Internet Governance Through Site Shutdowns:
The Impact of Shutting Down Two Major Commercial Sex Advertising Sites

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In the two weeks after the U.S. Congress passed a package of anti-sex trafficking bills on March 21, 2018, two of the largest online commercial sex advertising platforms ceased operation. On March 23, Craigslist voluntarily removed their personals section, which had been dominated by advertisements for commercial sex. And on April 6, the Department of Justice seized Backpage.com, the largest online platform for commercial sex advertisements. Our research examines the impact of these shutdowns on a variety of important outcome variables, notably prostitution arrests and violence against women–variables the prior literature has shown were impacted by the introduction of commercial sex advertising platforms.

We employ a generalized difference-in-difference model by exploiting cross-city variation in the pre-shutdown usage of the two shuttered sites. We find no causal effect of the shutdowns on any of the outcome variables we measure. Further analysis suggests that these null results are likely due to the fluidity of online markets. Our data show that the majority of advertisers and users of Backpage and Craigslist's personals quickly moved to other (often off-shore) commercial sex advertising portals.

Our results highlight the challenges governments face in reducing online sex trafficking, as the market for commercial sex advertising appears agile enough to quickly disperse to offshore sites after a few popular domestic sites are shut down. Our results have general implications for the governance of other illegal activities online.

Key words: Regulation, Sex trafficking, Internet governance, Commercial sex markets

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1. Introduction and Background

Digital exchanges have transformed many markets, including markets for commercial sex. Although prostitution is illegal in all but 10 counties in the United States, U.S.-based commercial sex exchanges have flourished online for more than a decade. In the early 2000s, Craigslist introduced an "erotic services section" (ERS) to its already popular site, and this quickly became a prominent source of commercial sex advertisements, shifting commercial sex markets online (Chan et al. (2019b), Farley (2006)). Similarly, Backpage.com was launched in 2004, and by 2015 was the dominant online commercial sex marketplace with $135 million in annual revenue (Savage and Williams 2018). In addition to advertisements for consensual commercial sex, both sites hosted advertisements

facilitating sex trafficking—which U.S. law defines as commercial sex "induced by force, fraud, or coercion, or in which the person induced to perform such act has not attained 18 years of age."[1]

The National Center for Missing and Exploited Children (NCMEC) reported their CyberTipline experienced an 846% increase in public reports of suspected child sex trafficking between 2010 to 2014, and that three quarters of these reports came from advertisements posted on Backpage.[2] A February 2017 Senate investigation, titled "Backpage.com's Knowing Facilitation of Online Sex Trafficking," found the site was a "hub" for "human trafficking, especially the trafficking of minors" by knowingly hosting ads for sex trafficked victims—including submitted ads describing the individual being sold for sex as "teen," "lolita," "young," "amber alert," "little girl," and "school girl."[3]

The owners of Backpage and other similar platforms were shielded from legal liability for hosting such advertisements by Section 230 of Communications Decency Act (CDA).[4] That legal immunity was removed on April 11, 2018 by the enactment of the "Allow States and Victims to Fight Online Sex Trafficking Act" (FOSTA), which eliminated the Section 230 liability shield for sites that "knowingly assist, facilitate, or support sex trafficking." This coincided with two major shocks to the market for commercial sex advertising. First, on March 23, Craigslist took down the "Personals" section for its U.S. site, a section which, following the removal of their ERS section in 2009 (Stone 2009), contained nearly all of Craiglist's commercial sex advertisements. Then, on April 6, the United States Department of Justice seized Backpage following a money laundering indictment against the site's founders (Wilber and Clozel 2018). Thus, the two largest commercial sex advertising platforms in the U.S. ceased operation within 2 weeks of each other. However, other websites, especially those outside of U.S. jurisdiction, continued to host advertisements for sexual services and prostitution.

The passage of FOSTA, and the shutdown of these two platforms, stirred controversy. FOSTA gave sex trafficking survivors, and state and local law enforcement agencies, new legal tools to investigate websites acting "in reckless disregard" to sex trafficking occurring on their sites. Supporters of the law argued that these legal tools would aid victims and incentivize commercial sex advertising platforms to better monitor and deter sex trafficking on their sites. Opponents argued that shuttering online markets for commercial sex would push the sex trade back onto the streets, subjecting commercial sex providers to increased violence as a result of not being able to screen their clients.[5] This led Senators Warren, Sanders, and Wyden to co-sponsor a December 2019 bill, reintroduced in March

[1] https://www.law.cornell.edu/uscode/text/22/7102

[2] https://www.govinfo.gov/content/pkg/CHRG-115hhrg28792/html/CHRG-115hhrg28792.htm

[3] https://www.courthousenews.com/wp-content/uploads/2017/02/Backpage-Report.pdf

[4] https://www.law.cornell.edu/uscode/text/47/230

[5] https://www.aclu.org/blog/criminal-law-reform/congress-proposes-fight-online-trafficking-harming-sex-workers, https://www.eff.org/deeplinks/2019/12/observing-international-day-end-violence-against-sex-workers-means-looking-closely

2022, requiring the Department of Health and Human Services to study "the health and safety impacts on sex workers from SESTA/FOSTA and the loss of access to certain online platforms."[6]

Our study addresses one of this bill's core questions by evaluating whether the near-simultaneous shutdown of Backpage.com and Craigslist's personal ads section affected a variety of important outcome variables, including prostitution arrests and violence against women–variables the prior literature found were directly impacted by the introduction of commercial sex platforms. Our analysis exploits variation in the pre-shutdown usage of these sites across U.S. cities as a measure of city-level treatment intensity, asking whether key outcome variables changed more after the shutdowns in cities that made heavier use of these sites than in other cities. We find no causal effect of the shutdowns on any of the outcome variables we measure. Further analysis suggests that the most likely explanation for these precisely estimated null results is the relatively low search and switching costs associated with Internet marketplaces (Goldfarb (2006b), Chen and Hitt (2002), Smith et al. (2001)). Consistent with this we find that within six months of the shutdown of Backpage and Craigslist there was a significant increase in the number of advertisements on and visitors to the remaining online commercial sex advertising platforms, an increase nearly commensurate with the reduction in advertisements and visitors to the shutdown sites. Moreover, roughly 75% of this increase was to offshore sites not subject to U.S. legal jurisdiction.

Our study highlights the difficulty in combating illegal activity and its consequences by shutting down a few high-profile online platforms. Combining our results with empirical research on other illegal activities online, we provide advice to policymakers interested in reducing sex trafficking facilitated by online platforms.

