Essays on the Economics of Prostitution and Sex Crimes

Riccardo Ciacci

Thesis submitted for assessment with a view to obtaining the degree of Doctor of Economics of the European University Institute

Florence, 12 December 2018

European University Institute
Department of Economics

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"It is our choices, that show what we truly are, far more than our abilities."

Albus Dumbledore (J.K. Rowling)

"I explored the theoretical and empirical implications of the assumption that criminal behavior is rational, but again "rationality" did not imply narrow materialism. It recognized that many people were constrained by moral and ethical considerations, and they did not commit crimes even when these were profitable and there was no danger of detection. However, police and jails would be unnecessary if such attitudes always prevailed. Rationality implied that some individuals become criminals because of the financial and other rewards from crime compared to legal work, taking account of the likelihood of apprehension and conviction, and the severity of punishment."

Gary Becker

European University Institute

Abstract

Department of Economics

Doctor of Philosophy

Essays on the Economics of Prostitution and Sex Crimes

by Riccardo CIACCI

This thesis consists of three chapters devoted to analyse the determinants of prostitution and sex crimes.

The first chapter, jointly co-authored with Micaela Sviatschi (Princeton), finds evidence that adult entertainment establishments and sex crimes behave as substitutes. We build a daily panel that combines the exact location of not-self-reported sex crimes with the day of opening and exact location of adult entertainment establishments in New York City. We find that these businesses decrease daily sex crime by 13% per police precinct. The results imply that the reduction is mostly driven by potential sex offenders frequenting these establishments rather than committing crimes.

The second chapter shows that improving prostitutes' outside options deter prostitution. Specifically, this chapter fills the gap between two strands of the literature suggesting that unilateral divorce should decrease prostitution as a result of higher wives' welfare. I build a unique panel data set for the U.S to test this prediction. Differences in the timing of entry into force of unilateral divorce laws across U.S. states provide a quasi-experimental setting allowing to estimate the effect of unilateral divorce laws on female prostitution (proxied by female prostitutes' arrests). Using a diff-in-diff estimation approach, I find that unilateral divorce reduces prostitution by about 10%. I explore several mechanisms that could rationalize my findings. The mechanism that fits best the empirical evidence is one where unilateral divorce improves the option value of getting married by increasing wives' welfare. As a result, the supply of prostitution declines.

Finally, in the third chapter I rely on a recent economic literature, including Chapter 1 of this thesis, reporting evidence on how sex crime and prostitution behave as substitutable activities. This chapter makes use of variation in fines for sex purchase in Sweden to analyse the relationship between criminalising the purchase of prostitution and rape; and finds that higher fines for sex purchase increase rape on impact.

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To the pursuit of happiness,

Chapter 1

The Effect of Adult Entertainment Establishments on Sex Crime: Evidence from New York City

(joint with María Micaela Sviatschi)

1.1 Introduction

Sex crimes, including sexual violence, are a major public health concern. Apart from the large psychological and physical burden, these crimes also lead to public health issues including unintended pregnancies, induced abortions and sexually transmitted infections.[1] However, little is known about how to prevent sex crimes, including sexual abuse and rape. Several have argued that rape is simply a substitute for consensual sex (Thornhill and Thornhill, 1983; Thornhill and Palmer, 2000a; Thornhill and Palmer, 2000b). Thus, having access to substitutes such as adult entertainment or paid-for sex (i.e. prostitution) may reduce the incidence of such crimes. Yet, little causal evidence has been produced to support this claim.

This paper examines whether the presence of adult entertainment establishments (strip clubs, gentlemen clubs and escort girl services) reduces sex crimes. Adult establishments may include prostitution, although it is generally illegal. While these clubs and services may reduce sex crimes if individuals use them instead of committing sex crimes (Posner, 1992; Dever, 1996), they may increase sex crimes if they reinforce the view of women as objects, leading to more violence against them (Brownmiller, 1993).[2] One of the main challenges of evaluating whether adult entertainment is a substitute for sex-related crime is the difficulty of gathering data that allows for a causal interpretation of the effect of adult entertainment establishments on such crimes. Sex crimes are thought to be under-reported, and related data is often protected by privacy laws.

This paper exploits a unique data set with daily precinct-level crime information from New York City (NYC). We construct a new data set on adult entertainment establishments that includes the names and addresses of the establishments, providing precise geographic information. We complement this with information on establishment registration dates from the New York Department of State and Yellow Pages, which we use to define when an establishment opened. We categorize adult entertainment establishments by New York Police Department (NYPD) precincts to match

[1] A 2007 national study of the Department of Justice estimated that 18% of American women experienced rape (or attempted rape) at least once in their life.

[2] In addition, assuming that sex crimes are an increasing function of the number of sex workers, adult entertainment businesses may rise this sort of crimes by increasing the number of sex workers.

crime data from the "Stop-and-Frisk" program. The crime data include hourly information on crimes observed by the police, including sex crimes. The data set covers the period from January 1, 2004 to June 30, 2012. Since these crimes are reported by the police, it minimizes the biases associated with self-reported data on sex crime. We check the robustness of the results using police complaint data.

Using variation in the date of registration of adult entertainment establishments, we show that opening these establishments in particular areas decreases the number of sex crimes committed nearby. We find that the presence of an adult entertainment establishment in a given precinct leads to a 13% daily reduction in sex crime in the precinct. This estimated coefficient comes from the preferred specification that includes fixed effects at the precinct, year, month, day-of-the-year, day-of-the-week and holiday level, and precinct-year time trends.[3] The results are robust to different regression models and to using police complaint data.

The main identification assumption is that the opening date of an adult entertainment establishment is exogenous to any other factor affecting sex crime. Since opening a business in NYC requires a long bureaucratic procedure, we can take the date of registration as a quasi-natural experiment to study the effect of these businesses on sex crime. In addition, we exploit cross-section daily variation in sex crimes across precincts within the city.[4] Therefore, since adult entertainment businesses were not opened in response to precinct-specific trends in reported sex crime, we can exploit the exogenous variation in openings at different time periods in different precincts to obtain the causal effects of adult businesses on sex crime.

The second focus of this paper is to understand the mechanism behind the effect of adult entertainment on sex crimes. One potential mechanism is that these establishments offer services that may substitute sex crimes, leading potential sex offenders to become adult customers of such businesses. Recently, scholars have argued that adult entertainment establishments might also offer prostitution services, they refer to them also as indoor prostitution. Adult entertainment establishments might provide a way for the whole transaction to occur behind closed doors (Farley, 2003.[5] In addition, even if adult entertainment establishments do not offer paid sex they offer other services that can be considered as substitutes for sex crimes.

We find considerable evidence that sex crime is reduced when potential sex offenders frequent adult entertainment establishments. We find that at night, the effect of the establishments is negative and larger in absolute value than our benchmark. This suggests that these establishments are most effective at preventing sex crimes from being committed at night. Since the majority of adult entertainment establishments are only open at night, and the demand for their services is higher at that time of day, the results suggest that potential sex offenders prefer to use these services rather than commit sex crimes. Therefore, these results suggest that potential criminals consider sex crime and adult entertainment establishment services as substitute activities, as Farley, Bindel, and Golding (2009) documents by interviewing men who purchase prostitution. Dahl and DellaVigna (2009) identify a similar mechanism in which violent movies have an incapacitation effect: they reduce the crime rate by

[3] One potential concern is that the opening date is different from the registration date. However, the results are robust to conducting the analysis at the week or month level.

[4] A precinct is a geographical division of neighborhoods within a city. We follow the 77 precincts of the NYPD.

[5] Indoor prostitution is any kind of sex work that happens behind closed doors (as opposed to street prostitution). Indoor prostitution includes massage parlors and saunas, brothels, strip clubs, and escort prostitution (Urban Justice Center, 2005; Shively et al., 2012). In the US, indoor prostitution is the major source of prostitution: according to the Urban Justice Center (2005) the indoor market constitutes roughly the 85% of all sex work activity.

keeping potential offenders off the streets and in the cinemas. The only difference is that potential sex offenders do not commit sex crimes simply due to incapacitation (i.e. time constraint), but because they substitute sex crimes with services offered in adult entertainment establishments.

We also use our data to rule out three other mechanisms. First, we find that opening adult establishments does not affect other types of crimes, which demonstrates that the results on sex crimes are not driven by an increased police presence on the streets. This also rules out the hypothesis that these businesses may attract other types of criminals such as drug dealers as well. Second, we find that sex crimes are not moving to other areas, which shows that there are no negative spillover effects on bordering precincts.[6] Third, we also check if there is a reduction in street prostitution.[7] The number of street sex workers would decline if they started working in adult entertainment establishments or if they moved to other precincts due to the increased competition. However, we find no effects on the number of street sex workers and no reallocation to bordering precincts. This suggests that the results are not driven by a reduction in potential victims who are now avoiding the area or by a reduction in sex crimes against sex workers.[8]

This is the first paper to study the casual impact of adult entertainment establishments on sex crimes. The study contributes to the economics of crime literature by focusing on sex-related crimes; there is little evidence regarding how to prevent them. While most of the literature has focused on theories of control, labor markets and the role of deterrence policies (Card and Dahl, 2011; Munyo and Rossi, 2013; Bobonis, Gonzalez-Brenes, and Castro, 2013; Aizer, 2010; Amaral, Nishith, and Bhalotra, 2018; Iyengar, 2009; Kavanaugh, Maria Micaela, and Trako, 2018; Miller and Segal, 2016), this paper focuses on the role of services for men that may substitute for sex crimes. Moreover, while most of the focus has been on domestic violence, in this paper we analyze the effects of introducing adult entertainment options in an area on rape and sexual harassment in nearby public spaces, which may have other unexpected consequences such as reducing women's economic mobility. For example, Borker, 2017 shows that women choose to attend lower-ranked schools than men in order to avoid sexual harassment from men on the street.

This paper is closely related to two recent studies of the effects of decriminalizing prostitution. Cunningham and Shah, 2017 exploit an unperceived decriminalization of indoor prostitution in Rhode Island; their estimates are based on a year-state specification. Bisschop, Kastoryano, and Klaauw (2017) study the effect of street prostitution in special red-light zones, also using annual estimates.[9] Both papers find that decriminalizing prostitution decreases sex crimes against sex workers.

We make four contributions to this literature. First, while previous studies have

[6]These results are consistent with previous studies that have shown that increasing the number of police officers on the street does not displace crime to other areas (Di Tella and Schargrodsky, 2004; Draca, Machin, and Witt, 2011).

[7]Scholars found that about 70% of (street) sex workers have been victims of sex crimes due to their job (Farley, 2003).

[8]This is consistent with the fact that sex workers represent a small proportion of the total reported sex crimes, given the illegal nature of their work (Bridgett and Robinson, 1999).

[9]In 1980 in Rhode Island prostitution law was amended and prostitution was degraded from a felony to a misdemeanor. The legislators removed the section that addressed committing the act of prostitution itself, yet street solicitation, running a brothel, and pimping remained illegal. Therefore, indoor prostitution was "de iure" decriminalized. However, Arditi, 2009 argued that this decriminalization occurred by mistake, so probably neither legislators nor citizens realized that the amendment created a legal vacuum.

focused on how the decriminalization of prostitution affects sex crimes, we find evidence that adult entertainment establishments can reduce sex crimes even in a setting where prostitution is illegal.[10] While the decriminalization of prostitution is a contentious issue, adult entertainment establishments are generally legal around the world, although there are often strict regulations governing where they can be located.[11] The results in this paper imply that the regulation of adult entertainment establishments is one way to address sex crimes. Moreover, it is a viable alternative that is less ethically challenging than legalizing prostitution and can achieve similar effects. Second, we complement previous papers, that used year and state variation, by analyzing the short-term effects using daily precinct data within a city as well as non-self-reported data. Third, by shedding light on the mechanisms linking adult establishments and the incidence of sex crimes, the results have several policy implications. The fact that the effects are driven by potential customers and that there is no increase in other crimes suggests that these establishments can have positive effects on reducing sex crimes without the negative externalities often associated with decriminalizing prostitution (such as an increase in the use of drugs or violent crimes against sex workers).[12] However, it could be argued that adult entertainment establishments should be supervised since some of their customers are potential sex offenders. Finally, we complement the previous literature by showing direct evidence that opening adult entertainment businesses generates positive externalities on sex crime for the whole population: sex crimes are reduced for both sex workers and non-sex workers.[13]

The paper proceeds as follows. In the next section, we provide background information on adult entertainment establishments in NYC. Section 1.3 presents the data. Section 1.4 discusses the identification strategy and the possible threats. Section 1.5 shows the results of our specification. Section 1.6 discusses the possible mechanisms that could be driving the effect. The last section summarizes the findings and offers concluding remarks.

1.2 Background information on adult entertainment establishments

1.2.1 Adult entertainment establishments in NYC

The New York State Department of State classifies adult entertainment establishments as businesses that *regularly feature movies, photographs, or live performances that emphasize "specified anatomical areas" or "specified sexual activities" and excludes minors*

[10]Although in the United States (except Nevada) prostitution is illegal, there is a lack of agreement about how to legislate against it. European countries such as Germany, the Netherlands and Belgium legalized and regulate prostitution via licenses, while Sweden and Norway opted to criminalize the *purchase* of prostitutes rather than the supply of such services. In 2014 the European Parliament passed a resolution to follow the Swedish model.

[11]The legalization of prostitution is one of the most frequently discussed topics related to gender issues. For example, *The Economist* has published many articles on this debate. See, e.g., Basin and Farly, *Prostitution debate*, September 6, 2010); *A job like any other*, August 8, 2014; *A personal choice*, August 9, 2014.

[12]In a theoretical model, Lee and Persson, 2015 show that decriminalizing prostitution increases the size of the sex market by reducing the costs of entry. Using country cross-sectional data, Cho, 2018; Cho, Dreher, and Neumayer, 2013 argue that legalized prostitution leads to an expansion of the prostitution market, and an increase in human trafficking.

[13]These results are in line with Cunningham and Shah, 2017, who show that decriminalizing prostitution affects the health outcomes of both sex workers and non-sex workers.

by reason of age. We define such businesses more narrowly, only considering four types – strip clubs, gentleman's clubs, adult entertainers and escort girl services.

In the early 1990s the NYC Division of City Planning published a report on the nature and impact of adult entertainment establishments on the city

In October 1995, following this study, the New York City Council amended its zoning regulations to restrict the location and size of adult entertainment establishments and to disperse such businesses across different areas (i.e. decrease their concentration in certain neighborhoods).[14]

The New York City zoning amendment applies to all sorts of *adult establishments*, including adult bookstores and adult cinemas, that are not studied in this article. The amendment does not ban adult establishments; it simply requires that they: (1) must be located at least 500 feet from a school, house of worship, day care center, or residential district; (2) must be located at least 500 feet from any other adult establishment; (3) must be limited to one establishment per zoning lot; and (4) must not exceed 10,000 square feet of floor space. None of these features are related to the distribution of sex crimes.

1.2.2 Adult entertainment establishments and indoor prostitution

Recent literature has documented that most prostitution takes place indoors in massage parlors and saunas, brothels, strip clubs, and escort prostitution services (Farley, 2005; Urban Justice Center, 2005). Hence, the adult entertainment establishments considered in this article may represent a share of the prostitution market.

The US prostitution market in stratified into three segments.[15] The lowest rung of the ladder is formed by outdoor prostitution (i.e. street prostitutes), which is usually run by pimps. Hence, street prostitutes lack control about their choice of clients, earnings and health checks. They also tend to be younger and are more likely to be victims of violence, to be arrested or to be drug addicted. Strip clubs and gentlemen's clubs comprise the medium rung of the ladder. In this sector prostitution is run as a business; prostitutes might lack control over their clients but enjoy higher earnings, safer controls and more frequent health checks. Self-employed escort girls occupy the top rung. In this market segment, prostitution is professionalized: since prostitutes are not *pimped*, they have control over their customers, earnings, health status and "careers."

Nonetheless, even sex workers on the medium and high rungs face many difficulties. A recent paper documents the close connection between strip clubs, gentleman's clubs and escort girls services to prostitution in NYC (Urban Justice Center, 2005). The majority of indoor prostitutes studied in this report lived precarious lives, and encountered similar problems faced by street-based prostitutes, including violence, constant fear of police interference, and a lack of substantive support services.

1.3 Data

NYC is divided into five boroughs: the Bronx, Brooklyn, Queens, Manhattan and Staten Island. The data are organized in a panel of observations of 77 police precincts in NYC from January 1, 2004 to June 30, 2012. We combine two sets of data: police stops and adult entertainment establishment data. For robustness checks, we use police complaint data.

[14] For further information see Department of State New York State, 1998

[15] For further information, see Church et al., 2001, Albert, 2002, Shively et al., 2012 and Ciacci, 2017.

1.3.1 Sex crimes: "Stop-and-Frisk" data set

Sex crimes in the main specification are drawn from the NYPD "Stop-and-Frisk" data set which provides information on each "Stop-and-Frisk" encounter. This data set has three convenient features. First, it minimizes the problem of self-reporting of sex crimes, since the data comes directly from what the NYPD saw in the street. Previous studies have relied on self-reported measures, which most likely suffer from a high degree of non-random under-reporting. There are multiple reasons why respondents may under-report, including fear of the aggressor and the social stigma associated with victims of these crimes. Second, this data set can be easily used at the daily level since crimes are counted according to when the officers report it. Other data sets document information about crimes that happened during a given time period without documenting the number of occurrences. Thereby, it is difficult to compare them or to use them at the daily level. Third, the "Stop-and-Frisk" data have information on the exact position, hour and day of the crime, which is crucial for the analysis. Furthermore, this data set includes prostitutes' and sex abusers' demographic characteristics such as age, gender and race, which we use to disentangle the mechanisms behind the effects.

The "Stop-and-Frisk" data set contains 7,478 stops for sex crimes (sexual abuse and rape) in NYC.[16] Table 1.1 presents the summary statistics of sex crimes per day. We observe that on average only 0.0313 sex crimes were committed in each precinct per day. Sex crime data have substantial variation over years and precincts. Figure 1.1 shows that the number of sex crimes stayed constant from 2004–2007, after which they peaked, dropped, and increased again. In addition, the data does not present any similar pattern over boroughs.

The total number of sex crimes presents considerable differences across boroughs. Table 1.2 (Panel A) shows that sex crimes are concentrated in the borough of Manhattan (3,844 during the 8.5-year study period). Brooklyn and Queens have roughly half as many sex crimes as Manhattan (1,464 and 1,646, respectively). These patterns motivate the inclusion of geographical fixed effects, time trends and clustered variance at the precinct level in the main specification.

Since the total number of sex crimes also varies by season, we include month fixed effects in the analysis. Table 1.2 (Panel B) presents these results. The fewest sex crimes are committed in the winter. There is also substantial variation in the number of sex crimes committed across precincts within a given borough. For example, in Manhattan the highest proportion of sex crimes is concentrated in Precinct 14 (28%), followed by Precinct 13 (16%).[17]

Men commit 95% of sex crimes, and the percentage of such crimes committed by men on weekdays vs. weekends is relatively constant (Table 1.3). Sex crimes are not concentrated on particular days of the week (Figure 1.2) or particular hours of the day.[18]

[16] Appendix Section A.1 contains precise information on the categories used to count sex crime occurrences.

[17] Precincts 13 and 14 are both located in midtown Manhattan. The former is primarily a commercial and entertainment-oriented precinct. The latter is home to several residential complexes, insurance companies and major health care facilities. Further descriptions are available in the NYPD database.

[18] Table A.1 in Appendix Section A.2 shows the total number of sex crimes committed on weekends vs. weekdays and divides weekend days into four different parts: morning (6 A.M. to 12 P.M.), afternoon (12 P.M. to 6 P.M.), evening (6 P.M. to 12 A.M.) and night (12 A.M. to 6 A.M.).

1.3.2 Adult entertainment establishments

The second data set was obtained from Reference USA and provides information on all registered adult entertainment establishments from 2004–2012 in NYC. It contains data about the year when each establishment was registered, the number of employees in each establishment and its geographic coordinates. Using businesses' records such as the Yellow Pages, Superpages, and the NY State Department of State records, we match almost every establishment with an opening and/or registration date, and sometimes also with a closing date.[19]

We use these two data sets to construct a panel counting the total number of establishments in each precinct for each day of the period of observation. We mainly used three sources to determine the opening date of the establishments. The first two are the Yellow Pages and Superpages, which are telephone directories of businesses organized by category. Advertising a business in these directories is free, and it takes at most five business days to get an establishment advertised after applying online. Since owners have to supply their name and phone number, the ads are likely to be accurate. The third source is the Department of the State of NY, which records every business in the state; for each business it provides detailed information including jurisdiction, address, current entity status, etc. In some cases the names of the establishments are different from those they used to register with the Department of the State of NY's database, so they cannot be matched. This problem does not apply to Yellow Pages and Superpages, since the name of the registered business is the same as that used to register with Reference USA.

The number of adult entertainment establishments increased significantly during the period of observation from 76 in 2004 to approximately 280 in 2012. Thus, the data include roughly 200 openings of adult entertainment establishments during the 8.5-year study period. We use this variation to identify the effect of adult entertainment establishments on sex crime. Figure 1.3 displays the evolution of adult entertainment establishments during the sample period. Appendix Section A.3 shows the geographic evolution of such establishments across precincts.

Column (2) of Table 1.2 shows that adult entertainment establishments' openings are concentrated in Manhattan (75%, 150 out of 206) and in the summer (34%, 70 out of 206). Table A.2 shows that the openings are roughly equally distributed between weekends and weekdays (90 vs. 116, respectively). Figure 1.4 illustrates that openings are not more likely to take place on a particular day of the week. The distribution of sex crimes over days of the week looks balanced: sex crimes do not appear to happen more often on a given day. Given these findings, we conclude that openings do not take place more likely on any particular day of the week.

1.3.3 Sex crimes: complaint data set

To check the robustness of our results in Section 1.5.4 we also use data on sex crimes from two different versions of the NYPD Complaint Data Historic.

First, we use the disaggregated data set at the daily level. We refer to this data set as the Complaint Disaggregated data set. This data set contains all valid felony, misdemeanor, and violation crimes reported by legal complaint to the NYPD. In this data set crimes are recorded according to the time range in which they took place (i.e. for each crime a starting date and an ending date can be reported and in some cases one of the two is missing). While the information is recorded, the classification is

[19] We were able to match 90% of the adult entertainment establishments found in Reference USA. In our data set we observe only a closure of such establishments.

carried out in this way since the NYPD is concerned with how long the crime lasted. Yet, for our purposes we need to quantify how many times that crime occurred in a certain number of days.

Second, we use the aggregate version at the yearly level of the NYPD Complaint Data Historic. We refer to this data set as the Complaint Aggregated data set. This data set also contains all valid felony, misdemeanor, and violation crimes reported by legal complaint to the NYPD. However, this data set accumulates total crimes occurred at the precinct and year levels. This allows us to precisely quantify the number of times a certain offense takes place. This data set will be useful to compare the distribution of sex crimes across the two data sources (i.e. "Stop-and-Frisk" and Complaint). Unlike the former database, these two data sets do not include any information on the aggressor. Moreover, none of these two data sets geocodes the location of sex crimes, but includes the precinct of occurrence, which allows for precinct-by-precinct comparisons.

Both data sets only include valid complaints. Complaints judged unfounded due to reporter mistakes or misinformation (or invalid due to internal errors) are excluded, since they are not reflected in official figures and thus are not considered to have occurred in a criminal context.[20] Also, since *mala prohibita* crimes do not require a complaint report, they may not be represented accurately, or at least in the Complaint Disaggregated data set. Such incidents are usually recorded using other department forms, such as arrests and summonses. These offenses include (but are not limited to) certain drug, trespassing, theft of service, and prostitution charges.

Appendix Section A.8 compares descriptive statistics between the complaint data set and the "Stop-and-Frisk" data set. The distribution of sex crimes in the complaint data set is substantially similar to that of the "Stop-and-Frisk" data set.[21]

Unlike the "Stop-and-Frisk" data set, data from the NYPD Complaint Data Historic might include domestic violence cases as sex crimes. This seems plausible for two reasons. First, the domestic violence category does not exist in this data set. Second, since these crimes happen indoor and are self-reported the victim might report domestic violence cases as sex crime.

1.4 Identification strategy

Similar to Dahl and DellaVigna (2009), we estimate the following specification:

$$\log\left(Sex\,Crime_{pt}\right) = \beta Adult\,Enter_{pt} + \Gamma X_{pt} + \varepsilon_{pt} \tag{1.1}$$

The dependent variable is the logarithm of one plus the number of sex crimes committed in precinct p on a given day t.[22] $Adult\,Enter_{pt}$ denotes the total number of adult entertainment establishments in precinct p for day t. This variable accumulates the opened businesses up until day t. X_{pt} represents a set of seasonal and geographic control variables: indicators for precinct, year, month, day of the week, day of the year and holidays, and geographic (precinct level) year trends. All standard errors are clustered at the precinct level.

The identification strategy relies on the exogeneity of variation in the time of openings and registration of adult entertainment establishments across precincts in

[20] Investigation reports are not included either, in order to guarantee relevance and lessen extraneous material.

[21] Figures similar to those explored for the "Stop-and-Frisk" data set are available upon request.

[22] We use $\log(1+y)$ since our dependent variable takes a value of 0 on days that no sex crimes were committed. In Section 1.5 we test the robustness of this functional form.

NYC. The main assumption is that opening and registration dates are exogenous in a model of daily crime. Given that opening a business in NYC requires a long bureaucratic procedure we can take the day as random. Since our specification is daily, this amounts to the opening date of a business being exogenous to any other factor affecting sex crime. The comparability of the treatment and control groups boils down to the comparability of NYC police precincts over time. Thus, our specification captures any confounding factor that varies at the precinct or day level. The inclusion of precinct time trends ensures that $\hat{\beta}$ is not capturing any effect simply due to temporal changes in trends by precinct.[23]

One potential threat could be measurement error in the dependent variable and/or the explanatory variable. On the one hand, measurement error in the former could easily arise if we do not observe all the sex crimes committed in NYC (i.e. if sex crimes are committed but are not seen by the officers). However, assuming that the measurement error is random, this problem would produce larger standard errors, suggesting that the level of statistical significance of the coefficient is smaller than what we found. Measurement error is an issue in every crime data set, and even more in data related to sex crimes. Measurement error in the crime economics literature is mostly due to victims choosing not to report the crime (especially sex crimes). Nonetheless, we believe using the "Stop-and-Frisk" data set minimizes this concern since victims do not decide whether or not to report the crime. Therefore it seems reasonable to assume that there is less measurement error than in data sets based on complaints. On the other hand, measurement error in the explanatory variable might arise if these businesses are not registered in the Reference USA database. In this case, assuming that this measurement error is random would lead to attenuation bias, suggesting that the population regression function's coefficient is negative but larger in absolute value than our estimates.[24]

1.5 Results

This section shows that adult entertainment businesses can reduce sex crimes by 13% per day per precinct. This result is robust to different specifications and to using different data sets to measure sex crimes. Moreover, effects are persistent over time and there is no evidence of the existence of pre-trends. Future openings of adult entertainment establishments have no effect on sex crimes.

1.5.1 The effect of adult entertainment establishments on sex crime

Table 1.4 presents the results. Column (1) presents the correlation between the opening of an adult entertainment establishment and sex crimes including precinct fixed effects. Columns (2) and (3) add month and year fixed effects. In all the specifications the coefficient is statistically significant and negative, indicating that having an adult entertainment establishment in a certain precinct is negatively associated with the number of sex crimes.

[23] A critique of this specification could be that the stable unit treatment value assumption is not satisfied, since the number of adult entertainment establishments in a precinct could affect the number of sex crimes in bordering precincts. We address this issue in the mechanism analysis (when we explore the *potential victims channel*).

[24] There is no reason to believe that some adult entertainment establishments would prefer not to appear in Reference USA since their activity is totally legal. Yet even if this were the case, there is no evidence to suggest that such mismeasurement would not be random.

Since it is plausible that crime patterns may differ throughout the week, during the year and in holidays, Columns (4)–(6) present the results based on the day-of-the-week, day-of-the-year and holiday indicators, respectively. The results do not change.

Column (7) presents the results with the inclusion of precinct-year trends, which increases the absolute value of the coefficient. This pattern suggests that omitted variables were attenuating the estimated coefficient. This is the preferred specification, it shows that having an adult entertainment establishment decreases the number of sex crimes by roughly 13% per day in a particular precinct .[25]

1.5.2 Sensitivity to model specification changes and the definition of the dependent variable

This section explores the robustness of the results to different specifications. First, we replace the day-of-the-year and holiday indicators with exact-day indicators so that each day in the study period has its own fixed effect that captures any day-to-day differences. Second, we include precinct-month trends instead of precinct-year trends. Third, we include different precinct trends based on every month of each year and drop the precinct-year trends. The main difference is that precinct-year trends were varying in each precinct across years, while these are varying across each month of the year. For example, in this specification January 2004 has a different trend than both February 2004 and January 2005. Columns (1) to (3) in Table A.3 report the results of these three specifications. All estimates are negative and statistically significant in each of the three specifications, and the magnitude of the effect does not change.

Column (4) presents the estimates of Equation (1), but only for sex crimes committed by male offenders. As before, we include all the fixed effects and precinct time trends in the specification. The results do not change, which is consistent with the fact that male offenders commit the large majority of sex crimes. In line with these results, Column (5) displays the outcomes of running this regression using the inverse hyperbolic sine (IHS) transformation of the dependent variable.

Table A.4 presents the results of using different transformations of the dependent variable. First, we apply the IHS transformation. In our main specification the dependent variable is $\log(1+y)$, while in this specification using the IHS it becomes $\log\left(y+\left(y^2+1\right)^{\frac{1}{2}}\right)$. The IHS is commonly used where there are fat tails (Pence, 2006). Column (1) of Table A.4 shows the results of running such a regression. In line with our main findings, the estimated coefficient is statistically negative but larger in absolute value.

Another concern could be that the effect is driven by extreme values of the dependent variable. To address this issue, Columns (2) and (3) of Table A.4 correspond, respectively, to a probit and a linear probability model (LPM henceforth) using a dummy variable that takes a value of 0 when no sex crimes are committed, and 1 otherwise. The coefficient of interest is negative and statistically significant at standard levels in the LPM. Finally, we estimate the model in levels form and find a negative, statistically significant coefficient in this case as well (Column(4) of Table

[25] Taking into account the transformation of the dependent variable, the effect can be computed using the following formula:

$$\frac{\partial \log(y)}{\partial x} = \frac{\partial \log(1+y)}{\partial x}\frac{\partial \log(y)}{\partial \log(1+y)} = \beta\frac{1+y}{y} \simeq \hat{\beta}\frac{1+\bar{y}}{\bar{y}} = -0.4\%\frac{1+0.0313}{0.0313} = -13.18\%$$

A.4). In this specification, an extra establishment decreases sex crimes by 0.0076 units. This is equivalent to a 23% reduction.[26]

Our findings are also robust to changes in the time unit of the regression. Table A.5 shows the estimated coefficient if we run our main specification at weekly frequency. In the next section, we show the estimated coefficient at the monthly level as well, and the results do not change.

1.5.3 Falsification test

In this section we investigate whether the decrease in sex crimes is caused by a contemporaneous increase in adult entertainment establishments or by its leads or lags. This exercise is similar to the one carried out by Dustmann and Fasani, 2016 and serves as a falsification test since, if the identification assumption holds, future values of adult entertainment establishments should have no effect on sex crimes.

Our setting has two features that should be taken into account. First, the identification relies on the exogeneity of the variation in the timing of the openings and registration of adult entertainment establishments across precincts in NYC. Yet, our data set does not specify the exact opening day, which could be days or weeks after the registration date. Thus, we collapse the data set at the precinct-month level. Second, the regressor of interest accumulates the number of adult entertainment establishments in a certain precinct. As Table 1.2 shows, there were 206 openings in the sample period. Hence, even collapsing the data set at the monthly level, the correlation of adjacent changes is extremely high (0.9983). Given these two features we include the first lag and lead of the main regressor and we estimate the following regression model:

$$\log\,(Sex\,Crime_{pt}) = \sum_{j=-1}^{1} \beta_j Adult\,Enter_{p,t+j} + \Gamma X_{pt} + \varepsilon_{pt} \tag{1.2}$$

where X_{pt} includes month fixed effects, year fixed effects, precinct fixed effects and precinct-year time trends. Column (1) in Table 1.5 presents the results using only the contemporaneous (i.e. $j = 0$) number of adult entertainment establishments. In this regression the coefficient of interest is negative and statistically significant. These results are in line with our main specification's findings. Column (2) displays the results of running Equation (3) using only the forward value of the main regressor (i.e. $j = 1$). We find that the number of future adult entertainment establishments has no effect on contemporaneous sex crimes. Column (3) shows the results using only the lag of the main regressor (i.e. $j = -1$) as the regressor. It has a sizable and significant effect on contemporaneous sex crimes, showing some persistence of the effect. Column (4) includes the leads and lags, as in Equation (2). In line with the identification assumption, we find that future values of the main regressor have no effect on sex crimes (this coefficient even flips the sign). Moreover, there is more evidence that the effect persists: the lagged value of the main regressor preserves its size and statistical significance. The loss of significance of the contemporaneous value of the main regressor is also due to a decrease in precision: standard errors more than double when comparing Column (1) to Column (4). However, this is not

[26]This last specification is the most sensitive to extreme values, which is probably why the estimated coefficient is the largest (in absolute value) of all the specifications considered. Appendix Section A.9 presents all the results in levels. The results are larger in absolute value but do not change.

the case for the forward value of the main regressor: its standard error increases very slightly and the coefficient even flips sign.[27]

1.5.4 Representability of "Stop-and-Frisk" data set

Sex crimes drawn from the "Stop-and-Frisk" data represent only a share of all the sex crimes in NYC. If such data were not representative of all the sex crimes occurring in NYC, our findings would not be either.

In this section we address this issue in two different ways. First, we use high-frequency data drawn from the NYPD's historical complaints data set that fit into our specification. Second, we use aggregate (low-frequency) data to determine whether the "Stop-and-Frisk" data set is representative of the patterns of all sex crimes recorded by the NYPD.

Disaggregated complaint data at high frequency

We build a database that includes complaint sex crimes and perform the same analysis as for our main specification. Columns (1) and (2) of Table 1.6 present the results of this regression using the logarithmic transformation or the IHS, respectively.[28] In both cases, the coefficient of interest is statistically negative at standard levels and larger in absolute value than the estimated coefficient of the main specification, indicating that indoor sex crimes decrease as well. We find that the opening of an adult entertainment business decreases sex crimes by approximately 7%.[29] The fact that the magnitude of the estimated coefficient is not statistically different from using the "Stop-and-Frisk" data set even if the magnitude of the effect is different suggests that the results are not driven by biases in the "Stop-and-Frisk" data.[30] On the contrary, the fact that the standard errors associated with the estimated coefficients are almost twice as large as using the "Stop-and-Frisk" data suggests that the complaint data set, as expected, might suffer from random measurement error. If this is the case, the population regression coefficients of the regression models considered in Columns (1) and (2) of Table 1.6 are statistically significant at lower levels of significance.

