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

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Abstract

In the two weeks following the U.S. Senate's passing of the FOSTA-SESTA anti-sex trafficking bill, two of the largest online platforms for commercial sex advertisements in the United States were shut down. On March 23, 2018, Craigslist voluntarily removed their personals section, which had been dominated by advertisements for paid sex. And on April 6, the United States Department of Justice seized Backpage.com, the largest online platform for commercial sex advertisements and a "hub" for online sex trafficking. Our research examines the impact of these coinciding shutdowns on a variety of outcome variables including supply and demand for commercial sex online, prostitution arrests, the volume of sex trafficking reports, and violence against women in the form of female homicides and rapes.

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 that the shutdown of Backpage and Craigslist's personals did not cause a change in the number of reported sex trafficking cases or prostitution arrests; nor did these shutdowns bring about the harms that some had feared in the form of increased female homicides or rapes. We believe these non-results are likely caused by the fluidity of online markets. Our data show that the majority of advertisers and users of Backpage and Craigslist's personals quickly gravitated toward other remaining (often off-shore) commercial sex advertising portals.

Our results demonstrate the challenges that governments will face in their efforts to reduce online sex trafficking, as the market for online commercial sex advertising appears agile enough to quickly disperse and relocate after the two most popular sites are shut down. Our results have general implications for Internet governance with respect to illegal online activities.

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Keywords: Regulation, Sex trafficking, Internet governance, Commercial sex markets

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1. Introduction

Digital exchanges have transformed many marketplaces, including those that facilitate commercial sex. Although prostitution is illegal in all but 10 counties in the United States, US-based commercial sex exchanges have flourished online for more than a decade. Craigslist, one of the largest classified advertising websites online, introduced an "erotic service section" (ERS) in the mid 2000's. The website Backpage.com was launched in 2004, and by 2015 it was the dominant online commercial sex marketplace with annual revenue of $135 million in 2014 [1].

In addition to hosting advertisements posted by providers of consensual commercial sex, Backpage also hosted advertisements facilitating human sex trafficking—which U.S. law defines as commercial sex that "is induced by force, fraud, or coercion, or in which the person induced to perform such act has not attained 18 years of age" [2]. In August 2011, the National Association of Attorneys General called Backpage a "hub" for "human trafficking, especially the trafficking of minors" [3]. In 2015, the National Center for Missing and Exploited Children (NCMEC) reported that their CyberTipline experienced a 846 percent increase in reports of suspected child sex trafficking between 2010 to 2015, and that nearly three quarters of all the child sex trafficking reports submitted NCMEC's CyberTipline by the public related to Backpage ads [4]. A 2017 Senate investigation committee concluded that Backpage knowingly facilitated sex trafficking on its site, including the trafficking of minor victims [5].

Though underage prostitution and sex trafficking are illegal, the owners of Backpage and other similar platforms were protected by Section 230 of Communications Decency Act (CDA), which shielded them from liability for content posted to their site by others [6]. That legal immunity was removed in April 2018, when two significant events shocked the market for commercial sex advertising. First Craigslist took down its "Personals" section for its U.S. site on March 23rd, 2018, in response to Senate's passing of two anti-sex trafficking bills - the Allow States and Victims to Fight Online Sex Trafficking Act (FOSTA) and Stop Enabling Sex Traffickers Act (SESTA), which eliminated the section 230 liability shield for any sites that "knowingly assist, facilitate, or support sex trafficking." This "Personals" section has been dominated by commercial sex advertisements and, after it was removed, such advertisements were no longer posted on Craigslist. Two weeks later, on April 6th, 2018, the United States Department of Justice seized Backpage, following a money laundering indictment against the site's founders [7, 8]. FOSTA-SESTA became law 5 days after the seizure of Backpage, and thus the two largest sites in the US for online advertising of commercial sex were no longer offering such services.

The passage of FOSTA-SESTA stirred controversy. Advocates argued that FOSTA-SESTA was neces-

sary to increase the incentives for online commercial sex platforms to remove advertisements pertaining to potential trafficking victims. Those opposed to the bill argued that shuttering online markets for "consensual" commercial sex would push the sex trade underground or onto the street, and subject commercial sex providers to increased violence as a result of not being able to screen their clients [1][2][3]. Based on these concerns, several Senators introduced a bill in December 2019 that would require 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." [9]

Our study empirically evaluates the effect of the two FOSTA-SESTA-related site shutdowns on a variety of outcomes. Specifically, our research asks whether the April 2018 shutdown of Backpage and Craigslist's personal ads section affected the volume of human sex trafficking reports, prostitution arrests, the online supply and demand for commercial sex, or violence against commercial sex providers in the form of female homicides and rape incidents.

While Backpage and Craigslist's personal ads section were shut down everywhere, we exploit variation in the pre-shutdown usage of these sites across US cities as a measure of the intensity of the treatment on each city, asking whether outcomes changed more after the shutdowns in cities that made heavier use of these sites than they did in cities that made lighter use. We implement a generalized version of the difference-in-differences model with a continuous treatment variable, introduced by Card (1984) [10] in his seminal paper on the minimum wage, and frequently employed in policy evaluation literature. We find no causal effect of the shutdowns on the volume of sex trafficking reports, prostitution arrests, or the number of female homicides and rape cases. We believe the most likely explanation for this non-result is the relatively low search and switching costs associated with Internet marketplaces [11, 12, 13]. Consistent with this explanation, our data show that within a month of the shutdown of Backpage and Craigslist "Personals" section, there was an increase in the number of advertisements and visitors to the remaining online commercial sex advertising platforms that was nearly commensurate with the reduction in advertisements and visitors to the shut down sites.

Our study makes several key contributions to the academic understanding of online markets for illegal activities. First, our study provides an evidence-based analysis of an important policy intervention that affected two of the most-used sites for an illegal activity. Second, our study highlights the difficulty in com-

[1] https://www.aclu.org/blog/criminal-law-reform/congress-proposes-fight-online-trafficking-harming-sex-workers

[2] https://www.eff.org/deeplinks/2019/12/observing-international-day-end-violence-against-sex-workers-means-looking-closely

[3] https://khanna.house.gov/media/press-releases/release-reps-ro-khanna-barbara-lee-senators-elizabeth-warren-ron-wyden

bating illegal activity—or its consequences—by shutting down individual high-profile online marketplaces or hubs. Combining our results with empirical research on other illegal online activities such as piracy and drug trafficking, we provide advice to policymakers interested in reducing sex trafficking facilitated by online platforms.