2. Related literature

Prior work on online markets for commercial sex is sparse. In the Information Systems literature, Chan and Ghose (2014) find that the entry of Craigslist into U.S. cities caused a 15.9% increase in HIV infections, an increase more related to unpaid sex than to commercial sex. Likewise, Chan et al. (2019b) find that Craiglist's entry caused a 17.6% increase in prostitution cases, including an increase in commercial sex providers offering niche sexual services. In the economics literature, Cunningham et al. (2021) find that Craigslist's entry into commercial sex advertisements caused a 10-17% decrease in female homicides, likely by providing more mechanisms to screen dangerous clients and the movement of dangerous street prostitution indoors.

Rather than studying the introduction of online marketplaces as these prior studies do, we study their removal following a policy intervention. As such, our study is informed by research on regulating

[6] https://www.wyden.senate.gov/news/press-releases/wyden-introduces-safe-sex-workers-study-act, https://www.congress.gov/bill/117th-congress/senate-bill/3758

commercial sex. Cho et al. (2013) empirically analyze data from 150 countries and find that legalizing commercial sex is associated with an increase in sex trafficking. Similarly, Jakobsson and Kotsadam (2013) find that criminalizing commercial sex reduces overall sex trafficking. These studies imply that enforcement against online commercial sex markets could reduce illegal behaviors even if they do not meaningfully reduce the ability to transact in such markets. In contrast, Cunningham and Shah (2018) find that decriminalization in Rhode Island increased the size of the commercial sex market but reduced reported incidences of rape and female gonorrhea by 30% and 40% respectively. Thus, actions that reduce the volume of prostitution could increase negative outcomes if they affect the safety of commercial sex providers. As such, our research is related not just to the regulation of commercial sex, but also to combating crimes against women (e.g., Miller and Segal (2019)). Based on this prior literature, we study the outcome variables of prostitution cases, sex trafficking cases, and violence toward women, given that prior studies show that the introduction of online markets impacted or could impact these outcomes.

In our research, however, illegal prostitution is facilitated through Internet portals, and enforcement involves the removal of the two largest commercial sex advertising sites online. Thus, our research is also informed by prior work on Internet governance of illegal activities, notably online piracy. Much of the literature on combating online piracy has focused on supply-side enforcement (Dey et al. 2018)—disrupting access to piracy sites in an effort to reduce piracy and increase legal consumption of content. Some of this research suggests that shuttering or blocking a few large piracy websites shifts pirated supply and demand to other sites, with little effect on legal consumption (Poort et al. (2014), Aguiar et al. (2018)). Other studies find that when a sufficiently large number of sites or suppliers are disrupted, some consumers reduce piracy and increase legal consumption (Danaher and Smith (2014), Danaher et al. (2020), Reimers (2016)).

Results from combating online drug trafficking are similarly divided by the intensity of enforcement. Soska and Christin (2015) and Décary-Hétu and Giommoni (2017) find that enforcement against one or two large drug trafficking sites (e.g., The Silk Road) merely dispersed traffic to remaining online drug marketplaces. However, Chan et al. (2019a) find that enforcement against a number of online drug suppliers (without shutting down the sites) led to a decrease in illegal transactions without a compensating increase in traffic to other sites.

Thus, it is unclear whether closing two major commercial sex platforms will impact the outcomes we study. Because online marketplaces eliminate the geographic constraints from offline transactions (Brynjolfsson et al. 2003), after closing an online advertising portal buyers and sellers may quickly migrate to other portals. This, however, requires that buyers and sellers incur search and switching costs, including the cost of finding other sites, and learning to trust and use them. On one hand, search (Goldfarb and Tucker 2019) and switching costs (Chen and Hitt 2002) are often lower online

than offline. Goldfarb (2006a) shows consumers can easily switch to competitor websites when their targeted website experiences a denial of service attack. However, Smith and Brynjolfsson (2001) and Hann and Terwiesch (2003) show that online search costs can be substantial. Ghose et al. (2012) establishes the importance of search costs on user behavior, especially on mobile devices. In short, even though online markets can rebound after a shutdown as participants migrate to other sites, that rebound may be incomplete if search/switching costs cause some participants to leave the market.

Enforcement may also induce changes in outcomes via signaling. The "broken windows theory" of crime (Wilson and Kelling 1982) explains how visible signs of unpoliced crime send signals to criminals that enforcement is low, effectively encouraging crime. Conversely, actions against a few prominent websites that facilitate illegal activity could signal that enforcement is strong, deterring illegal activity at other sites. Thus, it is unclear how the shutdowns of Backpage and Craigslist's personals section may affect outcomes in the marketplace for commercial sex.

The primary contribution of our research is to evaluate this question by analyzing the effect of the near-simultaneous shutdowns of Backpage.com and Craigslists' personals section on outcome variables that prior academic studies and ongoing policy debates suggest are relevant. We examine prostitution arrests and sex trafficking, as the prior literature relates these outcomes to commercial sex. We examine female homicides and rapes given the connection in the literature between these outcomes and the introduction of commercial sex advertising platforms. Our study also contributes generally to understanding whether supply-side enforcement online can deter crime, a subject of interest in many settings, including drug trafficking and copyright enforcement. Together, we provide evidence as to which theoretical frameworks appear to govern participants in illegal Internet markets, with general implications for Internet governance.

3. Data

Our study combines five distinct data sets.

3.1. Sex Trafficking, FBI Uniform Crime Report and US Census Data

Our first data set is from the FBI's Uniform Crime Report (UCR). From UCR reports of "Persons Arrested" we obtained monthly prostitution arrests in 2017 and 2018. While changes in prostitution arrests may reflect changes in prostitution volume or the intensity or efficacy of law enforcement, they provide the best available metric of whether disrupting the online commercial sex market impacted prostitution activity. From "Offenses Known to Law Enforcement" we obtained monthly rape incidents in 2017 and 2018.[7] Rape cases are known to be under-reported, but as long as under-reporting is consistent across periods, or unrelated to treatment intensity, it will be differenced out

[7] Our data exclude the state of Florida because law enforcement agencies in Florida only report data semi-annually. Note that rape cases in the UCR data cannot be separated by gender and thus include both males and females.

in our empirical model. Finally, from the "Supplemental Homicide Report" we obtained monthly female homicides reported during 2017 and 2018. Our data on rapes and homicides do not allow us to measure specifically the number of these crimes related to prostitution. As such, and consistent with Cunningham et al. (2021), we measure the effect of the shutdowns on total female homicides and rape cases. Because each law enforcement agency is attached to a jurisdiction, we can map reports to the city in which they occurred. UCR data provide a useful measure for our analysis given its broad coverage (95%) across the U.S. population and its regular use in the academic literature studying commercial sex markets, notably Chan et al. (2019b) and Cunningham et al. (2021).