Aggregated complaint data at low frequency

This section explores whether sex crimes in the "Stop-and-Frisk" data set are representative of all sex crimes recorded in NYC. Using the complaints data set with our specification is problematic, since the occurrence of such crimes is not recorded on a daily basis. To solve this problem we use low-frequency data about all sex crimes

[27] Appendix Figure A.3 shows the estimated coefficient with the respective 95% confidence intervals of the regression model associated with Column (4). Table A.6 shows the results of running the same analysis but using the IHS transformation as the dependent variable. The results do not change. Moreover, Appendix Figure A.4 shows the estimated coefficient with the respective 95% confidence intervals of the regression model associated with Column (4) for IHS transformation.

[28] Table A.11 in Appendix Section A.9 shows the results of running such regressions in levels.

[29] In this case, computations differ since the average value of the dependent variable is 0.1118. Therefore, using the same formula as before

$$\frac{\partial \log(y)}{\partial x} = \frac{\partial \log(1+y)}{\partial x}\frac{\partial \log(y)}{\partial \log(1+y)} = \beta\frac{1+y}{y} \simeq \hat{\beta}\frac{1+\bar{y}}{\bar{y}} = -0.7\%\frac{1+0.1118}{0.1118} = -6.96\%$$

[30] This might seem plausible since "Stop-and-Frisk" data depend on surrounding characteristics, such as: number of people in the street, number of officers in the street, other types of crimes happening in the surroundings, etc. Note that this issue is also addressed by the inclusion of exact-day indicators in Table A.3.

committed in NYC, which is available from the NYPD.[31] This data already calculate the number of occurrences of each crime. Yet, since this data is at the precinct-year level, we cannot use it in our main specification or rely on the identification assumption. Therefore we run the following specification:

$$Sex\,CrimeSF_{pt} = \delta Sex\,CrimeNYPD_{pt} + \Gamma X_{pt} + \varepsilon_{pt} \tag{1.3}$$

where $Sex\,CrimeSF_{pt}$ and $Sex\,CrimeNYPD_{pt}$ are sex crimes from the "Stop-and-Frisk" and NYPD data set, respectively, and X_{pt} include year fixed effects, precinct fixed effects and precinct-year time trends. The correlation δ captures whether sex crimes from the two data sets are correlated, netting out time and geographic differences. Column (3) in Table 1.6 shows the results for this specification. These findings demonstrate that even if the year-to-year changes and geographic distribution differ across the two data sets, and even if the number of sex crimes in the "Stop-and-Frisk" data set is lower than in the NYPD data set (7,478 reported sex crimes in the former, compared to 52,910 in the latter), the sex crimes from the "Stop-and-Frisk" data set can be representative of all sex crimes recorded in NYC.

Even taking precinct and year fixed effects and year trends into account, we find that sex crimes drawn from the "Stop-and-Frisk" data set are closely correlated with the complaint sex crimes. Column (4) of Table 1.6 includes precinct-year trends, and we find that sex crimes drawn from the "Stop-and-Frisk" data set represent around 27% of complaint sex crimes. In other words, for every four complaint sex crimes, there is one sex crime from the "Stop-and-Frisk" data set. The results at the IHS level are substantially similar. As a further robustness check, Appendix Table A.7 shows the same regression but using the Complaint Disaggregated data set at the daily level as the regressor. These two different measures of sex crimes are positively significantly correlated in this regression as well.

1.5.5 Placebo test: randomization inference

To address the concern that the data are highly serially correlated across precincts, all our regressions are clustered at the precinct level. Yet, this section presents a further test to explore this concern. In this section we present the results of randomizing the number of adult entertainment establishments across precincts.[32]

Appendix Figures A.5 and A.6 present the results of randomizing the number of opened establishments stratified at the borough level with 1,000 permutations. In the latter, the red vertical line represents the estimated coefficient in our main specification. The intersection between the red vertical line and the estimated distribution could be interpreted as the probability of finding the same effect found in our main specification by chance.

Figure A.6 shows that finding the same estimated coefficient as in our main specification is extremely unlikely: out of 1,000 permutations, none could replicate the estimate. This finding seems to exclude the possibility that our estimates were driven by serial correlation across precincts.[33] Appendix Section A.11 presents the same figures without stratifying at the borough level.

[31] http://www1.nyc.gov/site/nypd/stats/crime-statistics/historical.page

[32] Similar approaches and results are developed in Pinotti, 2017 and Aglasan, Guiteras, and Palloni, 2017.

[33] These results are robust to using 10,000 permutations. Figures are available upon request.

1.6 Mechanisms driving the effect of adult entertainment establishments on sex crimes

This section explores three mechanisms that can help explain the decrease in sex crimes caused by adult entertainment establishments: police channel, potential victims channel and potential criminals channel. Each of these mechanisms can be tested using our database.

First, it could be the case that adult entertainment establishments reinforce security in the precinct if more police officers are assigned to the area. In this case, a decline in sex crimes could reflect a general decline in crime due to the higher number of officers present in the area after an establishment is opened (police channel).[34] Given our identification strategy, this would imply that the number of police officers increases at the same time (i.e. on the same day) that a new adult entertainment establishment opens in a certain precinct. Second, women may be avoiding precincts where adult entertainment businesses have opened and are moving to bordering precincts where there are no establishments. Thus, the decline in crime could be explained by a reduction in potential victims. It could also be the case that adult entertainment establishments are employing potential street sex workers who, in absence of opportunities for indoor prostitution, would work on the streets. If most sex crimes are committed against street sex workers, adult entertainment establishments might reduce sex crimes by merely providing protection to street workers (potential victims channel). Finally, potential offenders might prefer to use adult entertainment establishmentsâĂŹ services instead of committing sex crimes (potential criminals channel).

1.6.1 Police channel

The ideal way to explore the police channel is to use data about the number of police officers working in each NYC precinct on each day. However, since this data is not publicly available, to explore this mechanism we estimate the effect of adult entertainment businesses on other crimes, such as the number of stops for drugs use and the number of burglaries from the "Stop-and-Frisk" data set. Table 1.7 presents the results of this specification. Each specification resembles Equation (1) but with a different dependent variable – the number of stops for drug use (Column (1)) and the number of burglaries (Column (3)). In these specifications we cluster the variance at the precinct level and include precinct, year, month, day-of-the-week, day-of-the-year and holiday indicators and precinct-year trends.

If sex crimes decline because there are more police officers in the area when an adult entertainment establishment opens, we should also find a decrease in the number of crimes that are more frequent and easier to control, such as burglaries and drug use. However, we find no effect of adult entertainment establishments on these crimes, suggesting that an increase in security is not the main channel behind the decline in sex crimes.

Furthermore, the results of this specification suggest that adult entertainment establishments have no effect on crimes other than sex crimes (e.g. drugs and burglaries, which might be affected by the number of these establishments). Columns (2) and (4) repeat the same analysis but using the IHS transformation of the two crimes, and again there is no significant effect. These results do not support the police channel. In Appendix Section A.12 we provide further analysis and evidence

[34] Draca, Machin, and Witt, 2011 and Di Tella and Schargrodsky, 2004 provide evidence on how increasing the number of police officers reduces crime.

by analyzing the effect of adult entertainment establishments on 10 different crimes. Table A.19 shows that we find no empirical evidence supporting the police channel. Furthermore, in Section A.13 of the Appendix we run all the robustness checks using these 10 different crimes. No other crime presents a decrease pattern similar to that of sex crimes. These findings therefore do not support the police channel.[35]

1.6.2 Potential victims channel

To explore the potential victims channel, we estimate two models. First, to determine whether adult entertainment establishments are changing the location of street sex workers, we estimate Model (1) but replace the dependent variable with street prostitution stops. If this were the case, we would observe that the number of adult entertainment establishments has a negative effect on the number of street prostitutes. The results of this specification are reported in Columns (1) and (2) (Panel A) of Table 1.8. We find no statistically significant effect on this new outcome. This result suggests that there has not been a reallocation of street sex workers to adult entertainment businesses, and it rules out the possibility that the decline in crime is driven by a reduction of street sex workers who could be the main potential victims of sex crimes in the street.[36]

The New York State Division of Criminal Justice Services classifies loitering as including "loitering for Prostitution."[37] Thus, Columns (3) and (4) in Table 1.8 present the same analysis but for loitering. Both coefficients are positive and not statistically significant. Hence, we conclude that there is no evidence that the reduction in sex crimes is due to a reallocation of outdoor sex workers to indoor venues.

Second, we also analyze whether there is a spillover effect caused by women moving to other precincts. If women are simply avoiding precincts with adult entertainment establishments, we should observe an increase in sex crime in neighboring precincts. We consider a specification with 22 precincts in which we group precincts on the basis of their geographic position. For example, we group Precincts 1, 5 and 7 together; Precincts 6, 9, 10 and 13 together, and so on. A complete list of groupings is available in Appendix Section A.14. If the effect found is only due to women avoiding precincts with adult establishments, then we would observe sex crimes moving from one precinct to another. Therefore, this would imply that sex crimes are increasing in precincts with no establishments but which have neighboring precincts with at least one establishment. If this were the case, the total effect in larger precincts should compensate and be closer to zero than the main estimated coefficient. If sex crimes are not moving, the coefficient should still be negative and larger in absolute value since we are taking into account larger geographic units.

Panel B in Table 1.8 presents the results. We still find a negative effect on sex crimes. Since in these regressions there are only 22 precincts, standard errors could be smaller due to the smaller number of clusters. Therefore, Columns (3) and (4) in Panel B present the same regressions but using wild cluster-bootstrap methods. The results do not change. Overall, the findings do not support the notion that women avoid precincts where adult entertainment establishments are located. In Appendix

[35] These findings are in line with Linz et al., 2004.

[36] A further concern is that sex crimes transfer from other women to indoor prostitutes. Three points are worth mentioning in this regard. First, Section 1.5.4 provides evidence against this since indoor sex crimes decrease as well. Second, we acknowledge this concern would be difficult to address since there is evidence in the literature that prostitutes in the U.S. rarely report sex crimes (Anderson, 2004). Third, there is evidence in the literature that adult entertainment establishments provide protection to their workers, making this concern particularly unlikely (Church et al., 2001; Shively et al., 2012).

[37] For further information, see Urban Justice Center, 2005.

Section A.15, we also perform other robustness checks which provide further evidence that sex crimes are not moving to neighboring precincts.

1.6.3 Potential criminals channel

To address the potential criminals channel, we focus on sex crimes committed at night. If potential criminals prefer to use adult entertainment establishmentsâĂŹ services rather than commit sex crimes, the effect should be larger when the supply of the services offered by these establishments is higher. It seems plausible to assume that the supply of these services is higher at night, given that most of these establishments are only open at night.

We divide the day into four quarters – morning (from 6 A.M. to 12 P.M.), afternoon (from 12 P.M.to 6 P.M.), evening (from 6 P.M.to 12 A.M.) and night (from 12 P.M.to 6 A.M.) – and create four corresponding dummy variables and saturate the model with the interactions. Table 1.9 presents the results of the fixed effect at evening and night, and their corresponding interactions. As benchmarks, Columns (1) and (3) of this table present the results for our logarithmic transformation and IHS, respectively, without the interactions. Columns (2) and (4) present the results of the fully saturated model. The results in Table 1.9 corroborate the initial finding: the two interaction coefficients are jointly statistically significant and negative at the 1% level. In addition, their total effect is statistically different from zero at the 10% level. These results imply that we cannot reject the potential criminals channel.

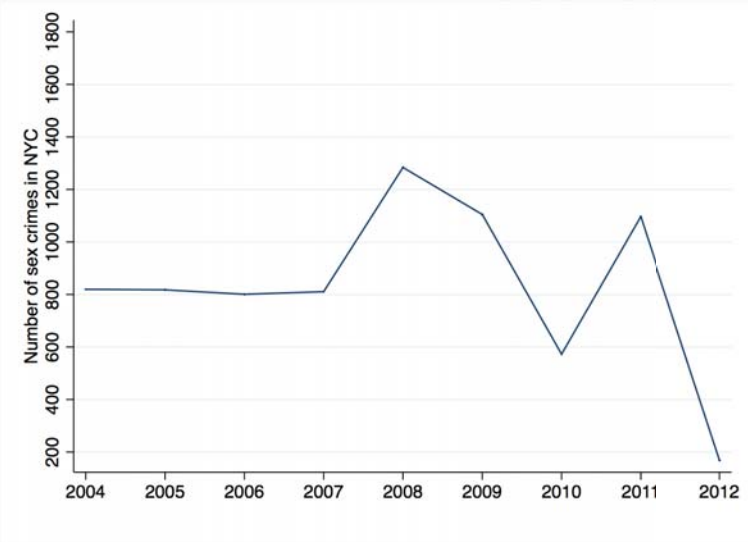
1.7 Conclusion

This paper presents the first causal estimates of the effect of adult entertainment establishments on sex crimes. Using high-frequency daily data for all NYC, we find that opening adult entertainment establishments reduces sex crimes by 13%, and that these effects are driven by potential customers who substitute sex crimes with services provided by adult entertainment businesses.

These results have several policy implications. First, while previous academic and policy research has focused on the role of deterrence policies, here we focused on an alternative tool – providing legal substitute services. Second, adult entertainment establishments appear to be a viable alternative to decriminalizing prostitution. Indeed, their effect on rape is similar to the one of decriminalizing prostitution, but prostitution law is a contentious issue, regulation of these establishments is not. Third, the fact that these services are legal may explain why we do not find an increase in other types of crimes. Fourth, the results show that providing substitute services may have positive externalities not only for sex workers but also for all women in the areas where these businesses opened.

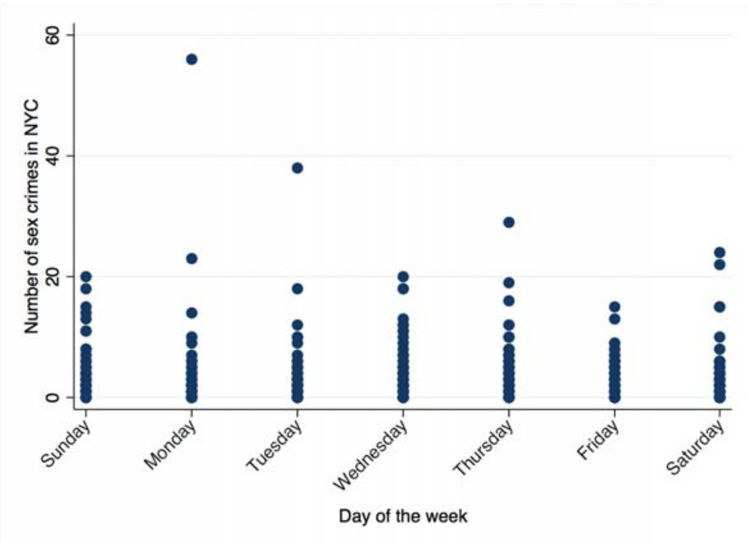
Figures & Tables

FIGURE 1.1: Evolution of sex crimes in NYC from January 2004 to June 2012



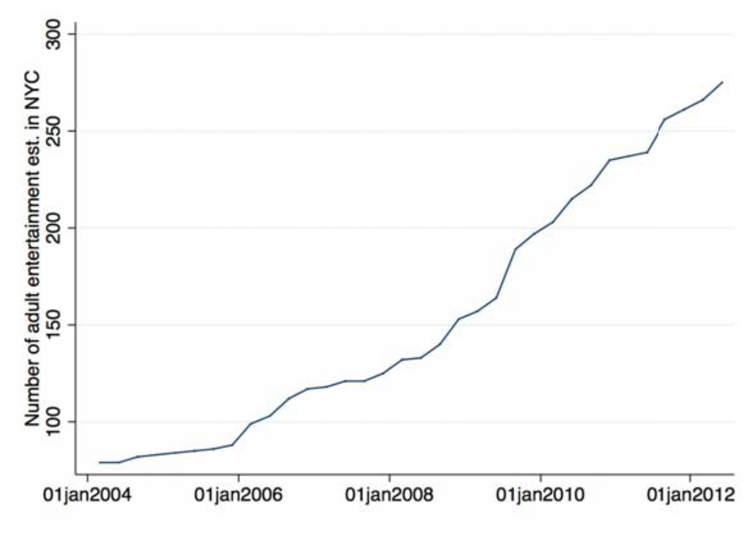
Notes: This figure shows the evolution of sex crimes in NYC between January 1, 2004 and June 30, 2012. For this picture, data has been collapsed yearly. Note in 2012 we only have data till June.

FIGURE 1.2: Distribution of sex crimes over days of the week



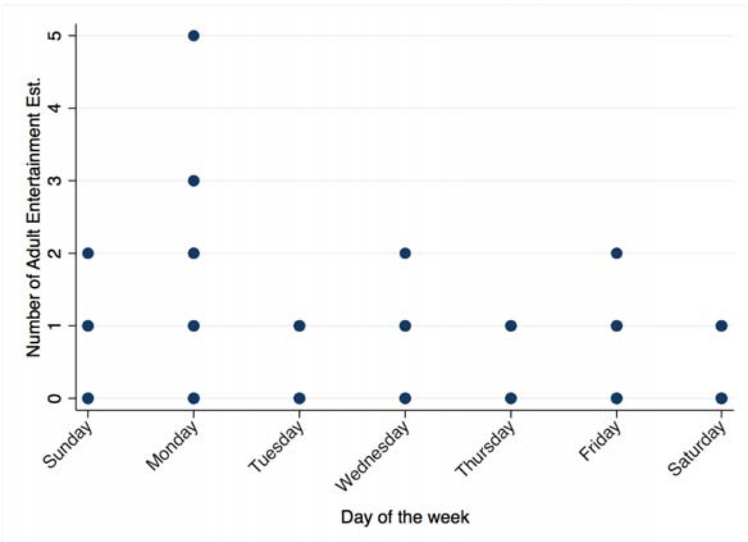
Notes: This figure shows the distribution of sex crimes across days of the week in NYC during the study period.

FIGURE 1.3: Evolution of adult entertainment establishments from January 2004 to June 2012



Notes: This figure shows the evolution of adult entertainment establishments in NYC during the study period.

FIGURE 1.4: Opening of adult entertainment establishments by day of the week



Notes: This figure shows the distribution of the day of opening of adult entertainment establishments across days of the week in NYC during the study period.

TABLE 1.1: Total number of sex crimes by day of the week

	(1) Sex crimes	(2) Adult enter. est.
Observations	238,931	238,931
Mean	0.031	1.957
Standard Deviation	0.341	5.128

Notes: This table presents descriptive statistics (mean and standard deviation) during our sample period for sex crimes and adult entertainment establishments. The two statistics are computed using daily data.

TABLE 1.2: Total number of sex crimes and openings by borough and season

Panel A

	Sex crimes by borough	Openings by borough
The Bronx	454	10
Brooklyn	1,464	20
Manhattan	3,844	150
Queens	1,646	24
Staten Island	170	2
Total	7,478	206

Panel B

	Sex crimes by season	Openings by season
Winter	1,554	42
Spring	1,894	39
Summer	2,115	70
Fall	1,915	55
Total	7,478	206

Notes: Panels A and B present the distribution of sex crimes and openings of adult entertainment establishments in our sample period by NYC borough and season, respectively.

TABLE 1.3: Total number and frequency of sex crimes committed by gender

	Sex crimes by male offenders (per day)	Percentage over total
Weekend	2,431	95.9%
-Friday	1,013	96.85%
-Saturday	712	95.57%
-Sunday	706	94.89%
Weekdays	4,776	96.62%
Total	7,207	96.38%

Notes: This table presents the distribution of sex crimes committed by male offenders by day of the week. Column (1) presents the absolute frequency, while Column (2) presents the percentual frequency. As expected, male offenders commit almost 90% of all such crimes. Further sex crimes are not concentrated on weekends.

TABLE 1.4: The effect of adult entertainment establishments on sex crimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adult Entertainment Est.	-0.00209**	-0.00214**	-0.00215**	-0.00215**	-0.00215**	-0.00215**	-0.00401*
	(0.000855)	(0.000947)	(0.000947)	(0.000947)	(0.000948)	(0.000948)	(0.00217)
Observations	238,931	238,931	238,931	238,931	238,931	238,931	238,931
Clustered variance at Precinct level	Y	Y	Y	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y	Y	Y	Y
Year FE		Y	Y	Y	Y	Y	Y
Month FE			Y	Y	Y	Y	Y
Day of the week FE				Y	Y	Y	Y
Day of the year FE					Y	Y	Y
Holiday FE						Y	Y
Precinct Trends							Y
Mean of Sex Crime	0.0313	0.0313	0.0313	0.0313	0.0313	0.0313	0.0313
Std Deviation of Sex Crime	0.3405	0.3405	0.3405	0.3405	0.3405	0.3405	0.3405

Notes: This table presents the results of running $\log\,(Sex\,Crime_{pt}) = \beta Adult\,Enter_{pt} + \Gamma X_{pt} + \varepsilon_{pt}$. The dependent variable is the logarithm of one plus the number of sex crimes committed in precinct p on a given day t. $Adult\,Enter_{pt}$ denotes the total number of adult entertainment establishments in precinct p on day t. This variable cumulates all the opened businesses up to day t. X_{pt} is a set of seasonal and geographic control variables: indicators for precinct, year, month, day-of-the-week, day-of-the-year and holidays, and geographic (at precinct level) year trends. All standard errors are clustered at the precinct level. Note that besides the classical year and month fixed effects, our daily specification allows us to include day-of-the-week, day-of-the-year and holiday fixed effects to capture deeper variation due to timing factors. In each column we add a different control. Clustered standard errors at the precinct level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

TABLE 1.5: Falsification test

	(1)	(2)	(3)	(4)
Adult Entertainment Est. (t+1)		-0.0273		0.0163
		(0.0172)		(0.0216)
Adult Entertainment Est.	-0.0314*			-0.0160
	(0.0185)			(0.0399)
Adult Entertainment Est. (t-1)			-0.0320*	-0.0311*
			(0.0180)	(0.0182)
Observations	7,854	7,777	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

TABLE 1.6: Effect of adult entertainment establishments on sex crimes using complaint data set

	(1)	(2)	(3)	(4)
	Log	IHS	Sex crimes	Sex crimes
	Sex crimes	Sex crimes	Stops	Stops
Adult Entertainment Est.	-0.00672*	-0.0134*		
	(0.00396)	(0.00791)		
Sex crimes, NYPD			0.193*	0.265*
			(0.106)	(0.139)
Observations	238,931	238,931	693	693
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	N/A	N/A
Day of the week FE	Y	Y	N/A	N/A
Day of the year FE	Y	Y	N/A	N/A
Holiday FE	Y	Y	N/A	N/A
Precinct Trends	Y	Y		Y

Notes: Clustered standard errors at the precinct level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

TABLE 1.7: Police channel

	(1) Log drug stops	(2) IHS drug stops	(3) Log burglaries	(4) IHS burglaries
Adult Entertainment Est.	0.00539	0.0108	-0.00769	-0.0154
	(0.00797)	(0.0159)	(0.0137)	(0.0274)
Observations	238,931	238,931	238,931	238,931
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: This table presents the results of exploring the police channel. Columns (1) and (3) present the results of the baseline regression, while Columns (2) and (4) present the results for the IHS of drug stops and burglaries drawn from the Stop-and-Frisk data set, respectively. If sex crimes are decreasing because the number of officers increases in precincts where an adult entertainment establishment opens, other crimes should also decrease–particularly crimes that happen more frequently and that are easier to catch, such as drug stops and burglaries. Clustered standard errors at the precinct level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 1.8: Potential victims channel

	(1)	(2)	(3)	(4)
Panel A				
	Log street prostitutes	IHS street prostitutes	Log loitering	IHS loitering
Adult Entertainment Est.	-0.000636	-0.00127	0.00149	0.00299
	(0.00114)	(0.00227)	(0.000997)	(0.00199)
Observations	238,931	238,931	238,931	238,931
Clustered variance at Precinct level	Y	Y	Y	Y
	(1)	(2)	(3)	(4)
Panel B				
	Log sex crimes	IHS sex crimes	Log sex crimes	IHS sex crimes
Adult Entertainment Est.	-0.00686***	-0.0137***	-0.00686**	-0.0137**
	(0.00223)	(0.00446)	(0.00334)	(0.00668)
Observations	68,266	68,266	68,266	68,266
Clustered variance at Precinct level	Y	Y	Wild	Wild
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Panel A presents the results of exploring the potential victims channel. Columns (1) and (2) present the results for the baseline regression using log and IHS of street prostitutes. If sex crimes are decreasing because street prostitutes, who were victims of sex crimes before, are now working in adult entertainment establishments we would observe a statistical negative estimated coefficient. The results suggest that this is not the case. Columns (3) and (4) repeat the analysis using as dependent variable the stops for loitering. Panel B presents results for the baseline regression using log and IHS of sex crimes but using bigger precincts. These precincts were chosen according to their geographic distance. A complete list of the new precincts can be found in the appendix. If women are avoiding precincts where adult entertainment establishments open, we should find either a statistically negative but smaller estimated coefficient in absolute value, a statistically positive coefficient or a coefficient that is statistically equal to zero . In both cases the estimated coefficients are negative and larger in absolute value than the ones in our baseline regression. This evidence rejects the potential victims channel. Clustered standard errors at the precinct level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 1.9: Potential criminal channel

	(1) Log sex crimes	(2) Log sex crimes	(3) IHS sex crimes	(4) IHS sex crimes
Adult Entertainment Est.	-0.00114*	-0.000373	-0.00229*	-0.000746
	(0.000619)	(0.000292)	(0.00124)	(0.000584)
Dummy Evening		0.00160**		0.00320**
		(0.000715)		(0.00143)
Dummy Night		0.00105		0.00211
		(0.00101)		(0.00202)
Interaction Evening		-0.000954*		-0.00191*
		(0.000567)		(0.00113)
Interaction Night		-0.00146		-0.00292
		(0.000939)		(0.00188)
Observations	955,724	955,724	955,724	955,724
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y
p-value joint effect		0.0841		0.0841
p-value		0.00792		0.00792

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

Chapter 2

The Effect of Unilateral Divorce on Prostitution: Evidence from Divorce Laws in U.S. States

2.1 Introduction

Prostitution is a gender issue. According to HG.org, 2017, out of the total arrests for prostitution in the U.S., 70% are female prostitutes, 20% are either male prostitutes or pimps and the remaining 10% are prostitutes' clients.

Since the 1960s, fighting prostitution has been a key target of many American policy interventions (Shively et al., 2012).[1] Recently, there have been important policy debates on prostitution (Della Giusta, 2016; Yttergren and Westerstrand, 2016). In particular, in 2014 the European Parliament voted in favour of a resolution to criminalize the purchase of prostitution. According to this school of thought, whether it is forced or voluntary, prostitution is a violation of human rights and human dignity. Prostitution laws aside, little is known about how to reduce prostitution.

In this paper, I explore the effect of a seemingly unrelated policy on prostitution activities, namely, the approval of unilateral divorce laws in several US states. Although the link between divorce regimes and prostitution may look weak at first sight, there are several channels through which such a relationship could be established. For example, because unilateral divorce law alters the bargaining position of partners within married couples relative to more rigid divorce regimes where mutual consent is required, introducing such a divorce law could impinge on prostitution via downward shifts in its demand and supply. On the one hand, it could be argued that those married men who are prostitutes' clients become more reluctant to purchase their services because their wives could dissolve their marriage more easily under unilateral divorce. As a result, this change in clients' behavior would translate into a reduction in the demand for prostitution. On the other hand, the threat of unilateral divorce may improve the condition of married women, and therefore make marriage a more attractive option, leading to a fall in the supply of prostitution. In either of these two cases, enacting unilateral divorce laws reduces the amount of prostitution in equilibrium.

[1] The first "reverse sting" operation to catch prostitutes' clients took place in Nashville, Tennessee in 1964. Ten years later St. Petersburg, Florida spent large amounts of financial resources toward arresting male customers, applying some of the main principles that were later used in the so-called "Nordic Model" (i.e. criminalizing the purchase of prostitution). In the same year, Eugene, Oregon started the first shaming campaign in which, names and/or photos of prostitutes' clients were publicized. Likewise, San Francisco opened in 1995 the first school to re-educate arrested sex buyers. The vast majority of these policies aimed at fighting prostitution activities by reducing its demand.

By the same token, one could think of alternative mechanisms that instead imply an increase in the amount of prostitution. For instance, it could be argued that unilateral divorce laws are likely to increase the number of divorces in the short run, and therefore lead to a rise in the share of single people in the population. To the extent that single men demand more prostitution services than married men and insofar as single women supply more prostitution services than married women, these two forces jointly could lead to a larger amount of prostitution in equilibrium.

In view of the previous mechanisms, it seems relevant to evaluate which is the sign and size of the causal effect of unilateral divorce of prostitution, as well as to identify its underlying mechanism. Indeed, the nature of this effect could change people's prior beliefs on these two issues. If the effect is negative, this could generate a trade-off for those who oppose divorce and prostitution: barriers to divorce would imply higher levels of prostitution. Conversely, if the effect is positive, this would reinforce their beliefs.

This paper addresses this issue by exploiting a quasi-natural experiment provided by differences in the timing of implementation of unilateral divorce laws across U.S. states. Such differences enable one to use a difference-in-difference approach (DiD hereafter) to identify the potential causal effect of such laws on arrested female prostitutes. Notice that arrests for female prostitution is used as a proxy for the amount of prostitution, an activity for which there is very scant information being an illegal practice.[2] To implement the DiD approach, two sources of data are combined: the month in which unilateral divorce laws become effective in each U.S. state and information on arrested crimes drawn from the agency-level UCR (Uniform Crime Reporting) database. The evidence provided in this paper relies on the plausible identification assumption that the month in which unilateral divorce laws become effective in each state was correlated neither with any crime pattern nor in particular with any prostitution pattern.

To assess the credibility of the previous identification assumption, I use an event study methodology as well as a graph to investigate the parallel trends hypothesis of control and treated groups in a time window close to the date of the policy intervention. The evidence in this respect credibly shows that the effect on prostitutes' arrests occurs after the entry into force of the law and that prior to the intervention date treated and control groups share a common underlying trend.

My main finding is that unilateral divorce laws reduce arrests for female prostitution by roughly 10%. Such a reduction takes place in the first year after the implementation of the law. Since around 60,000 female prostitutes are arrested on average in the U.S. each year, the above-mentioned estimate implies a reduction of approximately 6,000 women arrested for prostitution. According to HG.org (2017) estimates, this decrease yields a reduction of about \$16.4 million for American taxpayers. As for the decrease in the overall number of female prostitutes, one can make a guess by using information drawn from Fondation-Scelles (2012), which reports about 1 million prostitutes in the US during the 2000s. Using such a figure and my estimated effect, a simple back-of-envelope calculation points out that unilateral divorce laws could lead to a reduction of 100,000 prostitutes.

Moreover, since in various states no-fault divorce laws went into effect slightly before unilateral divorce laws were enacted, a concern could be that the former divorce laws also played an important role in the decline of arrested female prostitutes to the extent that these laws reduced the cost of divorce relative to no-divorce (i.e.

[2] Both variables are bound to move together if arrests intensity for prostitutes is fairly constant over time, an assumption which I cannot directly test but which I take as plausible.

traditional) regimes. Using the month in which no-fault divorce laws entered into force as a further control in the DiD specification, I find that it does not change the previous estimate of the causal effect. An interpretation of this result is that no-fault divorce laws do not change the bargaining structure within couples but they merely reduce the costs of filing for a divorce.

Next, I consider the potential mechanisms that could be driving the results. These mechanisms range from a general decline in the number of arrests for all sorts of crimes to changes in both the demand and supply of prostitution. First, I explore if unilateral divorce laws led to a general reduction in arrests for crimes not connected to prostitution per se. Using data on police officers and on women arrested for robberies, drugs crimes/usage and vandalism' (three crimes with higher frequency than prostitution) I find that these alternative crimes are not affected by the implementation of unilateral divorce laws.

Second, I examine whether unilateral divorce changed the demand of prostitution. Three separate data sets are used to capture different features of such demand. In particular, data on the number of searches in internet of several words connected to prostitution are used to proxy online demand of prostitution; panel-survey data are used to analyze if men's views towards prostitution change after the men get divorced; and data on the number of unmarried men are used to proxy the demand of prostitution by unmarried men. In none of these exercises, I find empirical support to shifts in the demand of prostitution being due to unilateral divorce laws.

Finally, I look at supply-driven mechanisms stemming from changes in the value of marriage as an outside option to prostitution. In particular, I focus on a potential increase of wives' wages and improvement of conditions in marriage for wives (i.e. wives' welfare) that resulted from the wives' higher bargaining power when unilateral divorce laws go into force.[3] Using data on the real average wage of wives across U.S. states, I do not find empirical evidence to support that unilateral divorce laws affect wives' real wages. Then, I analyze whether there is evidence on unilateral divorce law improving wives' conditions in marriage. If this were the case, it seems plausible to conjecture that only female prostitutes in age of marriage and being fertile would exit prostitution since they would be the main recipients of an improvement in wives' welfare (see, e.g., Edlund and Korn (2002) and Edlund (2013)). To test this hypothesis, I split the data on arrested female prostitutes into different age groups and find that female prostitutes in marriage and fertile age are the main driver of the estimated reduction in arrested female prostitutes.

This paper contributes to three different lines of research. First, there is a growing literature in economics and other social sciences that has studied prostitution both from theoretical and empirical viewpoints (see, inter alia, Cameron 2002; Edlund and Korn 2002; Cameron and Collins 2003; Della Giusta, Di Tommaso, and Strøm 2009; Immordino and Russo 2014; Immordino and Russo 2015a; Immordino and Russo 2015b). However, so far this research has focused on analyzing how a policy intervention regarding prostitution regulation affects other crimes. For example, Jakobsson and Kotsadam (2013), Cho, Dreher, and Neumayer (2013), and Lee and Persson (2015) have studied the link between human trafficking and prostitution, while Ciacci and Sviatschi (2016), Cunningham and Shah (2017), and Bisschop, Kastoryano, and Klaauw (2017) analyze how changes in prostitution policies or business establishments connected to prostitution affect sex crimes. However, to the best of

[3] These two mechanisms (i.e. increase of wives' wages and improvement of conditions in marriage for wives) were suggested in Edlund and Korn, 2002 , where it is claimed that female prostitutes earn high fees for their services as a compensation of forgone marriage opportunities.

my knowledge, this is the first paper that looks at how a policy intervention in a different market than the prostitution market affects the latter.