2. Background

2.1. Commercial Sex Advertising Platforms and Section 230 Protection

Technology has reshaped markets for prostitution. Craigslist, an online classified website, was founded in San Francisco Bay Area in 1995, and by 2010 it covered more than 400 cities and regions in the United States [14]. In 2002, Craigslist expanded its listings to include commercial sex advertisements by launching an erotic service section (ERS). ERS was listed in Craigslist's service section, and was added on the front-page of the Craigslist site in different cities over time [15]. Opening first on the west coast and in the largest eastern cities and later expanding to other parts of the country, ERS became a prominent source of commercial sex advertisements, shifting the sale of sex from offline markets toward online channels. In 2005, an average of 2,500 new commercial sex advertisements were posted to Craigslist evey day [16, 17]. A typical solicitation ad would include contact information of the provider, hourly rates, provocative photographs, and the provider's ethnic background and physical characteristics [18]. After receiving significant criticism for fostering prostitution and other illegal activities, Craigslist closed its erotic service section in 2009 and replaced it with an adult service section, announcing that postings would be reviewed by employees [19]. This adult section was blocked in 2010 after claims by state attorneys general that the ads helped facilitate prostitution and human sex trafficking [20]. However, despite the actions taken, these kind of advertisements still found a home in Craigslist's 'personals' section and appeared in abundance there, vastly outnumbering the volume of legitimate personals ads [21].

Another free advertising website, Backpage.com, launched in 2004 and soon become the second largest online classified site in the world and the leading digital marketplace for commercial sex [22]. The predominant market on Backpage.com was its adult section, which contained multiple subcategories of sex services [23]. As the dominant publisher of commercial sex ads, it received roughly 140,000 new advertisements per day in the U.S. in the six months before it was shutdown, and reportedly provided its owners with $500 million in revenue since its inception in 2004 [24]. As early as 2011, law enforcement began to accuse Backpage of being the "Walmart" for sex trafficking of both adults and juveniles. Over the years, support for this position came from a number of non-government organizations, legal experts, and law enforcement officials.

Among them, the National Center for Missing and Exploitted Children (NCMEC), stated that more than 74% of reports related to child sex trafficking filed from the public to NCMEC concerned an advertisement on Backpage [4].

Until 2018, commercial sex advertising websites like Craigslist and Backpage were shielded from liability for content posted to their sites by Section 230 of the 1996 Communications Decency Act [25]. Section 230 states that "no provider or user of an interactive computer service shall be treated as the publisher or speaker of any information provided by another information content provider." In essence, Section 230 protects websites from all civil and criminal liability for content posted to their site by others, except for liability imposed by federal criminal or intellectual property law [26]. When sex trafficking victims and their families brought cases against Backpage, U.S. courts consistently found that Section 230 shielded Backpage from legal liability for any harm resulting from advertisements posted to their site. Such findings also implied protection for Craigslist and other sites that hosted advertisements for sexual services.

2.2. FOSTA-SESTA and the Shutdown of Backpage

On March 21st 2018, the Stop Enabling Sex Traffickers Act (SESTA) and Allow States and Victims to Fight Online Sex Trafficking Act (FOSTA), referred to as "FOSTA-SESTA package", passed the Senate and it was signed into law by President Donald Trump on April 11th in the same year [27]. The twin bills were written largely in response to allegations of child sex services being sold on Backpage, and they made it illegal to knowingly "assist, facilitate, or support sex trafficking" [27]. This effectively removed the liability shield that online websites involved in these activities had previously enjoyed. Five days before the passage of FOSTA-SESTA, Backpage was seized by the United States Department of Justice on charges of money laundering related to prostitution [7, 8].

In late March 2018 Craigslist decided to discontinue its personals ads section, likely in reaction to the imminent passage of FOSTA-SESTA [28]. As previously mentioned, commercial sex advertisements appeared to vacate Craigslist following this. However, other websites, especially those offshore and not subject to US jurisdiction, continued to host advertisements for sexual services and prostitution. While our natural experiment thus involves the shuttering of the two most prominent marketplaces for online sex work advertisements, many such markets continued to exist on the Internet [25].

Despite passing in both the House and Senate by large bipartisan majorities, FOSTA-SESTA was a controversial law [26]. On one hand, it allows state and local officials to protect victims of sex trafficking and it allows sex trafficking victims to sue websites for civil damages caused in facilitating their victimization [25]. On the other hand, FOSTA-SESTA has been criticized for its potential risks to free speech on the Internet,

for the possibility that it would be ineffective in its goals, and for its potential unintended consequences regarding the safety of consensual participants in the commercial sex trade [29].

To be clear, our research does not evaluate the overall impact of the FOSTA-SESTA law and its enhanced criminal and civil protections. Rather, we evaluate the impact the near-simultaneous shutdown of the two most prominent online marketplaces for commercial sex that coincided with the law's passage.

3. Related literature

3.1. Online Markets for Commercial Sex

There is a large literature in the marketing, management, and information systems disciplines related to the effects of moving markets online [30, 31, 32]. However, specific to our context, the literature on online markets for sex or sex work is much sparser. Chan and Ghose (2012) find that the entry of Craigslist into US cities caused a 15.9 percent increase in the incidence of HIV infections in those cities, though they find this related more to unpaid sex rather than paid commercial sex [33]. Specific to commercial sex, Chan et. al. (2016) find that Craiglist's entry also caused a 17.6% increase in prostitution cases, including an increase in sex workers providing niche sexual services [16]. Cunningham and Kendall (2016) find that because prostitution is illegal in the United States, contracts between buyers and sellers in the market for sex work are difficult to enforce through courts [34]. As a result, the emergence of a popular online review site for escorts created a reputation mechanism and correlated with a trend toward better behavior between buyers and sellers in this market. In a similar vein, Cunningham et. al. (2019) found that the introduction of Craigslist's online market for commercial sex advertisements caused a 10-17 percent decrease in female homicides. They suggest that this was the result of service providers having more mechanisms to screen dangerous clients when using online platforms and the movement of dangerous street prostitution indoors [15].

3.2. Internet Governance of Illegal Behaviors

Rather than studying the effect of the introduction of an online marketplace as the aforementioned studies do, we study the attempt to deter an illegal activity taking place in an online market. As such, while the aforementioned studies provide relevant context, they do not provide definitive answers as to what would happen when some of the largest sites in the online commercial sex market are disrupted or shut down. When a site is shut down, traffic to that site may easily move to other alternative sites providing the same function. In fact, these sites may even be registered in regions where the agency trying to enforce against

the illegal activity has no jurisdiction. In that sense, our research is informed by prior work on enforcement against crime and, in particular, the literature on Internet governance of illegal activities.

With regard to Internet governance, perhaps the most studied parallel in the information systems and economics literature is the attempt to enforce copyright against digital piracy. We draw this parallel as researchers in this area have focused much of their attention on supply-side anti-piracy measures [35], which involves shutting down piracy sites or making them harder to find in an effort to reduce piracy and to increase legal sales of entertainment content. Results in this literature are mixed, as some research finds that disrupting access to major piracy sites or taking down content from such sites can reduce piracy and increase legal consumption [36, 37, 38]. Other studies find that when piracy sites are disrupted both supply and demand merely shift to other piracy sites and domains [39, 40] with little effect on overall piracy levels. The latter set of studies demonstrate not only a shift to other existing piracy sites but also the rapid emergence of new sites to take the place of those that were shut down [40].