Our second data set comes from IBM's Traffik Analysis Hub (TA Hub). These data contain sex trafficking cases aggregated from NGO incident reports, law enforcement data, and news reports for 3,142 U.S. counties on a monthly basis from Jan 2017 to Dec 2018. TA Hub applies a de-duplication process to eliminate double counting.[8] We note that this is a limited and imperfect measure of actual incidents of sex trafficking, and discuss the implications of this limitation below.

Our third dataset is from the 2018 U.S. Census and includes the population of each Craigslist and Backpage city in our data. We use population to scale our data because our outcome variables should increase with population. Following Cunningham et al. (2021), we drop cities with population less than 100,000 so that outcomes in low population cities don't skew our results. This leaves us with 153 cities. The first four rows of Table 1 present summary statistics at the city-month level for the first three data sets, including sex trafficking cases, prostitution arrests, female homicides and rapes per 100,000 population.

Table 1 **Summary statistics**

Variable	N	Mean	Std. Dev.	Min	Max
Observations on city-monthly level					
Sex trafficking cases (per 100,000)	3,358	0.104	0.677	0	17.057
Prostitution arrests (per 100,000)	3,528	2.776	9.225	0	140.314
Female homicide (per 100,000)	3,312	0.290	0.518	0	5.895
Rape (per 100,000)	3,408	10.658	12.460	0	131.446
Ads volume on other sites (per 100,000)	1,800	1,775.957	2,791.42	0.406	24,341.02
Observations on city level					
Treatment intensity	153	5,358.994	4,216.676	430.708	37,273.857

[8] Note that the FBI led a national sex trafficking sting in October 2017 (`https://www.fbi.gov/news/stories/operation-cross-country-xi`), causing the number of cases in our data to spike 6.63 standard deviations above their average during this month. Because this spike was random with respect to our experiment, and unrelated to actual changes in sex trafficking caused by our experiment, we drop this month from our data. Our results remain the same in sign and significance if it is included, but with larger standard errors.

3.2. Website Visitation and Advertisement Volume Data

Our fourth data set comes from Alexa.com and reports weekly page view percentages from U.S. Internet users to the top ten commercial sex advertising sites between 2017 and 2018.[9] Page view percentage measures the fraction of all page views to a website. We use this to proxy for the number of visits to each site. Figure 1 presents traffic trends to these websites from 2017 to 2018, along with the Herfindahl-Hirschman Index (HHI) of the market.[10] Note that visits to Craigslist personal ads are not included in Figure 1 as they cannot be separated from overall visits to Craigslist's primary domain/page. A list of these sites can be found in Table A1.

Our fifth dataset is provided by Marinus Analytics (a company founded in 2014 out of Carnegie Mellon Robotics) and contains city- and daily-level data on the volume of commercial sex ads placed on 14 of the most popular platforms (including Backpage and Craigslist personal ads) from November 2017 to October 2018. Figure 2 shows the volume of ads aggregated at the national and weekly level as well as the HHI of these 14 websites. Both graphs show similar results: listings at and visits to online commercial sex advertisements immediately dropped after the shutdowns, but quickly recovered to near pre-shutdown levels within 6 months. However, while Backpage and Craigslist were dominant before their shutdowns, after April 2018 the diminishing HHI indicates market diffusion. Row five of Table 1 shows the average monthly advertisements on sites other than Backpage and Craigslist.

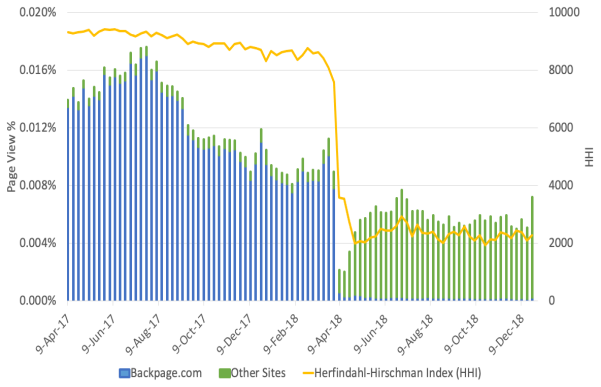

Figure 1 **Historic page view % of top 10 escort websites**

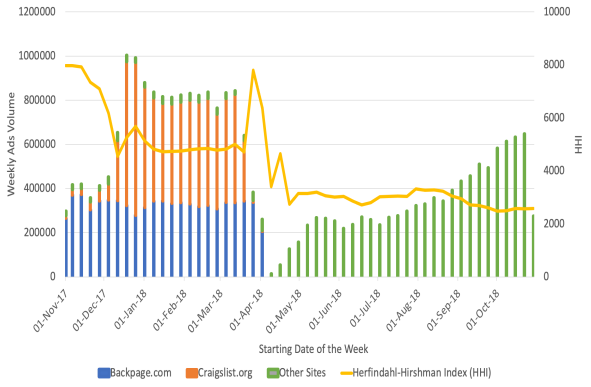

Figure 2 **Total ads volume by site, from Nov. 2017 to Oct. 2018**

[9] Alexa limits output from a query to ten sites.

[10] HHI measures market concentration ranging from 1 (least concentrated) to 10,000 (most concentrated). HHI is the sum of the squares of the percentage of traffic from each single site, namely, HHI $= \sum_{i=1}^{10} S_i^2$, where S_i is the percentage of traffic from site i.

3.3. Treatment Intensity

Although all Craigslist and Backpage cities were affected by the shutdowns, the "bite" of this treatment was stronger in cities where Backpage and Craigslist were used more heavily to advertise commercial sex, and weaker where these sites were used less frequently. For each city, we aggregate the volume of commercial sex advertisements on Backpage and Craigslist from November 1, 2017 to the shutdown date. We compute the average (pre-shutdown) monthly sex advertisements on these two sites and divide this average by city population (in 100,000's). This represents a relative measure of the pre-shutdown city-level usage of Backpage and Craigslist for commercial sex advertisements, which we use as the "treatment intensity" of the shutdowns in each city.

Figure 8 shows the distribution of treatment intensity across cities in our data. The distribution is right-skewed, with outliers indicating cities with high numbers of sex advertisements per capita. Most outlier cities are transit, convention, or tourist destinations (e.g., Atlanta or Myrtle Beach), which is consistent with prior research showing that such cities exhibit a larger supply of commercial sex providers (Cunningham and Kendall 2011). Importantly, even excluding these outlier cities, there is significant cross-city variation in the treatment intensity of the shutdowns, which is essential to our identification strategy. Row six of Table 1 presents descriptive statistics for this variable.