Second, there is another stream of research in sociology, law and economics that evaluates the impact of unilateral divorce laws on different outcomes (see, e.g., Weitzman (1985), Gray (1998), Friedberg (1998), Edlund and Pande (2002), Gruber (2004), Rasul (2004), Rasul (2005), Alesina and Giuliano (2007), Stevenson and Wolfers (2006), Stevenson and Wolfers (2007), Stevenson (2008), Wickelgren (2007), and Voena (2015)). Yet, none on these papers deals with the effects of these laws on prostitution.

Finally, the results in this paper also contribute to a growing empirical literature on the relevance of several mechanisms at play in economic models of prostitution (see, e.g., Moffatt and Peters (2004), Gertler, Shah, and Bertozzi (2005), Levitt and Venkatesh (2007), Arunachalam and Shah (2008), Edlund, Engelberg, and Parsons (2009), Della Giusta (2010), Torre et al. (2010), Cunningham and Kendall (2010), Cunningham and Kendall (2011c), Gertler and Shah (2011), Islam and Smyth (2012), Arunachalam and Shah (2013), Sohn (2016), Logan and Shah (2013), Shah (2013), Cunningham and Kendall (2011a), Cunningham and Kendall (2013), Cunningham and Shah (2017), Bisschop, Kastoryano, and Klaauw (2017), Ciacci and Sviatschi (2016), and Cunningham and Shah (2016)). In particular, the underlying model behind the empirical approach taken in this paper relies on the theoretical setup developed by Edlund and Korn (2002), where a link is established between the marriage and prostitution markets to rationalize prostitutes' high earnings in terms of the forgone opportunity of such women in the marriage market.

The rest of the paper is organized as follows. Section 2.2 offers a brief overview of the prostitution market in the U.S., while Section 2.3 discusses the legislative context that led to the enactment of unilateral divorce laws across U.S. states. Section 2.4 proposes a conceptual framework explaining the main hypothesis tested throughout this paper. Section 2.5 describes the data sets used in this paper. Section 2.6 discusses the estimation approach and the main results obtained. Section 2.7 examines the identification assumption of the regression models. Section 2.8 tests the robustness of the results. In section 2.9, I empirically explore the numerous underlying mechanisms that might explain the findings of the paper. Finally, Section 2.10 concludes.

2.2 Background on the U.S. prostitution market

Prostitution is one of the most unsafe occupations in the U.S, worse than Alaskan fishermen, loggers, or oil rig workers. As reported by HG.org (2017), the death rates for prostitutes in the U.S. is 204 out of every 100,000; while for Alaskan fishermen is 129 out of every 100,000. On top of that, statistics about prostitutes are conservative since prostitution is illegal in the U.S. (it is only allowed in Nevada in brothels and certain areas of the state). As a matter of fact, prostitutes facing violence have nowhere to go without risking to get arrested themselves.

Dank et al. (2014) found that in 2007 in eight major U.S. cities prostitution generated a market value ranging from \$39.9 to \$290 million.[4] Furthermore, Pearl (1986) estimated that 16 U.S. cities spent on average \$15.3 million each year for prostitution control. More recently, Allard and Herbon (2003) found that prostitution arrests caused an expense of \$10.3 million only in the city of Chicago. According to HG.org

[4]The eight cities in the study are Denver, CO, Washing DC, San Diego CA, Miami FL, Seattle WA, Dallas TX, Kansas City MO, Atlanta GA.

(2017), the yearly average of around 70,000-80,000 arrests for prostitution costs $200 million to American taxpayers. Unsurprisingly, prostitution moves huge amounts of money in forms of both generated income and crime prevention.

Possibly, the large amounts of money that prostitution moves around might have originated the lack of agreement on prostitution law. Opponents to prostitution claim that prostitution is dehumanizing (e.g., Farley et al. (2004), Farley (2003), Farley (2004a), and Farley and Butler (2012)). According to this line of thought, prostitutes are victims of physical and psychological violence. For example, Farley (2004b) estimated that about 85% to 95% of prostitutes want to escape from prostitution, but have no other options for survival. By contrast, those supporting legalization of prostitution advocate that prostitutes chose to exchange their time and services for money as in any other job (e.g., TheEconomist (2004), Kempadoo (1999), Kempadoo (2007), and Kempadoo, Sanghera, and Pattanaik (2015)). Hence, it is criminalization of prostitution that worsen prostitutes' standards of living. They claim that, since prostitution cannot be stopped, legalizing it would be the only way to tax and "protect" prostitutes.

This ideological problem about how to regulate prostitution gains importance since the U.S. prostitution market is highly stratified. Thus, the effects of any given regulation of the prostitution market might differ across market segments. The prostitution market in the U.S. could be divided into three segments. On the lowest ladder, there are street prostitutes. Street prostitutes are usually controlled by pimps and thus make the least money. Further, they lack control over their choice of clients and are more likely to be victims of violence and to be arrested. Operating at the medium level there are those working indoors in brothels, massage parlours, gentlemen's clubs and strip-clubs. They usually enjoy better conditions than street prostitutes. Finally, escort girls comprise the highest level prostitutes. In this market segment, prostitutes have control over their choice of clients and "careers"; usually they are not controlled by a pimp, earn high wages and are less likely to be victims of violence. This group is the one that best fits the image of prostitutes depicted by supporters of legalized prostitution. Prostitution in the medium and high ladder of this stratification takes place indoor: that is why it is also known as *indoor prostitution*, while street prostitution is also known as *outdoor prostitution*.[5]

This study makes use of data of female prostitution arrests, which are more likely to represent outdoor prostitution than indoor prostitution. However, I also use information on indoor prostitution when the mechanisms linking unilateral divorce and prostitution are analyzed.

2.3 Legislative background: the Divorce Revolution

Traditionally, in the U.S. divorce was permitted only for grounds showing guilt of misconduct by any of the two spouses and had to be agreed mutually by both spouses (i.e. consent of the innocent party was required before a divorce was granted). Generally, such grounds were abandonment, cruelty, incurable mental illness, or adultery. The law was seen as inadequate, due to the major emotional and financial transaction costs involved in the verification of guilt of wrongdoing during the divorce process.

Thus, dissolution of marriages that were broken for mundane reasons (i.e. without misconduct by any spouse) was only possible if one of the two parties declared herself or himself guilty. In addition, since divorce had to be mutually agreed, the

[5] For further details on the stratification of the prostitution market in the U.S. see Shively et al. (2012).

belief was that whenever husbands wanted to divorce they would bribe their wives to get their consent, while if wives wanted to divorce they could not afford to bribe their partners.

However, since divorce was regarded to be against public interests, civil courts used to deny a divorce if there was evidence of cooperation between the two spouses, or if they tried to counterfeit the grounds for divorce. In fact, divorce could be barred even if one of the two spouses was found guilty. Recrimination, the suing spouse also found guilty; condonation, explicitly forgiving the misconduct or implicitly by continuing living together with the partner after knowing of it, and connivance, participating to the fault, such as organizing an adultery; were the three main reasons to refuse a divorce petition.

This law, not only required marital wrongdoing in order to file the divorce petition, but also punished spouses for such misbehaviour. Indeed, both husband and wife could be punished if they were found guilty of wrongdoing. If the husband was at fault he usually suffered the loss of child custody and the imposition of economic responsibilities; likewise if the wife was found at fault she might suffer the loss of alimony and child custody.

There was the tacit perception that abolition of fault grounds and mutual consent would eliminate the hypocrisy that incited the use of perjury and the forgery of evidence to surmount strict legal hurdles (Marvell, 1989; Rheinstein, 1955; Rheinstein, 1972; Mazur-Hart and Berman, 1977). On the one hand, guilt or innocence of the spouses would be irrelevant if no-fault divorce were available. On the other hand, consent of the partner would be useless if unilateral divorce were available.

In 1969 the California Family Law Act removed completely the requirements of fault as the basis of divorce and allowed spouses to file divorce without the consent of their partner. This Law Act established only two grounds for divorce: (i) irreconcilable differences; (ii) incurable insanity. Following Weitzman (1985), researchers have viewed this reform as the basis for both no-fault and unilateral divorce.

The focus of the reform was gender-neutral: it assumed that the divorcee was economically independent and employable. Consequently, this law established two major bases for alimony awards: the divorcees' employability and the length of the marriage. If any of the divorcees were not economically independent, this law also helped her/him to garner new-skills or to improve old ones to become self-sufficient.

The California Family Law Act started a period of movement to reform divorce laws in the U.S. known as "The Divorce Revolution" where various states followed suit. The movement gathered an apolitical consensus. Right-wingers viewed it as an expansion of personal rights and freedom. Left-wingers promoted it to impede women being locked in unfortunate marriages.

Unlike the case of California, "The Divorce Revolution" consisted of two steps: no-fault divorce and unilateral divorce. First, states moved to no-fault divorce regimes, which were already effective (with different degrees) in various states prior to 1950, while keeping mutual agreement. Next, states moved to unilateral divorce allowing the consent of only one spouse to dissolve legally the marriage. This second step, that was uncommon before the 60s, started in 1969 right after the California Family Law Act.

No-fault divorce does not change the bargaining structure within a marriage relationship. It solely reduces transaction costs by decreasing bargaining costs and eliminating financial penalties that could no longer be inflicted on at-fault spouses.

Indeed, no-fault divorce law eliminates the requirement of proof of guilt or innocence of either spouse. After the introduction of no-fault divorce law, marriage dissolution could be lodged on grounds such as "incompatibility" or"irreconcilable differences". Yet, it has to be agreed mutually by both partners. As a matter of fact, it was merely formulated to make marriage dissolution less dolorous and mournful.

Unilateral divorce goes a step further. It removes the property rights that mutual consent divorce gives either to the innocent spouse (for fault divorces) or to the spouse that does not want to get divorced (for no-fault divorces). Namely, unilateral divorce could change spouses' behaviour in two different ways. First, it allows spouses, who are unable to prove guilt of their partner or cannot afford to bribe their partner to file a divorce. Second, it changes bargaining power within the members of the couple.

Furthermore, no-fault divorces are more complex to code since the definition of what constitutes a no-fault divorce is much broader than the definition of unilateral divorce. In fact, the literature classified no-fault divorce in four categories: (a) living separate and apart as grounds for divorce; (b) incompatibility as grounds for divorce; (c) no-fault provisions added to traditional grounds as grounds for divorce; (d) no-fault is the sole ground for divorce (Elrod and Spector, 1997). These differences caused a wide disagreement between scholars using no-fault divorce dates (Vlosky and Monroe, 2002). An important point of divergence has been how to categorize fault-based laws that added "living separate and apart " provisions as no-fault laws. Even if such settlements consent to divorce without any proof of wrongdoing, the waiting period might be so long that renders the provision either too weak to be considered as no-fault or tantamount to a fault divorce law. The key difference is that true no-fault divorce laws are difficult to compare to legislative changes that just revise fault-based grounds.

Unilateral divorce laws are easier to code, the only difference is whether the provision requires a separation period or not. The literature has considered as unilateral divorce regimes either both provision with and without separation requirements or only provisions without separation requirements. Following Gruber (2004) I use unilateral divorce laws without separation for two reasons. First, since I code the law in a dummy variable, comparison of identical unilateral divorce laws seems more reasonable and accurate. Second, even if unilateral divorce laws without separation requirements usually became effective later than the ones with separation requirements, I do observe when such laws go into effect since my sample period spans from 1980 to 2014.

Finally, coding might differ on whether enactment dates or effective dates were used. The enactment date is the date in which a law is approved, while the effective date is the date in which a law enters into force. There can be a lag of some months between the enactment and the effective date. Coding the effective date is usually more laborious than coding the enactment date, since it necessitates to review the session laws of each state. Nevertheless, I use the effective date since it is the one that is crucial in legal actions.

2.4 Conceptual framework: The link between unilateral divorce and prostitution

At first sight the link between unilateral divorce and prostitution might not seem a clear-cut one. Nevertheless, there are a number of potential channels through which unilateral divorce might affect prostitution (e.g. demand, supply, etc.). For instance, it might be argued that unilateral divorce increases the number of single men, and since these men demand more prostitution, there is a rise in these activities. Likewise, another potential mechanism could be that unilateral divorce rises the number of single women, which implies that the supply of prostitution may grow and, as a result, prostitution in equilibrium increases as well.

This paper focuses on a specific mechanism that links two branches of the literature. The first one studies the effect of unilateral divorce on several outcomes related to wives' welfare. The second one deals with the interplay of the marriage and prostitution markets.

Coase theorem predicts that if there are zero transaction costs and transferable utility, moving from mutual to unilateral divorce does not have any effect on divorce rates. Unilateral divorce simply reassigns property rights but it does not change the outcome. Regardless of the divorce regime, only relationships with joint utility larger under marriage than under divorce survive. Therefore, the divorce rate would not change. However, both assumptions of the Coase theorem seem unrealistic in a marriage relationship. First, it is likely that bargaining is costly between spouses due to feelings and disdain. Second, utility might not be transferable between spouses.

Despite the predictions of the Coase theorem, moving from mutual to unilateral divorce entails huge redistributional differences between spouses. Under mutual consent divorce the spouse who wants to break the marriage is the one that should compensate the other one to get divorced. Conversely, unilateral divorce gives the property right to dissolve the marriage to the spouse who is better off with a divorce. Then, it is the spouse who wants to stay married the one who should compensate the partner to avoid divorce. Such distributional changes imply that the party seeking a divorce would be the one benefitting from the enforcement of unilateral divorce law.

The literature found that unilateral divorce law increases wives' welfare. Specifically, Stevenson and Wolfers (2006) find that unilateral divorce laws decrease female suicides, females murdered by their partners and domestic violence, while Alesina and Giuliano (2007) report evidence on how these laws decrease out-of-wedlock births and increase fertility rates in the first years of marriage. They also document that unilateral divorce laws reduce the number of never married women. In line with these results, Stevenson (2008) finds that unilateral divorce laws raise women's labor participation of both married and single women.

As for the prostitution market, Edlund and Korn (2002) argue that female prostitutes earn high wages, despite being a low skilled and labor intense job, since they are being compensated for forgone marriage opportunities. This implies that choosing to be a prostitute jeopardises one's marriage market prospects. Another key feature of this model is that wives sell to husbands a share of their custodial rights (i.e. reproductive sex) in exchange of a marriage compensation (i.e. a level of welfare) (Edlund, 2013). Indeed, custodial rights of children born out-of-wedlock used to belong only to the mother, while custodial rights of children born in a marriage belong to both parents. Combining this result with the fact that traditionally marriage has been an important source of pecuniary and non-pecuniary resources for women, implies that prostitution must pay better than other jobs compensating the opportunity cost of such forgone marriage market earnings.

Relying on the previous ideas, this paper suggests a mechanism that connects these two lines of research. The introduction of unilateral divorce law increases the bargaining power of the spouse seeking the divorce. Hence, in an unilateral divorce regime wives know they can get divorced irrespectively of their earnings.[6]

[6]Assuming husband's earnings were higher than wife's ones, under a mutual consent divorce regime if a husband wanted to get divorced, he could "bribe " his wife. Yet, wives could not afford

This feature makes marriage more attractive to women, by facilitating the breakup of "wrong " marriages. As a whole, in line with previous literature quoted above, unilateral divorce law boosts wives' welfare. Therefore, the main beneficiaries of the introduction of unilateral divorce law are women that prefer to get married, but would have opted to be prostitutes in the absence of such law. In doing so, they are able to exchange a share of their custodial rights for the marriage compensation.[7]

Finally, this paper focuses solely on the effect of unilateral divorce on prostitution in the short/medium run.

2.5 Data description

This section provides information about the data sets used throughout the paper. My econometrical analysis is based on two main data sets: the Uniform Crime Reporting which contains information on the number of arrested prostitutes for each agency-level in the U.S., and the effective date of unilateral divorce laws across U.S. states. Observations are matched at county and month level. Moreover, I use multiple data sets to carefully explore each of the potential mechanisms behind my findings.

2.5.1 Arrests for prostitution

Since historical data on the amount and intensity of female prostitution is not available, I use information of female prostitutes' arrests from agency-level UCR (Uniform Crime Reporting) sources as a proxy for this missing variable. This database contains information about monthly reports of arrests by age, sex, and race provided each year by law enforcement agencies in the U.S.. There are 29 main categories of offenses in this database. Such categories cover several sorts of offenses ranging from vandalism to gambling, and from prostitution to larceny. In addition, they are divided in subcategories for a total of 43 different offenses.[8] Each year, law enforcement agencies communicate their reports to the Federal Bureau of Investigation (FBI) who records such database as a periodic nationwide assessments of reported crimes not available elsewhere in the criminal justice system.

This data was downloaded from the Inter-university Consortium for Political and Social Research (ICPSR) web-page. ICPSR stores such information each year dividing it in five different components: (i) summary data, (ii) county-level data, (iii) incident-level data, National Incident-Based Reporting System (NIBRS), (iv) hate crime data, and (v) various, mostly nonrecurring, data collections. ICPSR recorded such data from 1980 to 2014 with the exception of 1984 which is missing.

With this available data sources, I construct a panel including monthly information at the county level on the ratio between the number of female prostitutes' arrests and the county population for the time period 1980-2014 (except 1984). Appendix Section B.3 presents detailed descriptive statistics of this data set.

to do so. Under unilateral divorce, a husband could still compensate his wife financially to avoid to get divorced. However, the wife should give her consensus.

[7]The main recipients of an increase in wives' welfare in marriage would be women that can get married and can exchange their "share" of custodial rights. Thus, prostitutes in a certain age interval should decrease either because prostitutes (in that age group) exit prostitution (i.e. stock effect) or because "potential" prostitutes (in that age group) prefer not to enter prostitution (i.e. inflow effect). I investigate this issue in Appendix Section B.1.

[8]In Appendix Section B.2 there is the complete list of offenses recorded in this database.

2.5.2 Divorce laws

In order to code unilateral divorce laws there are two important decisions to make: (i) whether to use the enactment date or the effective date of the law (ii) how to classify different unilateral divorce laws. In regards to (i), the enactment date is the date in which a law is approved, while the effective date is the date in which a law goes into effect. I use the effective date since this is when unilateral divorce petitions start to be filed. It could be that some divorce petitioners anticipated their behaviour since the law was already approved. Yet, they could not get divorced before the effective date.[9]

Regarding (ii), I focus on unilateral divorce laws without separation requirements in order to compare identical laws. It is difficult to compare unilateral divorce laws with and without separation requirements since the length of the required separation changes across states. Thereby, using unilateral divorce law with separation requirements would imply establishing a criteria to compare: (i) states with unilateral divorce law without separation requirements with states with unilateral divorce law with separation requirements (ii) states with unilateral divorce laws with separation requirements of different lengths. Since any of these criteria would be subjective I prefer to focus on unilateral divorce laws without separation requirements. Column (2) of Table 2.1 displays those states with unilateral divorce laws that required separation of spouses (Cáceres-Delpiano and Giolito, 2012).

Therefore, my main explanatory variable in the regression models estimated throughout the paper is a step dummy variable taking value 1 starting in the effective month of unilateral divorce law in a given state and taking value 0 previous to that date. This variable has been constructed updating Gruber (2004)'s data. As shown in Table 2.1, during my sample period there are six states that experienced a change of divorce law.

In addition, for comparability with unilateral divorce laws, I have also constructed a data set for dates of entry into force of no-fault divorce laws. Coding such law implies the problems discussed in Section 2.2. After reviewing the literature,Vlosky and Monroe (2002) suggest a decision criterion to code no-fault divorce laws which consists of four rules. Rule 1: In states where there is only a no-fault law, use the effective date of that law. Rule 2: In states where no-fault provision/s was/were added to traditional fault divorce law, use the effective date of such provision/s. Rule 3: Use the effective date for the law allowing the shortest separation period. Rule 4: Laws with explicit no-fault provisions supplant laws with no-fault *separate and apart* provisions.[10] I follow their coding of no-fault divorce laws effective date and I restrict again my attention to laws without separation requirements (i.e. Rules 1 and 2). [11]

[9]There can be a lag of at most one year between the enactment date and the effective date. Further, the effective date might be postponed, rendering the enactment date even less important. For further details about using effective dates instead of enactment dates see Vlosky and Monroe (2002). It is important to use an objective criteria to classify these laws since it could impact my identification assumption and findings. Even if in this setting, since intuitively it could not seem plausible that the effect is immediate, using either of the two dates should not affect results considerably.

[10]See Table 2 and Table 3 of Vlosky and Monroe (2002) for further information.

[11]Appendix Section B.4 presents further information about the classification followed to code unilateral divorce laws across U.S. states.

2.5.3 Supplementary data sets used

On top of the previous data sets, use is made of information about arrests for other crimes different from prostitution, number of police officers hired in each state, as well as on proxies for both demand and supply of prostitution. Data on other crimes is drawn from the agency-level UCR database which allows to compute crime rates at county level.

In this paper I use "The Police Employee" data set to measure the number of officers per state's population. This data set contains annually collected data about law enforcement officers and civilians employed by police departments, and their respective rates per location's population from 1971 to 2016.[12] The UCR Program defines law enforcement officers *as individuals who ordinarily carry a firearm and a badge, have full arrest powers, and are paid from governmental funds set aside specifically for sworn law enforcement representatives*. Whereas, civilian employees include personnel such as clerks, radio dispatchers, meter attendants, stenographers, jailers, correctional officers, and mechanics provided that they are full-time employees of the agency. In addition, the totals given for sworn officers comprise not only the patrol officers on the street but also the officers assigned to various other duties such as administrative and investigative positions and special teams.

As a proxy for the demand of prostitution, I use data about searches of words connected to the demand of prostitution in Google.com which are drawn from Google Trends. Since those records are geo-located, I collected the counts for the number of times each word was searched in Google.com for each county and month in the U.S. This data spans from 2004 to 2017.

Another data set used in this respect refers to divorcees' opinions about prostitution which is drawn from a longitudinal survey, more precisely from the 1st, 2nd, 3rd and 4th waves of the Youth Parent Socialization Survey (YPSS). This survey was designed to study political socialization and was implemented by the Survey Research Center and Center of Political Studies of the University of Michigan. This study started in 1965 and collected data in three other different waves that respectively took place in 1973, 1982 and 1997. There is a total of 934 respondents (458 men and 476 women) in the four waves. This data is available from the ICPSR web-page as well.

Since the YPSS data collected information on the marital status of their respondents, it is known whether an individual who was previously married got divorced during the following waves. Further, this survey collected information on topics that respondents disliked.[13] Replies were classified in multiple categories, among which there was prostitution. With this database I can see if for the sample of the survey there is a statistical significant correlation between getting divorced and disliking prostitution.[14]

The last database is the monthly Current Population Survey (CPS), which is an employed-focused cross-sectional survey. The U.S. Census Bureau of Labor and Statistics administers the CPS monthly to around 60,000 U.S. households. The survey collects information about a number of variables connected to employment status of each household member aged 15 years old or older. Such information is

[12]Year 1972 is missing, although there is no reason to believe it is missing due to any special pattern of hired officers.

[13]Namely, the survey states topics respondents were "least proud of".

[14]The question of the survey is: " What are the things you are least proud of as an American?". The answer connected to prostitution is "Immorality in general; low morals; deterioration in moral standards; also specific actions--e.g. drinking, gambling, overexposure; lewdness in behavior or in mass media or literature; pornography, prostitution".

provided by an adult member of the household. A multistage stratified statistical sampling scheme selects sample households. Such households are surveyed for 4 consecutive months, interviews are stopped for 8 and eventually are surveyed back for 4 additional months. The sample represents the civilian non-institutional population. The CPS data used in this paper extends from 1980 to 2014.[15]

2.6 Estimation approach and main results

In this section, I explore the causal effect of unilateral divorce laws on arrests of female prostitutes. First, I present my identification strategy that exploits reasonable exogenous variation of the timing in which unilateral divorce laws became effective across U.S. states. Next, I discuss my econometric specification in detail. Finally, I report the main empirical results uncovered by the regressions.

2.6.1 Identification assumption and regression model

The results of this paper rely on the identification assumption that the months in which unilateral divorce laws became effective in the six states treated during my sample period were not chosen due to any reason related to crime in general and prostitution in particular. Yet, this concern can be easily dismissed since, to the best of my knowledge, there is no historical evidence supporting that crime rates might have affected such effective dates.

Knowledge of the legislative background is crucial to assess the credibility of the identification assumption. As I explained in Section 2.2 "The Divorce Revolution" was caused mainly by the inadequacy of traditional divorce laws and was driven by an apolitical consensus of both liberals and conservatives. Fault grounds and mutual agreement encouraged couples even to perjure and falsify evidence to obtain a divorce. Introduction of divorce laws would reduce the use of perjury, by eliminating either mutual consensus, fault grounds or both. Moreover, conservatives supported divorce since they saw it as an widening of personal rights, whereas liberals backed it to thwart women being locked in dismal marriages.

Another potential concern is that there could be an omitted variable affecting simultaneously the effective date of unilateral divorce laws and female prostitutes arrests. For example, it could be that the women's rights movement affected both variables. However, this possibility again seems unlikely due to two reasons. First, historically women's right movements have been in favour of unilateral divorce, but such movement did not have a clear position on prostitution: feminists supported both actions against and in favour of prostitution. Therefore, it does not seem likely that the women's right movement, fostering the "The Divorce Revolution" played any role in prostitution regulation. Second, in spite of "The Divorce Revolution" there has not been yet a "Prostitution Revolution" nor any other movement changing prostitution laws systematically.[16]

A final concern to my identification assumption is displacement of female prostitutes, clients or police officers among different states. These issues should be analyzed carefully since they could violate the Stable Unit Treatment Value Assumption (SUTVA). Yet, I could not find any evidence nor any plausible reason suggesting

[15]The CPS data used in this paper are drawn from the Uniform Extracts of the CPS ORG. Center for Economic and Policy Research. 2017. CPS ORG Uniform Extracts, Version 2.2.1. Washington, DC.

[16]Currently, the only state in the U.S. that have legalized prostitution is Nevada. Nevada introduced unilateral divorce laws and legalized prostitution in different years: unilateral divorce law became effective in 1967, while prostitution was legalized in 1971.

that prostitutes, clients or police officers could move among states depending on their divorce regimes.[17]

Using data at county level increases precision and improves comparability across treated and control units. As a matter of fact, it is more reasonable to compare smaller geographical units, such as counties, instead that states as a whole. In addition, if my specification were at year level the identification assumption would be less plausible. Indeed, it seems likely that other progressive social policies might become effective in the same year in which unilateral divorce law entered into force. If this happens systematically in the treated states, my estimates might be capturing the joint effect of both unilateral divorce and other progressive laws. Yet, it is much less likely that such changes in social policies occurred exactly in the same month in which unilateral divorce law became effective.

More precisely, the identification assumption in this paper corresponds to the parallel trends hypothesis in the DiD estimation approach. In other words, the only difference among treated and control counties is that the formers were treated. Should they have not been treated, they would have experienced the same evolution of control counties.

This paper considers two control groups: the never treated and the treated before 1980. In fact, since this study makes use of data spanning from 1980 to 2014, but many U.S. states promulgated unilateral divorce laws before 1980, I proceed to include such states in the control group. In the control group there are also the standard never treated units.

In particular, the following regression model is considered here

$$log(1 + Prostitution_{csmy}) = \beta Unilateral_{smy} + \alpha_m + \alpha_y + \alpha_c + \alpha_c * y + \varepsilon_{csmy} \quad (2.1)$$

where $Prostitution_{csmy}$ is the number of female prostitutes arrests per 1,000,000 inhabitants in county c of state s, in month m of year y.[18] α_m, α_y, α_c, are respectively month, year and county fixed effects; $\alpha_c * y$ is a county-year linear trend; $Unilateral_{smy}$ is the main regressor of interest, namely, a dummy variable taking value zero before the effective month of unilateral divorce and value 1 in the month in which the unilateral divorce law becomes effective and afterwards.[19] As for states that were treated before 1980, $Unilateral_{smy}$ takes always value 1 for them; whereas, for states that were treated after 2014 or have never been treated $Unilateral_{smy}$ takes value zero.

Taking the logarithmic transformation of the dependent variable is common in crime economics, mainly because data presents extreme values that may skew the results. In addition, since arrests might take zero values, I use $log(1+Prostitution_{csmy})$ as dependent variable.

Notice that the specification considered in this paper is quite demanding since it takes into account that crime patterns respond to seasonal changes, and that these patterns might differ between counties within the same state. In fact, beyond having

[17] Since this paper finds that unilateral divorce decreases prostitution by improving prostitutes' outside option, a possible concern could be that entry into force of unilateral divorce could cause prostitutes from surrounding states to move to that state to exit prostitution. However, I did not find any evidence supporting this hypothesis.

[18] Arrests of female prostitutes per 1,000,000 inhabitants is computed as the number of arrested female prostitutes divided by population and multiplied by 1,000,000. Same computations are made for data on other crimes.

[19] As a robustness check I also consider year-month fixed effects.

the common year fixed effects, there are also month fixed effects, county fixed effects and county-year trends.

2.6.2 Results

Panel A of Table 2.2 shows the results of estimating model (1). Column (1) includes county-year trends and county fixed effects, whereas in column (2) I add year fixed effects, in column (3) I introduce month fixed effects and in column (4) I add year-month fixed effects. In column (1) the estimated coefficient is negative and statistically significantly different from zero at 5% level. After adding year and month fixed effects, in columns (2) and (3), the estimated coefficient is similar in size and statistically different from zero at 10% level. There could be concerns about the level of significance of these results, hence, for ease of comparison Table 2.2 reports the p-values associated to the null of zero effect for each estimated coefficient. It is reassuring to find that such p-values range between 0.046 and 0.055. In particular, note that the significance of my results is not affected by the inclusion of year-month fixed effects (i.e. column (4)).

After easy back-of-the-envelope computations, the coefficient estimates in column (3) indicate that unilateral divorce laws decrease female prostitutes arrests by roughly 10%.[20] Since in my data set on average around 60,000 female prostitutes are arrested each year in the U.S., this finding implies that unilateral divorce law could cause a decrease of 6,000 women arrested for prostitution in the whole country. According to HG.org (2017) estimates, this decrease could yield a reduction of approximately $15 million for American taxpayers.[21] The size of this effect could be compared to Allard and Herbon (2003)'s results, who found that prostitution arrests caused an expense of $10.3 million only in the city of Chicago. Therefore, unilateral divorce law helps a state to save around 1.5 times the cost of arrests for prostitutes in Chicago.

It is not straightforward to link these findings to the number of prostitutes based on arrests for female prostitution. According to Fondation-Scelles (2012) there are around 1 million female prostitutes in the U.S. Hence, assuming that the found effect of a reduction of 10% of female prostitutes arrests is the same as that on female prostitutes, implies that female prostitutes in the U.S. would decrease by 100,000 women if unilateral divorce law were effective in all states.

My findings rely on the quasi-experimental design given by the effective month of unilateral divorce laws across U.S. states, but since my dependent variable spans from 1980 onwards my identifying variation comes from only six states and not from all the adopting states. Thereby, there is the risk that these six states could have a specific reaction to the event. Yet, I did not find either any evidence or any plausible reason supporting this hypothesis.

It is important to stress that the external validity of my findings should be interpreted carefully. Prostitution market works differently in developing and developed countries (Farley et al., 2004). Further, unilateral divorce laws were enacted after a period of discussion in the U.S. that led slowly to full social acceptance of divorce. It would be difficult to extrapolate my results to developing countries and to countries

[20] These computations simply take into account the structure of my dependent variable to compare it to a standard log-level specification. Precisely, $\frac{\partial \log(y)}{\partial x} = \frac{\partial \log(1+y)}{\partial x} \frac{\partial \log(y)}{\partial \log(1+y)} = \beta \frac{1+y}{y} \simeq \hat{\beta} \frac{1+\bar{y}}{\bar{y}} = -6.8\% \frac{1+1.9}{1.9} = -10.4\%$

[21] According to HG.org (2017), 80,000 arrests cost $200 million. Thus, 60,000 cost $150 million to the taxpayers and a decrease of 10% implies a decrease of $15 million. While, on average at state level such decrease would amount to $300,000.

that enforced divorce due to foreign influences without having an internal social movement arising such change.

There are several mechanisms that might explain the reduction of arrested female prostitutes associated to unilateral divorce laws. These mechanisms range from change in the number of police officers enforcing the law, to shifts of either the demand of prostitution or the supply of prostitution. After presenting evidence in favour of my identification assumption (Section 2.7) and discussing the robustness of the results (Section 2.8), I explore thoroughly each one of these mechanisms in Section 2.9.

2.7 Concerns about the identification assumption

This section tests the parallel trends hypothesis. In the literature, ascertaining whether the identifying assumption of parallel trends is reasonable (in a setting as the one considered in this paper) has been carried out mainly in two ways: i) using the event study methodology and ii) visual inspection of pre-treatment trends. The first approach builds on a regression model that estimates different coefficients over time, before and after the date of the treatment. The second approach relies on visual inspection of pre-treatment trends to assess whether control and treated units were on the same trend prior to the treatment.

2.7.1 Event study

I analyze an event study for three years before and five years after unilateral divorce laws became effective in each one of the six states.[22] If prostitution were decreasing in treated counties prior to the effective month of unilateral divorce, then the estimated coefficients of the dichotomous variables prior to the event would be negative and jointly significantly different from zero. If prostitution started decreasing after unilateral divorce law became effective in each state, the reverse would be true: then the estimated coefficients of the dichotomous variables after the event would be negative and jointly different from zero.

In order to evaluate a long time window with monthly data I group the dichotomous variables in groups of twelve month before and after the month unilateral divorce laws became effective in each of the six treated states in the sample. Therefore, there are nine periods: three periods before and five after the event, and period 0. The excluded indicator, as usual, is $t = -1$, twelve months prior unilateral divorce becomes effective.

Figure 2.1 plots the estimated coefficients of this event study. On the horizontal axis there is the event time (the number of periods prior and posterior to the change in unilateral divorce law in spells of twelve months), whereas on the vertical axis there is the size of the coefficient measured according to its effect on the dependent variable in the main specification. Each dot in the graph is an estimated coefficient, each coefficient is depicted with its own confidence interval at both 90% and 95% significance levels.

As can be observed in Figure 2.1, the coefficients prior to the occurrence of the event are positive, while the coefficients after the occurrence of the event are all negative. The coefficients estimating the effect of the policy one year and two years after its introduction are statistically significant at standard levels suggesting that most of the effect takes place in the first and second year after unilateral divorce

[22] In Appendix Section B.5 I carefully explain the methodology followed for the event study analysis.

enters into force. Further, the coefficients prior to the occurrence of the event are not jointly statistically different from zero, while the coefficients after the occurrence of the event are jointly statistically negative at 1% level. Hence, this evidence supports that the effect was not temporary.