Studies on Internet governance in the context of combating drug trafficking by targeting underground websites that connect buyers and sellers are similarly divided. On one hand, Soska and Christin (2015) find that the shutdown of The Silk Road (once the prominent dark web market for drug trafficking) led to the emergence of a much more dispersed and elaborate set of online anonymous marketplaces for illegal drugs [41]. Décary-Hétu and Giommoni (2017) find that enforcement against other underground drug trafficking sites caused similar dispersion of the market and had little lasting impact on the volume of illegal transactions [42]. On the other hand, Chan et. al. (2019) find that enforcement actions against a number of the suppliers on such sites led to a decrease in future illegal transactions on these sites without a compensating increase on other sites, suggesting a deterrence effect [43]. Thus, in drug trafficking as well as anti-piracy enforcement, the literature is divided on whether targeting online sellers and marketplaces for illegal behaviors can deter such behaviors. Drawing a parallel to our context, it is unclear whether enforcement against online prostitution or sex trafficking by means of shutting down two major advertising platforms will affect the outcomes we study. This is true not just because the link between online markets for sex and these outcomes is uncertain, but also because it is theoretically ambiguous whether shutting down a few major sites will actually cause any meaningful disruption to the online market.

3.3. Supply-Side Enforcement and Search Costs

Because online marketplaces eliminate the geographic constraints associated with offline transactions [44], when buyers and suppliers at an online advertising portal find their site shutdown, they could in theory migrate quickly to another portal serving the same function regardless of location. Such migration, however,

involves switching costs which include things like seeking out the other available sites, learning to use such sites, and trusting these sites. On one hand, such costs are often lower online than they were offline, and they are more under the control of the owners of the websites [12]. Goldfarb (2006) [45] documents that consumers can easily switch to the website of a competitor when their originally targeted website is experiencing a denial of service attack. On the other hand, Hann and Terwiesch (2003) [46] demonstrate that the friction associated with search costs online (at least in the context of searching prices) are more substantial than previously expected, preventing perfectly "flat" markets online. Ghose et. al. (2012) [47] also establish the importance of such search costs on user behavior, especially on mobile devices. Thus, even though online markets could rebound after a shutdown if participants in the market migrate to other sites, the rebound may be incomplete if search costs and frictions dissuade some participants from doing so.

3.4. The Signaling Effects of Enforcement

Even if the market for online advertisements for sex work were to migrate to one or more remaining sites, the shutdown of two major sites for these advertisements could have a chilling effect on the market. The "broken windows" theory of crime explains how visible signs of unpoliced crime send signals to criminals that enforcement is low, effectively encouraging crime [48]. The reverse may also be true; when highly prominent advertising portals for sex work are shut down, it could send a signal to participants in the market that enforcement will be strong and thus dissuade participation in the market.

This broken windows theory may well be relevant in the context of commercial paid sex. Cho, Dreher, and Neumayer [49] empirically analyze data from 150 countries and find that countries where commercial sex is legalized experience an increase in human trafficking following legalization, relative to countries that did not legalize it. Similarly, Jakobsson and Kotsadam [50] find that criminalizing commercial sex markets reduces overall human sex trafficking. Thus, an action that signals enforcement against paid commercial sex markets and sex trafficking could reduce these illegal behaviors even if that action does not meaningfully reduce the ability to transact in such markets online.

Such a signaling effect might not have only positive outcomes, however. Cunningham and Shah (2017) [51] find that the decriminalization of sex did increase the size of the commercial sex market, but it also reduced reported incidences of rape by 30% and cases of female gonorrhea by 40%. If enforcement actions create a chilling effect that reduces the size of the online commercial sex market, it is possible that they could also push the remaining market underground and into less safe conditions, potentially leading to worse outcomes for women who voluntarily or involuntarily participate in the market.

3.5. The shutdown of Backpage and Craigslist's Personals

It is clear from the previous literature that Internet governance of criminal activities – especially through supply-side enforcement actions like site shutdowns – is not well enough understood to easily predict the effect that shutting down Backpage and Craigslist's personals section would have on markets for commercial sex and their consequences. Even existing theories of enforcement against crime and of search costs on the Internet leave the answer to our research questions ambiguous. The primary contribution of our study is to resolve this theoretical ambiguity by empirically evaluating the effect of these shutdowns on a number of hypothesized outcomes, and we are aware of no other empirical studies on the effects of enforcement actions against online markets for commercial sex. We examine the effect on prostitution arrests and sex trafficking as these are directly related to commercial sex, and we examine the effect on female homicides and rapes due to connections made in prior literature between these outcomes and the introduction of online platforms facilitating the commercial sex market.

However, this study also contributes more generally to our understanding of whether and how supply-side online enforcement actions can deter criminal behavior, a subject of interest in drug trafficking, copyright enforcement and in many other areas of study. As a result, we also provide suggestive evidence as to which theoretical frameworks appear to govern the behaviors of participants in illegal Internet markets, which has general implications for Internet governance overall. These implications are discussed in section 6.

4. Data

We combine five distinct data sets in this study.

4.1. Sex Trafficking Data

Our first set of data is from IBM's Traffik Analysis Hub (TA Hub). IBM's TA Hub is a partnership across sectors and industries including government agencies, law enforcement, financial institutions and NGOs, who share global data of human trafficking. The TA Hub platform enables partners to combine data from multiple sources and formats and uses AI technologies for consistent formatting, analysis and output. These data contain the number of sex trafficking cases aggregated from NGO incident reports, law enforcement data, news, etc. for 3,142 counties on a monthly basis from Jan 2017 to Dec 2018. TA Hub applies a de-duplication process to the data to eliminate individual cases being double counted as a result of both a law enforcement report and an NGO report, for example. We acknowledge that the number of cases reported here is merely a proxy for the true number of cases, as changes in the level of enforcement or the

rate of reporting could influence these numbers even while the number of trafficking cases remains constant[4]. Nonetheless, the TA Hub data remains the best available data on the volume of sex trafficking across various geographic regions, and thus we include it as an outcome variable potentially affected by the shutdowns, though we will draw inferences cognizant of this limitation.