4. Models and Results

Our identification strategy is based on the generalized form of the difference-in-difference model with a continuous treatment intensity variable, where treatment intensity represents the "bite" of the treatment on each city. If the shutdowns had any effect on a particular outcome variable, we would expect to see larger changes in cities where the treatment had a stronger bite (a higher concentration of advertisements). See Card (1992) for the seminal publication of this methodology, and Danaher et al. (2020) for a recent implementation in the Information Systems literature.

The value of this approach is that, although cities clearly self-select into treatment intensity, our identification strategy relies only on treatment intensity being exogenous with respect to changes in the outcome variables over time. We can partially test this assumption by asking whether treatment intensity is a moderator of the time trend during the pre-period, which is the generalized form of the parallel trends assumption.

It also makes sense to test whether our model can detect a causal effect when one clearly exists. Fortunately, we have such a scenario. Figure 2 shows that the remaining online advertising portals for commercial sex began to experience rapid growth after Backpage and Craigslist's personals were shut down. This, coupled with the rapid growth in visits to these sites observed in Figure 1, makes it straightforward to conclude that this shift to other sites was causal. As such, we first estimate our generalized difference-in-differences model with commercial sex advertisements on other sites

(besides Craigslist and Backpage) as the dependent variable, to see if it produces results consistent with a causal increase in ads on other sites.

4.1. Effect of Shutdown on Advertisements on Alternate Sites

To ask whether the shutdown of Craigslist and Backpage caused the increase in commercial sex advertisements on other sites, we estimate the following model:

$$y_{it} = \beta_0 + \beta_1^t month_t + \beta_2^t \, Treatment \; Intensity_i * month_t + \mu_i + \varepsilon_{it} \tag{1}$$

where y_{it} is the volume of commercial sex advertisements on other websites per 100,000 population in city i during month t, $month_t$ is a vector of dummy variables for each month (omitting March 2018, the month before the intervention), $Treatment\ Intensity_i$ is the pre-shutdown treatment intensity in city i, μ_i is a city-level fixed effect and ε_{it} is the error term. We divide our dependent variable (i.e. alternate site ads) by population in 100,000 to control for city size and so that the scale is commensurate with our measure of treatment intensity, yielding easily interpretable coefficients.

In (1), β_1^t represents the predicted change in commercial sex advertisements from other sites in a hypothetical city where the pre-shutdown penetration rate of Backpage and Craigslist (i.e. treatment intensity) was zero. This approximates a "control" city. The coefficient of interest, β_2^t, indicates the degree to which treatment intensity moderates the time trend of the outcome variable. Under the identifying assumption we would expect β_2^t to be 0 for all pre-shutdown months. After the shutdown, β_2^t indicates the causal effect of the shutdown on the outcome variable.[11]

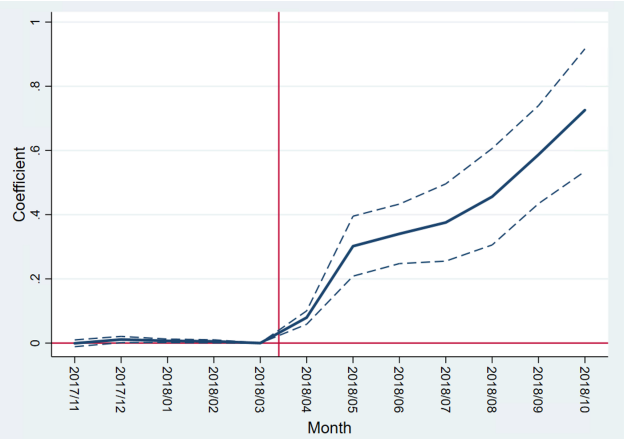

Figure 3 **Moderation of trend for ads volume on other websites per 100,000 population by treatment intensity in 153 cities**

[11] Consistent with the prior literature, we estimate fixed-effect OLS on our outcome variables per 100,000 population. However, because our outcomes represent right-skewed count data, we have also estimated all models using both log-log and fixed effects Poisson regressions. These results yield the same sign and significance as those reported in the paper.

Figure 3 plots the β_2^t coefficients, and their 95% confidence intervals, obtained from the fixed effect OLS estimation in (1). Before the shutdown, β_2^t for each month is small and hovers around zero (a Wald test fails to reject the null hypothesis that these coefficients are jointly zero at alpha = 0.05), indicating that treatment intensity is not correlated with the time trend of commercial sex advertising volume on other sites before the shutdown. Immediately following the shutdown, however, β_2^t becomes positive and statistically significant, with magnitude growing over time. This indicates the growth of advertisements on alternate sites was larger in cities more affected by the shutdowns.[12] This exercise highlights that our difference-in-difference model can identify and estimate a causal effect where we have a strong prior belief that one exists. Moreover, the magnitude of the estimate is reasonable—the estimate of 0.73 in month seven after the shutdown suggests that, controlling for population, shutting down 1 Backpage/Craigslist advertisement in a city caused an increase of 0.73 in the number of advertisements in that city on alternate sites. This is consistent with the raw data in Figure 2, where advertisements on other sites grew to well over 50% of pre-shutdown ads on Backpage of Craigslist.

Based on these results, we next estimate the average effect of the shutdown on outcomes during the post-shutdown period using the following model:

$$y_{it} = \beta_0 + \beta_2 * \textit{Treatment Intensity}_i * After_t + \mu_i + \lambda_t + \varepsilon_{it} \tag{2}$$

where the variables are defined as in (1) but now $After_t$ is a dummy variable set to one if the month observed is after the shutdowns and λ_t represents the vector of month fixed effects. Under the identifying assumption, β_2 represents the average causal effect of the shutdowns during the entire post-shutdown period. Our estimate of β_2 when the outcome is commercial sex advertisements posted on alternate sites (per 100,000 population) is reported in column (1) of Table 2. We report robust standard errors clustered at the city level in parentheses. We observe that the coefficient of interest is 0.405 and is statistically significant ($p<0.001$). This indicates that shutting down 1 Backpage/Craigslist advertisement per 100,000 population resulted in an increase of 0.405 advertisements (per 100,000 population) on remaining sites after the shutdowns, though we know from Figure 3 that this was smaller at first and larger by the end of the post-period.

Because FOSTA-SESTA is a U.S. law, it can't be enforced against websites outside of U.S. jurisdiction, meaning that new U.S. regulations may cause illegal activity to shift to offshore sites. To test for this, we split commercial sex advertising websites into offshore sites and domestic sites, and report estimates for the causal increases in each category in columns (2) and (3) of Table 2. Our estimates indicate that each advertisement eliminated from Backpage and Craiglist caused an

[12] The OLS estimates underlying Figure 3 are shown in column 1 of Table A2 of the Appendix.

increase in advertisements on offshore sites 3.5 times as large as the increase to domestic sites. Thus, a key element of the rebound appears to be that the commercial sex market moved predominantly to offshore sites that could still reach U.S.-based customers and advertise U.S.-based providers.