As a whole, these findings support the identification assumption because the decrease in arrested female prostitutes happened after the policy intervention. The reduction started in the first year after unilateral divorce law became effective and there is evidence that such effect was permanent.

2.7.2 Parallel trends

The usual parallel trends graph plots data for control and treated units over time to evaluate the pre-treatment patterns of both groups. The easiest setting to use this type of graphs is when the policy intervention happens simultaneously for every treated unit. In these cases, by plotting the trend of the control and treatment group prior to the policy intervention it is easy to assess whether the two trends for the groups were parallel or not.

In this paper the policy intervention date is not the same for all treated units and there are two distinct control groups. First, the effective date differs across states. This entails two problems: how to compare treated units among themselves and how to compare such units to the control group. Second, in my sample there are states that never approved unilateral divorce (I refer to them as never-treated) and others that changed the divorce regime prior to my sample period (I refer to them as already-treated).

To overcome the first issue I normalize to 0 the treatment date for all treated counties, as I did in the event study. Then, I computed the average of the dependent variable at each normalized month for every treated county. To overcome the second issue, I compute the average of the dependent variable at each policy intervention date for every control county, this yields six different trends, one for each of the six policy intervention dates.[23] In order to compare the results to the event study, each period consists of twelve months and the number of periods prior and posterior to the policy intervention is as in the event study.

Figure 2.2 shows the trends for the treated group and both control groups: never-treated and already-treated. On the horizontal axis there is the event time (the number of periods prior and posterior to the change in unilateral divorce law in spells of twelve months), while on the vertical axis the value of the dependent variable is depicted. Cumulating the data for each twelve month prior and posterior to the policy intervention creates the exact same number of periods of the event study graph with the only difference that in the latter period $t = -1$ is omitted.

Figure 2.2 shows that the treated group and the two control groups are parallel prior to period 0. On top of that, the treated group shows a small reduction in periods 0, 1 and 2. Yet, this graph is useful to assess whether treated and control units are parallel before the entry into force of unilateral divorce law. While, in order to determine the magnitude of the decrease it is more useful to examine Figure B.1 and Figure B.6. This evidence is in line with the findings of the event study.

[23] By averaging the control group trends for each policy intervention date, this procedure takes into account that there might be seasonal effects. A similar approach is used in Figure II of Ayres and Levitt (1998).

2.8 Robustness checks

This section deals with the robustness of the results. Firstly, it explores whether these results are robust to changes of the dependent variable. Next, it explores to what extent these results are sensitive to changes in the main specification.

2.8.1 Sensitivity to changes in the definition of the dependent variable

There might be the concern that my findings rely on the chosen transformation of the dependent variable (i.e. $\log(1+y)$). Thus, in what follows, I consider specifications of the dependent variable to analyze whether the previous results persist. First, I consider the Inverse Hyperbolic Sine transformation. Second, I run a Linear Probability Model. Lastly, I consider a specification where the dependent variable is in levels.

The Inverse Hyperbolic Sine Transformation (hereafter, IHS) is an alternative to taking the $log(1+y)$ for dependent variables that take zero values. The IHS is defined as $\log\left(y + \left(y^2+1\right)^{\frac{1}{2}}\right)$. Panel B of Table 2.2 shows the results of running the same regression as in Section 2.5 but taking the IHS of the dependent variable. As can be observed, the findings using the IHS are similar in both sign and size to the ones of the main regression. In fact, after easy back-of-the-envelope computations alike the ones for the estimated coefficient of the main regression, the effect estimated by the IHS is -9.2%.[24]

Despite the dependent variable is in logs, there could be the concern that the results are driven by extreme observations of the dependent variable. To assess this issue, I replace the dependent variable with a binary variable taking value 1 for every positive value of the dependent variable and 0 otherwise. Panel C of Table 2.2 shows the results of running a Linear Probability Model (hereafter, LPM). Column (1) of such table displays the estimated coefficient without year and month fixed effects, column (2) adds year fixed effects, column (3) adds month fixed effects and column (4) adds year-month fixed effect. The estimated coefficients are always negative and statistically different from zero at 5%. These results suggest that the introduction of unilateral divorce law is associated with a reduction of 1.8 percentage points of the probability of arresting a female prostitute.

As a last robustness check, Panel D of Table 2.2 considers a specification where the dependent variable is in level form (i.e. the number of female prostitutes arrests per 1,000,000 inhabitants). Columns (1), (2), (3) and (4) of Panel D of Table 2.2 show that the estimated coefficients are negative and statistically significant. Column (3) considers the full specification, where the estimated coefficient is negative and statistically different from zero at 10%. Such a coefficient is approximately $-.77$. On average, there are roughly 2 arrested female prostitutes per 1,000,000 inhabitants per county and month. Accordingly, the decrease caused by unilateral divorce law is much larger than the one estimated by the other specifications. This might be due to the extreme values of the dependent variable that are not transformed in this specification and push up the estimated coefficient.

Summing up, the evidence presented in this subsection supports a negative causal effect of unilateral divorce on female prostitutes' arrests, irrespectively of the chosen functional form of the dependent variable.

[24]Precisely, $\frac{\partial \log(y)}{\partial x} = \frac{\partial IHS(y)}{\partial x}\frac{\partial \log(y)}{\partial IHS(y)} = \beta\frac{\sqrt{1+y^2}}{y} \simeq \widehat{\beta}\frac{\sqrt{1+\bar{y}^2}}{\bar{y}} = 8.1\%\frac{\sqrt{1+(1.9)^2}}{1.9} = -9.2\%$

2.8.2 Sensitivity to model specification changes

Next, I analyze whether the results found in this paper depend on other specification issues, like the choice of the control group and choice of the treatment. It might be that using only one of the two control groups changes substantially the results of the regression. Further, since no-fault divorce and unilateral divorce reforms took place almost contemporaneously, it might be that the estimated effect is due to the former instead of the latter.

Table 2.3 shows the results of running the main regression using only one of the two control groups. Estimated coefficients of these regression models should be interpreted cautiously since they are computed using a biased restricted sample. This exercise is only useful to test whether the estimated coefficient of the main regression is statistically equal to the coefficients of the restricted samples. Column (1) only uses the already-treated control group, whereas column (2) uses the never-treated control group. Both columns show results for the full regression model (i.e. with all the controls used in my main specification). The estimated coefficients are negative in both columns, but different from zero only in column (1). More importantly, in both regressions the estimated coefficients are not statistically different from the estimated coefficient of the main regression. Such evidence indicates that the two control groups produce similar results.

As for no-fault divorce laws, I make use of the effective month of no-fault divorce laws in two different ways. First, I add no-fault divorce as a control variable. Second, I replace the unilateral divorce dates with the no-fault divorce dates. Since no-fault divorce does not need proof of wrongdoing or innocence, researchers theorized that it does not change the bargaining structure within a relationship (Gruber, 2004). Yet, it reduces bargaining costs and financial penalties, and as a consequence reduces bargaining costs as well. If the observed decline in arrested female prostitutes is caused by no-fault divorce laws instead of unilateral divorce laws, then using such variable as a control variable should reduce (in absolute value terms) the size of the estimated coefficient and its statistical significance. Table 7 displays the estimated coefficients of running the main regression of the paper adding no-fault divorce dates as a dichotomous control. Such control takes value 1 in the month no-fault divorce law become effective and in the following months, and 0 before the effective date.[25] As can be inspected in Table 2.4, the estimated coefficients are not statistically different from the ones of the main regression.[26] This supports that no-fault divorce laws did not play an important role in the reduction of arrested female prostitutes.

Table 2.5 shows the results of running a specification that replaces the effective month of unilateral divorce laws with the effective month of no-fault divorce laws. There are two insights for this specification. On the one hand, it can be viewed as a double check that no-fault divorce laws are not leading to a reduction of arrested female prostitutes. In fact, if this were the case then the month in which no-fault divorce law became effective should be negative and statistically different from zero. On the other hand, this regression can be seen as a placebo test. If unilateral divorce laws are not causing the decay in arrested female prostitutes, changing such dates with almost contemporaneous dates should find similar results.

As can be seen in Table 2.5 no-fault divorce laws do not appear to cause the reduction in arrested female prostitutes. Indeed, the estimated coefficients in columns

[25] This variable is coded in the same way than the treatment variable.

[26] The point estimate is even slightly larger in absolute value than the one of the main specification.

(1), (2), (3) and (4) are insignificant and much smaller in size than the ones of the main regression.

In sum, the evidence provided above shows the robustness of the main regression to the choice of the control group and to no-fault divorce laws.

2.9 Potential mechanisms

My main finding so far is that unilateral divorce law decreases arrested female prostitutes in the U.S. There are several mechanisms that could lead to such decline. This section explores each one of them by combining multiple data sets.

First, it could be argued that the estimated decrease in arrested female prostitutes could simply be explained by a general decrease in crime rates contemporaneous to the introduction of unilateral divorce laws. For example, it could be that unilateral divorce law might have an effect on crimes committed by women as a whole. Although it is not clear the mechanism that would lead to such effect, if unilateral divorce law decreases all sort of crimes committed by women then I would find a decrease in female prostitution arrests but this decrease would not be related to prostitution per se. This is the first mechanism this section explores.

Second, to analyze the potential mechanisms related to prostitution I use a simplified version of the model introduced by Edlund and Korn (2002). These authors argue that the aggregate demand of prostitution $D(p, n)$ is a function of p, the price of commercial sex; and n, the number of single men, whereas, the aggregate supply of prostitution $S(n)$ is simply a function of the number of unmarried (single) women n.[27] Thus, p, n are endogenously determined in the model.

Since in equilibrium demand is equal to supply, equating them determines p as a function of n (i.e $p = p(n)$). Yet, in order to compute the equilibrium values of p and n, an extra equation is needed. According to their model such equation is the non-arbitrage condition that connects marriage market to prostitution market: in an interior equilibrium, where there are both married women and prostitutes, revenues from the two activities must be equal. As a consequence, p, the wage earned by prostitutes, is equal to w, the wage earned in the labor market by wives, plus the compensation p_m, paid in equilibrium to married women by their partners. These two curves (i.e $p = p(n)$, computed from the equilibrium condition $D(p, n) = S(n)$, and $p = w + p_m$) determine the equilibrium of the prostitution market, as shown in Figure 2.3.

Hence, according to this simple model, there are two mechanisms related to the prostitution market that might decrease the number of female prostitutes, explaining the findings of this paper:

- It might be that unilateral divorce increases w , that is the wage earned by wives.
- It might be that unilateral divorce increases the compensation p_m paid in equilibrium to wives by their husbands.

Since in this model $D(p, n)$ and $S(n)$ are functions of the endogenous variables p and n, there is no way in which unilateral divorce law could affect these two curves. However, one could think of channels through which unilateral divorce might affect either the demand or the supply of prostitution. For example, it could be that

[27] In their model there is the same amount of women as men and, since marriage is monogamous, the number of single men and women is the same.

progressive laws, such as unilateral divorce law, shape men's and women's preferences towards women's rights and as a result towards prostitution. Hence, these two mechanisms will be examined as well in the sequel.

2.9.1 Fight against crime mechanism

This subsection explores whether the decrease in arrested female prostitutes is related to a general decrease in arrests. There are many explanations that could cause a general decrease in arrests. For instance, it might be that in the very same month than unilateral divorce law becomes effective in a certain state, the number of police officers decreases in the majority of counties of such state.[28] This seems unlikely since police officers are hired yearly, while unilateral divorce laws might become effective in any month of the year; yet it could be an explanation for the results of the paper.[29]

To test if unilateral divorce affects officers, I run a specification where the dependent variable is the number of officers. Namely, since this data set is at state-year level, I consider the following regression model:

$$Officers_{sy} = \beta Unilateral_{sy} + \alpha_y + \alpha_s + \alpha_s * y + \varepsilon_{sy} \tag{2.2}$$

where $Officers_{sy}$ is the number of officers per 1,000 inhabitants in state s and year y, and the rest of the variables follows the same nomenclature as in the main regression. This regression model captures any change of officers due to the entry into force of unilateral divorce at state-year level. For example, if systematically in the same year unilateral divorce laws become effective the number of hired police officers decrease (increase), then we would expect β to be negative (positive). Table 2.6 displays the results of running specification (2). Columns (1) to (4) show the results of using the dependent variable in levels, columns (5) to (8) use the dependent variable in logs. Columns (1) and (2) present the results for the sample period 1971 to 2016 respectively without and with state-year trends. Columns (5) and (6) present results for this same regression but using the dependent variable in logs. Across these four specifications the estimated coefficient flips sign, is small in absolute value and it is not statistically significant in any of them.

Since this data set spans from 1971 to 2016, but my main specification considers from 1980 to 2014 there could be the concern that unilateral divorce decreases officers only during my sample period. To this extent, I also run specification (2) using the same sample period as in the main specification. Columns (3) and (4) respectively show the results of running specification (2) in levels, using the restricted sample between years 1980 and 2014, without and with state-year trends. Columns (7) and (8) repeats this same analysis but with the dependent variable in logs. Also in this case, results are inconclusive. In fact, the estimated coefficient flips sign depending on the specification of the dependent variable and, more importantly, it is not statistical significant in any of the four regressions considered. All in all, I do not find any empirical evidence supporting that unilateral divorce has an impact on officers.[30]

[28] A possible explanation could be that contemporaneously to the introduction of unilateral divorce police's budget reduces and so the number of officers decreases.

[29] There are alternative potential mechanisms involving police officers to explain the findings of the paper. For instance, it could be that contemporaneously to the introduction of unilateral divorce law police officers become less strict in arresting criminals or decrease their working hours. Even if implausible these mechanisms would be able to explain the findings of this paper.

[30] Appendix Section B.6 presents the results of the same analysis using the yearly change (i.e. first difference) of the number of officers per 1,000 inhabitants and the growth rate of the number of officers per 1,000 inhabitants as dependent variables. Again, I find no evidence supporting this mechanism.

Another potential mechanism is that unilateral divorce law could decrease all sorts of crimes committed by women. If this were true, the found decline in arrested female prostitutes could be explained by a general reduction in crimes committed by women. If unilateral divorce laws did not affect either police officers' behaviour, nor crimes committed by women, running a regression with women arrested for crimes different than prostitution will yield estimated coefficients which are statistically equal to zero.

To test this hypothesis, I consider a specification similar to the main regression but where I change the dependent variable. I use three different dependent variables: women arrested for robberies, vandalism and drugs crime/usage.[31] If unilateral divorce laws are shaping police officers' behaviour, or decreasing their number, then I should observe a decrease for these crimes as well. In fact, robberies, vandalism and drugs crimes occur more frequently than prostitution and are easier to catch, therefore, if either police's behaviour or women's crime behaviour are changing, these crimes would change as well.[32]

Table 2.7 shows the results of running my main regression using data on women arrested for such crimes. Column (1), (3) and (5) show the results using as the dependent variable $\log(1+y)$, while column (2), (4) and (6) repeat these computations for the IHS of the dependent variable. Regarding robberies, the estimated coefficients are close to zero and are not statistically different from zero for both regressions. As for drugs, the estimated coefficients are insignificant as well, but larger in absolute value for both $\log(1+y)$ and IHS. As for vandalism, the two estimated coefficients are positive and not statistically different from zero.

Having established that there is no empirical evidence supporting that unilateral divorce altered neither police officers' behaviour nor crime patterns of women, in the rest of this section I explore each of the other potential mechanisms that could explain the decrease in arrested female prostitutes through a reduction of female prostitution in equilibrium.

2.9.2 Demand mechanisms

The estimated reduction in the arrests for female prostitution might be driven by a decrease of the demand of prostitution. Indeed, there are many mechanisms through which unilateral divorce could shift the demand of prostitution. For example, Edlund and Korn (2002) assume that unmarried men demand more prostitution than married men. Thus, by increasing the number of male divorcees and, as a result, the number of single men, unilateral divorce may lead to a rise in the demand for prostitution. Another example could be that unilateral divorce laws change people's attitudes, pushing up in turn the demand of prostitution.

[31] This regression analysis has two main features. First, it uses crimes committed only by women since unilateral divorce might change men's behaviour. Indeed, assuming that on average male incarceration decreases the likelihood that women marry (Charles and Luoh, 2010), and that on average women (i.e. wives) used to own less resources than men (i.e. husbands), implies that the introduction of unilateral divorce by increasing wives' bargaining power (w.r.t. mutual consent divorce) should decrease crimes committed by men. As a consequence, using crimes committed by men would turn out to be uninformative to study the aforementioned mechanism. Second, this analysis makes use only of crimes not connected to prostitution since crimes related to prostitution (e.g. rape, sexual offenses, loitering, homicides, etc.) could be affected by unilateral divorce not via a general decrease in arrests (Urban Justice Center, 2005; Cunningham, DeAngelo, and Tripp, 2017; HG.org, 2017).

[32] A similar approach is used in Ciacci and Sviatschi (2016).

In the sequel I test whether this mechanism is supported by the data using three different data sets which proxy different features of the demand of prostitution. [33]

Internet searches

The first data set used is drawn from Google trends. Cunningham and Kendall (2010), Cunningham and Kendall (2011c), and Cunningham and Kendall (2013) claim that "overall, online solicitation represents an augmentation of the prostitution market".[34] Indeed, according to these researchers internet allowed prostitutes to (i) reach more easily a larger pool of potential clients, (ii) build reputations for their services and (iii) use screening to filter out unwanted clients.

Therefore, using Google trends I gathered data about searches of different words. First, I consider different synonyms of "prostitute". Second, I consider the word "sex". Next, I consider words connected to indoor prostitution such as "stripper", "strip club" and "escort". Finally, I consider words connected to websites known for matching customers and prostitutes.[35] The Erotic Review is one of the most important websites that matches prostitutes and clients in the U.S.[36] It seems plausible that if the demand of prostitution exhibited a change in those years, the searches of such words should have changed too.

Since Google trends data set is at state-month level, in this case the regression is at that level as well. Then, I run the following regression:

$$Searches_{smy} = \beta Unilateral_{smy} + \alpha_m + \alpha_y + \alpha_s + \alpha_s * y + \varepsilon_{smy} \tag{2.3}$$

Where, $Searches_{smy}$ stands for the number of searches of a certain word; and the rest of terms follow the same nomenclature as in the main regression. If unilateral divorce increases (decreases) the demand of prostitution, the estimated coefficient should be positive (negative) and significant.

Google trends data is available since 2004. Table 2.8 displays the estimated coefficients after running such regressions for the largest sample I have (i.e. 2004 to 2017). While, Table 2.9 displays the estimated coefficients after running such regressions till 2014 to match partially the sample period of my main regression. Panel A, B and C respectively show results in levels, logs and IHS.[37] Evidence is inconclusive: estimated coefficients flip signs across regressions in both tables. The majority of estimated coefficients are statistically zero, some are statistically positive and no coefficient is statistically negative, in other words, I do not find any piece of evidence supporting that unilateral divorce decreases the demand of prostitution. Therefore, these findings suggest that unilateral divorce does not reduce the demand of prostitution.

[33] In addition, Appendix Section B.7 explores a supplementary Demand Mechanism connected to Edlund and Korn, 2002.

[34] Dank et al. (2014) also highlighted the expansion of internet use to match clients and prostitutes.

[35] Namely, "The Erotic Review", "Erotic Review" (easier and faster version to search in Google), "Craiglist", "Backpage" and "Backpage erotic". I cannot consider "Craiglist erotic" since it was not searched in Google enough times (i.e. it was searched so rarely Google does not keep track of the number of times).

[36] This website has been used in the literature to collect data on prostitutes and customers (Cunningham and Shah, 2017)

[37] Sample size varies across columns since Google trends data is available only for states where the number of searches is not close to zero. Searches of certain words were close to zero in some states. Yet, this was not the case for any treated state.

Preferences of divorced men

Unilateral divorce law might affect the demand of prostitution indirectly. For example, it could be that it is the act of getting divorced that affects people's attitudes instead of unilateral divorce law.

In order to study this instance I use data from the Youth Parent Socialization Survey (YPSS). This survey started in 1965 and had other three waves respectively in 1973, 1982 and 1997. Since the YPSS followed individuals during these three waves, using this data I can see how divorced people changed. The same data set has been used by Edlund and Pande (2002) to show that, after getting divorced, women become left-wing.

In particular, to proxy the demand of prostitution I can use changes in male opinions about prostitution. As a matter of fact, this survey measured the dislike of their respondents towards various issues, one of these issues is prostitution. Consequently, I can observe if, after getting divorced, men said that they dislike prostitution more or less often than before.

In this case, I run the following regression model:

$$Dislike\, Prostitution_{iw} = \beta_1 divorced_{iw} + \beta_2 divorced_{iw} * male_i + X_{iw}\delta + \alpha_i + \alpha_w + \varepsilon_{iw} \tag{2.4}$$

where $Dislike\, Prostitution_{iw}$ is a dummy variable taking value 1 if the respondent i expresses dislike towards prostitution in the wave of the survey w, X_{iw} is a vector of characteristics that includes gender of the respondent and marital status in the w wave of the survey and α_i, α_w are respectively individual and wave fixed effects. Finally, $divorced_{iw}$ is a dichotomous variable that takes value 1 if individual i was divorced in wave w of the survey. In addition, standard errors are clustered at school code level.

This regression exploits the variation of being divorced across successive waves of the survey for a given individual to compute the correlation between being divorced and disliking prostitution, and being a divorced man and disliking prostitution. If being divorced is correlated with greater disliking of prostitution β_1 would be positive. Likewise, if divorced men dislike more prostitution than married men, β_2 would be positive.

Column (1) of Table 2.10 shows the results of regression model (3). Both β_1 and β_2 are not statistically significant. Furthermore, β_2 is positive suggesting that being a divorced man is correlated with more aversion towards prostitution. To double check these findings column (2) of Table 2.10 pools together respondents whose marital status is divorced or separated. Column (1) considers only respondents who said that were divorced, while in column (2) being separated or divorced is considered to be the same. Once, I pool together these two groups β_2 is negative suggesting that there could be mild evidence that being divorced is correlated with openness towards prostitution. However, both β_1 and β_2 are again not statistically significant.

Notwithstanding, it might be that is the first time men get divorced when men change their preferences toward prostitution. Since the YPSS considers the marital status of respondents in wave w, if this were the case, this would bias my results.[38] Consequently, as a further check, the last two columns of Table 2.10 (i.e. namely, columns (3) and (4)) consider respondents who claimed to be divorced/separated in a previous wave of the YPSS as divorced and/or separated. As an example, suppose individual j was divorced in wave 2 and married again in wave 3, column (1) would consider such individual as divorced in the former and married in the latter;

[38] As a matter of fact, a respondent could get divorced in an earlier wave and then get married again.

whereas, column (3) would consider such individual as divorced in both periods. Column (4) does the same pooling together both divorced and separated individuals. Column (3) and (4) of Table 2.10 show that both β_1 and β_2 are again not statistically different from zero.

Unmarried men

The last dimension in which I test whether unilateral divorce shifts the demand of prostitution is using data on unmarried men. According to Edlund and Korn (2002) unmarried men demand more prostitution than married men. Hence, finding that unilateral divorce is associated with a decrease in unmarried men might be evidence that the demand of prostitution declines, leading to a reduction in arrested female prostitutes.

To compute the number of unmarried men per state I use monthly data of the Current Population Survey (CPS) between 1980 and 2014. Therefore, since CPS data is at state level I collapse my data set at state level and run the following regression:

$$Unmarried\,men_{smy} = \beta Unilateral_{smy} + \alpha_m + \alpha_y + \alpha_s + \alpha_s * y + \varepsilon_{smy} \tag{2.5}$$

where $Unmarried\,men_{smy}$ is either the number of unmarried men per 1,000,000 inhabitants in state s at month m and year y or its growth rate. The other variables follow the same notation of regression model (2). Column (1) and (2) of Table 2.11 respectively show the results using as dependent variable the number of unmarried men per 1,000,000 and its growth rate.[39] Column (3) shows the results for the logarithmic transformation of the number of unmarried men per 1,000,000. As Table 2.11 shows the estimated coefficients are positive and not statistically different from zero.These results suggest that unilateral divorce does not affect the number of unmarried men.[40]

In a nutshell, this subsection does not find any empirical evidence that unilateral divorce law shifts the demand of prostitution. Thereby, this evidence supports that the decline found in arrested female prostitutes is not caused by a decay of the demand of prostitution.

2.9.3 Supply mechanisms

So far, I have not found any empirical evidence supporting that unilateral divorce law decreases the number of arrests of crimes in general nor that it affects the demand of prostitution. Therefore, I am left with the only alternative that such law could have reduced the supply of prostitution, I refer to such channels as "supply mechanisms". As explained at the beginning of this section, I proceed to test the two supply mechanisms suggested by Edlund and Korn (2002): wives' wage and marriage compensation.

[39] I run both regressions since it could be argued that the number of unmarried men does not vary substantially over months.

[40] Note that this result does not contradict the marriage compensation mechanism since according to this mechanism unilateral divorce improves wives' welfare. First, the effect of unilateral divorce law on the marriage market is a composite effect depending on the effect of such law on other sub-populations (not only on prostitutes). Second, it might be that prostitutes do not enter or exit prostitution with the hope of getting married but do not get married in the end.

Wives' wage

The non-arbitrage condition between marriage and prostitution in Edlund and Korn (2002) establishes that p, the wage earned by prostitutes, must be equal to w, the wage earned in the labor market by wives, plus p_m, the compensation paid in equilibrium in the marriage market. If unilateral divorce law increases w, prostitution in equilibrium will decrease.

Thus, it seems plausible that, since unilateral divorce law bolsters women's rights, it could lead to an increase in wives' wages. An increase in w makes marriage more attractive to women causing that some women could prefer to exit prostitution.

In order to test this hypothesis this subsection makes use of monthly CPS data to compute the average real wage of married women across states in the U.S. Similar to regression model (4) I run the following specification:

$$W_{smy} = \beta Unilateral_{smy} + \alpha_m + \alpha_y + \alpha_s + \alpha_s * y + \varepsilon_{smy} \tag{2.6}$$

where W_{smy} stands for wives' average real wage in state s in month m of year y, while the rest of terms follow the same notation as in regression models (2) and (4).

Column (1) of Table 2.12 shows the result of this specification using as dependent variable wives' average real wage in logs, while column (2) report results for wages in levels.

Table 2.12 shows that the estimated coefficients of such regressions are both close to zero and not statistically different from zero. This finding supports that the decay found in arrested female prostitutes is not caused by an increase in wives' wages.[41]

Marriage compensation

As discussed in Section 2.4, an increase in wives' welfare is tantamount to an increase in p_m. If unilateral divorce law increases p_m, following Edlund and Korn (2002), prostitution declines. I refer to this as the marriage compensation mechanism.

The compensation p_m paid in equilibrium in the marriage market can be interpreted as the compensation husbands pay (both pecuniary and non-pecuniary) to wives. According to Edlund (2013), p_m is a compensation for custodial rights. In other words, traditionally women are the solely guardian of children for out-of-wedlock births (i.e. births outside of marriage), while, within marriage the guardians of a child are her/his parents. Hence, within marriage women sell a share of their custodial rights to their husbands and p_m is what they get in exchange. Thus, if unilateral divorce increases p_m, the main beneficiaries will be women that can get married and have kids, in other words, women who are in age of marriage and fertility.

To test this hypothesis, I restrict my sample to women that are in both marriage and fertile age. Despite the fact that in many states the minimum age to get married is 18 years old, usually people get married older. In my sample period the median marriage age in the U.S. for women is 24.8 years old.[42] In addition, Alesina and Giuliano (2007) studied the effect of unilateral divorce on fertility and used 49 years

[41] Note that considering the impact of unilateral divorce on labor force participation of wives would be uninformative on this (i.e. wives' wage) mechanism. As a matter of fact, it could be that labor force participation of wives rises after the introduction of unilateral divorce due to an improvement in wives' bargaining position within the household.

[42] I computed the median age between 1980 and 2014 of women at first marriage from the U.S. Census Bureau. The median is 24.8 years old and the average is 24.5 years old.

old as the boundary age for women. Accordingly, I restrict to women between 25 and 49 years old and I refer to this group as women in marrying-fertile age.[43]

If unilateral divorce increases p_m, the reduction in arrested female prostitutes would be larger (in absolute value) in the marrying-fertile age group than for other age groups. Thereby, I run the main regression separately for women in the marrying-fertile age and in other ages.[44] Comparison of the estimated coefficients for the two groups determines whether the impact of unilateral divorce law across these two age groups differs or is the same.

Table 2.13 shows the results of running the main regression for these two samples of women. Column (1) and (3) show the results using $\log(1 + y)$ as dependent variable, while column (2) and (4) use the IHS transformation. Comparing columns (1) and (3), and columns (2) and (4) I find that the estimated coefficients for women in marrying-fertile age are much larger (in absolute value) than their counterparts for other ages.

To provide a further test since estimated coefficients are insignificant across both samples, equation (6) presents a regression model that separates the number of arrested prostitutes according to the two previously defined age groups.

$$\begin{aligned} log(1 + Prostitution_{acsmy}) = \beta_1 Unilateral_{smy} + \beta_2 \alpha_a * Unilateral_{smy} \\ + \alpha_a + \alpha_m + \alpha_y + \alpha_c + \alpha_c * y + \varepsilon_{acsmy} \end{aligned} \tag{2.7}$$

The difference with respect to the main specification (i.e. equation (1)) is that this regression model takes into account the age group a of the arrested prostitutes. α_a is a dummy variable taking value 1 if the arrested prostitutes are in in the marrying-fertile age group and 0 if they are not. Running this regression allows to test whether unilateral divorce has a different effect according to the age group. Indeed, β_1 captures the effect of unilateral divorce law on arrested prostitutes not in the marrying-fertile age group, while $\beta_1 + \beta_2$ captures the effect of such law on arrested prostitutes in the marrying-fertile age group.

Hence, testing if unilateral divorce has a different effect on arrested prostitutes in the marrying-fertile age group is equivalent to test whether β_2 is different from zero. Columns (5) and (6) run this regression model respectively using $\log(1 + y)$ as dependent variable and the IHS transformation. In both cases the age fixed effect (i.e. α_a) is positive and statistically different from zero, indicating that there are more arrested prostitutes in that age group. Most importantly, in both regressions $\hat{\beta}_1$ is negative but it is not different from zero, while, $\hat{\beta}_2$ is negative and different from zero at 5% level pointing that the reduction in arrested female prostitutes is larger (in absolute value) in the marrying-fertile age group. [45]

[43] The relative size of the two samples is fairly balanced since around 60% of my sample falls in the marrying-fertile age range (Table B.3). Moreover, it is important to note that only having data on prostitutes' prices would not be informative to check the marriage compensation mechanism. A potential threat to this approach is that since according to Edlund, Engelberg, and Parsons (2009) prostitutes' prices are higher for women between 21 and 40 years old, if unilateral divorce law decreases the number of prostitutes in marrying-fertile age due to a rise in p_m, I might find an ebb in average prostitutes' prices only because some of the prostitutes with highest prices are exiting the market.

[44] Edlund and Korn (2002) model aside, running this regression also tests whether unilateral divorce has an impact on the supply of prostitution as a whole. If unilateral divorce decreases the supply of prostitution as a whole, without affecting the marriage compensation, there is no reason to believe that the effect of this law on prostitution differs across age groups.

[45] A possible concern could be that these findings are driven by the inclusion of arrested prostitutes older than 49 years old in the comparison group (i.e. in the group "Other ages). To this extent, Appendix Section B.8.1 replicates the analysis using only arrested prostitutes between 17 and 24 years

As a double-check I run the same event study as in Section 2.7 but restricting the sample first to arrested female prostitutes in marrying-fertile age and then to arrested female prostitutes in other ages. If the reason for the decline in arrested female prostitutes is a rise in p_m, then female prostitutes in marrying-female age will be driving the results. In other words, the event study would show that the reduction in arrested female prostitutes, is due to a reduction in female prostitutes in marrying-fertile age.

Figure 2.4 and 2.5 respectively show the results of the event study for arrested female prostitutes in marrying-fertile age and in other ages. As Figure 2.4 shows, after unilateral divorce laws become effective, arrested female prostitutes in marrying-fertile age decrease. In fact, all the estimated coefficients prior to the event are non-negative and jointly not statistically significant, while all the estimated coefficients after the event are negative and are jointly statistically significant. Whereas, the same cannot be said about arrested female prostitutes in other ages: simple visual inspection of the graph makes clear that unilateral divorce does not seem to have any effect on this group. As a matter of fact, for this regression, both the estimated coefficients prior and posterior to the entry into force of unilateral divorce law are not statistically significant.[46] This evidence supports that unilateral divorce laws increase p_m, which in turn makes marriage more attractive to prostitutes and, hence decreases female prostitution in equilibrium.[47]

Overall, this evidence provides additional support on unilateral divorce law increasing p_m.[48]

An important strand of the literature is in line with this evidence. Stevenson and Wolfers (2006) find that unilateral divorce decreases female suicides, females murdered by their partners and domestic violence. According to Stevenson and Wolfers (2006), unilateral divorce transfers bargaining power toward the abused spouse, potentially stopping the mistreatment in extant relationships. As far as the abused spouse is usually the wife, this channel implies an increase in wives' welfare, and consequently a rise in p_m. Alesina and Giuliano (2007) suggest that unilateral divorce makes marriage more attractive since the exit option is easier. According to these authors, unilateral divorce makes people feel less locked in marriages, so women (even women planning child bearing) are more likely to accept marriage. Alesina and Giuliano (2007) find that unilateral divorce decreases both out-of-wedlock fertility and never-married women, while, it does not affect in-wedlock fertility. Thereby, the total fertility rate declines. In other words, with an easier "exit option" shot-gun marriages become less threatening. Such results are coherent with my findings in two ways. First, these results are in line with an increase in p_m, since they find empirical evidence supporting that unilateral divorce law makes marriage more attractive to women, because "exiting it" is easier. Second, a share of the decrease in never-married women could be explained by the decay of female prostitutes caused by such law.

old in the comparison group. In addition, Section B.8.3 replicates this analysis for indoor prostitution. Results do not change.

[46] Note that the graph for arrested female prostitutes in other ages is more precise. Hence, the lack of a pattern in this case cannot be linked to lack of precision in estimates.

[47] Likewise, in Appendix Section B.8.2 I compare the parallel trends graphs of the two restricted samples (i.e. marrying-fertile age vs other ages).

[48] It could be argued that the model developed in Edlund and Korn (2002) suits better indoor prostitution than street prostitution. Thus, finding empirical evidence in favour of the same mechanism also for indoor prostitution is reassuring.