4.2. FBI Uniform Crime Report Data

The second data set is from the "Persons Arrested", "Offenses Known to Law Enforcement" and "Supplemental Homicide Reports" (SHR) sections of the FBI's Uniform Crime Report (UCR) program. The UCR compiles official data on crime in the United States with nearly 18,000 city, county, university and college, state, and federal law enforcement agencies voluntarily reporting crime data to the FBI. The report of "Persons Arrested" under UCR counts one arrest for each instance in which a person is "arrested, cited or summoned for an offense". From this we obtained the number of monthly prostitution arrests each law enforcement agency reported to UCR in 2017 and 2018. From the "Offenses Known to Law Enforcement" section we obtained number of monthly rape incidents reported by each agency for 2017 and 2018. Because each law enforcement agency is attached to a jurisdiction, we are able to map rape incidents and prostitution arrests to the city in which they occurred. Finally, from the SHR section we obtained the number of monthly female homicides reported during 2017 and 2018, and we mapped the location of each to the city in which they occurred[5]. As with our sex trafficking data, the UCR data has limitations. The number of prostitution arrests may reflect the volume of prostitution activity, but it may also be influenced by changes in the intensity of law enforcement activity, or in the ability of law enforcement to uncover prostitution activity and make arrests. Rape incidents must be reported in order to appear in our data, and thus are likely undercounted. Finally, the data do not separately identify crimes specifically related to the commercial sex industry, and so we examine the effect of the shutdowns on aggregate measures of specific crimes, rather than just crimes stemming from the commercial sex industry. In spite of these limitations, UCR data provide a useful measure for our analysis given its common use in the literature and that it has broad coverage (95%) across the US population. We will again draw inferences appropriately based on the limitations of our data.

[4]Highlighting this, we note that the FBI led a major national sex trafficking sting in October 2017 [52], causing the number of cases in our data to spike 6.63 standard deviations above their average during this month. Because this spike in law enforcement activity was random with respect to our experiment, and unrelated to actual changes in sex trafficking caused by our experiment, and because such a large spike would drive the bulk of our results, we drop this month from our data. All results remain the same in sign and significance if it is included, but standard errors become meaningfully larger.

[5]Our data on rape cases excludes the state of Florida because the law enforcement agencies in Florida only report these data semi-annually.

4.3. US Census Data

Our third dataset is from the US Census in 2018 and includes the population of each Craigslist and Backpage city in our data. Because each of the outcome variables we measure is expected to increase with population, we use these population data to scale our data and report all outcome variables as incidences per 100,000 population (e.g. prostitution arrests per 100,000 population). Following prior work, we drop cities with population less than 100,000 so that outcomes in low population cities do not skew our results, but we show are results are not sensitive to this cutoff in robustness checks. This leaves us with 153 cities in our data.

In the first four rows of Table 1 we present summary statistics for the first three data sets, including human sex trafficking cases, prostitution arrests, female homicides and rapes per 100,000 population. The level of observation is a city-month, and so our data indicate that in the average city during the average month, we observe (for example) 2.776 prostitution arrests and 0.290 female homicides per 100,000 population. There is, however, wide variation in the data, which are generally right-skewed.

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	24341.02
Observations on city level					
Monthly average number of ads pre-shutdown	153	22,274.471	34,803.993	516.400	293,570
Treatment intensity	153	5,358.994	4,216.676	430.708	37,273.857

4.4. Website Visitation and Advertisement Volume Data

The fourth set of data was derived from Alexa.com, which captures the number of visits or pages viewed at a website by unique Alexa toolbar users (multiple visits from the same user count as only one visit). Specifically, we obtained the page view percentage by US Internet users to the top ten commercial sex advertising sites between 2017 and 2018 [6]. Page view percentage measures the fraction of all page views going to a website. We consider this a reflection of consumer demand for commercial sex services facilitated

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

through online advertisements at each site. Figure 1 presents the traffic trends (i.e. demand) at the top 10 escort websites (including Backpage) from 2017 to 2018. Note that visits to Craigslist personal ads are not included in Figure 1 and Figure 2 as they cannot be separated from overall visits to all of the advertisements for other products and services available at this site. A list of these sites can be found in Table A1.

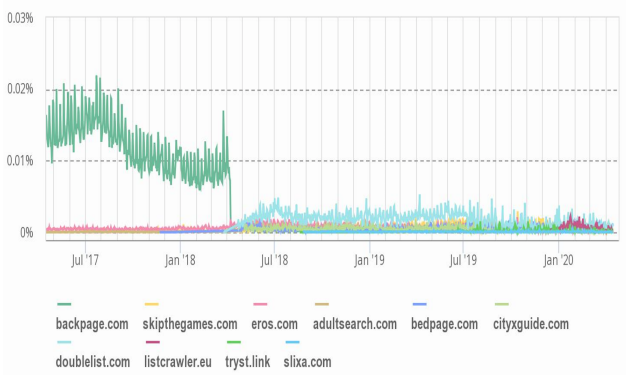


Figure 1: Historic page view % of top 10 escort websites

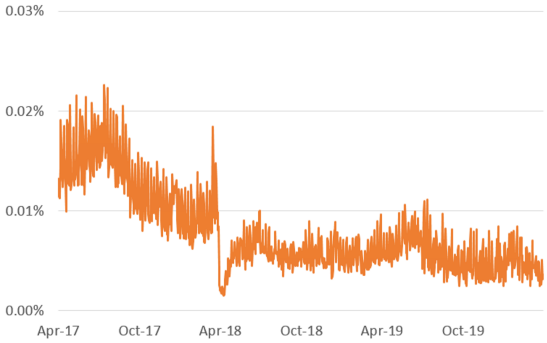


Figure 2: Total web traffic (page view %) of top 10 escort websites

Figure 1 demonstrates that prior to April 2018, Backpage was the most visited commercial sex advertising platform, and that after April 2018 the market was fragmented across dozens of websites with no single website achieving Backpage's formerly dominant position. The most popular advertising portal in our data has only 5-10% of total page views received by Backpage at its peak in 2017. Although no single advertising portal demonstrates the level of demand formerly held by Backpage, it may simply be that demand for these services dispersed. Figure 2 shows the total web traffic to the top 10 escort websites. In this figure, we observe that online commercial sex demand quickly recovered much of the way to pre-shutdown levels. By one month after the shutdown, demand at the top 10 sites had recovered to about 50 percent of pre-shutdown levels, and by six months later demand had recovered to 64 percent of its pre-shutdown level. These numbers understate the total recovery as the "long tail" of commercial sex websites outside of the top 10 likely also have received some traffic.

Our last set of data 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. We use this as a measure of the supply of online advertisements for commercial sex. Table 2 shows the volume of ads aggregated at the national and weekly level[7], as well as the Herfindahl-Hirschman Index (HHI)

[7]Due to the policy of the data provider, we are required to keep the websites in the data set anonymous. However, one can imagine that there is a strong correlation between the top 10 most visited sites and the top 14 sites with the most advertisements.

of the market calculated for shares of the 14 websites we track[8]. Similar to the demand figures presented earlier, this table shows that the supply of commercial sex advertisements fell after the shutdowns but that the amount of ads slowly returned to near their previous levels within 6 months after the shutdowns. While this recovery may be understated because advertisements likely also increased on sites outside of the top 14, it may also be overstated if there were more duplicate ads (the same advertisement on two or more sites) after the shutdowns. Although total ad volume recovered meaningfully in the months following the shutdowns, the HHI clearly diminished. This is consistent with what we saw in the site visitation data, indicating that after the shutdowns supply was dispersed over a larger number of websites.