Given that the market for commercial sex ads rebounded after the shutdowns, but may not have fully recovered, we next estimate models (1) and (2) for a variety of outcome variables that have been theoretically linked as potential outcomes of these shutdowns.

Table 2 OLS regression results for (3)

Dependent variable (per 100,000)	**Ads volume on other sites**	**Offshore sites**	**US sites**	**Sex trafficking**	**Prostitution arrests**	**Female homicide**	**Rape**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$rate_i * \text{After}_t$	4.049e-01***	3.556e-01***	1.072e-01***	-4.276e-07	5.326e-05	-8.409e-07	3.911e-05
	(5.605e-02)	(5.054e-02)	(1.351e-02)	(2.680e-06)	(3.210e-05)	(5.787e-06)	(5.508e-05)
Constant	4.864e+02**	5.074e+02**	-1.179e+02**	1.050e-01***	2.669e+00***	2.917e-01***	1.058e+01***
	(1.785e+02)	(1.677e+02)	(4.362e+01)	(5.579e-03)	(6.410e-02)	(1.099e-02)	(1.037e-01)
Observations	1800	1728	1232	3358	3528	3312	3408
Clusters	153	153	136	146	147	138	142
R-square	0.813	0.796	0.749	0.208	0.899	0.391	0.946
Back-of-Envelope Calculation	187.492%	161.637%	338.292%	-2.518%	5.733%	-1.603%	2.005%

Robust standard errors clustered at city level in parentheses: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

4.2. The Effect of the Shutdowns on Sex Trafficking and Prostitution Arrests

We first estimate model (1) when the outcome variable is sex trafficking cases per 100,000 population, plotting the resulting β_2^t coefficients and their 95% confidence intervals in Figure 4. We do the same for prostitution arrests and plot these estimates in Figure 5.

For sex trafficking cases, β_2^t hovers around the same level for almost the entire pre-period. The negative values appear to result from a somewhat high number of sex trafficking cases in high treatment intensity cities in March 2018 (our left-out "baseline" month). However, in all other pre-period months there is little correlation between treatment intensity and monthly changes in sex trafficking cases, and even the difference between March 2018 and the rest of the pre-period is statistically indistinguishable from 0 (Wald test p-value is 0.95). Likewise in Figure 5 we observe that treatment intensity is not correlated with pre-period monthly changes in prostitution arrests (p=0.99). Thus, in the absence of the shutdowns, we might expect treatment intensity to continue to be uncorrelated with these trends.[13] In the post-shutdown period, we see no clear evidence of a relationship between treatment intensity and changes in the number of reported sex trafficking cases or prostitution arrests. The time period after the shutdowns appears similar to that beforehand for both of these variables, suggesting little or no effect of the shutdowns on these outcomes.

The average effect of the shutdowns in the post-period is found in columns (4) (sex trafficking) and (5) (prostitution arrests) of Table 2. Both average treatment effects are nearly zero and statistically

[13] The OLS estimates underlying these figures can be found in column 2 and 3 of Table A2 in the Appendix.

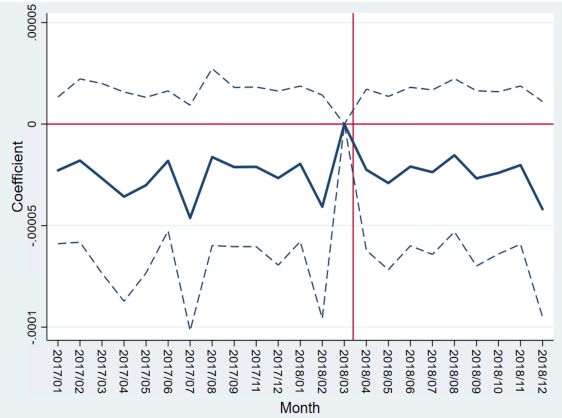

Figure 4 **Moderation of trend for number of sex trafficking cases per 100,000 population by treatment intensity in 146 cities**

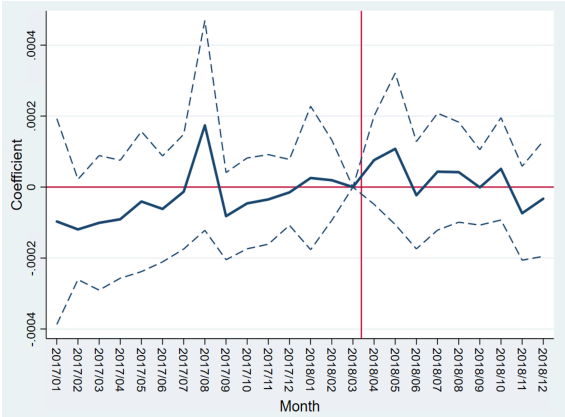

Figure 5 **Moderation of trend for number of prostitution arrests per 100,000 population by treatment intensity in 147 cities**

insignificant with alpha=0.05, although the prostitution arrests variable has a p-value of 0.099. Even if the effect of the shutdowns on these outcomes is statistically insignificant, the implied changes might be practically significant. To explore whether the point estimates for our coefficients represent practically significant changes, we take the coefficient estimates and apply them to the observed data, estimating the counterfactual prostitution arrests and sex trafficking cases in each city if there had been no shutdowns (i.e., if treatment intensity were 0). We then compute the causal change for each city, sum these up, and divide by the total number of counterfactual cases/arrests to impute the implied percent change in each outcome caused by the shutdowns. We report these back-of-the-envelope calculations in the last row of Table 2, where the point estimates from our models indicate a 2.5% decrease in sex trafficking cases and a 5.7% increase in prostitution arrests.

4.3. The Effect of the Shutdowns on Female Homicides and Rape Cases

Although the shutdowns had little impact on sex trafficking cases or prostitution arrests, it is still possible that they made women engaged in commercial sex less safe. Estimates of β_2^t from model (1) for female homicides and rapes (per 100,000 population) are pictured in Figure 6 and Figure 7 respectively, with the underlying estimates in columns 4 and 5 in Table A2. Again, we find no statistically significant relationship between treatment intensity and the time trend of these variables during the pre-period, with β_2^t hovering around zero in both graphs. This trend largely continues in the post-period, with little evidence that the shutdown of Backpage or Craiglist caused a change in female homicides or rapes. We also estimate model (2) for these two variables, reporting our results in columns (6) - (7) of Table 2. These coefficients are both close to zero and statistically insignificant, and the estimated effect on these outcomes is also small in magnitude (-1.6% for homicides and 2% for rapes). Thus, we find no evidence of an impact of the shutdowns on the safety of women as measured by female homicides or rapes.