2.10 Concluding remarks

This paper uses a quasi-natural experiment setting provided by differences in the timing of entry into force of unilateral divorce laws across U.S. states to study the effect of such laws on the amount of female prostitution (proxied by arrest of female prostitutes in the absence of any other reliable information on this illegal activity). My main finding is that unilateral divorce law decreases female prostitution arrests by roughly 10% in the year after entry into force of such divorce laws. This estimate of the causal effect translates into a reduction of about 6,000 women arrested for prostitution in the U.S. According to HG.org (2017) estimates, this decrease in prostitution arrests yields a reduction of about $15 million for American taxpayers.

To explore the credibility of the identification assumption behind the previous causal effect, two different methodologies are used: an event study and visual inspection of parallel trends in control and treated groups (states) in a time window close to the policy intervention. I find conclusive evidence that the causal effect occurs after the entry into force of the law and that prior to the policy intervention such groups exhibited similar trends.

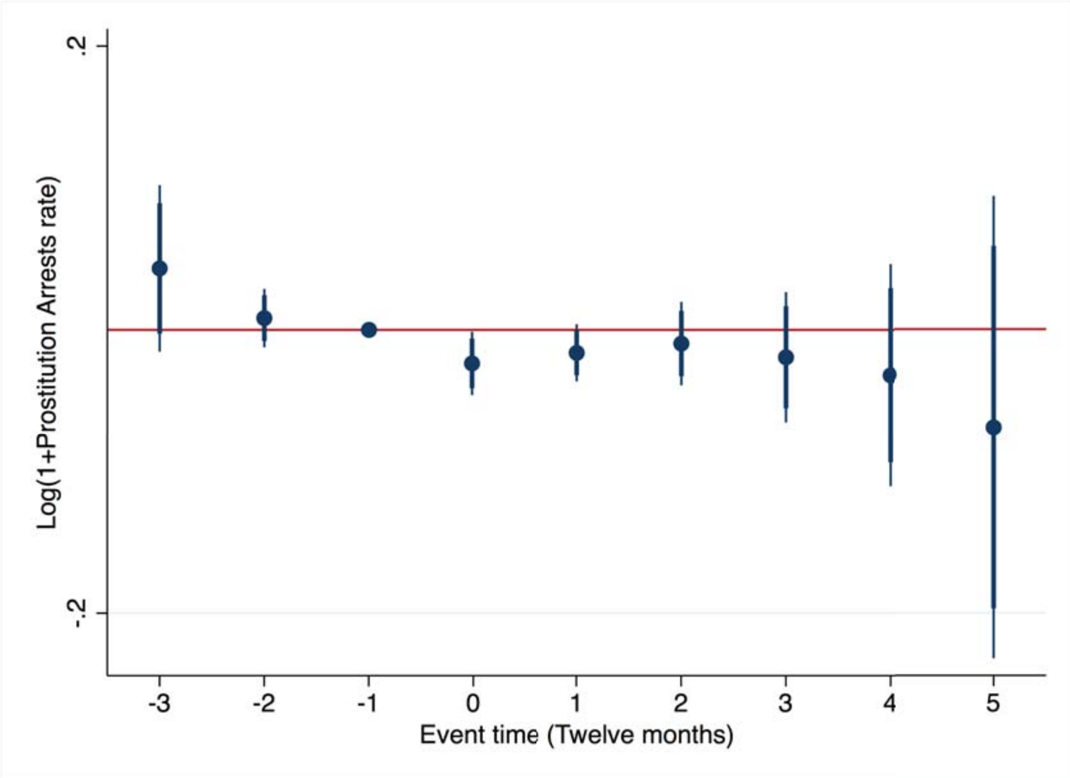
Next, I consider each of the underlying channels that could be driving the results. The explored mechanisms range from changes in police officers' effectiveness in fighting crimes to shifts in the demand and supply of prostitution. To identify the latter, I rely on the well-known model of the link between marriage and prostitution markets proposed by Edlund and Korn (2002). First, I explore if unilateral divorce laws causes either a decrease in police officers or a general decline in arrests for all sorts of crimes. Using respectively data on hired officers and women arrested for robberies, vandalism and drugs, I do not find empirical evidence in favor of this mechanism. Next, I examine if unilateral divorce laws shift the demand of prostitution. Three different data sets are used to capture distinct features of the demand of prostitution: (i) number of searches of words linked to prostitution as a proxy for the online demand of prostitution; (ii) panel-survey data about views on prostitution of divorced men; and (iii) data on the number of unmarried men in each state as a proxy of the overall demand of prostitution by unmarried men. In none of these data sets, I find evidence that unilateral divorce law shifts the demand of prostitution.

Next, I test for shifts in the supply of prostitution. I explore if unilateral divorce law affects wives' real wages. To the extent that such laws increase female bargaining power in a married couple, they would increase the value of marriage through higher wages, so that the supply of prostitution would decline. Again, the empirical evidence on this issue does not support this mechanism. Finally, I examine if unilateral divorce law improves wives' conditions in marriage (i.e. wives' welfare). The existing literature (see, e.g., Edlund and Korn, 2002; Edlund, 2013) seems to suggest that the main beneficiaries of an improvement in wives' welfare would be women in marriage and fertile age. Therefore, I split the sample of arrested female prostitutes into different age groups and check how they respond to unilateral divorce laws. I find that female prostitutes in marriage and fertile age are the main driver of the reduction in arrested female prostitutes that follows the implementation of these divorce laws.

Hence, the overall evidence presented in this paper points out that the main mechanism through which unilateral divorce laws have a causal effect on prostitution is by improving women's compensation when married (through transfers from husbands to wives) which subsequently leads to a reduction in the supply of prostitution. Since the empirical evidence presented earlier does not yield support to a rise in the demand for prostitution, reduced supply would translate into a smaller amount of prostitution in equilibrium. To the best of my knowledge, this is one of the first papers to show that improving prostitutes’ outside option deters prostitution.

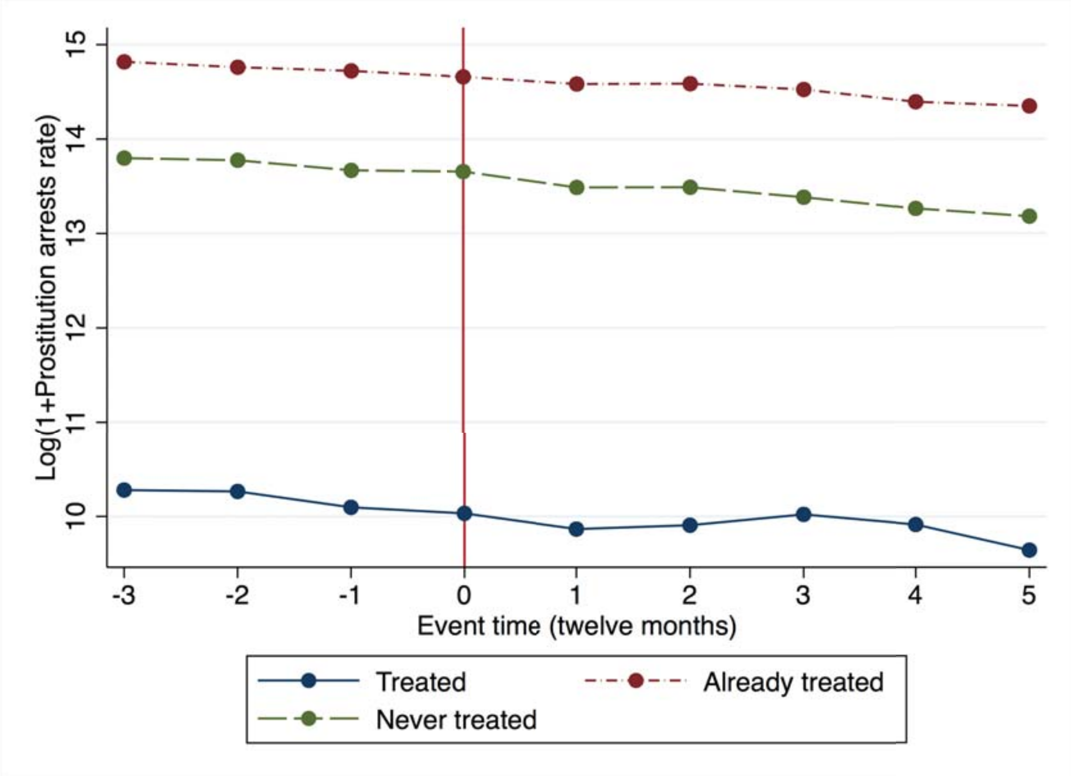
Figures & Tables

FIGURE 2.1: Event study



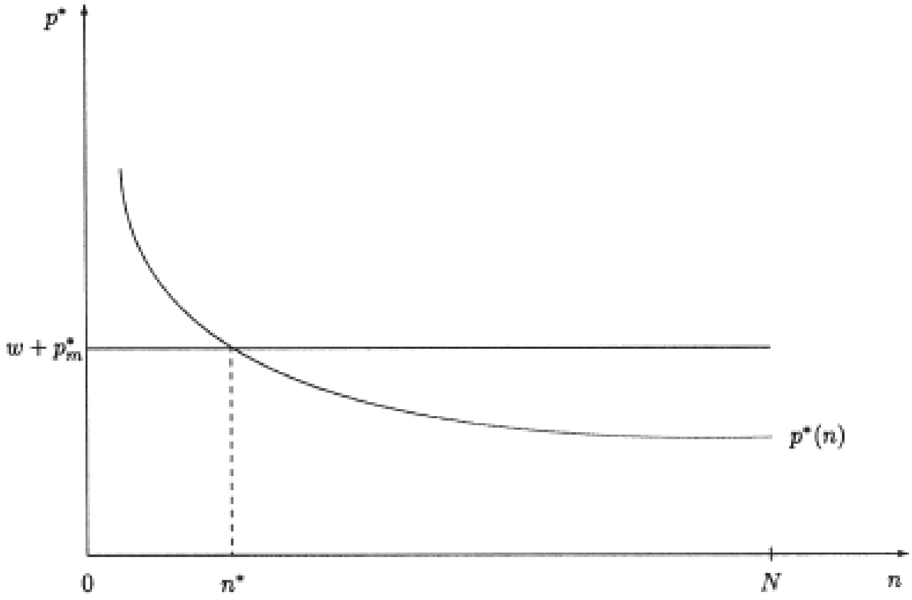
Notes: This figure plots the estimated coefficients of the event study analysis three years prior and five posterior to the enter into force of unilateral divorce law. On the horizontal axis there is the event time, each period lasts twelve months (e.g. period 0 comprises the month in which unilateral divorce law becomes effective and eleven moths after that). On the vertical axis the coefficients are measured in terms of their effect on the dependent variable. The coefficients are measured relative to the omitted coefficient ($t = -1$). For each coefficient the dot graphs the point estimate, while the length of the lines graphs confidence intervals at both 90% and 95% level. The pattern of the estimated coefficients is consistent with the identification assumption: they show absence of a strong pre-trend and a trend break after the enter into force of unilateral divorce law. In fact, the two coefficients prior to the event (i.e. -3 and -2) are not negative and are not jointly statistically significantly different from zero, whereas, the coefficients after the event (i.e. 0, 1, 2, 3, 4 and 5) are negative and jointly statistically significantly different from zero. Furthermore, the estimated coefficients in the first and second year after the introduction of the policy (i.e. 0 and 1) are individually statistically different from zero at standard significance levels.

FIGURE 2.2: Parallel trends between treated and control groups



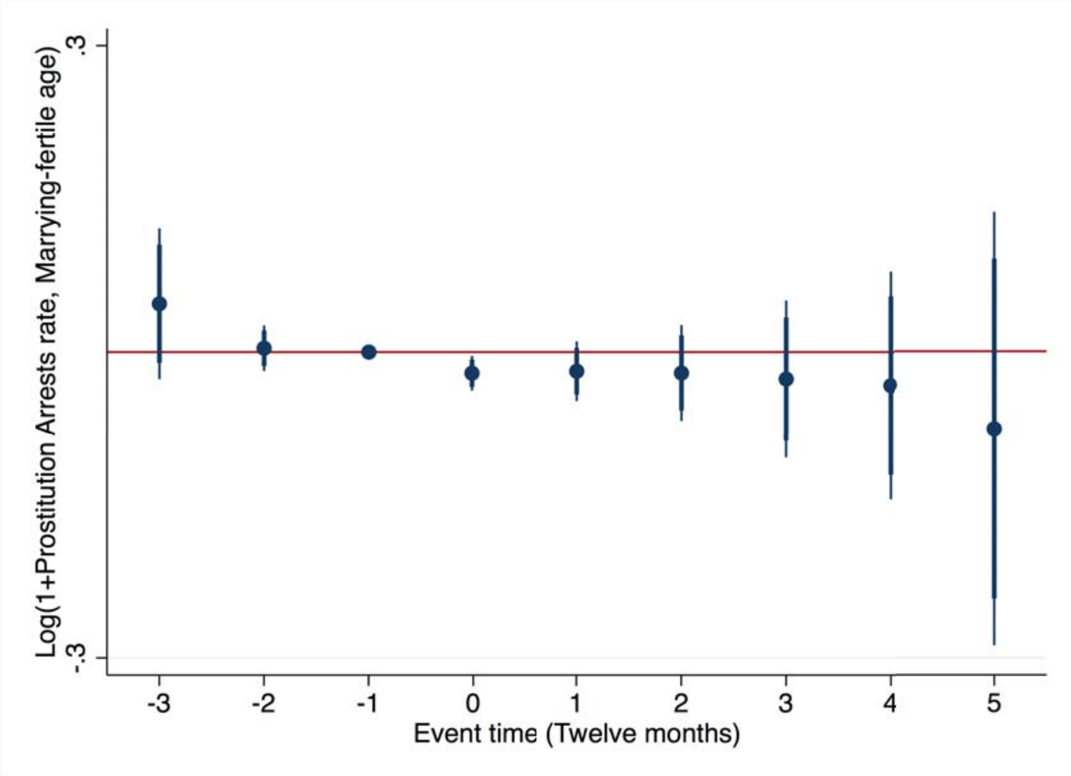
Notes: This figure plots the trends of the treated and control groups three years prior and five after the enter into force of unilateral divorce law. On the horizontal axis there is the event time, each period lasts twelve months (e.g. period 0 comprises the month in which unilateral divorce law becomes effective and eleven moths after that). On the vertical axis there is the average value of the dependent variable in that period of time. The treated group's trend is an average for each treated county. Details on the computations of the control groups' trend can be found in the paper. This figure shows that treated and control groups seem to be on the same trend prior to the enter into force of unilateral divorce law.

FIGURE 2.3: Marriage and prostitution market equilibrium



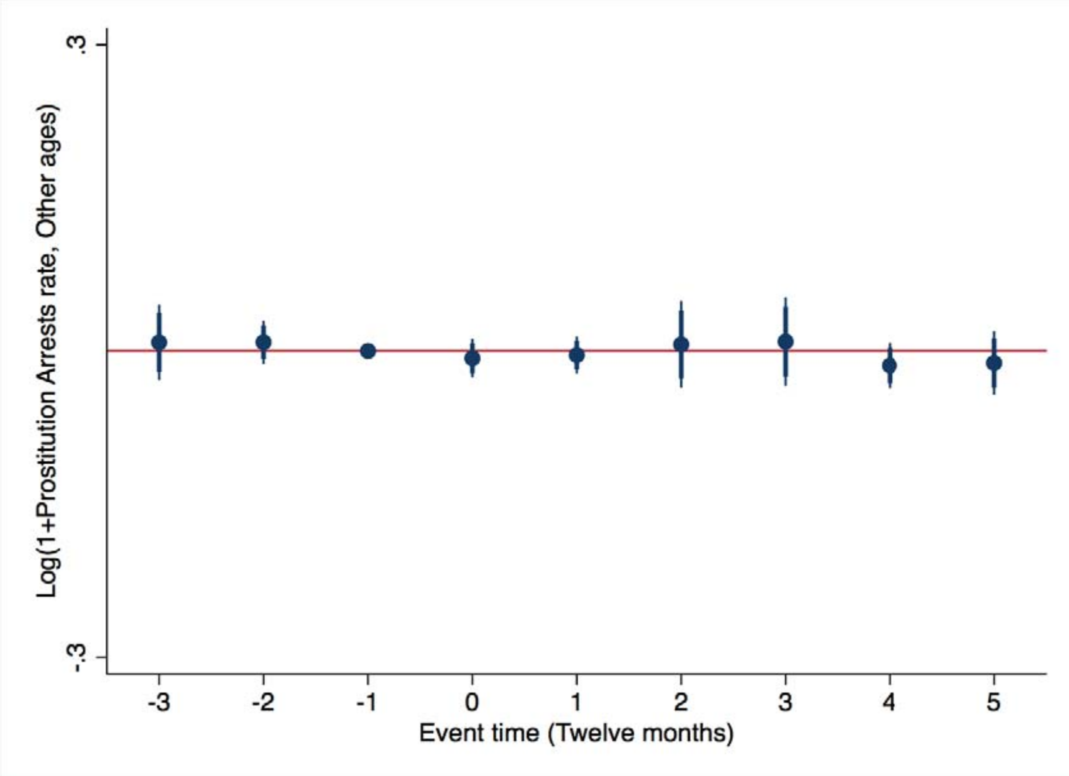
Source: Edlund and Korn (2002).

FIGURE 2.4: Event study for arrested female prostitutes in marrying-fertile age



Notes: This figure plots the estimated coefficients of the event study analysis three years prior and five posterior to the enter into force of unilateral divorce law for the sample in marrying-fertile age. On the horizontal axis there is the event time, each period lasts twelve months (e.g. period 0 comprises the month in which unilateral divorce law becomes effective and eleven moths after that). On the vertical axis the coefficients are measured in terms of their effect on the dependent variable. The coefficients are measured relative to the omitted coefficient ($t = -1$). For each coefficient the dot graphs the point estimate, while the length of the lines graphs confidence intervals at both 90% and 95% level. The pattern of the estimated coefficients is consistent with the identification assumption: they show absence of a strong pre-trend and a trend break after the enter into force of unilateral divorce law. In fact, the two coefficients prior to the event (i.e. -3 and -2) are not negative and are not jointly statistically significantly different from zero, whereas, the coefficients after the event (i.e. 0, 1, 2, 3, 4 and 5) are negative and jointly statistically significantly different from zero. This evidence is consistent with the "Marriage Compensation" mechanism.

FIGURE 2.5: Event study for arrested female prostitutes in other ages



Notes: This figure plots the estimated coefficients of the event study analysis three years prior and five posterior to the enter into force of unilateral divorce law for the sample in "other ages". On the horizontal axis there is the event time, each period lasts twelve months (e.g. period 0 comprises the month in which unilateral divorce law becomes effective and eleven moths after that). On the vertical axis the coefficients are measured in terms of their effect on the dependent variable. The coefficients are measured relative to the omitted coefficient ($t = -1$). For each coefficient the dot graphs the point estimate, while the length of the lines graphs confidence intervals at both 90% and 95% level. The pattern of the estimated coefficients is not consistent with the identification assumption: both coefficients prior and posterior to the event are not statistically significant. Note that coefficients are considerably more precise for this age group than for marrying-fertile age. This evidence is consistent with the marriage compensation mechanism.

TABLE 2.1: Effective months of entry into force of unilateral divorce laws

	Unilateral Divorce (updated Gruber (2004)) (1)	Unilateral Divorce with Separation Requirements (updated Caceres-Delpiano & Giolito (2012)) (2)
Alabama	1971	
Alaska	1935	
Arkansas		
Arizona	1973	
California	1970	
Colorado	1972	
Connecticut	1973	
District of Columbia		1 year 1977
Delaware	1968	
Florida	1971	
Georgia	1973	
Hawaii	1972	
Idaho	1971	
Illinois		2 years, August 1984
Indiana	1973	
Iowa	1970	
Kansas	1969	
Kentucky	1972	
Louisiana		1 year, pre 1968
Maine	1973	
Maryland		5 years; later 2 years pre-1968
Massachusetts	1975	
Michigan	1972	
Minnesota	1974	
Mississippi		
Missouri	September 2009	2 years, 1973
Montana	1973	
Nebraska	1972	
Nevada	1967	
New Hampshire	1971	
New Jersey	January 2007	18 months, 1971
New Mexico	1933	
New York	October 2010	
North Carolina		1 year, pre-1968
North Dakota	1971	
Ohio		1 year, 1974
Oklahoma	1953	
Oregon	1971	
Pennsylvania		3 years, 1980; 2 years, January 1991
Rhode Island	1975	
South Carolina		3 years; later 1 year, 1969
South Dakota	January 1985	
Tennessee		
Texas	1970	
Utah	January 1987	3 years, pre-1968
Vermont		6 months, pre-1968
Virginia		2 years, pre-1968
Washington	1973	
West Virginia	September 2001	2 years; later 1 year, pre-1968
Wisconsin	1978	
Wyoming	1977	

Notes: This table reports the effective of entry into fore of unilateral divorce laws across U.S. states. It reports the effective year for states where unilateral divorce law entered into force prior to 1980, and the effective month for states where unilateral divorce law entered into force during my sample period (i.e. between 1980 and 2014). Column (1) of this table updates Gruber (2004), while column (2) updates Cáceres-Delpiano and Giolito (2012).

TABLE 2.2: Main results

Panel A: Log(1+y)	(1)	(2)	(3)	(4)
Unilateral	-0.0719**	-0.0687*	-0.0682*	-0.0685*
	(0.0351)	(0.0349)	(0.0349)	(0.0349)
	[0.046]	[0.055]	[0.056]	[0.055]
Panel B: IHS	**(1)**	**(2)**	**(3)**	**(4)**
Unilateral	-0.0848**	-0.0814*	-0.0808*	-0.0812*
	(0.0413)	(0.0411)	(0.0411)	(0.0411)
	[0.046]	[0.053]	[0.055]	[0.054]
Panel C: LPM	**(1)**	**(2)**	**(3)**	**(4)**
Unilateral	-0.0179**	-0.0182**	-0.0181**	-0.0182**
	(0.0088)	(0.0088)	(0.0088)	(0.0088)
	[0.047]	[0.043]	[0.045]	[0.044]
Panel D: Levels	**(1)**	**(2)**	**(3)**	**(4)**
Unilateral	-0.8309*	-0.7661*	-0.7619*	-0.7699*
	(0.4209)	(0.4467)	(0.4462)	(0.4473)
	[0.054]	[0.093]	[0.094]	[0.092]
Observations	1,252,282	1,252,282	1,252,282	1,252,282
Clustered variance at State level	✓	✓	✓	✓
County FE	✓	✓	✓	✓
County Year Trends	✓	✓	✓	✓
Year FE		✓	✓	
Month FE			✓	
Year-Month FE				✓

Clustered standard errors at state level in parentheses, p values in brackets.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: This table displays the estimated coefficients of running specification (1). Data is at county-month level. Standard errors are clustered at state level. Column (1) includes county fixed-effects and county year-trends, column (2) adds year fixed-effects, column (3) adds month fixed-effects and column (4) uses year-month fixed effects.

TABLE 2.3: Robustness check: different control groups

VARIABLES	(1) Only Already Treated	(2) Only Never Treated
Unilateral	-0.0746**	-0.0535
	(0.0351)	(0.0348)
Observations	904,570	487,728
Clustered variance at State level	✓	✓
County Year Trends	✓	✓
County FE	✓	✓
Year FE	✓	✓
Month FE	✓	✓

Clustered standard errors at state level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: This table displays the estimated coefficients of running specification (1) using only one of the two control groups. Data is at county-month level. Standard errors are clustered at state level. Column (1) restricts to already-treated, while column (2) restricts to never-treated.

TABLE 2.4: Robustness check: including the effective month of no-fault divorce law as control

VARIABLES	(1) Log(1+y)	(2) Log(1+y)	(3) Log(1+y)	(4) Log(1+y)
Unilateral	-0.0736*	-0.0690*	-0.0684*	-0.0689*
	(0.0369)	(0.0364)	(0.0364)	(0.0364)
Observations	1,252,282	1,252,282	1,252,282	1,252,282
Clustered variance at State level	✓	✓	✓	✓
County FE	✓	✓	✓	✓
County Year Trends	✓	✓	✓	✓
Year FE		✓	✓	
Month FE			✓	
Year-Month FE				✓

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Notes: This table displays the estimated coefficients of running specification (1) including No-Fault divorce effective month as a control variable. Data is at county-month level. Standard errors are clustered at state level. Column (1) includes county fixed-effects and county year-trends, column (2) adds year fixed-effects, column (3) adds month fixed-effects and column (4) uses year-month fixed effects.

TABLE 2.5: Robustness check: using the effective month of no-fault divorce law as treatment

VARIABLES	(1) Log(1+y)	(2) Log(1+y)	(3) Log(1+y)	(4) Log(1+y)
No-Fault	-.00980	-0.0167	-0.0168	-0.0165
	(0.0111)	(0.0129)	(0.0129)	(0.0128)
Observations	1,252,282	1,252,282	1,252,282	1,252,282
Clustered variance at State level	✓	✓	✓	✓
County FE	✓	✓	✓	✓
County Year Trends	✓	✓	✓	✓
Year FE		✓	✓	
Month FE			✓	
Year-Month FE				✓

Clustered standard errors at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table displays the estimated coefficients of running specification (1) replacing No-Fault divorce effective month as main regressor (i.e. replacing Unilateral divorce with No-Fault divorce). Data is at county-month level. Standard errors are clustered at state level. Column (1) includes county fixed-effects and county year-trends, column (2) adds year fixed-effects, column (3) adds month fixed-effects and column (4) uses year-month fixed effects.

TABLE 2.6: Potential mechanisms: fight against crime mechanism

VARIABLES	(1) Officers	(2) Officers	(3) Officers	(4) Officers	(5) Log Officers	(6) Log Officers	(7) Log Officers	(8) Log Officers
Unilateral	-0.00382	0.0361	-0.0116	-0.0210	0.00713	0.0153	0.0207	0.0166
	(0.0702)	(0.0849)	(0.0846)	(0.0752)	(0.0580)	(0.0762)	(0.0427)	(0.0262)
Observations	2,250	2,250	1,750	1,750	2,250	2,250	1,750	1,750
Clustered variance at State level	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State Year Trends		✓		✓	✓	✓		✓
Sample	1971-2016	1971-2016	1980-2014	1980-2014	1971-2016	1971-2016	1980-2014	1980-2014

Clustered standard errors at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table displays the estimated coefficients of running specification (2). Data is at state-year level. Standard errors are clustered at state level. Columns (1) to (4) use the dependent variable in levels, columns (5) to (8) use the dependent variable in logs.

TABLE 2.7: Potential mechanisms: fight against crime mechanism

VARIABLES	(1) Log(1+y) Robbery	(2) IHS Robbery	(3) Log(1+y) Drugs	(4) IHS Drugs	(5) Log(1+y) Vandalism	(6) IHS Vandalism
Unilateral	-0.00172	-0.00221	-0.0655	-0.0809	0.0256	0.0277
	(0.00836)	(0.0102)	(0.0906)	(0.102)	(0.0589)	(0.0681)
Observations	1,252,282	1,252,282	1,252,282	1,252,282	1,252,282	1,252,282
Clustered variance at State level	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
County Year Trends	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓

Clustered standard errors at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table displays the estimated coefficients of running specification (1) using female robberies, vandalism and drugs arrests as dependent variable. Data is at county-month level. Standard errors are clustered at state level. Column (1), (3) and (5) use $\log(1+y)$ as dependent variable, while column (2), (4) and (6) use the IHS transformation as dependent variable.

TABLE 2.8: Potential mechanisms: demand proxied by Google Trends data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
			Synonyms of prostitute							Indoor prostitution			Websites			
VARIABLES	Prostitute	Bitch	Call Girl	Whore	Hot babe	Hooker	Hustler	Sex	Stripper	Strip club	Escort	The Erotic Review	Erotic Review	Craiglist	Backpage	Backpage Erotic
Panel A: Levels																
Unilateral	1.548	2.343	-2.319	-0.990	2.811	3.317**	-0.653	0.553	1.114**	3.012	1.029	-1.094	-4.382	0.495	3.525	5.044*
	(1.967)	(2.817)	(2.754)	(1.647)	(2.533)	(1.454)	(0.673)	(1.970)	(0.444)	(3.207)	(2.623)	(4.080)	(5.692)	(6.766)	(6.135)	(2.442)
Panel B: Logs																
Unilateral	0.0661	0.0449	-0.123	-0.00262	0.130***	0.0600	0.00443	0.0120	0.0196	0.0389	0.0261	-0.0528	-0.0669	-0.0514	-0.0648	0.0492
	(0.0594)	(0.0397)	(0.0915)	(0.0776)	(0.0454)	(0.0715)	(0.0531)	(0.0265)	(0.0493)	(0.0835)	(0.0373)	(0.215)	(0.204)	(0.196)	(0.201)	(0.0526)
Panel C: Levels																
Unilateral	0.0641	0.0455	-0.145	-0.00180	0.140**	0.0560	0.00256		0.0179	0.0360	0.0268	-0.0524	-0.0666	-0.0667	-0.104	0.0235
	(0.0712)	(0.0436)	(0.118)	(0.0931)	(0.0627)	(0.0869)	(0.0678)		(0.0629)	(0.0982)	(0.0393)	(0.245)	(0.228)	(0.258)	(0.244)	(0.0700)
Observations	8,262	8,262	7,452	8,262	7,128	8,262	8,262	8,262	8,262	8,262	8,262	7,128	5,994	8,262	8,262	2,430
Clustered variance at State level	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State Year Trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Clustered standard errors at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table displays the estimated coefficients of running specification (3). Data is at state-month level. Standard errors are clustered at state level. Each column includes state fixed-effects, state-year trends, year fixed-effects and month fixed-effects. Sample: January 2004 to December 2017.

TABLE 2.9: Potential mechanisms: demand proxied by Google Trends data, sample 2004-2014

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Synonyms of prostitute								Indoor prostitution				Websites			
VARIABLES	Prostitute	Bitch	Call Girl	Whore	Hot babe	Hooker	Hustler	Sex	Stripper	Strip club	Escort	The Erotic Review	Erotic Review	Craiglist	Backpage	Backpage Erotic
Panel A: Levels																
Unilateral	0.715	2.192***	-1.204	-0.0193	5.390***	1.167	0.880	1.589	0.772	2.527	1.749	-0.466	-4.501	3.638	3.019	8.429***
	(2.360)	(0.807)	(3.036)	(2.210)	(1.778)	(3.396)	(0.919)	(3.102)	(0.662)	(5.095)	(4.379)	(4.820)	(6.735)	(7.269)	(5.430)	(0.710)
Panel B: Logs																
Unilateral	0.0251	0.0709	-0.0843	0.0368	0.181***	0.0444	0.0562	0.0322	0.0314	0.0572	0.0433	-0.0991	-0.105	-0.00573	-0.163	0.00747
	(0.0626)	(0.0460)	(0.0909)	(0.0889)	(0.0347)	(0.0644)	(0.0600)	(0.0409)	(0.0551)	(0.106)	(0.0583)	(0.132)	(0.174)	(0.308)	(0.291)	(0.148)
Panel C: Levels																
Unilateral	0.0641	0.0455	-0.145	-0.00180	0.140**	0.0560	0.00256		0.0179	0.0360	0.0268	-0.0524	-0.0666	-0.0667	-0.104	0.0235
	(0.0712)	(0.0436)	(0.118)	(0.0931)	(0.0627)	(0.0869)	(0.0678)		(0.0629)	(0.0982)	(0.0393)	(0.245)	(0.228)	(0.258)	(0.244)	(0.0700)
Observations	8,262	8,262	7,452	8,262	7,128	8,262	8,262	8,262	8,262	8,262	8,262	7,128	5,994	8,262	8,262	2,430
Clustered variance at State level	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State Year Trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Clustered standard errors at state level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: This table displays the estimated coefficients of running specification (3). Data is at state-month level. Standard errors are clustered at state level. Each column includes state fixed-effects, state-year trends, year fixed-effects and month fixed-effects. Sample: January 2004 to December 2014.

TABLE 2.10: Potential mechanisms: demand proxied by YPSS data on opinions

VARIABLES	(1) Dislike Prostitution	(2) Dislike Prostitution	(3) Dislike Prostitution	(4) Dislike Prostitution
Divorced	-0.0174		0.00623	
	(0.0255)		(0.0311)	
Divorced & Male	0.0471		-0.0333	
	(0.0395)		(0.0383)	
Divorced/Separated		0.0305		0.0153
		(0.0280)		(0.0275)
Divorced/Separated & Male		-0.0259		-0.0464
		(0.0319)		(0.0320)
Observations	3,736	3,736	3,736	3,736
Clustered variance at School-code level	✓	✓	✓	✓
Individual FE	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓

Clustered standard errors at state level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: This table displays the estimated coefficients of running specification (4). Standard errors are clustered at school-code level.

TABLE 2.11: Potential mechanisms: demand proxied by number of unmarried men

VARIABLES	(1) Unmarried	(2) Unmarried growth	(3) Unmarried Log(y)
Unilateral	421.7	0.00216	0.0119
	(487.1)	(0.00186)	(0.0149)
Observations	20,400	20,300	20,400
Clustered variance at State level	✓	✓	✓
State FE	✓	✓	✓
Year FE	✓	✓	✓
Month FE	✓	✓	✓
State Year Trends	✓	✓	✓

Clustered standard errors at state level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: This table displays the estimated coefficients of running specification (5). Data is at state-month level. Standard errors are clustered at state level. Each column of the table uses a different dependent variable. Column (1) uses number of unmarried men, column (2) uses growth rate of the number of unmarried men, while column (3) uses number of unmarried men in logs. Each column includes state fixed-effects, state-year trends, year fixed-effects and month fixed-effects.

TABLE 2.12: Potential mechanisms: wives' wage

VARIABLES	(1) Log Average Married Women's Real Wage	(2) Average Married Women's Real Wage
Unilateral	0.000558	-0.0407
	(0.0162)	(0.142)
Observations	20,400	20,400
Clustered variance at State level	✓	✓
State FE	✓	✓
Year FE	✓	✓
Month FE	✓	✓
State Year Trends	✓	✓

Clustered standard errors at state level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: This table displays the estimated coefficients of running specification (6). Data is at state-month level. Standard errors are clustered at state level. Each column of the table uses a different dependent variable. Column (1) uses average married women's real wage in logs, column (2) uses average married women's real wage in levels. Each column includes state fixed-effects, state-year trends, year fixed-effects and month fixed-effects.