To capture the potential displacement effect of the shutdown, we calculate the number of ads from other sites (excluding the shutdown sites Backpage and Craigslist) per 100,000 population for each city in our data set, the summary statistics of which are reported in Table 1.

Table 2: Total ads volume by site, from Nov. 2017 to Oct. 2018

Date		Ads Volume	Herfindahl-Hirschman Index (HHI)
3/12/2018	10 days before Craigslist personals shutdown	842,060	5002.658
3/26/2018	10 days before Backpage shutdown	383,697	7795.564
4/9/2018	3 days after Backpage shutdown	13,293	3404.296
5/7/2018	1 month after Backpage shutdown	233,296	3147.573
10/22/2018	6 months after Backpage shutdown	647,836	2573.619

Of course, changes in any of these variables from before the shutdowns to after may be attributed to any number of factors changing over time. Our identification strategy involves cross-city variation in the penetration level of Backpage and Craigslist advertisements for commercial sex. Though all Craigslist and Backpage cities were affected by the shutdowns, the "bite" of this treatment is different depending on how heavily these sites were used to advertise sex work in a given city. For example, in a hypothetical city where there were no commercial sex advertisements on either of these sites, the treatment would have arguably had little to no effect.

4.5. Treatment Intensity

For each city, we aggregate the daily volume of commercial sex advertisements on Backpage and Craigslist from November 1, 2017 to the date the advertisements were no longer available. We compute the average

[8]The Herfindahl-Hirschman Index (HHI) measures the market concentration of the industry with its scale ranging from 1 (least concentrated) to 10,000 (most concentrated). Here HHI is calculated as the sum of the squares of the percentage of ads from each single site, namely, HHI $= \sum_{i=1}^{14} S_i^2$, S_i is the percentage of ads from site i.

number of (pre-shutdown) monthly sex ads on these two sites and divide this average by the city population (in 100,000's). This treatment intensity is used as a relative measure of the pre-shutdown city-level usage of Backpage and Craigslist for commercial sex advertisements and thus we consider it the "treatment intensity" of the shutdowns in each city. Following the prior literature [15], we remove cities with fewer than 100,000 population as very small cities would otherwise have a disproportionate influence on our estimates, but we show in robustness tests that our results are not sensitive to this particular cutoff.

We illustrate the distribution of treatment intensity (Backpage/Craigslist ad penetration rate) across cities in a histogram in Figure 3. The distribution is right-skewed with some outlier values on the right tail. These outliers in the histogram are the cities with high numbers of sex ads relative to population size. We note most of these cities are transit, convention, or tourist destinations (such as Atlanta or Myrtle Beach), which is consistent with prior research showing that such cities tend to exhibit a larger supply of commercial sex providers [53]. Importantly, even without the outlier cities, it is clear that there is a good deal of cross-city variation in the treatment intensity of the shutdowns, which is essential to our identification strategy. The descriptive statistics for this variable are presented in Table 1.

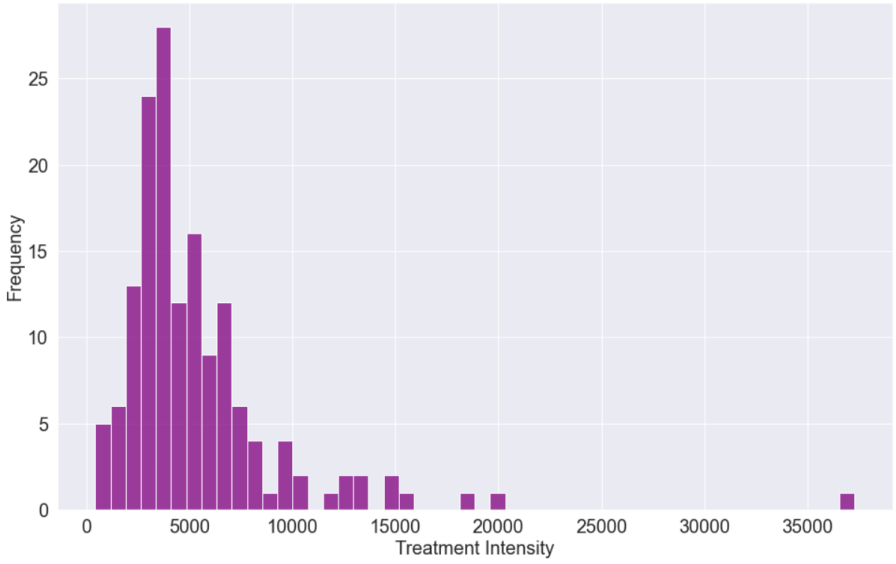


Figure 3: Histogram of the treatment intensity

5. Models and Results

The goal of this study is to investigate the effect of the shutdown of Backpage and Craigslist's commercial sex advertising platforms on a variety of outcomes. Specifically, we examine the effect on total reported sex

trafficking cases, prostitution arrests, female homicides, and female rape cases. Our identification strategy is based on the generalized form of the difference-in-difference model with a continuous treatment intensity variable, where this treatment intensity variable represents the "bite" of the treatment on each city. If the shutdowns had any effect on a particular outcome, we would expect to see larger changes in that outcome in cities where the treatment had a stronger bite and smaller changes where the treatment had a weaker bite.[9]

As mentioned, we use the number of Craiglist and Backpage prostitution advertisements (in the months before the shutdowns) per 100,000 population with a city as our measure of that city's treatment intensity, as cities with a higher concentration of advertisements would be more affected by the shutdowns than cities with lower concentrations. We acknowledge that cities are self-selecting into treatment intensity, in that individuals in each city are choosing whether to post advertisements for commercial sex on the two sites. However, our difference-in-difference identification does not rely on treatment intensity being exogenous with respect to our outcome variables, but rather on treatment intensity being exogenous with respect to month-to-month *changes* in the outcome variables. Not only is this assumption more plausible, but we can partially test it by asking whether treatment intensity is correlated with month to month changes in the outcome variables in the pre-period, which is the generalized form of the parallel trends assumption.

It makes sense to test whether our model is specified in such a way so as to detect a causal effect when one clearly exists. Fortunately, we have such a scenario at our disposal. Table 2 clearly shows that online advertising portals for commercial sex other than Backpage and Craiglist immediately began to experience a rapid growth in advertising volume (supply) after Backpage and Craigslist's personals were shut down. Coupled with the rapid growth in demand on these sites observed in Figure 2, it is straightforward to conclude that this was causal: the most popular sites were shut down, and advertisers and buyers immediately shifted toward other sites. As such, we first estimate our generalized difference-in-differences model with advertisements for commercial sex on other sites (besides Craigslist and Backpage) to see if it produces results consistent with the causal increase in ads on other sites that we can clearly observe.