Figure 6 **Moderation of trend for female homicides per 100,000 population by treatment intensity in 138 cities**

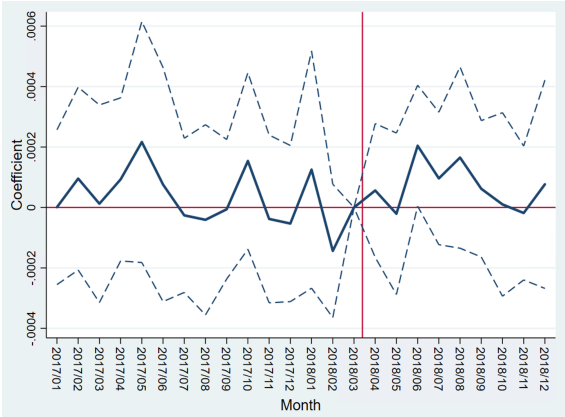

Figure 7 **Moderation of trend rape incidents per 100,000 population by treatment intensity in 142 cities**

Our results in Table 2 are robust to a variety of specifications; these include changing the population cutoff for including cities in our analysis, dropping cities with low incidence of the dependent variable, and right censoring treatment intensity so that outlier cities with an extremely high number of sex ads per 100,000 population do not disproportionately impact our results. In all cases the difference-in-difference estimators remain close to zero and statistically insignificant, at times swapping signs due to the proximity to zero. These robustness tests can be found in Table A3, Table A4, Table A5, and Table A6 of the Appendix.

5. Discussion and Conclusion

Our study uses cross-city variation in the pre-shutdown usage of two dominant commercial sex advertising platforms to study the effect of their shutdown on a variety of policy-relevant outcome variables. These variables include two outcomes the prior literature has shown were directly impacted by the introduction of commercial sex advertising platforms (prostitution arrests and violence against women), and an additional policy-relevant outcome variable (reports of sex trafficking incidents). We conclude that the shutdown of Backpage.com and Craigslist's personals section had no causal impact on any of the outcome variables we study.

Additional analyses suggest that these null-results arise because markets for commercial sex advertising were able to quickly move to other online platforms after the shutdown of Backpage and Craigslist's personals. Our generalized difference-in-differences model shows a causal relationship between the shutdown of Backpage and Craigslist "Personals" and an increase in listings on and traffic to other commercial sex advertising sites. There are two primary explanations for this. First, switching costs appear to be low for both advertisers and potential customers in this market. The ease of finding new sites is reflected in the fact that the Google Trends relative index for search terms

like "escort sites" or "Backpage alternative site" jumped from below 25% to 100% in late April 2018, and remained elevated after that,[14] and that nearly all of the remaining websites included in our data appear in the top twenty results returned by Google for these search terms. Once found, these sites were also easy to learn, as the alternate sites all provided similar functionality and look-and-feel to Craigslist and Backpage, displaying advertisements divided into sections by US cities.

A second reason for the ease with which online commercial sex advertising markets were able to relocate is that, absent the ability to block foreign sites, domestic laws targeting online behavior have limited effectiveness against offshore sites. While the FOSTA-SESTA law may deter sex trafficking within US jurisdiction by allowing individual survivors, and state and local prosecutors, to bring legal action against domestic sites that knowingly facilitate sex trafficking, it offers no such remedies against sites located outside the jurisdiction of US laws. This limitation is reflected in our data in two ways. First, our data show that over 75% of the causal increase in advertisements at remaining sites went to offshore commercial sex advertising sites. Second, several of the remaining commercial sex advertising sites in our data moved their registration from US-based to international addresses around the time of the shutdown of Backpage and Craigslist personals. For example, doublelist.com moved its registration from a US address to Paris in March 2018, and tryst.link moved from a US address to the UK in August 2018 (see Table A1). This likely explains why the common finding of deterring crime by targeting several prominent perpetrators of illegal activity (the so-called "broken windows theory") does not appear effective here. Any deterrent effect is limited when other sites engaged in the illegal activity can easily move outside of the jurisdiction of domestic laws.

As with any empirical analysis, our study has several limitations. First, each of our outcome variables come with limitations that constrain our ability to draw inferences, as described in section 3. We note that this is particularly true for available measures of the reported incidents of sex trafficking. Future research should seek to develop additional measures of this important outcome variable as a way to better understand how changes in the commercial sex advertising market impact the ease and prevalence of online sex trafficking. We also note that our results are limited to the nine months of post-shutdown activity in our data, but it seems unlikely that effects would begin to emerge after this period given that the compensating usage of alternative sex advertising sites still appears to be increasing at the end of our data time frame.

Our study has important implications for the ability to enforce US laws in the context of illegal online activity such as online sex trafficking. Our results imply that targeting a small number of prominent sex advertising portals is unlikely, by itself, to be effective in combating sex trafficking given the fluidity of online markets for illegal activity. Internet governance through site shutdowns is

[14] See Figure 9 in the Appendix.

generally difficult because traffic can easily move outside the jurisdiction of domestic laws. Combining this result with analogous findings in copyright enforcement, we suggest that ISP-level website blocking may be more suited to policing some types of illegal online activities, including online sex trafficking. First, it may be easier to coordinate the blocking of a large number of websites engaged in illegal activity than it is to shut down the same sites through enforcement. Second, website blocking can be applied to sites located offshore, allowing law enforcement to disrupt access to any site facilitating the illegal activities in question. In the context of digital piracy, Danaher et al. (2020) show that website blocking can be effective at shifting consumers from illegal piracy to legal media consumption. However, we cannot say from available evidence whether such disruptions would have similar effects in the context of sex trafficking.