TABLE 2.13: Potential mechanisms: marriage compensation

VARIABLES	(1) Log(1+y) Marrying-Fertile age	(2) IHS Marrying-Fertile age	(3) Log(1+y) Other ages	(4) IHS Other ages	(5) Log(1+y) Joint regression	(6) IHS Joint regression
Unilateral	-0.0739	-0.0880	-0.0174	-0.0227	-0.0286	-0.0348
	(0.0466)	(0.0555)	(0.0158)	(0.0187)	(0.0242)	(0.0287)
Dummy Marrying -Fertile age					0.0813***	0.096***
					(0.0174)	(0.0207)
Unilateral*Dummy Marrying-Fertile age					-0.0402**	-0.0476**
					(0.0183)	(0.0218)
Observations	1,252,282	1,252,282	1,252,282	1,252,282	2,504,564	2,504,564
Clustered variance at State level	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
County Year Trends	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓

Clustered standard errors at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table displays the estimated coefficients of running specification (1) for marrying-fertile age sample and for "other ages" sample . Data is at county-month level. Standard errors are clustered at state level. Each column of the table uses a different dependent variable. Column (1) uses $\log(1+y)$ of the marrying-fertile age group, column (2) uses the IHS transformation of the marrying-fertile age group, column (3) uses $\log(1+y)$ of "other ages" group and column (4) uses the IHS transformation of "other ages" group. Column (5) and (6) show the results of running equation (6).

Chapter 3

The link between prostitution laws and rape crimes: Evidence on alternative mechanisms

3.1 Introduction

The European Union Agency for Fundamental Rights (hereafter, FRA) issued the first official report on violence against women in 2014.[1] The report, titled *Violence Against Women: An EU-wide Survey*, documents that 1 out of 3 women in the EU has been victim of physical or sexual violence at least once since the age of 15. In particular, for that same age group, it was found that 11% of women have been victims of sexual violence and 5% (a group of around 9 million) have been victims of rape. It is pointed out that the main psychological consequences for the victims of such crimes are depression, anxiety, loss of self confidence and panic attacks.

In addition, it is well acknowledged that, even in Western countries, rape is still a gender issue: women are drastically over-represented among victims of this crime. This feature is common to all countries, including those where gender violence is severely punished, like in Scandinavia. For example, according to the Swedish National Council for Crime Prevention (Bra), six times as many women as men stated in 2014 that they have been victims of sex offenses in Sweden.[2]

Yet, from the data gathered in the FRA report it also emerges that in about 35% of the cases the victim did not report these crimes.[3] Possibly this lack of precise information has led rape to become a *forgotten* issue in the literature on crime economics.[4]

In this respect, recent economic literature (Cunningham and Shah, 2017; Bisschop, Kastoryano, and Klaauw, 2017; Ciacci and Sviatschi, 2016) has found evidence that prostitution and rape tend to behave as substitutes: higher prostitution rates are associated with lower rape crimes. In light of this evidence, a relevant question is whether criminalizing the purchase of prostitution affects rape.

This paper empirically explores the effect of criminalizing the purchase of prostitution on rape using data from Sweden. Research on this issue will allow social politicians to design crime policies for rape and regulations for the prostitution market according to their objectives.

My main finding is that there is a positive correlation between fines for sex purchase and rape. I argue that this correlation seems to be a lower bound to the causal effect of fines for sex purchase on rape. Next, I explore the channels through which

[1] European Union Agency for Fundamental Rights, 2014

[2] The precise figures are 1.8% of women and 0.3% of men.

[3] Own computations based on Table 3.4 of the report.

[4] According to Ideas (Repec) there are only 478 articles published with rape as a keyword in the whole economic field.

fines for sex purchase might affect rape and conclude that the mechanism that best fits the evidence is a behavioural mechanism.[5] This mechanism suggests that a higher number of fines for sex purchase is perceived by individuals as increasing the probability of being apprehended for this crime. As a result, the behaviour of customers changes.[6] In line with prostitution and rape being substitutes, a higher probability of being apprehended for sex purchase drives customers out of prostitution and pushes them into committing more rape crimes. In addition, this paper uses an instrumental variable approach to study if such association is causal. It finds evidence that one extra fine for sex purchase increases rape by roughly 1%.

This paper contributes to a growing line of research in economics that studies prostitution either theoretically (Edlund and Korn, 2002; Cameron, 2002; Cameron and Collins, 2003; Della Giusta, Di Tommaso, and Strøm, 2009) or empirically (Cameron, Collins, and Thew, 1999; Moffatt and Peters, 2004; Gertler, Shah, and Bertozzi, 2005; Gertler and Shah, 2011; Arunachalam and Shah, 2008; Della Giusta et al., 2009; Edlund, Engelberg, and Parsons, 2009; Della Giusta, 2010; Cunningham and Kendall, 2011a; Cunningham and Kendall, 2011b; Cunningham and Kendall, 2011c). In particular, it contributes to a strand of the literature addressing the effects of different prostitution law (see, inter alias,Lee and Persson, 2015; Cho, Dreher, and Neumayer, 2013; Jakobsson and Kotsadam, 2013).

The rest of the paper is organized as follows. Section 3.2 describes the data sets used in this paper. Section 3.3 presents OLS estimates and test the robustness of such results. In Section 3.4, I explore the potential mechanisms leading to the main findings of the paper. Section 3.5 addresses the issue of causality of my estimates. Finally, Section 3.6 concludes.

3.2 Data

In January 1999 Sweden ban banned the purchase of prostitution, becoming in this way the first country to introduce this type of regulation. More specifically, since January 1999 prostitutes' customers in Sweden faced the risk of receiving a fine. In this paper, I use data on the number of fines for sex purchase and rape in short time windows (months).

The data used in this paper comes from "The Swedish National Council for Crime Prevention" (also known as and hereafter, Bra). Bra is the most important institution for crime data collection in Sweden. Among other types of crime data, it collects data of crimes reported to police officers. Hence, it provides detailed information on the number of rapes and on the number of fines for sex purchase since the enforcement of the ban in 1999.[7]

For each of the 21 regions of Sweden, I have collected data about reported rapes and issued fines for sex purchase at monthly level between 1997 and 2014 . Figure 3.1 shows the number of rapes and fines for sex purchase during the sample period considered in this paper. Two features are worth highlighting. First, there is considerable regional variation in fines for sex purchase. Second, both variables exhibit an upward trend during the sample period.

[5] A growing line of research in economics discusses the application of recent developments in behavioural economics to crime economics (Winden and Ash, 2012).

[6] Possibly this mechanism is also due to the recent expansion of networks of prostitutes' customers. A number of studies discuss the existence and expansion of such networks (e.g., see Pearl (1986), Hernandez (2003), Monto (2004), Farley, Bindel, and Golding (2009), and Sanders (2013).)

[7] Data on other sorts of crimes are drawn from this source as well.

Table 3.1 shows summary statistics for rapes and fines for sex purchase. Rapes are classified according to the age of the victim and the place where the crime occurred. Data show that the main victims are presumably women aged 15-18 years old and middle aged. Furthermore, most of rape in this data-set takes place indoors. Another noticeable feature is that for all variables the mean is greater than the median, as illustrated by the right-skewed distribution of rape displayed in Figure 3.2.

In addition, this paper also makes use of data on the number of police officers hired by each region from 1997 to 2014 to account for the degree of enforcement of the law. This data is drawn from "The Swedish Police". Since police recruitment take place each year this variable does not exhibit monthly variation within a given year.

Finally, in Section 3.4 and Section 3.5 I use data drawn from "The Swedish Meteorological and Hydrological Institute", Statistics Sweden, Google and "The Swedish Transport Agency". From "The Swedish Meteorological and Hydrological Institute" I collect data on weather conditions, whereas data on population (including civil status), immigration, economic conditions, consumption and number of employees in pubs and hotels are drawn from Statistics Sweden.[8] From Google Trends I drew data on the number of searches of different words related to prostitution across regions in Sweden while from Google Maps I collect data on the distances from the capital city of each region to the closest airport. Lastly, data on the number of passengers travelling to/from airports in Sweden each month are drawn from "The Swedish Transport Agency".[9]

3.3 Results

In order to explore the association between fines for sex purchase and rape I consider the following regression model:

$$log(1 + rape_{rmy}) = \beta fines_{rmy} + \alpha_r + \alpha_m + \alpha_y + \alpha_r * y + \gamma officers_{ry} + \varepsilon_{rmy} \quad (3.1)$$

where r stands for region, m for month and y for year. The dependent variable is $log(1+rape_{rmy})$ since rape takes value 0 for some months in some regions, $fines_{rmy}$ is the number of fines for sex purchase issued by police officers in region r in month m and year y; α_r, α_m, α_y are respectively fixed effects for region, month and year; $\alpha_r * y$ is a region-year trend and the control variable $officers_{ry}$ is the number of police officers in region r in year y since police officers are hired by regions every year.[10] Variation comes from the different number of issued fines for sex purchase within and between regions across time.

Following the stream of the literature reporting some degree of substitutability between prostitution and rape, I expect to find that criminalizing the purchase of prostitution would boost rape. In effect, as fines for sex purchase make the purchase of prostitution more expensive, rape offences increase.[11] Even if the penalty associated to each crime considerably differs (being much higher for rape), criminalizing

[8]Data on consumption and number of employees in pubs and hotels only span from 2007 to 2013.

[9]In this database data in 2005 for a few airports are missing.

[10]I control for the number of officers hired in each region following a strand of the literature that found that increasing officers decreases crime rate (see, inter alias, Di Tella and Schargrodsky, 2004; Draca, Machin, and Witt, 2011).

[11]Note that it might also be that prostitutes' customers rape prostitutes (i.e. do not pay for sex purchase) now that prostitution is more expensive.

sex purchase increases only the penalty associated to prostitution and therefore this could push some prostitutes' customers to commit rape.

There are two potential caveats that prevent these estimates from being causal: endogeneity and reverse causality. As for endogeneity, there might be an omitted variables bias stemming from a third variable simultaneously affecting both fines and rape. Notwithstanding this, it is worth noting that the regression model above is highly demanding since it includes fixed effects at region, month and year level, plus region-year trends to capture any variation at seasonal or geographical levels. Yet, this issue is further addressed in Sections 3.3.1 and 3.3.2.

Reverse causality arises from the concern that rape could affect prostitution. A strand of the literature has found that around 60% to 70% of prostitutes have been victims of rape (Farley and Barkan, 1998; Farley et al., 2004) and that rape of prostitutes rarely ends in conviction of aggressors (Anderson, 2004; Sullivan, 2007). In particular, prostitutes could prefer to avoid regions which experience large numbers of rape. This would cause my OLS estimates to be downward biased.[12] This issue is addressed in Section 3.3.3.

Moreover, the dependent variable could be measured with error since rapes (unlike fines for sex purchase) are under-reported. If such measurement error is random, this would cause the OLS estimates to be less precise.

Table 3.2 shows the results of running regression model (1) where the variance of the error term is clustered at region level. In column (1) the only controls included are region, year and month fixed effects. The estimated coefficient on the number of fines for sex purchase is positive and statistically significant at 10% level. Columns (2), (3) and (4) include, respectively, either the number of officers, region-year trends or both. In the three cases the estimated coefficient becomes larger than in column (1) and happens to be statistically significant at 5% level. According to this analysis, the correlation between fines for sex purchase and rape is roughly .14%.[13]

3.3.1 Sensitivity to model specification changes and to functional forms of dependent variable

First, I address concerns about the functional form chosen for the previous regression model. There could be concerns that my findings are driven either by extreme values of rape or by the chosen functional form in logs. Table 3.3 reports results for different specifications. Column (1) presents results for a Linear Probability Model (hereafter, LPM) where the dependent variable is a dummy variable taking value 1 if any positive number of rapes occurred and 0 otherwise. Column (2) uses the Inverse Hyperbolic Sine (hereafter, IHS) transformation of the dependent variable.[14] Second, to address potential confounding factors varying at year-month level, Column (3) adds year-month fixed effects as a control, so that this specification becomes the most demanding among the ones considered so far.

Columns (1) and (2) of Table 3.3 exhibit positive significant coefficients for both regressions. Thus, this finding shows that results are not driven by either extreme

[12]Since my OLS estimates are positive, reverse causality implies that the population regression coefficient is larger than my estimates.

[13]Given the functional form of my dependent variable this estimate is the semi-elasticity of the number of rapes with respect to the number of fines for sex purchase.

[14]The IHS transformation is defined as $log\left(y + (y^2+1)^{1/2}\right)$. It is a popular alternative functional form to $log(1+y)$ when the dependent variable might take a zero value.

values or by the chosen functional form. Column (3) finds that inclusion of year-month fixed effects does not *worsen* the statistical significance of my estimates. Indeed, the estimated coefficient is positive and significant at 1%. This result suggests that confounding factors varying seasonally do not lead to the positive relationship found between fines and rape.

Another potential concern is that results are driven by the low number of clusters. The main specification clusters variance at treatment regional level. Yet, since Sweden has 21 regions, there is a problem of few clusters. It is well known that whenever the number of clusters is low (approximately below 40), standard errors might lead to over-rejection (i.e. confidence intervals might be too narrow). The literature suggests several solutions to overcome this issue.[15] In this paper, Table 3.4 presents results using wild-cluster bootstrap.[16] Columns (1) and (2) present, respectively, the results using the dependent variable in logs and IHS. Since the coefficients are positive and statistically significant in both columns, I find no evidence against the possibility that my findings are driven by narrow standard errors. In particular, in this case standard errors are smaller using wild-cluster bootstrap. In this respect, an additional check is to test the significance of the estimated coefficient inf the main specification using a t-distribution with *number of groups - regressors* degrees of freedom. As can be inspected, estimates remain statistically significant at standard levels in this case as well.

3.3.2 Confounding factors

Another potential concern regarding the positive correlation between fines for sex purchases and rape is that it could be driven by a confounding factor that increases contemporaneously the purchase of prostitution and rape. If this is the case my OLS estimates would be upward biased. This section addresses this issue regressing the number of fines on a list of plausible confounding factors.

This approach is more robust than simply including the variable in the right hand-side and check that the estimated coefficient does not change (Pei, Pischke, and Schwandt, 2018).[17] The data-sets used in this section are drawn from Statistics Sweden and from the Swedish Meteorological and Hydrological Institute.

Specifically, I check whether it could be that either population, economic conditions, weather, immigration or leisure and degree of alcoholism in the region are behind the positive correlation found between fines for sex purchases and rape. Indeed, it sounds plausible that any of these variables could affect at the same time both fines for sex purchase and rapes. For example, it could be that when leisure and alcohol spending increase, both sex purchases and rape boost as well. However, this set of variables is not available at monthly frequency. In effect, weather indicators (temperature and precipitation) vary at quarterly level, while population growth, economic conditions, immigration and leisure and alcohol sectors variables are only available at yearly frequency. Namely, I run the following regression model:

$$y_{rt} = \beta fines_{rt} + \alpha_r + \alpha_t + \alpha_r * y + \gamma officers_{rt} + \varepsilon_{rt} \tag{3.2}$$

where y_{rt} is a dependent variable at the available frequency t of the data proxying a confounding factor and the variables on the right hand side follow the labeling used in regression model (1).

[15] For further details see Cameron and Miller (2015).

[16] For further details on wild-cluster bootstrap see Cameron, Gelbach, and Miller (2008)

[17] This approach is also preferable because several of these variables could be poorly measured.

Table 3.5 shows results for population. In columns (1) and (3) the dependent variables are respectively male population and female population, while in column (2) and (4) the dependent variable is the growth rate of these variables. The four estimated coefficients are either negative or small in absolute value, and not statistically different from zero. These results point that there is no evidence that a variation in population causes an increase in fines for sex purchase not captured by the main specification. Table 3.6 performs the same test and presents results in the same fashion as in Table 3.5 but separating population in four categories according to civil status: married men, married women, single men and single women. As can be inspected, estimated coefficients are also insignificant and negative.

Feminist literature (Schwendinger and Schwendinger, 1983; Bailey, 1999) linked economic conditions to rape and prostitution. According to this hypothesis, low employment rates could increase both prostitution demand and rape. To address this hypothesis, Table 3.7 presents the results when I consider overall and male and female employment as proxies of regional economic conditions. Columns (1), (3) and (5) use respectively male employment, female employment and total employment as dependent variable.[18] Columns (2), (4) and (6) use as dependent variable the growth rate respectively of male, female and total employment. In this case, coefficients flip signs across specifications. In addition, the estimated coefficients of regressions that use growth rates as dependent variables become close to zero. More importantly, all six coefficients are insignificant at standard levels. These findings do not support that the positive relationship between fines for sex purchases and rape is due to economic conditions.

Next, I use two variables that measure weather conditions though it seems unlikely that weather conditions could affect my estimates having included monthly fixed effects. Namely, I use average temperature and precipitation as dependent variables of my main specification to check that this is not the case. Columns (1) and (2) of Table 3.8, respectively, report the results for such regressions. Coefficients are positive, but statistically insignificant. Thus, as expected, weather does not seem to affect fines for sex purchase in the main regression model.

Recent mass sexual assaults were linked to immigration.[19] A potential concern is that the positive correlation between fines for sex purchase and rape comes from regions that experienced higher rates of immigration. To address this issue, columns (1) and (3) in Table 3.9 use male and female immigration as dependent variables, while columns (2) and (4) respectively use male and female immigration growth as dependent variables. Male immigration is negatively correlated with fines for sex purchase, while when considering its growth the estimated coefficient flips signs. Regarding female immigration, the estimated coefficient is negative in both specifications. Yet, in all these four regressions the estimated coefficients are insignificant. As a result, this empirical evidence does not support immigration as a confounding factor increasing both fines for sex purchase and rape.

A last concern could be that booms in leisure and alcohol consumption might be leading my findings. To tackle this issue, I make use of four different variables to measure both demand and supply. Namely, I use average spending in pubs and hotels to proxy demand for leisure and alcohol; and employees working in pubs and hotels to proxy supply.[20] Table 3.10 displays the results of using these variables. Columns (1) and (3) respectively use consumption in pub and hotels, while, columns

[18] These variables measure the flow of gainful employment. Any person who has worked for at least one hour per week (in a certain month) is defined as gainfully employed.

[19] see, e.g., Wikipedia contributors, 2018a; Wikipedia contributors, 2018b

[20] Due to data availability, this database spans from 2007 to 2013.

(2) and (4) use employees working in pub and hotels. Columns (1) and (3) reveal that fines for sex purchase are positively correlated with both variables proxying demand. Yet, coefficients are not statistically different from zero. As regards supply, estimated coefficients are negative and not statistically different from zero.

In sum, in view of the previous results I conclude that there is no strong evidence that my findings are affected by a confounding factor increasing both fines for sex purchase and rapes.

3.3.3 Falsification tests: leads and lags

This section tests the robustness of my results to reverse causality. It does so by regressing current rape on future and past values of fines for sex purchase to test whether leads or lags, besides current fines for sex purchases, also determine rapes.

Regressing current rape rates on future fines for sex purchase helps checking whether future fines are negatively or positively correlated with rape. If this correlation is negative it would support reverse causality. Table 3.11 reports the results of regressing current rape on future and past fines for sex purchase where leads and lags respectively span five months pre and post the present value. Columns (1) and (2) of Table 3.11 present the results using the dependent variable in defined in logs and IHS. Two particular findings stand out. First, $t + 3$, $t + 4$ and $t + 5$ fines do not seem to be correlated with current rape , while $t + 1$ and, in particular, $t + 2$ seem to be negatively correlated with rape. This evidence supports reverse causality. Second, the positive relationship between fines for sex purchase and rape is only due to current fines: lags of the number of fines seem to have no impact on current rape.

All in all, these findings support that fines for sex purchase and rape are positively related in the short run. Furthermore, in line with the discussion in Section 3.3, they point out that reverse causality might be an issue. Namely, rape seem to decrease (future) fines for sex purchase. This suggests that the OLS estimates might be downward biased.

3.3.4 Placebo test: Randomization inference

This section shows the results of randomizing fines for sex purchase across different time periods. This exercise is useful as a further robustness check to test whether current fines for sex purchase are correlated with rape.

Figure A.10 presents the results of randomizing the number of fines for sex purchase stratified at time period level with 1,000 permutations. The red vertical line depicts the estimated coefficient in my main specification. The intersection between the red vertical line and the estimated distribution could be interpreted as the probability of finding by chance an estimated coefficient as large as the estimated coefficient of the main regression. Figure A.10 shows that this probability is extremely low: only 87 regressions out of 1,000 could replicate such estimate.

This evidence points that current fines for sex purchase are indeed positively correlated with rape. Hence, this result offers further support that fines for sex purchase affect rape in the short run.

3.3.5 Placebo test: Crimes not related to rape or prostitution

Since data on rape and fines for sex purchase drawn from Bra exhibit time trends another potential concern is that their positive relationship could be due to spurious correlation. This section addresses this issue by regressing three types of crimes different from rapes which also exhibit trends on my main regressor, namely number of fines. In particular, I use threat crimes, damaged vehicles and violent crimes. Table 3.12 displays the results for each of these three variables. If spurious regression outcome were behind my results, one would expect significant estimated coefficients in these regression

However, in all these three regression models the estimated coefficient associated to the number of fines is always close to zero and statistically insignificant at standard levels, dismissing in this way spurious regressions as a source of concern.

3.4 Mechanism

This section explores if there is empirical evidence than the positive association between fines for sex purchase and rape is led by a specific mechanism.

3.4.1 Supply of prostitution

Shifts of the supply of prostitution could affect both fines for sex purchase and rape. For instance, given substitutability between prostitution and crime, a general decrease in the supply of prostitution could affect fines for sex purchase and boost rape.[21]

To address this concern, I gather data about three variables to proxy the supply of prostitution. The first one is the number of *pimps*.[22] It seems plausible that a fall in the number of *pimps* would result in a decrease in the number of prostitutes as well. In Sweden even if selling sex is not penalized, promoting prostitution (i.e. making money out of prostitutes, such as *pimping*) is a crime. For this reason, Bra also collects data on the number of convicted pimps.

The second and third variables are, respectively, the number of humans trafficked for sexual purposes and the number of humans trafficked for non-sexual purposes. Human trafficking is narrowly linked to prostitution. Indeed, one of the main aims of the criminalization of the purchase of prostitution is to decrease human trafficking.[23] Table 3.13 presents the results using the main regression model above but replacing the dependent variable with: *pimps* (column (1)), human trafficked for sexual purposes (column (2)), human trafficked for non-sexual purposes (column (3)) and human trafficked (the sum of the last two variables, column (4)). In the case of human trafficking, data is available only for a subset of our sample, starting either in 2003 (human trafficking for sexual purposes) or in 2005 (human trafficking for non-sexual purposes).

Table 3.13 shows that the estimated coefficients in these regressions are not statistically different from zero. Moreover, these coefficients are either positive or considerably close to zero. Overall, these results do not support that my findings are driven by a shift of the supply of prostitution.

[21] Given that prostitution and rape seem to be substitutes.

[22] Pimp (or procurer) means a person, especially a man, who controls prostitutes and arrange customers for them, usually in return for a share of the earnings.

[23] Several studies documented the close connection between prostitution and human trafficking, see among others Kulick (2005), Holmström, Siring, and Kuosmanen (2008), Statens Offentliga Utredningar (2010), Nordic Council of Ministers (2008), Riskpolisstyrelsen (2011), Waltman (2011), and Ekberg ().

3.4.2 Demand of prostitution

It could also be the case that shifts in the demand of prostitution affect both fines for sex purchase and rape. There is scant information worldwide on demand of prostitution, and even less in Sweden, where purchase of sex an illegal activity. However, a strand of the literature (Cunningham and Kendall, 2010; Cunningham and Kendall, 2011c; Cunningham and Kendall, 2013) reports that the prostitution market is expanding through internet and that online solicitation represents an important expansion of this market. Therefore, to proxy demand for prostitution I make use data draw from Google Trends data on the number of times words such as prostitution, brothel, porn, strip-club (and, their Swedish translations, bordell, porr and strippklubb) were *googled* on the web.[24]

Tables 3.14 and 3.15 show, respectively, the estimated coefficient of using as dependent variables each of the above-mentioned googled words . If the positive relationship between fines for sex purchase and rape were to be due to a change in the demand of prostitution, I would expect to find at least one significant estimated coefficient in these regressions. Tables 3.14 and 3.15 show that the estimated coefficients flip sign across regression models and are statistically insignificant. Hence, I conclude there is no empirical evidence supporting that the mechanism leading to my findings is connected to changes in the demand of prostitution.

3.4.3 Behavioural mechanism

As pointed out above, Sweden has been the first country to ban the purchase of sex following the approval of such regulation in 1999. There is evidence that in the next few years after the ban, there were doubts about how strictly it would be enforced (Bucken-Knapp, 2010; Skarhed, 2010; Kuosmanen, 2011; Skilbrei and Holmström, 2016). As a result, it was not clear initially for prostitutes' customers how the new regulation would affect the probability of being apprehended for sex purchase. To explain the positive correlation between fines for sex purchase and rape, this paper suggests that prostitutes' customers might use the current number of fines for sex purchase to gauge the probability of being apprehended for such an activity.[25] Under the maintained assumption of substitutability between both crime activities, everything else equal, a higher probability of being arrested when consuming prostitution would reduce this demand while increasing the substitutable crime of committing rapes.

In view of these considerations, this section provides a test of the plausibility of that mechanism. Specifically, I check whether the positive relationship between fines for sex purchase and rape changes over time. If current fines are initially used to proxy the probability of being apprehended for sex purchase, such positive correlation should be larger in those periods close to the introduction of the ban where the learning process would be still at work. By contrast, it should decrease later on, once prostitutes' customers got more acquainted with the law, therefore relying less on this probability updating strategy.

To explore this channel I interact fines with years fixed effects. Figure 3.4 shows the estimated coefficients and respective 90 % confidence intervals of running this

[24]Prostitution is the same word in English and Swedish. Note that Google Trends data is available since 2004 and is not available for regions where the number of searches of a given word is low and below a certain threshold.

[25]Prostitutes' customers could be aware of issued fines for sex purchase due to network effects for which there is evidence in the literature (e.g., see Pearl (1986), Hernandez (2003), Monto (2004), Farley, Bindel, and Golding (2009), and Sanders (2013).)

regression with the dependent variable in logs. There are two main features to be highlighted. First, the largest effect is reached the year after the ban was introduced (i.e. 2000) and it is statistically significant at 1%. Second, coefficients decrease over time. This pattern is clear by observing the standard errors of my estimates, as time passes they get narrower. This finding also supports that this effect is temporary. Table 3.16 shows the results of running such a regression both in logs and using IHS transformation. Results are robust to both functional forms.

Figure 3.5 shows the yearly average of fines for each year in the sample period after the ban is introduced. This figure shows that the yearly average varies substantially. This evidence dismisses the concern that results of Table 3.16 might be driven by any clear pattern in the yearly average of fines for sex purchase.

3.5 Causality: IV estimates

Even if OLS estimates show that there is a positive association between fines for sex purchases and rape, it is still unclear whether there is a causal relationship between these two variables. Indeed, mainly two issues threat causal inference in my previous setting: endogeneity and reverse causality.

Instrumental variable (hereafter, IV) estimation has the benefit to be a solution to both problems. This section makes use of IV estimation to explore if fines for sex purchase have a causal impact on rape.

Sex tourism is broadly defined as tourism for commercial sex purposes. It has been documented that prostitutes' customers travel to countries where prostitution is tolerated to buy sex there (see, inter alias, Jeffreys, 1999; Oppermann, 1999; Farley, Bindel, and Golding, 2009).[26] This is likely to be a more widespread practice in countries where the purchase of sex is outlawed.[27] Consequently, being close to airports increases the likelihood prostitutes' customers in that region travel to buy sex elsewhere in order to avoid being penalized in their home country. Furthermore, passenger traffic seems to be the best variable to proxy sex tourism.

Therefore, I instrument fines for sex purchase with the number of monthly departing and arriving passengers to the main airport in the region divided by the distance to such airport.[28] In particular, I collected data of the distance from the capital city of the region to the main airport of that region.[29] Then, I collected data about the monthly number of passengers for such airports.

Passengers to airports are good instruments in this setting. First, they are plausibly randomly assigned with respect to crime patterns (i.e. passengers do not plan when to travel due to any reason connected to crime). Second, it does not seem plausible that they can affect rape in other ways than via prostitution. Sex tourism exploits differences in the regulation of prostitution across countries. So prostitutes' customers travel to other countries where prostitution is more tolerated, or even legal. Yet, to the best of my knowledge, this is not the case for rape since this crime is neither legal nor tolerated in any country. Hence, there is no reason to believe that departing or arriving passengers could directly affect rape. Third, these instruments affect the (potential) endogenous regressor: fines for sex purchase are less likely to

[26]Note that in a number of European countries prostitution is legal or regulated via licenses (e.g. Denmark, Germany, the Netherlands, etc.).

[27]In this respect, the Government of Sweden is even considering extending the ban to sex purchased abroad (Johansson and Koch, 2017).

[28]Namely, as instruments I use $\frac{Departing\ passengers_{rmy}}{Distance_r}$ and $\frac{Arriving\ passengers_{rmy}}{Distance_r}$.

[29]Throughout this analysis I define the main airport as the closest airport to the capital city of the region.

occur in months with sex tourism (i.e. months with many departing passengers and few arriving passengers). In other words, departing passengers in a given region should decrease fines for sex purchase, while arriving passengers should increase it.[30] Moreover, by dividing the number of passengers by the distance to the airport, passengers in regions close to the main airport carry more weight.

Table 3.17 displays the estimates of the first stage. Column (1) shows the results clustering at region level, while, column (2) uses wild-cluster bootstrap at region level. Results do not change with wild-cluster bootstrap. Such results show that both the instruments are statistically significant. In addition, as expected, departing passengers decrease fines for sex purchase, while arriving passengers increase it. These results are in line with the theoretical predictions.

Table 3.18 presents the findings of running the reduced form regression. Columns (1) and (2) respectively display the results using the dependent variable in logs or in IHS transformation clustering at region level. Columns (3) and (4) repeat the same analysis using wild-cluster bootstrap at region level. Coefficients are statistically significant at standard levels across all four regression models. In addition, the sign of the estimated coefficient for each instrument is the same as in the first stage.

Table 3.19 shows results of running the second stage regression. Columns (1) and (2) respectively report results using rape in logs and in IHS transformation. In both regression models the estimated coefficient is positive and statistically different from zero. The results are consistent across the two models, though the estimates in logs are slightly more conservative. The last row displays the F-statistic. The F-statistic is large supporting the instruments are strongly correlated with the endogenous independent variable.

Given the functional form of my dependent variable, the IV estimates suggest that an extra fine for sex purchase increases rape by roughly 0.89%-1.4%. Since during my sample period there have been on average 11.4 rapes per region and month, these estimates suggest that roughly 11 fines for sex purchase increase rape by 1. Moreover, during my sample period there have been in total 5933 fines for sex purchase. Hence, during this period of time the ban increased rape by about 50%.[31]

Even if at first sight these estimates could seem large this is not the case. In effect, these estimates are quite conservative when compared to the literature values. (Cunningham and Shah, 2017) found that decriminalization of indoor prostitution decreased rape by 30% over 5 years in Rhode Island. It seems plausible to think that in a similar time period criminalizing the purchase of prostitution might have the same impact, but with the opposite sign. This is exactly what I find but the effect happens to be smaller: criminalizing prostitution increased rape in Sweden by 50% over 15 years rather than by 30% in 5 years.

3.6 Conclusion

This paper analyzes the connection between the market of rape and prostitution law taking into account the substitutable relation between the two that emerged in the recent literature (Cunningham and Shah, 2017; Bisschop, Kastoryano, and Klaauw, 2017; Ciacci and Sviatschi, 2016). Specifically, this paper studies the effect of criminalizing the purchase of prostitution on rape.

[30] This is a monotonicity assumption. In view of this assumption, it is easy to embed my analysis in a Local Average Treatment Effect (LATE) framework.

[31] Note that during my sample period rape has more than doubled in Sweden.

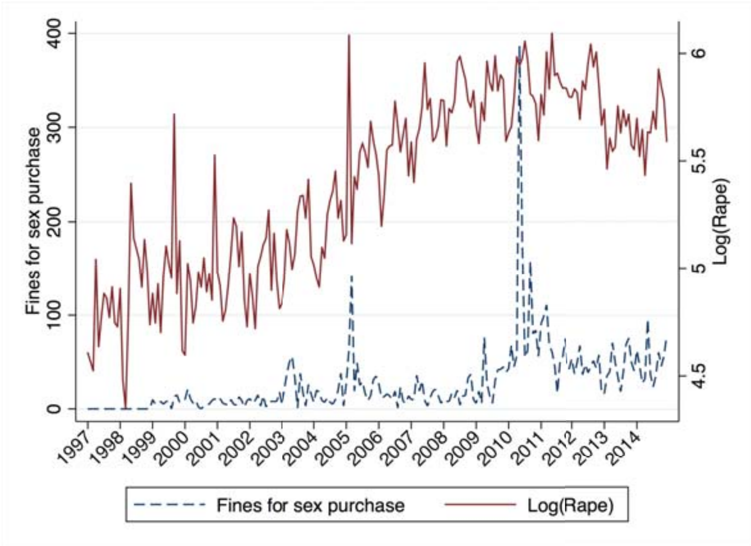
I find several pieces of evidence documenting that criminalizing the purchase of prostitution is positively related to rape. This result is robust to different specifications. Yet, I argue that it is likely to be downward biased due to reverse causality.

Furthermore, I explore whether this positive relationship could be due to a behavioural mechanism where prostitutes' customers interpret fines for sex purchase as the probability of being apprehended for this crime. Using an IV estimation procedure, this paper finds evidence that the relationship between these two variables is likely to be causal, such that criminalizing the purchase of prostitution increased rape by roughly 50% on impact.

These findings have several policy implications. First, criminalizing the purchase of prostitution increases rape. Second, this ban does not seem to affect other crimes. Third, most of the increase arises from the uncertainty of the enforcement of the ban (i.e. fines are initially used to gauge the probability of being apprehended for sex purchase). This result is line with the literature asserting that less experienced prostitutes' customers are more likely to be caught in police operations (Monto, 2004) and that the ban has not been enforced very strictly (Bucken-Knapp, 2010).