5.1. 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:

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

[9] See Card (1994) [10] for the seminal example of this implementation or Danaher et. al. (2020) [37] for a recent implementation in the information systems literature.

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 of the shutdown of Backpage and Craigslist 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 100,000s both 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 the volume of commercial sex advertisements from other sites in a hypothetical city, where if the pre-shutdown penetration rate of Backpage and Craigslist (i.e. treatment intensity) is 0, one might think of this as approximating a "control" city. The coefficient of interest, β_2^t, indicates the degree to which month to month changes in the outcome variable correlate with treatment intensity. The identifying assumption is that no correlation would exist in the absence of the treatment, implying that β_2^t should be 0 for all pre-shutdown months (our equivalent of the parallel trends assumption in a standard difference-in-difference model). After the shutdown, β_2^t indicates how treatment intensity correlates with post-shutdown changes in the outcome variable, and under the identifying assumption it would indicate the causal effect of the shutdown on the outcome variable.

In Figure 4 we plot all of the coefficients β_2^t along with their 95% confidence intervals from our fixed effect OLS estimation of (1). We note that before the shutdown, β_2^t for each month is small and hovers around zero (a Wald test could not reject the null hypothesis that these coefficients are jointly zero at alpha = 0.05), indicating that treatment intensity is not correlated with month-to-month changes in the commercial sex advertising volume on other sites before the shutdown. Immediately following the shutdown, β_2^t turns positive and statistically significant, and its magnitude grows over time. This indicates that growth of advertisements on alternate sites was larger in cities that were more affected by the shutdowns. By month seven after the shutdowns, alternate sites ads in a city that had 1 pre-shutdown Backpage/Craigslist advertisement per 100,000 people grew by 0.73 advertisements per 100,000 population more than a city that had no pre-shutdown ads on Backpage/Craigslist.[10] Of course, this is exactly what one would expect if advertisers on Backpage and Craigslist shifted their advertising to other sites: commercial sex advertisements at the other sites would increase more in cities where there had been more commercial sex advertisements on Backpage and Craigslist. But this exercise highlights that our difference-in-difference model can both pick up and estimate a causal effect where we have a strong prior belief that one existed. And the magnitude

[10] OLS estimates underlying Figure 4 can be found in column 1 of Table A2 of the Appendix.

of the estimate is reasonable—the estimate of 0.73 in month six suggests that, controlling for population, shutting down 1 Backpage/Craigslist ad 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 Table 2, where ads on other sites grew to well over 50 percent of pre-shutdown ads on Backpage of Craigslist, but did not reach 100 percent by the final month for which we have data.

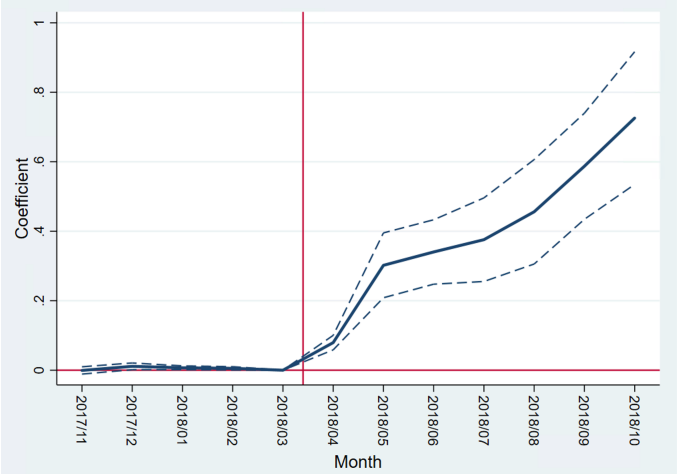


Figure 4: Moderation of trend for ads volume on other websites per 100,000 population by treatment intensity in 153 cities

Based on these results, one might want to estimate the average effect of the shutdown on the outcomes during the entire post-shutdown period. To this end, we estimate the following model:

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

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 (i.e. April 2018 or after) and λ_t now 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 3. 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 ad per 100,000 population resulted in an increase of 0.405 ads per 100,000 on remaining sites after the shutdowns, although we know from Figure 4 that this represents the average of a smaller increase in the first few months after the shutdown and a larger increase by the end of our data period. We do not have city-level data of traffic to these various commercial sex advertising websites, and thus we are unable to apply this model to visits

to the remaining sites, but we would expect to find similar results given the clear post-shutdown increase in visits to such sites in Figure 2.

Given that the majority of advertisements on Backpage and Craigslist were displaced to other sites after the shutdown, it is natural to ask whether these shutdowns could have any direct or indirect effect on outcomes potentially related to online markets for sex work. On one hand, by month seven each advertisement that was no longer available on Backpage and Craigslist appeared to result in only 0.73 more ads on the most popular remaining sites, which could imply a decrease in volume. Further, with advertisements now dispersed over a greater number of sites, increased search costs and diminished trust in the new sites could have disrupted the market to a degree, as has been observed when disrupting access to piracy sites [37]. On the other hand, existing literature in digital transformation suggests search costs associated with Internet portals are lower than offline search costs [13], so this dispersion of ads may have had little impact on the market. Moreover, the increase in ads on other sites was rising throughout our post-period, and it may have even reached a one-for-one parity with the number of shut down Backpage and Craigslist ads in the time period after our experiment. As buyers and sellers in this market may create connections that last for some time, it is not clear that a short-lived disruption would have much impact on the marketplace in question. To this end, we estimate models (1) and (2) for a variety of outcome variables that have been theoretically linked as potential outcomes of these shutdowns.

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

We do not have data on the actual number of prostitution-related transactions that take place, or the volume of sex-trafficking that occur, in the United States. Moreover, because these activities are illegal it is unlikely that such data exist. Instead, as described in section 4, our data record the number of prostitution arrests made in each city in each month as well as the number of sex trafficking cases reported across a variety of law enforcement agencies, news and media outlets, and non-profit organizations. We estimate model (1) when the outcome variable is sex trafficking cases per 100,000 population, and we plot all β_2^t coefficients along with their 95% confidence intervals in Figure 5. We do the same for prostitution arrests and plot these estimates in Figure 6.

For sex trafficking cases, we observe that β_2^t appears to hover around the same level for almost the entire pre-period. The negative values here appear to result from a somewhat high number of sex trafficking cases in high treatment intensity cities in March 2018 (our left-out month, and therefore the baseline month). However, in all pre-period months other than March 2018 there appears to be 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 appears to be statistically indistinguishable from 0. A Wald test of joint significance on these coefficients yields a p-value of 0.95. Likewise in the pre-period we see in Figure 6 that treatment intensity is not correlated with monthly changes in prostitution arrests (p-value of Wald test is 0.99). Thus, in the absence of the shutdowns, we might expect treatment intensity to continue to be uncorrelated with these trends.[11]

Indeed, 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.