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Appendix

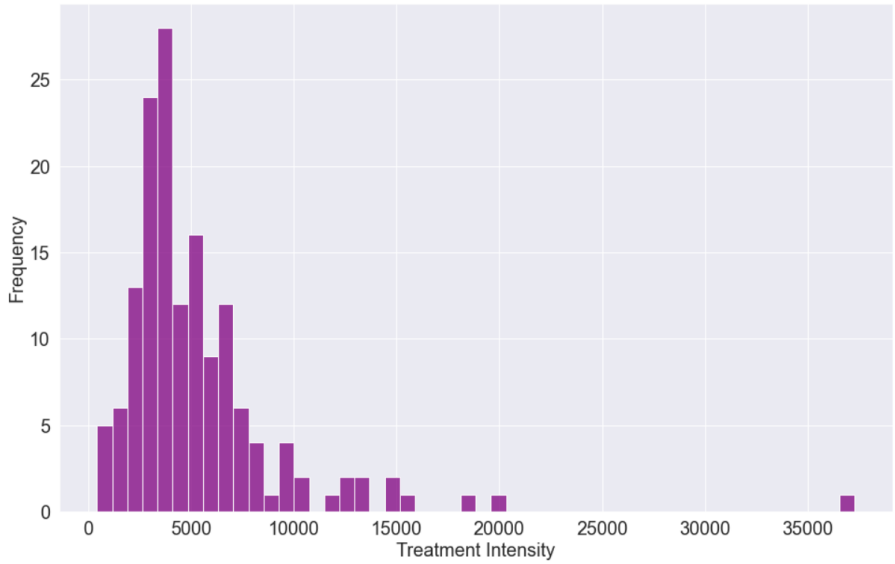

Figure 8 **Histogram of the treatment intensity**

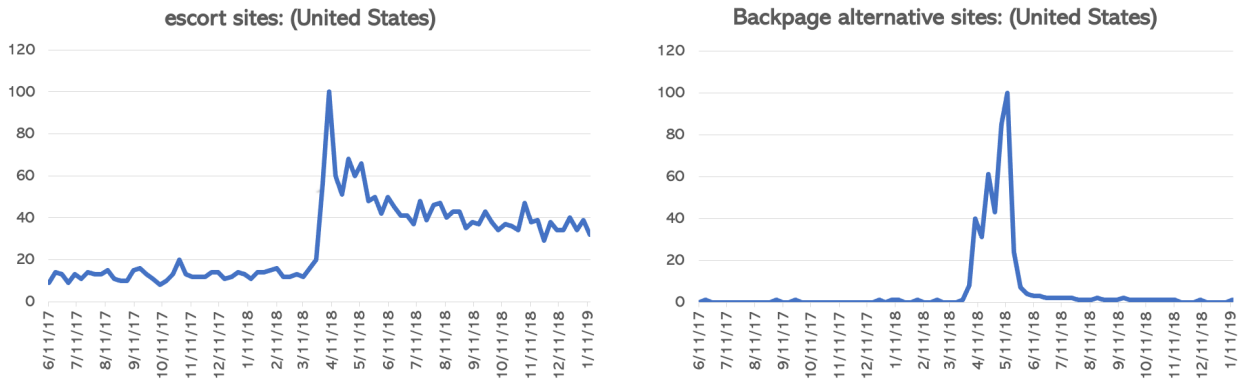

Figure 9 **Google Trends search index for "escort sites" and "Backpage alternative sites"**

Table A1 **Escort advertising sites domain change**

Website	Before	After
skipthegames.com	Europe	Europe
eros.com	Europe	Europe
adultsearch.com	US	US
bedpage.com	US	US
cityxguide.com	Hong Kong	Hong Kong
doublelist.com	US	Paris
listcrawler.com	Canada	Panama
tryst.link	US	UK (on 8/10/18)
slixa.com	UK	Paris (on 7/7/18)

Table A2 Regression results

Dependent variable (per 100,000)	**Ads volume on other sites**	**Sex trafficking**	**Prostitution arrests**	**Female homicide**	**Rape**
	(1)	(2)	(3)	(4)	(5)
$rate_i * month_{-15}$		-2.279e-05	-9.687e-05	1.604e-05	9.937e-07
		(1.827e-05)	(1.467e-04)	(2.514e-05)	(1.296e-04)
$rate_i * month_{-14}$		-1.794e-05	-1.193e-04	-4.678e-06	9.563e-05
		(2.035e-05)	(7.145e-05)	(1.745e-05)	(1.530e-04)
$rate_i * month_{-13}$		-2.676e-05	-1.007e-04	4.101e-06	1.240e-05
		(2.365e-05)	(9.593e-05)	(1.481e-05)	(1.654e-04)
$rate_i * month_{-12}$		-3.573e-05	-9.063e-05	4.674e-06	9.290e-05
		(2.607e-05)	(8.407e-05)	(1.933e-05)	(1.365e-04)
$rate_i * month_{-11}$		-3.005e-05	-4.068e-05	2.180e-05	2.166e-04
		(2.188e-05)	(9.995e-05)	(1.468e-05)	(2.017e-04)
$rate_i * month_{-10}$		-1.806e-05	-6.154e-05	6.665e-06	7.592e-05
		(1.742e-05)	(7.550e-05)	(1.726e-05)	(1.959e-04)
$rate_i * month_{-9}$		-4.622e-05	-1.288e-05	8.634e-06	-2.600e-05
		(2.811e-05)	(8.178e-05)	(2.193e-05)	(1.291e-04)
$rate_i * month_{-8}$		-1.624e-05	1.739e-04	4.098e-06	-4.078e-05
		(2.204e-05)	(1.496e-04)	(1.798e-05)	(1.592e-04)
$rate_i * month_{-7}$		-2.115e-05	-8.189e-05	-8.514e-06	-5.876e-06
		(1.983e-05)	(6.214e-05)	(2.728e-05)	(1.167e-04)
$rate_i * month_{-6}$			-4.587e-05	5.486e-05	1.540e-04
			(6.473e-05)	(5.021e-05)	(1.477e-04)
$rate_i * month_{-5}$	-6.309e-04	-2.105e-05	-3.472e-05	1.012e-05	-3.782e-05
	(5.306e-03)	(1.989e-05)	(6.389e-05)	(1.266e-05)	(1.404e-04)
$rate_i * month_{-4}$	1.115e-02*	-2.658e-05	-1.492e-05	1.108e-05	-5.319e-05
	(4.915e-03)	(2.168e-05)	(4.695e-05)	(1.898e-05)	(1.307e-04)
$rate_i * month_{-3}$	6.929e-03*	-1.956e-05	2.564e-05	-1.249e-05	1.254e-04
	(2.783e-03)	(1.937e-05)	(1.023e-04)	(1.759e-05)	(1.985e-04)
$rate_i * month_{-2}$	5.352e-03*	-4.069e-05	1.943e-05	4.284e-06	-1.437e-04
	(2.418e-03)	(2.783e-05)	(5.726e-05)	(1.794e-05)	(1.116e-04)
$rate_i * month_1$	7.947e-02***	-2.246e-05	7.603e-05	-1.057e-05	5.624e-05
	(1.056e-02)	(2.009e-05)	(6.304e-05)	(1.701e-05)	(1.119e-04)
$rate_i * month_2$	3.019e-01***	-2.902e-05	1.079e-04	3.157e-05	-2.069e-05
	(4.726e-02)	(2.159e-05)	(1.079e-04)	(2.302e-05)	(1.351e-04)
$rate_i * month_3$	3.404e-01***	-2.090e-05	-2.295e-05	1.980e-05	2.044e-04*
	(4.692e-02)	(1.975e-05)	(7.652e-05)	(1.713e-05)	(1.012e-04)
$rate_i * month_4$	3.757e-01***	-2.367e-05	4.342e-05	-3.060e-06	9.621e-05
	(6.090e-02)	(2.049e-05)	(8.332e-05)	(1.250e-05)	(1.109e-04)
$rate_i * month_5$	4.560e-01***	-1.534e-05	4.211e-05	1.244e-05	1.653e-04
	(7.607e-02)	(1.908e-05)	(7.139e-05)	(2.361e-05)	(1.518e-04)
$rate_i * month_6$	5.872e-01***	-2.672e-05	-8.583e-07	-1.117e-05	6.187e-05
	(7.728e-02)	(2.185e-05)	(5.374e-05)	(1.753e-05)	(1.141e-04)
$rate_i * month_7$	7.258e-01***	-2.401e-05	5.143e-05	1.934e-05	1.007e-05
	(9.661e-02)	(2.020e-05)	(7.297e-05)	(2.349e-05)	(1.536e-04)
$rate_i * month_8$		-2.019e-05	-7.369e-05	1.405e-05	-1.822e-05
		(1.972e-05)	(6.695e-05)	(1.606e-05)	(1.121e-04)
$rate_i * month_9$		-4.193e-05	-3.264e-05	-7.556e-06	7.676e-05
		(2.685e-05)	(8.224e-05)	(1.755e-05)	(1.743e-04)
Observations	1800	3358	3528	3312	3408
Clusters	153	146	147	138	142
R-square	0.863	0.212	0.899	0.399	0.946