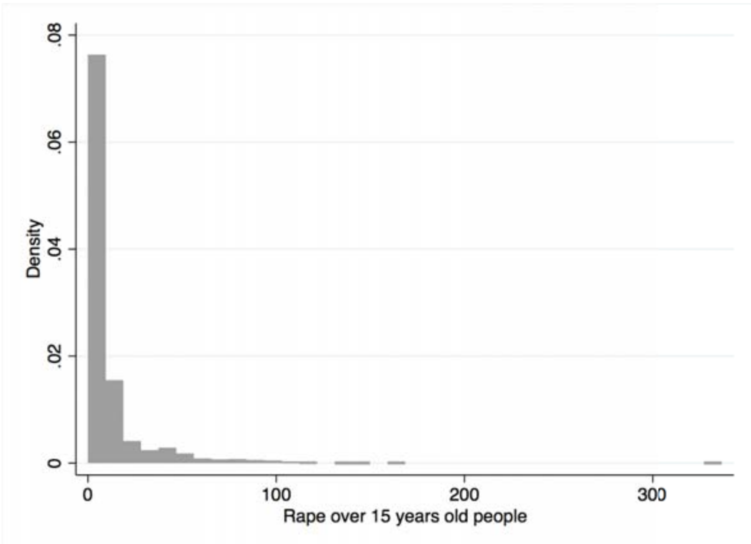
Figures & Tables

FIGURE 3.1: Evolution of fines for sex purchase and rape in Sweden



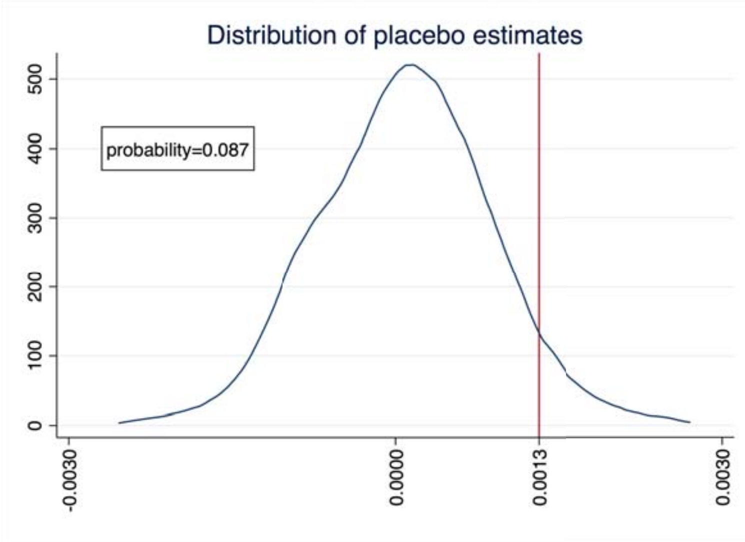
Notes: This figure shows the number of rapes (in logs) and fines for sex purchase in Sweden according to Bra during the period 1997-2014.

FIGURE 3.2: Distribution of rape



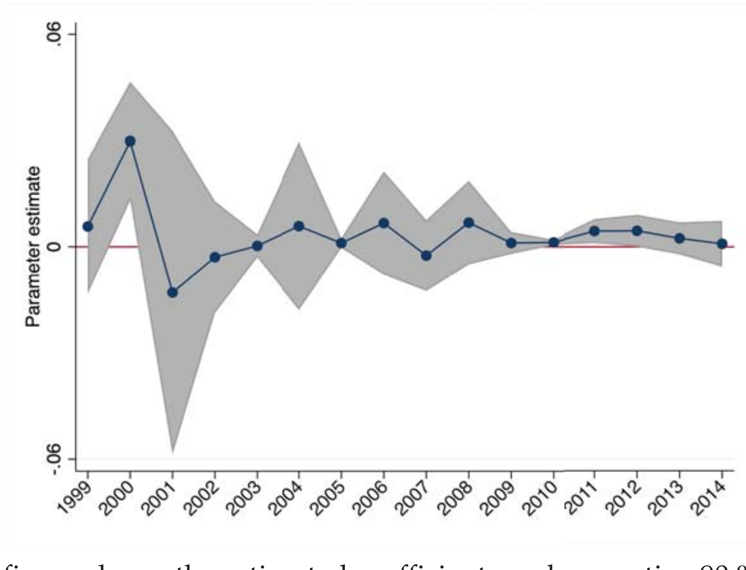
Notes: Histogram of rapes in Sweden according to Bra during the period 1997-2014.

FIGURE 3.3: Placebo test: randomization inference



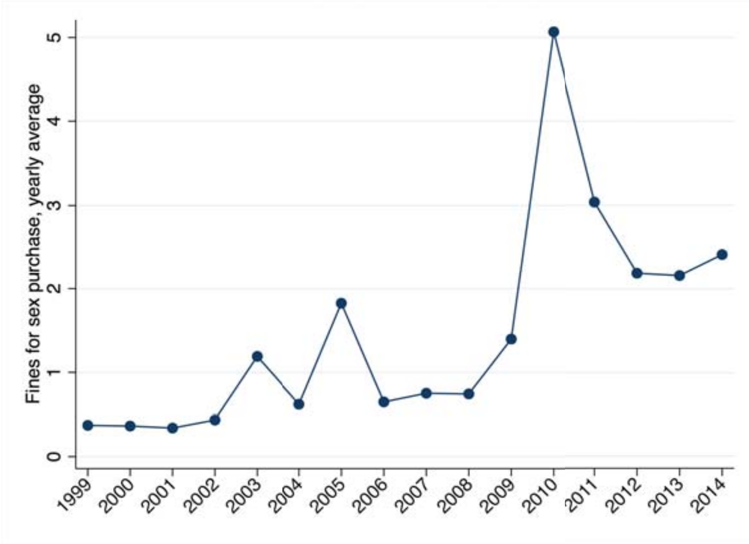
Notes: results of randomizing the number of fines for sex purchase stratified at time period level with 1,000 permutations. The red vertical line represents the estimated coefficient in my main specification. The intersection between the red vertical line and the estimated distribution could be interpreted as the probability of finding an estimated coefficient as large as my estimates by chance. Only 87 regressions, out of 1,000, could replicate the estimate.

FIGURE 3.4: Behavioral mechanism



Notes: This figure shows the estimated coefficients and respective 90 % confidence intervals of interacting fines for sex purchase with year fixed effects using the dependent variable in logs.

FIGURE 3.5: Yearly average of fines for sex purchase



Notes: This figure shows the yearly average of fines for sex purchase in Sweden during the period 1999-2014.

TABLE 3.1: Summary statistics

Rape, victim	mean	median	s.d.
b/15 and 17 y.o.	5.883598	3	11.97033
woman 18 y.o. or older	5.311067	0	13.10743
man 18 y.o. or older	.1840829	0	.6728784
Inside	8.812169	4	14.52782
Outside	2.566578	1	4.211443
Total	11.37875	6	18.06075
Fines for sex purchase	1.307981	0	7.35277
Observations 4,536			

TABLE 3.2: Regression results for Sweden

VARIABLES	(1) Log(1+Rape)	(2) Log(1+Rape)	(3) Log(1+Rape)	(4) Log(1+Rape)
Fines for sex purchase	0.00104*	0.00118**	0.00126**	0.00131**
	(0.000565)	(0.000432)	(0.000502)	(0.000470)
Observations	4,536	4,536	4,536	4,536
Clustered variance at Regional level	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
Regional Year Trends	NO	NO	YES	YES
# of Policemen	NO	YES	NO	YES
Mean of Rape	11.4	11.4	11.4	11.4
Std. Dev. of Rape	18.1	18.1	18.1	18.1

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.3: Robustness: specification

VARIABLES	(1) LPM	(2) IHS Rape	(3) Year-Month FE
Fines for sex purchase	0.000144**	0.00159***	0.00125***
	(5.63e-05)	(0.000543)	(0.000406)
Observations	4,536	4,536	4,536
Clustered variance at Regional level	YES	YES	YES
Region FE	YES	YES	YES
Year FE	YES	YES	NO
Month FE	YES	YES	NO
Regional Year Trends	YES	YES	YES
# of Policemen	YES	YES	YES
Year-Month FE	NO	NO	YES

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.4: Robustness: cluster correction

VARIABLES	(1) Log(1+Rape)	(2) IHS Rape
Fines for sex purchase	0.00131***	0.00159***
	(0)	(0)
Observations	4,536	4,536
Cluster	Wild	Wild

Clustered standard errors at region level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 3.5: Population

	(1)	(2)	(3)	(4)
VARIABLES	Male Pop	Growth rate of Male Population	Female Pop	Growth rate of Female Population
Fines for sex purchase	-10.03	0.000106	-8.364	0.000170
	(9.357)	(0.000266)	(8.496)	(0.000317)
Observations	378	357	378	357
Clustered variance at Regional level	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
Regional Year Trends	YES	YES	YES	YES
# of Policemen	YES	YES	YES	YES

Clustered standard errors at region level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 3.6: Civil status

VARIABLES	(1) Married men	(2) Growth rate of Married men	(3) Married women	(4) Growth rate of Married women	(5) Single men	(6) Growth rate of Single men	(7) Single women	(8) Growth rate of Single women
Fines for sex purchase	-4.364	-4.151	-3.407	-3.232	-5.376	-5.455	-4.255	-4.334
	(4.506)	(4.272)	(3.681)	(3.479)	(4.591)	(5.014)	(4.026)	(4.402)
Observations	378	357	378	357	378	357	378	357
Clustered variance at Regional level	YES	YES	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES
Regional Year Trends	YES	YES	YES	YES	YES	YES	YES	YES
# of Policemen	YES	YES	YES	YES	YES	YES	YES	YES

Clustered standard errors at region level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 3.7: Economic conditions

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Male Employment	Growth rate of Male Employment	Female Employment	Growth rate of Female Employment	Total Employment	Growth rate of Total Employment
Fines for sex purchase	-8.346	1.59e-05	-6.478	4.95e-05	-14.82	3.25e-05
	(6.809)	(0.000362)	(5.969)	(0.000379)	(12.75)	(0.000363)
Observations	378	357	378	357	378	357
Clustered variance at Regional level	YES	YES	YES	YES	YES	YES
Region FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES
Regional Year Trends	YES	YES	YES	YES	YES	YES
# of Policemen	YES	YES	YES	YES	YES	YES

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.8: Weather

VARIABLES	(1) Temperature	(2) Precipitation
Fines for sex purchase	0.00128	0.0315
	(0.00125)	(0.0260)
Observations	1,512	1,512
Clustered variance at Regional level	YES	YES
Region FE	YES	YES
Year FE	YES	YES
Month FE	YES	YES
Regional Year Trends	YES	YES
# of Policemen	YES	YES

Clustered standard errors at region level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 3.9: Immigration by sex

VARIABLES	(1) Male Immigration	(2) Growth rate of Male Immigration	(3) Female Immigration	(4) Growth rate of Female Immigration
Fines for sex purchase	-1.562	0.123	-1.232	-0.0109
	(1.297)	(0.622)	(1.030)	(0.619)
Observations	378	357	378	357
Clustered variance at Regional level	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
Regional Year Trends	YES	YES	YES	YES
# of Policemen	YES	YES	YES	YES

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.10: Hotels and pubs

VARIABLES	(1) Consumption pub	(2) Employees pub	(3) Consumption hotels	(4) Employees hotel
Fines for sex purchase	0.657	-0.139	0.141	-0.00205
	(0.930)	(0.198)	(0.113)	(0.0714)
Observations	145	145	147	147
Clustered variance at Regional level	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
Regional Year Trends	YES	YES	YES	YES
# of Policemen	YES	YES	YES	YES

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.11: Leads and lags

VARIABLES	(1) Log(1+Rape)	(2) IHS Rape
Fines for sex purchase t+5	1.44e-05	1.65e-05
	(0.000506)	(0.000533)
Fines for sex purchase t+4	0.000366	0.000579
	(0.000547)	(0.000622)
Fines for sex purchase t+3	0.00128	0.00143
	(0.000752)	(0.000938)
Fines for sex purchase t+2	-0.00125***	-0.00137***
	(0.000350)	(0.000411)
Fines for sex purchase t+1	-0.000855	-0.00102*
	(0.000507)	(0.000588)
Fines for sex purchase	0.00209***	0.00248***
	(0.000702)	(0.000803)
Fines for sex purchase t-1	-0.00143	-0.00163
	(0.000880)	(0.00100)
Fines for sex purchase t-2	-0.00112	-0.00123
	(0.000701)	(0.000776)
Fines for sex purchase t-3	-0.000203	-0.000182
	(0.000467)	(0.000517)
Fines for sex purchase t-4	0.000558	0.000727
	(0.000698)	(0.000803)
Fines for sex purchase t-5	0.000669	0.000758
	(0.000846)	(0.000906)
Observations	4,326	4,326
Clustered variance at Regional level	YES	YES
Region FE	YES	YES
Year FE	YES	YES
Month FE	YES	YES
Regional Year Trends	YES	YES
# of Policemen	YES	YES

Clustered standard errors at region level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 3.12: Placebo test: crimes not related to rape or prostitution

VARIABLES	(1) Log(Threat)	(2) Log(Damage veh.)	(3) Log(Violent Crimes)
Fines for sex purchase	-0.000129	0.000863	-4.40e-05
	(0.000171)	(0.000635)	(0.000187)
Observations	4,536	4,536	4,536
Clustered variance at Regional level	YES	YES	YES
Region FE	YES	YES	YES
Year FE	YES	YES	YES
Month FE	YES	YES	YES
Regional Year Trends	YES	YES	YES
# of Policemen	YES	YES	YES

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.13: Supply of prostitution

VARIABLES	(1) Log(1+Pimps)	(2) Log(1+Sex traff)	(3) Log(1+Non-sex traff)	(4) Log(1+Total traff)
Fines for sex purchase	0.00337	0.000850	-6.72e-06	0.000991
	(0.00265)	(0.000863)	(0.000248)	(0.00112)
Observations	4,536	3,024	2,016	2,016
Clustered variance at Regional level	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
Regional Year Trends	YES	YES	YES	YES
# of Policemen	YES	YES	YES	YES

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.14: Demand of prostitution

VARIABLES	(1) Log(1+Prostitution)	(2) Log(1+Strip Club)	(3) Log(1+Strippklubb)
Fines for sex purchase	0.00308	-0.00113	0.00358
	(0.00218)	(0.00124)	(0.00376)
Observations	2,376	1,980	1,980
Clustered variance at Regional level	YES	YES	YES
Region FE	YES	YES	YES
Year FE	YES	YES	YES
Month FE	YES	YES	YES
Regional Year Trends	YES	YES	YES
# of Policemen	YES	YES	YES

Clustered standard errors clustered at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.15: Demand of sex using Google trends

VARIABLES	(1) Log(1+Porn)	(2) Log(1+Porr)	(3) Log(1+Brothel)	(4) Log(1+Bordell)
Fines for sex purchase	-6.06e-05	5.24e-06	0.00116	-0.00701
	(0.000514)	(0.000477)	(0.00260)	(0.00831)
Observations	2,772	2,772	2,376	924
Clustered variance at Regional level	YES	YES	YES	YES
Region FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
Regional Year Trends	YES	YES	YES	YES
# of Policemen	YES	YES	YES	YES

Clustered standard errors at region level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 3.16: Behavioural mechanism

VARIABLES	(1) Log(1+Rape)	(2) IHS Rape
Fines in 1999	0.00576	0.00827
	(0.0110)	(0.0130)
Fines in 2000	0.0298***	0.0353***
	(0.00967)	(0.0111)
Fines in 2001	-0.0129	-0.0110
	(0.0264)	(0.0314)
Fines in 2002	-0.00294	0.000423
	(0.00919)	(0.0110)
Fines in 2003	0.000269	0.00103
	(0.00188)	(0.00222)
Fines in 2004	0.00586	0.00801
	(0.0138)	(0.0156)
Fines in 2005	0.00106	0.00106
	(0.000800)	(0.000880)
Fines in 2006	0.00673	0.00553
	(0.00847)	(0.00903)
Fines in 2007	-0.00248	-0.00565
	(0.00581)	(0.00662)
Fines in 2008	0.00685	0.00477
	(0.00686)	(0.00758)
Fines in 2009	0.00109	0.000910
	(0.00180)	(0.00191)
Fines in 2010	0.00125**	0.00149**
	(0.000443)	(0.000530)
Fines in 2011	0.00445**	0.00479**
	(0.00197)	(0.00221)
Fines in 2012	0.00453*	0.00558*
	(0.00261)	(0.00286)
Fines in 2013	0.00240	0.00542*
	(0.00263)	(0.00294)
Fines in 2014	0.000863	0.00425
	(0.00376)	(0.00430)
Observations	4,032	4,032
Clustered variance at Regional level	YES	YES
Region FE	YES	YES
Year FE	YES	YES
Month FE	YES	YES
Regional Year Trends	YES	YES
# of Policemen	YES	YES

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.17: IV: First stage

VARIABLES	(1) Fines for sex purchase	(2) Fines for sex purchase
Departing passengers	-2.920***	-2.920***
	(0.223)	(0.944)
Arriving passengers	3.811*	3.811***
	(1.845)	(0.362)
Observations	4,428	4,428
Clustered variance at Regional level	YES	Wild
Region FE	YES	YES
Year FE	YES	YES
Month FE	YES	YES
Regional Year Trends	YES	YES
# of Policemen	YES	YES

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.18: IV: Reduced form

VARIABLES	(1) Log(1+Rape)	(2) IHS Rape	(3) Log(1+Rape)	(4) IHS Rape
Departing passengers	-0.0262*	-0.0424**	-0.0262*	-0.0424**
	(0.0148)	(0.0180)	(0.0155)	(0.0182)
Arriving passengers	0.252***	0.360***	0.252***	0.360***
	(0.0741)	(0.0886)	(0.0976)	(0.117)
Observations	4,428	4,428	4,428	4,428
Clustered variance at Regional level	YES	YES	Wild	Wild
Region FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
Regional Year Trends	YES	YES	YES	YES
# of Policemen	YES	YES	YES	YES

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

TABLE 3.19: IV: Second stage

VARIABLES	(1) Log(1+Rape)	(2) IHS Rape
Fines for sex purchase	0.00812*	0.0133**
	(0.00482)	(0.00586)
Observations	4,428	4,428
Clustered variance at Regional level	YES	YES
Region FE	YES	YES
Year FE	YES	YES
Month FE	YES	YES
Regional Year Trends	YES	YES
# of Policemen	YES	YES
Mean of Rape	11.4	11.4
Std. Dev. of Rape	18.06	18.06
F-stat	1351.25	1351.25

Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix A

Appendix: The Effect of Adult Entertainment Establishments on Sex Crime: Evidence from New York City

A.1 Classification of crimes in the "Stop-and-Frisk" data set

The "Stop-and-Frisk" data set classifies crime using the following 113 categories. We classified sex crimes using categories 7, 18, 77, 87 and 88. A possible concern could be whether sex crimes contain public lewdness crimes. Such crimes are connected to sex crimes but are considerably different from them. Yet, as this table shows such crimes are classified in category 76.

1	ABANDONMENT OF A CHILD
2	ABORTION
3	ABSCONDING
4	ADULTERY
5	AGGRAVATED ASSAULT
6	AGGRAVATED HARASSMENT
7	AGGRAVATED SEXUAL ABUSE
8	ARSON
9	ASSAULT
10	AUTO STRIPPING
11	BIGAMY
12	BRIBE RECEIVING
13	BRIBERY
14	BURGLARY
15	COERCION
16	COMPUTER TAMPERING
17	COMPUTER TRESPASS
18	COURSE OF SEXUAL CONDUCT
19	CPSP
20	CPW
21	CREATING A HAZARD
22	CRIMINAL CONTEMPT
23	CRIMINAL MISCHIEF
24	CRIMINAL POSSESSION OF CONTROLLED SUBSTANCE
25	CRIMINAL POSSESSION OF COMPUTER MATERIAL
26	CRIMINAL POSSESSION OF FORGED INSTRUMENT
27	CRIMINAL POSSESSION OF MARIJUANA
28	CRIMINAL SALE OF CONTROLLED SUBSTANCE
29	CRIMINAL SALE OF MARIJUANA
30	CRIMINAL TAMPERING
31	CRIMINAL TRESPASS
32	CUSTODIAL INTERFERENCE
33	EAVES DROPPING
34	ENDANGER THE WELFARE OF A CHILD
35	ESCAPE
36	FALSIFY BUSINESS RECORDS
37	FORGERY
38	FORGERY OF A VIN
39	FORTUNE TELLING
40	FRAUD
41	FRAUDULENT ACCOSTING
42	FRAUDULENT MAKE ELECTRONIC ACCESS DEVICE
43	FRAUDULENT OBTAINING A SIGNATURE
44	GAMBLING
45	GRAND LARCENY
46	GRAND LARCENY AUTO
47	HARASSMENT
48	HAZING
49	HINDERING PROSECUTION
50	INCEST
51	INSURANCE FRAUD
52	ISSUE A FALSE CERTIFICATE
53	ISSUE A FALSE FINANCIAL STATEMENT
54	ISSUING ABORTION ARTICLES
55	JOSTLING
56	KIDNAPPING

57	KILLING OR INJURING A POLICE ANIMAL
58	LOITERING
59	MAKING GRAFFITI
60	MENACING
61	MISAPPLICATION OF PROPERTY
62	MURDER
63	OBSCENITY
64	OBSTRUCTING FIREFIGHTING OPERATIONS
65	OBSTRUCTING GOVERNMENTAL ADMINISTRATION
66	OFFERING A FALSE INSTRUMENT
67	OFFICIAL MISCONDUCT
68	PETIT LARCENY
69	POSSESSION OF BURGLAR TOOLS
70	POSSESSION OF EAVES DROPPING DEVICES
71	POSSESSION OF GRAFFITI INSTRUMENTS
72	PROHIBITED USE OF WEAPON
73	PROMOTING SUICIDE
74	PROSTITUTION
75	PUBLIC DISPLAY OF OFFENSIVE SEXUAL MATERIAL
76	PUBLIC LEWDNESS
77	RAPE
78	RECKLESS ENDANGERMENT
79	RECKLESS ENDANGERMENT PROPERTY
80	REFUSING TO AID A PEACE OR POLICE OFFICER
81	RENT GOUGING
82	RESISTING ARREST
83	REWARD OFFICIAL MISCONDUCT
84	RIOT
85	ROBBERY
86	SELF ABORTION
87	SEXUAL ABUSE
88	SEXUAL MISCONDUCT
89	SEXUAL PERFORMANCE BY A CHILD
90	SODOMY
91	SUBSTITUTION OF CHILDREN
92	TAMPERING WITH A PUBLIC RECORD
93	TAMPERING WITH CONSUMER PRODUCT
94	TAMPERING WITH PRIVATE COMMUNICATIONS
95	TERRORISM
96	THEFT OF SERVICES
97	TRADEMARK COUNTERFEITING
98	UNLAWFULLY DEALING WITH FIREWORKS
99	UNAUTHORIZED RECORDING
100	UNAUTHORIZED USE OF A VEHICLE
101	UNAUTHORIZED USE OF COMPUTER
102	UNLAWFUL ASSEMBLY
103	UNLAWFUL DUPLICATION OF COMPUTER MATERIAL
104	UNLAWFUL POSSESSION OF RADIO DEVICES
105	UNLAWFUL USE OF CREDIT CARD, DEBIT CARD
106	UNLAWFUL USE OF SECRET SCIENTIFIC MATERIAL
107	UNLAWFUL WEARING A BODY VEST
108	UNLAWFUL IMPRISONMENT
109	UNLAWFULLY DEALING WITH A CHILD
110	UNLAWFULLY USE SLUGS
111	VEHICULAR ASSAULT
112	OTHER
113	FORCIBLE TOUCHING

A.2 Sex crimes by hour and day

TABLE A.1: Total number of sex crimes by day of the week and time of the day

	Sex Crimes (per day)				
		Morning	Afternoon	Evening	Night
	Entire day	6 A.M. to 12 P.M.	12 P.M. to 6 P.M.	6 P.M. to 12 A.M.	12 A.M. to 6 A.M.
	(1)	(2)	(3)	(4)	(5)
Sex crime data					
Weekend	2,535	444	539	781	771
-Friday	1,046	253	243	322	228
-Saturday	745	78	157	253	257
-Sunday	744	113	139	206	286
Weekdays	4,943	1,567	1,154	1,359	863
Total	7,478	2,011	1,693	2,140	1,634

Notes: This table presents the distribution of sex crimes over weekdays and time of the day. Time of the day is divided in 4 shifts of 6 hours each: morning (6 am to 12pm), afternoon (12pm to 6pm), evening (6pm to 12 am) and night (12am to 6pm).

TABLE A.2: Total number of openings by day of the week

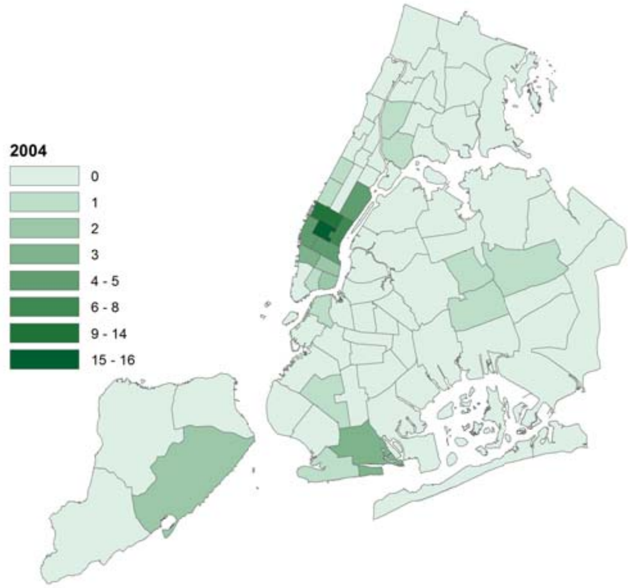
	Openings (per day)
Weekend (Friday-Sunday)	90
-Friday	30
-Saturday	20
-Sunday	40
Weekdays (Monday-Thursday)	116

Notes: This table presents the number of openings of adult entertainment establishments by day of the week.

A.3 Geographic evolution of adult entertainment establishments by precinct

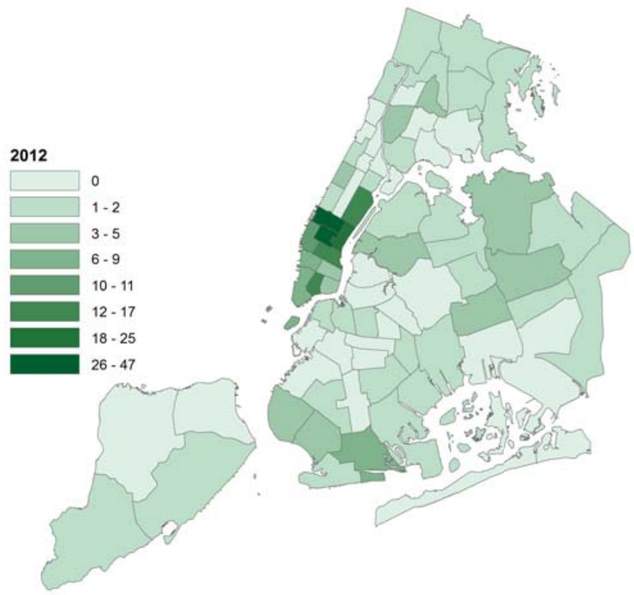
The two maps below show the evolution of adult entertainment establishments during our sample period. The maps show that there has been a substantial increase in the number of these businesses, not only by boroughs, but even between precincts within the same borough.

FIGURE A.1: Geographic distribution of adult entertainment establishments in NYC in 2004



Notes: This figure shows the geographic distribution of adult entertainment establishments in NYC on January 1, 2004, the first day of our sample period .

FIGURE A.2: Geographic distribution of adult entertainment establishments in NYC in 2012



Notes: This figure shows the adult entertainment establishments in NYC on June 30, 2012, the last day of our sample period.

A.4 Sensitivity to model specification changes and to definition of dependent variable

TABLE A.3: Additional specifications

	(1) Log sex crime	(2) Log sex crime	(3) Log sex crime	(4) Log Sex crime by men	(5) IHS of sex crime by men
Adult Entertainment Est.	-0.00414*	-0.00214**	-0.00442*	-0.00413*	-0.00825*
	(0.00220)	(0.000942)	(0.00245)	(0.00225)	(0.00451)
Observations	238,931	238,931	238,931	238,931	238,931
Clustered variance at Precinct level	Y	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y	Y
Exact Day FE	Y				
Precinct M Trends		Y			
Precinct Y M Trends			Y		

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.4: Robustness check

	(1) IHS	(2) Probit	(3) LPM	(4) Level
Adult Entertainment Est.	-0.00798*	-0.0165	-0.00455*	-0.00759*
	(0.00435)	(0.0106)	(0.00231)	(0.00434)
Observations	238,931	235,828	238,931	238,931
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes:For the probit model the estimated coefficient is showed. Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

A.5 Weekly regression

This section presents the results of the baseline regression but at a weekly frequency. Hence, we exchanged all the fixed effects varying daily for week fixed effects. The results are negative and statistically significant for both log, the IHS transformation and in levels.

TABLE A.5: Regression at weekly frequency

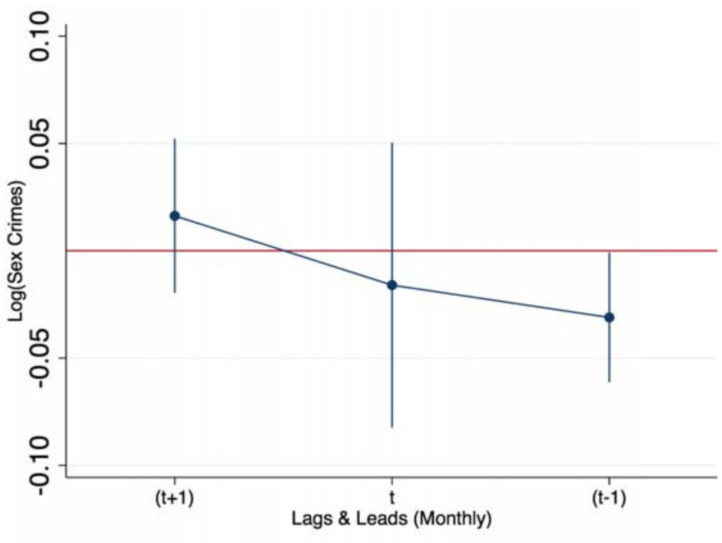
	(1) Log	(2) IHS	(3) Levels
Adult Entertainment Est.	-0.0172*	-0.0345*	-0.0529*
	(0.00884)	(0.0177)	(0.0302)
Observations	34,034	34,034	34,034
Clustered variance at Precinct level	Y	Y	Y
Precinct FE	Y	Y	Y
Year FE	Y	Y	Y
Month FE	Y	Y	Y
Week FE	Y	Y	Y
Precinct Trends	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

A.6 Falsification test

A.6.1 Log

FIGURE A.3: Falsification test



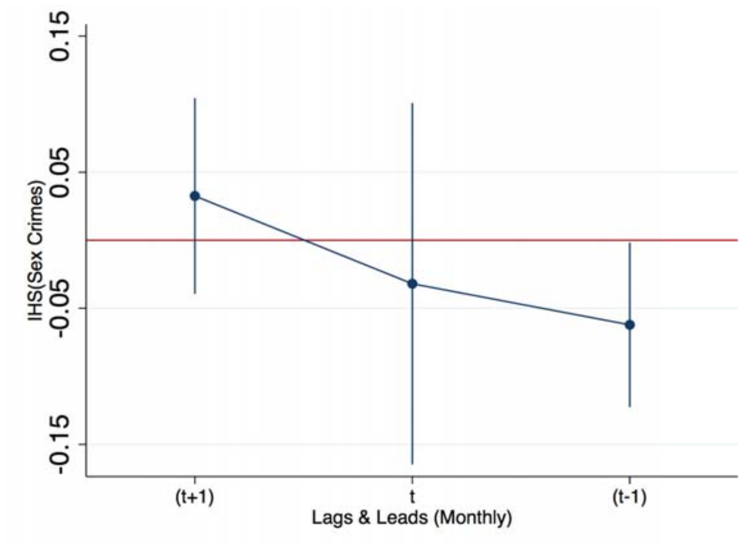
A.6.2 IHS

TABLE A.6: Falsification test using IHS

	(1) IHS	(2) IHS	(3) IHS	(4) IHS
Adult Entertainment Est. (t+1)		-0.0546		0.0325
		(0.0344)		(0.0432)
Adult Entertainment Est.	-0.0627*			-0.0320
	(0.0371)			(0.0798)
Adult Entertainment Est. (t-1)			-0.0640*	-0.0621*
			(0.0360)	(0.0363)
Observations	7,854	7,777	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

FIGURE A.4: Falsification test using IHS



A.7 Representability of "Stop-and-Frisk" data: further checks

TABLE A.7: Representability of "Stop-and-Frisk" data: complaint sex crimes at the daily level

	(1) Level Stop & Frisk	(2) Level Stop & Frisk
Level, Complaints	0.0383***	0.0391***
	(0.0114)	(0.0120)
Observations	238,931	238,931
Clustered variance at Precinct level	Y	Y
Precinct FE	Y	Y
Year FE	Y	Y
Month FE	Y	Y
Day of the week FE	Y	Y
Day of the year FE	Y	Y
Holiday FE	Y	Y
Precinct Trends		Y

Notes: Clustered standard errors at the precinct level in parentheses. ***$p<0.01$, **$p<0.05$, *$p<0.1$

A.8 Data set comparison

TABLE A.8: Total number of sex crimes. Summary statistics.

	(1) Stop and Frisk	(2) Complaint disaggregated	(3) Combined data set	(4) Complaint aggregated
Observations	238,931	238,931	238,931	693
Mean	0.0312977	.0804751	0.1117729	76.34921
Standard Deviation	0.3405145	0.3022442	0.4647225	40.44663

Notes: This table presents descriptive statistics for the three data sets used to measure sex crimes: "Stop-and-Frisk", Complaint Disaggregated and Complaint Aggregated. Furthermore, column (3) displays the descriptive statistics for the *Combined data set* resulting by joining both "Stop-and-Frisk" and Complaint Disaggregated. This latter data set is used in Section 1.5.4

TABLE A.9: Total number of sex crimes by borough and season. Absolute and relative frequencies.

Panel A: By Borough			
	Stop and frisk	Complaint dissaggregated	Complaint aggregated
The Bronx	454 (6.07%)	3,238 (16.84%)	9,790 (18.5%)
Brooklyn	1,464 (19.58%)	5,746 (29.88%)	17,100(32.32%)
Manhattan	3,844 (51.4%)	4,849 (25.22%)	11,890 (22.47%)
Queens	1,646 (22.01%)	4,806 (24.99%)	12,254 (23.16%)
Staten Island	170 (2.27%)	589 (3.06%)	1,876 (3.55%)
Total	7,478	19,228	52,910

Panel B: By Season		
	Stop and frisk	Complaint disaggregated
Winter	1,554 (20.78%)	4,896 (25.46%)
Spring	1,894 (25.33%)	5,551 (28.87%)
Summer	2,115 (28.28%)	4,634 (24.1%)
Fall	1,915 (25.6%)	4,147 (21.57%)
Total	7,478	19,228

Notes: Panel A and B presents the absolute frequencies of sex crimes in our sample period by and season respectively for the three data-sets used: "Stop-and-Frisk", Complaint Disaggregated and Complaint Aggregated. Relative frequencies in parentheses.