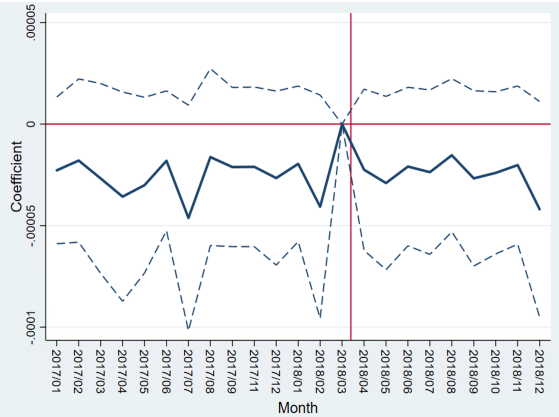


Figure 5: Moderation of trend for number of sex trafficking cases per 100,000 population by treatment intensity in 146 cities



Figure 6: Moderation of trend for number of prostitution arrests per 100,000 population by treatment intensity in 147 cities

Estimates of model (2) on the average effect of the shutdown across the entire post-period can be found in column 2 (sex trafficking) and column 3 (prostitution arrests) of Table 3.[12] We note that the estimate of the average treatment effect of the shutdowns on sex trafficking cases is nearly zero and statistically insignificant, while the effect on prostitution arrests is also small and indistinguishable from zero. Of course, the descriptive statistics made it clear that these outcome variables had relatively low volumes. However, even if the effect of the shutdowns on these outcomes is statistically insignificant, the reflected changes might be practically significantly. As such, to explore whether the point estimates for our coefficients represent "economically 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

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

[12] Note that the number of cities varies a bit for each outcome variable as each dataset has a few missing cities as described in section 4.

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 observed number of cases/arrests to impute the implied percent change in each outcome caused by the shutdowns. These back-of-the-envelope calculations are reported in the last row of Table 3, where our models estimate a statistically insignificant 2.5 percent decrease in sex trafficking cases and a marginally significant 5.7 percent increase in prostitution arrests (p=0.099).

We acknowledge that the number of prostitution arrests is an imperfect proxy for total volume of prostitution activity, as arrests may also be influenced by the volume or intensity of law enforcement activity, and this itself may change endogenously in reaction to the shutdowns. Similar arguments may be made regarding reported sex trafficking cases, and we discuss the ramifications of this for our conclusions in section 6.

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

Even if the shutdowns had little impact on sex trafficking cases or prostitution arrests, it is possible that they could have made women engaged in commercial sex less safe. If these women benefited from an ability to use online marketplaces to screen clients when advertising online, or if they shifted their work to more dangerous street prostitution, the volume of prostitution activity might not change but the safety of commercial sex providers might decline. We explore this by considering the number of female homicides and rape cases following prior work [15].

Estimates of β_2^t from model (1) for female homicides and rapes (per 100,000 population) are pictured in Figure 7 and Figure 8, respectively, with the underlying estimates in columns 4 and 5 in Table A2. Again, we find no statistical relationship between treatment intensity and month to month changes in these variables during the pre-period, with β_2^t hovering around zero in both graphs. In the post-period this trend largely continues, with little evidence that changes in female homicides or rapes across cities is related to the previous size of the commercial sex markets on Backpage or Craiglist in those cities.

We also estimate model (2) for these two variables and our report results in columns (4) - (5) of Table 3. These coefficients are both close to zero and statistically insignificant, and the estimated effect on these outcomes is also small in magnitude (-1.6 percent for homicides and 2 percent for rapes). Thus we find no evidence of an impact of the shutdowns on the safety of female commercial sex providers as measured by female homicides or rapes.

5.4. Robustness

We verify the robustness of our results by varying both the manner in which we segment the data as well as exploring different metrics of treatment intensity. The results of these models are found in the Appendix in



Figure 7: Moderation of trend for female homicides per 100,000 population by treatment intensity in 138 cities

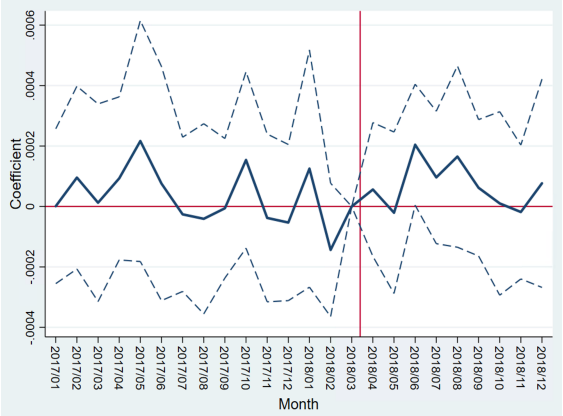


Figure 8: Moderation of trend rape per 100,000 population by treatment intensity in 142 cities

Table 3: OLS regression results for (3)

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

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

Table A3 (sex trafficking cases), Table A4 (prostitution arrests), Table A5 (female homicides), and Table A6 (female rapes). In each table, column (1) displays the results found in the main body in Table 3 for comparison while columns (2) through (5) display estimates from robustness checks.

5.4.1. Removing Cities with Extremely Low Crime Rates

Our dependent variables for crime (sex trafficking, prostitution arrests, female homicides and rapes) are right skewed with a disproportionate number of 0 values. As such, data in cities with extremely low crime rates (e.g., zero total sex trafficking case in both the pre- and post-periods) may suppress the magnitude of our coefficients of interest. As such, as a robustness test we remove cities with very low crime/illegal activities in the study period to determine how this impacts our results.

For the analysis on sex trafficking reports, we first remove the cities with 0 total sex trafficking cases reported for the two year study period 2017 - 2018. The coefficient estimate for β_2^t in this regression is

displayed in column (2) of Table A3. We further remove cities with fewer than 2 sex trafficking cases in the two-year study window. By doing this we retain around half of our original sample, 77 cities, cities covering more than 98% of total sex trafficking reports in our data set. The estimates for the coefficients of interest for these 77 cities are presented in column (3) of Table A3. We note that this change has no effect on the sign or significance of our findings and, if anything, the magnitude is even smaller when cities with only zero or one trafficking cases are dropped.