Robust standard errors clustered at city level in parentheses: $^{***}p < 0.001$, $^{**}p < 0.1$, $^{*}p < 0.05$

Table A3 OLS regression results for (3)

Dependent variable	Sex trafficking per 100,000 population				
	(1)	(2)	(3)	(4)	(5)
$rate_i * \text{After}_t$	-4.276e-07	-7.658e-07	1.052e-07	-1.096e-06	4.627e-07
	(2.680e-06)	(5.621e-06)	(6.146e-06)	(4.557e-06)	(2.153e-06)
Constant	1.050e-01***	1.686e-01***	1.932e-01***	1.063e-01***	7.587e-02***
	(5.579e-03)	(1.183e-02)	(1.344e-02)	(9.102e-03)	(4.670e-03)
Cities with at least 1 sex trafficking case reported	No	Yes	Yes	No	No
Cities with at least 2 sex trafficking cases reported	No	No	Yes	No	No
Right-censoring the treatment intensity to 15,000	No	No	No	Yes	No
Including cities with population size greater than 50,000	No	No	No	No	Yes
Observations	3,358	2,093	1,771	3,358	5,152
Clusters	146	91	77	146	224
R-square	0.208	0.206	0.206	0.208	0.195
Back-of-Envelope Calculation	-2.518%	-3.730%	0.207%	-6.107%	0.877%

Robust standard errors clustered at city level in parentheses: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A4 OLS regression results for (3)

Dependent variable	Prostitution arrests per 100,000 population				
	(1)	(2)	(3)	(4)	(5)
$rate_i * \text{After}_t$	5.326e-05	6.483e-05	6.610e-05	8.019e-05	3.500e-05
	(3.210e-05)	(3.900e-05)	(3.940e-05)	(5.228e-05)	(2.232e-05)
Constant	2.669e+00***	3.056e+00***	3.103e+00***	2.622e+00***	1.993e+00***
	(6.410e-02)	(7.916e-02)	(8.047e-02)	(1.002e-01)	(4.664e-02)
Cities with at least 1 prostitution arrest reported	No	Yes	Yes	No	No
Cities with at least 2 prostitution arrests reported	No	No	Yes	No	No
Right-censoring the treatment intensity to 15,000	No	No	No	Yes	No
Including cities with population size greater than 50,000	No	No	No	No	Yes
Observations	3,528	3,072	3,024	3,528	5,400
Clusters	147	128	126	147	225
R-square	0.899	0.898	0.898	0.899	0.878
Back-of-Envelope Calculation	5.733%	6.983%	7.127%	8.575%	3.8%

Robust standard errors clustered at city level in parentheses: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A5 OLS regression results for (3)

Dependent variable	Female homicides per 100,000 population				
	(1)	(2)	(3)	(4)	(5)
$rate_i * \text{After}_t$	-8.409e-07	-8.241e-07	-9.851e-07	-9.820e-07	5.947e-06
	(5.787e-06)	(5.810e-06)	(5.916e-06)	(6.303e-06)	(6.423e-06)
Constant	2.917e-01***	2.938e-01***	3.018e-01***	2.920e-01***	2.675e-01***
	(1.099e-02)	(1.100e-02)	(1.124e-02)	(1.188e-02)	(1.256e-02)
Cities with at least 1 female homicide reported	No	Yes	Yes	No	No
Cities with at least 2 female homicides reported	No	No	Yes	No	No
Right-censoring the treatment intensity to 15,000	No	No	No	Yes	No
Including cities with population size greater than 50,000	No	No	No	No	Yes
Observations	3,312	3,288	3,192	3,312	5,040
Clusters	138	137	133	138	210
R-square	0.391	0.390	0.386	0.391	0.248
Back-of-Envelope Calculation	-1.603%	-1.550%	-1.882%	-1.880%	8.556%

Robust standard errors clustered at city level in parentheses: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table A6 OLS regression results for (3)

Dependent variable	Rape incidents per 100,000 population				
	(1)	(2)	(3)	(4)	(5)
$rate_i * \text{After}_t$	3.911e-05	3.911e-05	3.911e-05	4.222e-05	-4.088e-05
	(5.508e-05)	(5.508e-05)	(5.508e-05)	(5.981e-05)	(3.901e-05)
Constant	1.058e+01***	1.058e+01***	1.058e+01***	1.058e+01***	1.103e+01***
	(1.037e-01)	(1.037e-01)	(1.037e-01)	(1.117e-01)	(8.154e-02)
Cities with at least 1 rape incident reported	No	Yes	Yes	No	No
Cities with at least 2 rape incidents reported	No	No	Yes	No	No
Right-censoring the treatment intensity to 15,000	No	No	No	Yes	No
Including cities with population size greater than 50,000	No	No	No	No	Yes
Observations	3,408	3,408	3,408	3,408	5,492
Clusters	142	142	142	142	229
R-square	0.946	0.946	0.946	0.946	0.486
Back-of-Envelope Calculation	2.005%	2.005%	2.005%	2.149%	-2.081%

Robust standard errors clustered at city level in parentheses: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$