We perform similar robustness checks on the other dependent variables, with results presented in columns 1 and 2 of Table A4 through Table A6 . We note that our results are robust in both sign and significance.[13]

5.4.2. Right-censoring Treatment Intensity

As we have mentioned, the cities with the highest treatment intensity in our data tend to be tourist, convention, or vacation cities, likely because prostitution is more common in such locations. However, we can see in Figure 3, that treatment intensity is right-skewed and that there are several outliers in the right tail. One might argue that such extreme outliers would have undue influence on the results of our OLS estimation. As well, even if our metric of treatment intensity is ordinaly correct (i.e. the cities with highest treatment intensity were indeed the most affected by the shutdowns), perhaps the ratio scale is off. As a check, we estimate our models after censoring treatment intensity, setting all values over 15,000 to a maximum of 15,000 ads per 100,000 population. The results after right censoring are reported in column (4) of Table A3 (sex trafficking), Table A4 (prostitution arrests) and Table A5 (female homicides), as well as Table A6 (for rape incidents). For all variables our results remain the same in size and significance to our baseline results, though we do note that the back-of-the-envelope calculation for sex trafficking cases increases in magnitude to a 6 percent drop—the largest change we observe across all robustness tests.[14]

5.4.3. Changing the Population Cutoff

As mentioned in the data section, and consistent with Cunningham et al. (2019) [15], we dropped all cities with fewer than 100,000 population as such cities have low incidence of crime and because, with population in the denominator, changes in crime in cities with very low population might skew our results. To test whether this choice has affected our results, we allow smaller cities into the data by using a population cutoff of 50,000 and we report these results in column (5) of from Table A3 to Table A6 for all four outcome variables. For sex trafficking, prostitution arrests, and rape cases, results are very similar to our baseline

[13]Note that all cities in our data set have at least 12 total rape incidents and so this restriction on the data is non-binding for that outcome variable.

[14]We observed similar results when censoring treatment intensity to 10,000 or 20,000 but do not report them here.

results. For female homicides, the sign of our result flips from negative to positive, although it remains statistically insignificant. Including these cities does, however, lead to an estimated 8.5 percent increase in female homicides, though this is statistically insignificant with a p-value of 0.356. This estimate might be considered economically meaningful given the nature of the outcome variable, but we note that it is driven by small cities (less than 100,000 population) where homicide rates are usually 0 and occasionally flip to 1, implying a low signal-to-noise ratio.

Overall, our robustness checks confirm the null results found in our preferred specification, with perhaps the exception that observe a statistically weak 8.5 percent increase in female homicides driven by cities with low population. Given the results in all other specifications and the weak p-value, we are unable to say whether there was an causal increase in female homicides in smaller cities, although we note that in large cities there is no evidence of such an effect.

6. Discussion and conclusion

In this study we use the 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. We conclude that the shutdown of Backpage and Craigslist personals section did not lead to an increase in violence against commercial sex providers, in the form of increased female homicides and rapes, that many critics had feared; nor did it lead to a decrease in reports of sex trafficking that many advocates had hoped for. The shutdowns also caused no change in the volume of prostitution arrests.

We believe these non-results arise from the fact that the markets for commercial sex advertising were able to quickly move to other online platforms after the shutdown of Backpage and Craigslist. Our generalized difference-in-differences model shows a causal relationship between the shutdown of Backpage and Craigslist "Personals" and increased supply and demand on other commercial sex advertising sites, indicated by the fact that advertisements on these newly popular sites grew more rapidly in the cities where Backpage and Craigslist's sex advertising markets had been more popular before the shutdowns. Our results also demonstrate that the number of advertisements on other alternative sites causally grew to at least 73 percent of Backpage and Craigslist's pre-shutdown levels within seven months of the shutdown.

The flexibility of Internet markets for commercial sex is also reflected in the fact that three 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, adlist24.com moved from US-based address to China in May 2018, doublelist.com moved to Paris in March 2018, and

girl-directory.com moved to the UK in May 2018.

6.1. Limitations

Our empirical analysis has several limitations. As we discussed in Section 4, our ability to measure outcomes is limited by the available data. Specifically, rape cases are known to be under-reported relative to the true number of rape incidents, and we are unable to limit female rapes and homicides to those associated with prostitution. Further, while prostitution arrests may proxy for total volume of prostitution activities, arrests may be influenced by the intensity or efficacy of law enforcement activity against prostitution and it is possible that enforcement activity itself shifted endogenously in response to the shutdown of Backpage and Craigslist. For example, with information more dispersed across a larger set of newly popular advertising portals, enforcement agencies may have found it more difficult to seek out prostitution or trafficking rings. Likewise, although IBM's TA Hub compiles trafficking data from multiple sources including financial institutions, non-government organizations and government agencies, one of these sources is cases reported by law enforcement, and so our data on trafficking may be subject to the same concerns (although this concern is more limited here due to TA Hub's monitoring of other data sources on sex trafficking). While each individual outcome variable comes with limitations that constrain our ability to draw inferences, we based our overall assessment on the preponderance of evidence rather than any one outcome.

Finally, we note that our results are limited to the nine months of post-shutdown activity in our data, although 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.

6.2. Regulatory Implications

Our study has implications for enforcement against efforts to reduce sex trafficking, and for Internet governance in general. With regard to efforts to reduce 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. Legislators and enforcement agencies may need to explore alternative strategies in their efforts to reduce trafficking and related illegal activities.

Although we are unaware of other work on the causal effects of enforcement against online commercial sex markets that could guide legislation and enforcement effort, prior work in other areas of Internet governance may provide useful guidance. Specifically, our finding that shutting down the two largest online portals for advertising sex work did not meaningfully disrupt the market is similar to findings by Aguiar et. al. (2018) [40] and Danaher et. al. (2020) [37] on anti-piracy enforcement. In the former, shutting down a popular

German piracy linking site caused pirates to disperse to other remaining sites and caused no change in legal film consumption (or illegal availability of films). In the latter, requiring Internet Service Providers (ISPs) to block access to The Pirate Bay also increased piracy at other sites without changing legal consumption. In both cases, and apparently in the case of shutting down Craigslist's personal ads and Backpage, the content contained on the original sites was either still available on—or moved to—a number of other sites, limiting any effect of the enforcement actions. Both piracy studies found evidence that low search costs on the Internet explained the ability for online markets to rebound quickly in reaction to removing access to a major site. These findings are also comparable to those of Soska and Christin (2015) on the dispersion of drug trafficking to alternate websites when The Silk Road was shut down

However, Danaher et. al. (2020) [37] also found that when UK courts forced ISPs to simultaneously block access to nineteen of the largest remaining piracy sites (and later over fifty more sites), these more intensive blocks were successful at reducing piracy and increasing legal consumption of motion picture content. The authors conclude that the difference in outcomes between blocking a few sites and blocking many sites stems from the fact that blocking many sites increased search costs sufficiently to deter some amount of illegal behavior, and the continuous enforcement through blocking more sites a year later continued this deterring effect.

While the nature of the harm is obviously very different, we suggest that these empirical results in the context of online piracy enforcement may be informative to policy efforts designed to reduce online sex trafficking. Specifically, our results on the shutdowns of Backpage and Craigslist's personals suggest that shutting down two portals for commercial sex advertisements does not represent a strong disruption to the market. It remains possible that, as with piracy, blocking access to a larger portion of sex advertising portals (either through direct intervention or through ISP-level blocking) might be significantly more disruptive to the market.

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Appendix

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: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$