A.9 Results in levels

A.9.1 Main results

TABLE A.10: Main results in levels

	(1) Levels	(2) Levels	(3) Levels	(4) Levels
Adult Entertainment Est.	0.00421**	0.00421**	0.00421**	0.00759*
	(0.00180)	(0.00180)	(0.00180)	(0.00432)
Observations	238,931	238,931	238,931	238,931
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y
Day of the year FE		Y	Y	Y
Holiday FE			Y	Y
Precinct Trends				Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

A.9.2 Representability of "Stop-and-Frisk" data set

TABLE A.11: Representability of "Stop-and-Frisk" Data

	(1) Levels complaints
Adult Entertainment Est.	-0.0155* (0.00921)
Observations	238,931
Clustered variance at Precinct level	Y
Precinct FE	Y
Year FE	Y
Month FE	Y
Day of the week FE	Y
Day of the year FE	Y
Holiday FE	Y
Precinct Trends	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

A.9.3 Mechanisms behind the effect of adult entertainment establishments on sex crime

TABLE A.12: Potential victims: Street prostitution

	(1) Levels street prostitutes	(2) Levels loitering
Adult Entertainment Est.	-0.00301 (0.00240)	0.00184 (0.00188)
Observations	238,931	238,931
Clustered variance at Precinct level	Y	Y
Precinct FE	Y	Y
Year FE	Y	Y
Month FE	Y	Y
Day of the week FE	Y	Y
Day of the year FE	Y	Y
Holiday FE	Y	Y
Precinct Trends	Y	Y

Notes: Panel A presents the results to explore the potential victims channel. Columns (1) and (2) present results for the baseline regression using $\log(1+y)$ and IHS of street prostitutes. If sex crimes are decreasing because street prostitutes, who were victims of sex crimes before, are now working in adult entertainment establishments we would observe a statistical negative estimated coefficient. Results suggest that this is not the case. Clustered standard errors at the precinct level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

TABLE A.13: Potential victims: Big precincts

	(1) Levels	(2) Levels
Adult Entertainment Est.	-0.0137**	-0.0137**
	(0.00499)	(0.00665)
Observations	68,266	68,266
Clustered variance at Precinct level	Y	Wild
Precinct FE	Y	Y
Year FE	Y	Y
Month FE	Y	Y
Day of the week FE	Y	Y
Day of the year FE	Y	Y
Holiday FE	Y	Y
Precinct Trends	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.14: Potential victims: Bordering precincts

	(1) Levels bordering precincts	(2) Levels bordering precincts
Adult Entertainment Est.	-0.0200	-0.0200
	(0.0173)	(0.0185)
Dummy No IP est. in bordering precinct	0.0147	0.0147
	(0.0204)	(0.0190)
Interaction	0.0316	0.0316
	(0.0244)	(0.0409)
Observations	77,575	77,575
Clustered variance at Precinct level	Y	Wild
Precinct FE	Y	Y
Year FE	Y	Y
Month FE	Y	Y
Day of the week FE	Y	Y
Day of the year FE	Y	Y
Holiday FE	Y	Y
Precinct Trends	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.15: Potential victims: Big precincts, night

	(1) Levels big precincts	(2) Levels big precincts	(3) Levels big precincts	(4) Levels big precincts
Adult Entertainment Est.	-0.00691**	-0.00541**	-0.00691**	-0.00541*
	(0.00249)	(0.00248)	(0.00337)	(0.00309)
Dummy Night		0.0216***		0.0216***
		(0.00748)		(0)
Interaction Night		-0.00300***		-0.00300**
		(0.000152)		(0.00146)
Observations	136,532	136,532	136,532	136,532
Clustered variance at Precinct level	Y	Y	Wild	Wild
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.16: Potential victims: Bordering precincts, night

	(1) Levels	(2) Levels	(3) Levels	(4) Levels
Adult Entertainment Est.	-0.00990	-0.00781	-0.00426	-0.00781
	(0.00859)	(0.0102)	(0.00332)	(0.0149)
Dummy No IP est. in bordering precinct	0.00726	0.00726	0.00204	0.00726
	(0.0101)	(0.0101)	(0.00805)	(0.0155)
Dummy Night		0.0161***		0.0161***
		(0.00521)		(0)
Interaction Night & No IP est. in bordering precinct		-0.00218		-0.00218
		(0.0105)		(1.304e+19)
Interaction Night		-0.00418		-0.00418
		(0.00642)		(0.00798)
Interaction No IP est. in bordering precinct	0.0160	0.0170	0.00792	0.0170
	(0.0123)	(0.0141)	(0.00733)	(0.0292)
Observations	155,150	155,150	155,150	155,150
Clustered variance at Precinct level	Y	Y	Wild	Wild
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y
p-value joint effect		0.840		1
p-value		0.838		1

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.17: Potential criminals

	(1) Levels	(2) Levels
Adult Entertainment Est.	-0.00380*	-0.00236
	(0.00218)	(0.00148)
Dummy Night		0.00593**
		(0.00252)
Interaction		-0.00288
		(0.00174)
Observations	477,862	477,862
Clustered variance at Precinct level	Y	Y
Precinct FE	Y	Y
Year FE	Y	Y
Month FE	Y	Y
Day of the week FE	Y	Y
Day of the year FE	Y	Y
Holiday FE	Y	Y
Precinct Trends	Y	Y
p-value joint effect		0.0817
p-value		0.102

Notes: Clustered standard errors at the precinct level in parentheses. ***$p<0.01$, **$p<0.05$, *$p<0.1$

TABLE A.18: Potential criminals

	(1) Levels	(2) Levels
Adult Entertainment Est.	-0.00191*	-0.000634
	(0.00108)	(0.000541)
Dummy Evening		0.00376***
		(0.00142)
Dummy Night		0.00303*
		(0.00181)
Interaction Evening		-0.00164*
		(0.000955)
Interaction Night		-0.00236
		(0.00153)
Observations	955,724	955,724
Clustered variance at Precinct level	Y	Y
Precinct FE	Y	Y
Year FE	Y	Y
Month FE	Y	Y
Day of the week FE	Y	Y
Day of the year FE	Y	Y
Holiday FE	Y	Y
Precinct Trends	Y	Y
p-value joint effect		0.0877
p-value		0.000526

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

A.10 Randomization inference

FIGURE A.5: Randomization inference stratified at borough level

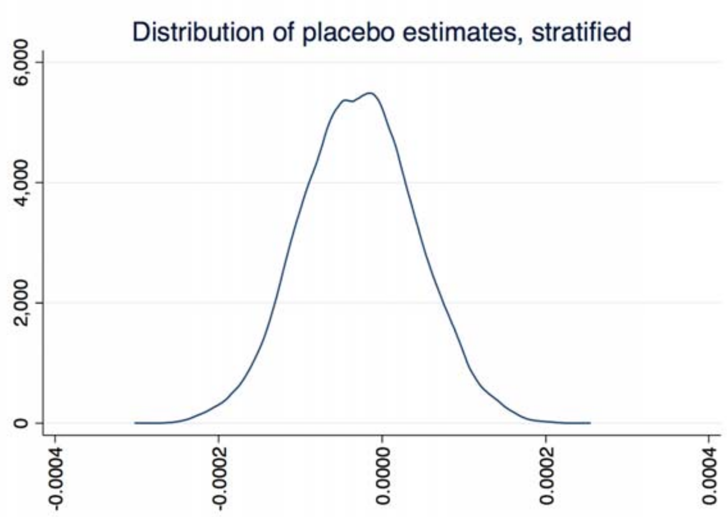
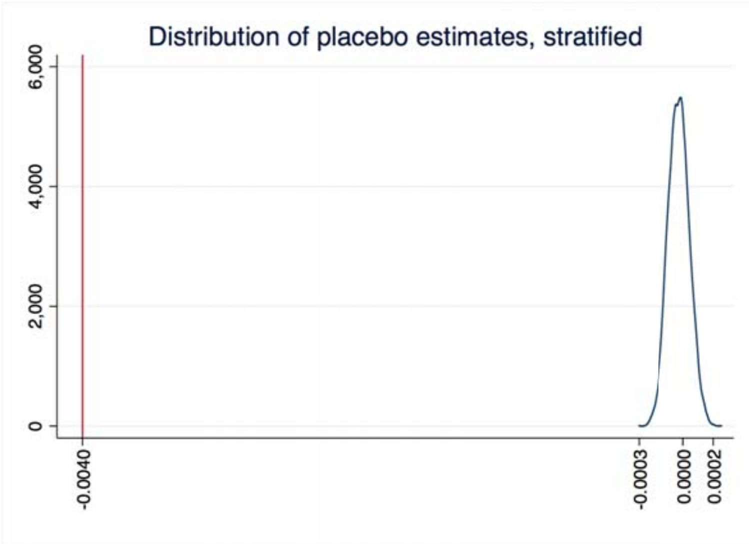


FIGURE A.6: Randomization inference stratified at borough level with estimated coefficient



A.11 Further checks: randomization inference without stratifying

This section presents the findings of running the same analysis as Section 5.5 without stratifying at the borough level. Since prostitution and sex crime patterns vary substantially across boroughs, there might be a concern that the results obtained in Section 5.5 are due to the stratification at the borough level.

Figures A.7 and A.8 below show the results of the estimated coefficient found with 1,000 permutations. The vertical red line represents our estimated coefficient (as in Figure A.6).

A simple visual inspection of the figures shows that there are no important differences in the findings even without stratification. It is important to note that without stratification, the estimated coefficients obtained by randomly permuting the number of establishments are less dispersed than stratifying (i.e. the support of the distribution depicted in Figure A.7 is smaller than that in Figure A.5). Figure A.8 compares such a distribution to our estimated coefficient: estimating our coefficient with randomization inference seems considerably more unlikely in this case. These results support our main finding that adult entertainment establishments decrease sex crimes.

FIGURE A.7: Randomization inference

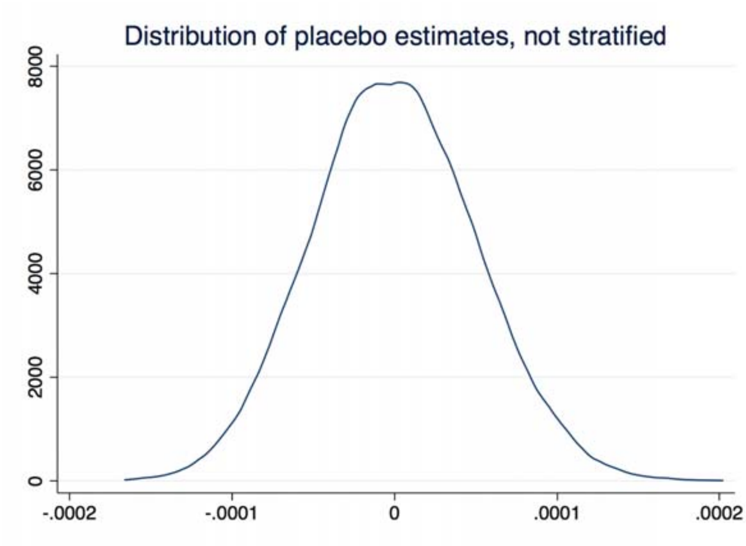
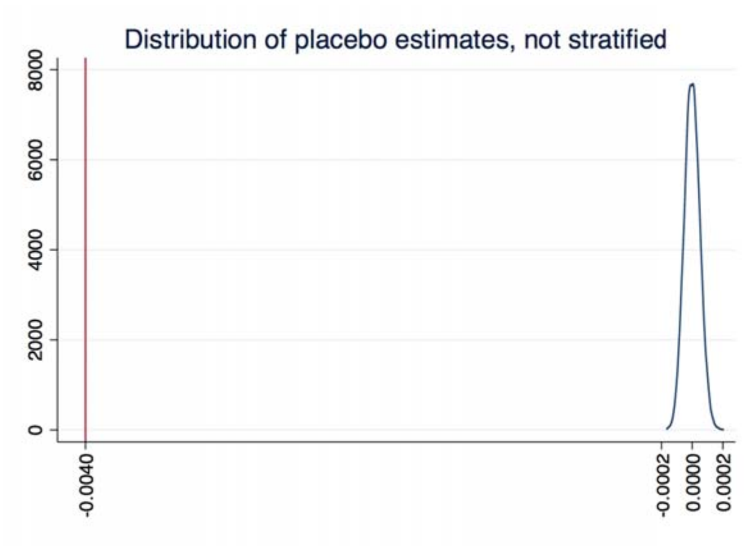


FIGURE A.8: Randomization inference with estimated coefficient



A.12 Police mechanism: further evidence

This section considers the effect of adult entertainment establishments on different types of crimes. Two features are important about such crimes. First, they should not be related to sex crimes, and there should not be a plausible mechanism of why adult entertainment establishments could affect them directly (i.e. other than via a change in the number of police officers). Second, it is preferable to select crimes that are easier to catch/control by officers compared to sex crimes. If it is a change in police presence that is driving the findings, then such crimes are much more likely to experience a decrease as well. [1] Table A.19 explores every sort of crime recorded in the "Stop-and-Frisk" data set that fulfills these two features.

[1]If it is a change in anything related to officers (e.g. their number or behavior) that is driving the decline in sex crimes, then crimes that are more easy to catch and control by police should be experiencing such a decrease as well.

Ten different crimes are presented in Table A.19: burglary, drug use, arson or fire, using a weapon, criminal mischief, murder, forgery, obscenity, graffiti and trespass. The table shows the estimated coefficient of running specification (1) using different transformations of a certain crime. Row (1) shows the effect using the logarithmic transformation, row (2) uses the IHS and row (3) uses the dependent variable in levels. All regressions (as in our main specification) have clustered standard errors at the precinct level and include precinct, year, month, day-of-the-year, day-of-the-week and holiday fixed effects, as well as precinct-year linear time trends.

It is important to note that the crimes presented in this section, in addition to sharing the two features listed above, are substantially different. Yet, there is no evidence that adult entertainment establishments decrease any of these crimes.

TABLE A.19: The effect of adult entertainment establishments on other crimes

	(1) Burglary	(2) Drug	(3) Arson	(4) Weapon	(5) Criminal Mischief	(6) Murder	(7) Forgery	(8) Obscenity	(9) Graffiti	(10) Trespass
Log	-0.00766	0.00541	-0.000393	-0.000854	-0.000870	0.000103	-0.0164	-1.14e-06	0.00123	0.0129
	(0.0137)	(0.00796)	(0.000564)	(0.000897)	(0.00274)	(0.000134)	(0.0113)	(1.23e-05)	(0.00199)	(0.00870)
IHS	-0.0155	0.0109	-0.000796	-0.00167	-0.00168	0.000232	-0.0328	-1.95e-06	0.00250	0.0256
	(0.0274)	(0.0159)	(0.00113)	(0.00180)	(0.00547)	(0.000264)	(0.0227)	(2.46e-05)	(0.00398)	(0.0174)
Levels	-0.0242	0.0182	-0.000290	-0.00183	-0.00141	0.000295	-0.0294	-5.36e-07	0.00426	0.0932**
	(0.0520)	(0.0249)	(0.00119)	(0.00248)	(0.00472)	(0.000285)	(0.0207)	(1.79e-05)	(0.00427)	(0.0392)
Observations	238,931	238,931	238,931	238,931	238,931	238,931	238,931	238,931	238,931	238,931

Notes: Clustered standard errors at the precinct level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

A.13 Further checks: falsification test

A.13.1 Police channel

As a further check of the police channel we run our falsification test using other crimes. If the reduction in sex crimes is due to an increase in officers near precincts where adult entertainment establishments open, then when controlling for a lagged and a forwarded value of our treatment variable such crimes should exhibit a similar decrease as well.

Tables A.20 to A.39 report the results of running Equation (2) using these crimes. No other crime has a similar pattern to sex crimes (i.e. only decrease in contemporaneous and lagged value with one of the two statistically significant). Hence, we find no empirical evidence that any other crime exhibits a decrease pattern similar to sex crimes. This finding goes against the police channel and further reinforces the credibility of our identification assumption.

TABLE A.20: Falsification test, Log(1+y)

	(1) Log(1+y) Burglary stops	(2) Log(1+y) Burglary stops	(3) Log(1+y) Burglary stops	(4) Log(1+y) Burglary stops
Adult Entertainment Est. (t+1)	-0.0592			-0.0966
	(0.0460)			(0.0642)
Adult Entertainment Est.		-0.0545		0.0809*
		(0.0431)		(0.0445)
Adult Entertainment Est. (t-1)			-0.0548	-0.0440
			(0.0419)	(0.0563)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precicnt level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.21: Falsification test, IHS

	(1) IHS Burglary stops	(2) IHS Burglary stops	(3) IHS Burglary stops	(4) IHS Burglary stops
Adult Entertainment Est. (t+1)	-0.118			-0.193
	(0.0920)			(0.128)
Adult Entertainment Est.		-0.109		0.162*
		(0.0862)		(0.0891)
Adult Entertainment Est.(t-1)			-0.110	-0.0879
			(0.0839)	(0.113)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.22: Falsification test, Log(1+y)

	(1) Log(1+y) Drug stops	(2) Log(1+y) Drug stops	(3) Log(1+y) Drug stops	(4) Log(1+y) Drug stops
Adult Entertainment Est. (t+1)	-0.0401**			-0.0232
	(0.0182)			(0.0357)
Adult Entertainment Est.		-0.0362*		-0.0158
		(0.0188)		(0.0314)
Adult Entertainment Est. (t-1)			-0.0337*	0.000965
			(0.0181)	(0.0348)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precicnt level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.23: Falsification test, IHS

	(1) IHS Drug stops	(2) IHS Drug stops	(3) IHS Drug stops	(4) IHS Drug stops
Adult Entertainment Est. (t+1)	-0.0802**			-0.0463
	(0.0365)			(0.0714)
Adult Entertainment Est..		-0.0723*		-0.0317
		(0.0375)		(0.0627)
Adult Entertainment Est. (t-1)			-0.0674*	0.00193
			(0.0362)	(0.0695)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.24: Falsification test, Log(1+y)

	(1) Log(1+y) Fire stops	(2) Log(1+y) Fire stops	(3) Log(1+y) Fire stops	(4) Log(1+y) Fire stops
Adult Entertainment Est. (t+1)	-0.00901			-0.00948
	(0.0137)			(0.0262)
Adult Entertainment Est.		-0.00998		0.00793
		(0.0131)		(0.0332)
Adult Entertainment Est. (t-1)			-0.00860	-0.00614
			(0.0131)	(0.0277)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.25: Falsification test, IHS

	(1) IHS Fire stops	(2) IHS Fire stops	(3) IHS Fire stops	(4) IHS Fire stops
Adult Entertainment Est. (t+1)	-0.0180			-0.0190
	(0.0274)			(0.0524)
Adult Entertainment Est.		-0.0200		0.0159
		(0.0262)		(0.0665)
Adult Entertainment Est. (t-1)			-0.0172	-0.0123
			(0.0263)	(0.0555)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.26: Falsification test, Log(1+y)

	(1) Log(1+y) Weapon stops	(2) Log(1+y) Weapon stops	(3) Log(1+y) Weapon stops	(4) Log(1+y) Weapon stops
Adult Entertainment Est. (t+1)	-0.0183**			-0.00566
	(0.00911)			(0.0244)
Adult Entertainment Est.		-0.0200**		-0.0263
		(0.00951)		(0.0258)
Adult Entertainment Est. (t-1)			-0.0171	0.0143
			(0.0107)	(0.0264)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.27: Falsification test, IHS

	(1) IHS Weapon stops	(2) IHS Weapon stops	(3) IHS Weapon stops	(4) IHS Weapon stops
Adult Entertainment Est. (t+1)	-0.0365**			-0.0113
	(0.0182)			(0.0488)
Adult Entertainment Est.		-0.0401**		-0.0526
		(0.0190)		(0.0516)
Adult Entertainment Est. (t-1)			-0.0343	0.0287
			(0.0214)	(0.0529)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.28: Falsification test, Log(1+y)

	(1) Log(1+y) Criminal mischief stops	(2) Log(1+y) Criminal mischief stops	(3) Log(1+y) Criminal mischief stops	(4) Log(1+y) Criminal mischief stops
Adult Entertainment Est. (t+1)	0.00972			0.0325
	(0.0307)			(0.0512)
Adult Entertainment Est.		0.00630		-0.0159
		(0.0296)		(0.0600)
Adult Entertainment Est. (t-1)			0.00928	-0.00529
			(0.0272)	(0.0419)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.29: Falsification test, IHS

	(1) IHS Criminal mischief stops	(2) IHS Criminal mischief stops	(3) IHS Criminal mischief stops	(4) IHS Criminal mischief stops
Adult Entertainment Est. (t+1)	0.0194			0.0649
	(0.0614)			(0.102)
Adult Entertainment Est.		0.0126		-0.0318
		(0.0592)		(0.120)
Adult Entertainment Est. (t-1)			0.0186	-0.0106
			(0.0544)	(0.0838)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.30: Falsification test, Log(1+y)

	(1) Log(1+y) Murder stops	(2) Log(1+y) Murder stops	(3) Log(1+y) Murder stops	(4) Log(1+y) Murder stops
Adult Entertainment Est. (t+1)	0.00173			-0.00605
	(0.00370)			(0.0132)
Adult Entertainment Est.		0.00224		0.00126
		(0.00355)		(0.0124)
Adult Entertainment Est. (t-1)			0.00317	0.00770
			(0.00333)	(0.00749)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precicnt level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y
Clustered variance at Precinct level				Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.31: Falsification test, IHS

	(1) IHS Murder stops	(2) IHS Murder stops	(3) IHS Murder stops	(4) IHS Murder stops
Adult Entertainment Est. (t+1)	0.00346			-0.0121
	(0.00739)			(0.0264)
Adult Entertainment Est.		0.00447		0.00252
		(0.00709)		(0.0249)
Adult Entertainment Est. (t-1)			0.00634	0.0154
			(0.00666)	(0.0150)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.32: Falsification test, Log(1+y)

	(1) Log(1+y) Forgery stops	(2) Log(1+y) Forgery stops	(3) Log(1+y) Forgery stops	(4) Log(1+y) Forgery stops
Adult Entertainment Est. (t+1)	-0.0859*			-0.0151
	(0.0431)			(0.0306)
Adult Entertainment Est.		-0.0924**		0.0126
		(0.0442)		(0.0408)
Adult Entertainment Est. (t-1)			-0.0957**	-0.0917**
			(0.0431)	(0.0368)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.33: Falsification test, IHS

	(1) IHS Forgery stops	(2) IHS Forgery stops	(3) IHS Forgery stops	(4) IHS Forgery stops
Adult Entertainment Est. (t+1)	-0.172*			-0.0303
	(0.0863)			(0.0612)
Adult Entertainment Est.		-0.185**		0.0252
		(0.0883)		(0.0816)
Adult Entertainment Est. (t-1)			-0.191**	-0.183**
			(0.0863)	(0.0735)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.34: Falsification test, Log(1+y)

	(1) Log(1+y) Obscenity stops	(2) Log(1+y) Obscenity stops	(3) Log(1+y) Obscenity stops	(4) Log(1+y) Obscenity stops
Adult Entertainment Est. (t+1)	0.000185			0.00231
	(0.000181)			(0.00230)
Adult Entertainment Est.		-3.63e-05		-0.00245
		(0.000373)		(0.00268)
Adult Entertainment Est. (t-1)			-2.35e-05	0.000212
			(0.000355)	(0.000217)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.35: Falsification test, IHS

	(1) IHS Obscenity stops	(2) IHS Obscenity stops	(3) IHS Obscenity stops	(4) IHS Obscenity stops
Adult Entertainment Est. (t+1)	0.000369			0.00463
	(0.000363)			(0.00460)
Adult Entertainment Est.		-7.27e-05		-0.00491
		(0.000745)		(0.00537)
Adult Entertainment Est. (t-1)			-4.70e-05	0.000424
			(0.000711)	(0.000434)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.36: Falsification test, Log(1+y)

VARIABLES	(1) Log(1+y) Graffiti stops	(2) Log(1+y) Graffiti stops	(3) Log(1+y) Graffiti stops	(4) Log(1+y) Graffiti stops
Adult Entertainment Est. (t+1)	-0.00338			-0.107***
	(0.0258)			(0.0307)
Adult Entertainment Est.		0.00798		0.0533
		(0.0262)		(0.0399)
Adult Entertainment Est. (t-1)			0.0154	0.0599*
			(0.0262)	(0.0346)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.37: Falsification test, IHS

	(1) IHS Graffiti stops	(2) IHS Graffiti stops	(3) IHS Graffiti stops	(4) IHS Graffiti stops
Adult Entertainment Est. (t+1)	-0.00677			-0.213***
	(0.0517)			(0.0613)
Adult Entertainment Est.		0.0160		0.107
		(0.0525)		(0.0798)
Adult Entertainment Est. (t-1)			0.0308	0.120*
			(0.0525)	(0.0692)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.38: Falsification test, Log(1+y)

VARIABLES	(1) Log(1+y) Trespass stops	(2) Log(1+y) Trespass stops	(3) Log(1+y) Trespass stops	(4) Log(1+y) Trespass stops
Adult Entertainment Est. (t+1)	-0.00611			0.0598
	(0.0287)			(0.0385)
Adult Entertainment Est.		-0.0127		-0.0755
		(0.0275)		(0.0590)
Adult Entertainment Est. (t-1)			-0.00877	0.00941
			(0.0284)	(0.0519)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.39: Falsification test, IHS

	(1) IHS Trespass stops	(2) IHS Trespass stops	(3) IHS Trespass stops	(4) IHS Trespass stops
Adult Entertainment Est. (t+1)	-0.0122			0.120
	(0.0574)			(0.0770)
Adult Entertainment Est..		-0.0255		-0.151
		(0.0549)		(0.118)
Adult Entertainment Est. (t-1)			-0.0175	0.0188
			(0.0567)	(0.104)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

A.13.2 Potential victims channel

Similar to the previous section, we check whether we find evidence in our falsification test that either street prostitution or loitering experiences a decrease contemporaneously to the openings of adult entertainment establishments. If this is the case, our falsification test should obtain results similar to those of sex crimes.

Yet, as can be observed in Tables A.38 to A.41, there is no evidence of any decrease in these two outcomes. These findings rule out the potential victims channel.

TABLE A.40: Falsification test, Log(1+y)

	(1) Log(1+y) Street prostitution	(2) Log(1+y) Street prostitution	(3) Log(1+y) Street prostitution	(4) Log(1+y) Street prostitution
Adult Entertainment Est. (t+1)	-0.00888			0.0303
	(0.0173)			(0.0420)
Adult Entertainment Est.		-0.0146		-0.0692
		(0.0165)		(0.0431)
Adult Entertainment Est. (t-1)			-0.0103	0.0294*
			(0.0171)	(0.0158)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.41: Falsification test, IHS

	(1) IHS Street prostitution	(2) IHS Street prostitution	(3) IHS Street prostitution	(4) IHS Street prostitution
Adult Entertainment Est. (t+1)	-0.0178			0.0606
	(0.0346)			(0.0841)
Adult Entertainment Est.		-0.0292		-0.138
		(0.0329)		(0.0862)
Adult Entertainment Est. (t-1)			-0.0205	0.0587*
			(0.0342)	(0.0317)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.42: Falsification test, Log(1+y)

	(1) Log(1+y) Loitering	(2) Log(1+y) Loitering	(3) Log(1+y) Loitering	(4) Log(1+y) Loitering
Adult Entertainment Est. (t+1)	0.0193			-0.0268*
	(0.0142)			(0.0140)
Adult Entertainment Est.		0.0228		0.0263
		(0.0143)		(0.0232)
Adult Entertainment Est. (t-1)			0.0233	0.0232
			(0.0145)	(0.0232)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.43: Falsification test, IHS

	(1) Log(1+y) Loitering	(2) Log(1+y) Loitering	(3) Log(1+y) Loitering	(4) Log(1+y) Loitering
Adult Entertainment Est. (t+1)	0.0193			-0.0268*
	(0.0142)			(0.0140)
Adult Entertainment Est.		0.0228		0.0263
		(0.0143)		(0.0232)
Adult Entertainment Est. (t-1)			0.0233	0.0232
			(0.0145)	(0.0232)
Observations	7,777	7,854	7,777	7,700
Clustered variance at Precinct level	Y	Y	Y	
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

A.14 List of larger precincts in potential victims channel

The 77 precincts are grouped into 22 âĂIJbigâĂİ precincts according to geographic proximity (see Table A.42). For instance, Precincts 1, 5 and 7 were grouped together, as were Precincts 6, 9, 10 and 13.

TABLE A.44: List of larger precincts to test the potential victims channel

Big Precinct	Formed by precincts
1	1, 5 and 7
2	6, 9, 10 and 13
3	14, 17 and 18
4	19, 20, 22 and 24
5	23, 25, 26 and 28
6	30, 32, 33 and 34
7	40, 41, 42, 43 and 44
8	46, 48 and 52
9	45, 47, 49 and 50
10	60, 61, 62 and 68
11	66, 70 and 72
12	71, 76, 77 and 78
13	79, 81, 84 and 88
14	63, 67, 69 and 73
15	83, 90 and 94
16	104, 108 and 114
17	75, 102 and 106
18	110, 112 and 115
19	100 and 101
20	103, 105 and 113
21	107, 109 and 111
22	120, 121, 122 and 123

A.15 Mechanisms behind the effect of adult entertainment establishments on sex crimes: potential victims channel

In this section we consider the possibility that women are simply avoiding precincts with at least one adult entertainment establishment in favor of those that have none. If this is the case, we should observe an increase in the number of sex crimes in these latter precincts. Indeed, if the estimated negative coefficient is due only to fewer women passing through precincts with at least one establishment, it implies that we should observe an increase in the bordering precincts that do not have any such establishments. Therefore, we restrict the sample to precincts with no adult establishments in bordering precincts, where one of these bordering precincts experienced at least one opening of an establishment at a later point in time. If it is true that the reduction in sex crimes we observe is merely due to women avoiding adult entertainment establishments, we should find that increasing the number of these establishments increases sex crimes in bordering precincts that do not have an adult entertainment establishment.

Hence, we consider a specification like Equation (1) but where the dependent variable is the number of sex crimes that occurred in the bordering precincts; we also add two explanatory variables. The first is a dummy variable taking a value of 1 if there is no indoor prostitution business in a bordering precinct. The second is the interaction between this dummy and the number of indoor prostitution businesses in the precinct of interest. If women are avoiding precincts with adult entertainment establishments, the interaction should be statistically significant and positive. In other words, sex crimes would be moving from precincts with adult establishments to those without them.

Table A.45 presents the results of this specification. Columns (1) and (3) present the results of our logarithmic transformation using, respectively, regular clustered errors at the precinct level and wild cluster-bootstrap (since the number of considered precincts decreases in this case). Columns (2) and (4) repeat the same analysis but for IHS. We find that the estimated coefficient is not statistically significant in any of our four specifications.[2]

A plausible explanation could be that women avoid precincts with adult entertainment establishments only at night. If this is the case, it may be that our previous specifications find no empirical evidence only because they are not separating sex crimes happening at night from those happening during the day. To address this issue, we run the previous specifications separating sex crimes according to the time of day. Table A.46 runs the same regressions as Panel B of Table 1.8 but separating sex crimes that occurred at night from those occurred during the day. The reasoning behind running these regressions is identical to the previous ones but applied at night.

As a benchmark, Column (1) of Table A.46 presents the results of using only the number of establishments (i.e. with neither a fixed effect for crimes committed at night nor the interaction between such fixed effect and the number of establishments). As expected, the estimated coefficient is statistically negative and lower in absolute value than the one in Panel B of Table 1.8. Columns (2) and (3) of Table A.46 report the coefficient of running this regression using our usual logarithmic transformation and the IHS, respectively. Columns (3) and (4) of Table A.46 repeat the same analysis using wild cluster-bootstrap methods due to the low number of clusters in this case.

If women avoid precincts with adult establishments at night, we should find that the estimated coefficient of the interaction term is either statistically significant and positive, or not statistically significant. In fact, if the decrease in sex crimes is due to women avoiding precincts with establishments at night, this would imply that at night sex crimes decrease in precincts with establishments but increase (or do not change) in other precincts. Therefore, the total effect of such establishments in larger precincts at night should be either positive or insignificant. In all four columns, the coefficient of the interaction term is statistically negative, suggesting that a decline in potential victims at night is not the main channel.

Table A.47 repeats the regressions of Table A.45 separating sex crimes happening at night from those happening during the day. In these regressions we are interested in the coefficient of the triple interaction between adult entertainment establishments, the dummy variable taking a value of 1 if there is no adult entertainment establishment in a bordering precinct, and the dummy variable taking a value of 1 for sex crimes committed at night. For ease of comparison, Columns (1) and (4), respectively, present the results of running the model only using the number of establishments and fixed effect and interaction (as in Table 16) for, respectively, regular clustered errors at the precinct level and wild cluster-bootstrap clustered errors at the precinct level. Columns (2) and (3) present the results of running the whole model for, respectively, our logarithmic transformation and IHS with regular clustered errors at the precinct level. Columns (4) and (5) repeat these computations using wild cluster-bootstrap clustered errors at the precinct level. The level of significance of the

[2]Likewise, the results of this table support the hypothesis that sex crimes are not moving to bordering precincts.

coefficient of interest (i.e. triple interaction) is shown in the table as the "p-value." Moreover, the row "p-value joint effect" shows the p-values associated with testing whether the total effect (i.e. the sum of the coefficients associated with our main regressor and its interactions) is zero. In our four regressions (i.e. Columns (2), (3), (4) and (5)) the coefficient of interest is statistically insignificant. These findings do not support the hypothesis that women avoid precincts that have adult entertainment establishments.

TABLE A.45: Potential victims channel

	(1) Log Bordering precincts	(2) IHS Bordering precincts	(3) Log Bordering precincts	(4) IHS Bordering precincts
Adult Entertainment Est.	-0.00853	-0.0171	-0.00853	-0.0171
	(0.00722)	(0.0144)	(0.00726)	(0.0145)
Dummy No IP est.	0.00280	0.00561	0.00280	0.00561
in bordering precinct				
	(0.00755)	(0.0151)	(0.00481)	(0.00962)
Interaction	0.0158	0.0317	0.0158	0.0317
	(0.0108)	(0.0216)	(0.0135)	(0.0270)
Observations	77,575	77,575	77,575	77,575
Clustered variance at Precinct level	Y	Y	Wild	Wild
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE A.46: Potential victims channel

	(1) Log Big precincts	(2) Log Big precincts	(3) IHS Big precincts precincts	(4) Log Big precincts precincts	(5) Log Big precincts precincts	(6) IHS Big precincts
Adult Entertainment Est.	-0.00370***	-0.00289**	-0.00579**	-0.00370**	-0.00289*	-0.00579*
	(0.00122)	(0.00122)	(0.00244)	(0.00180)	(0.00165)	(0.00331)
Dummy Night		0.00616**	0.0123**		0.00616***	0.0123***
		(0.00282)	(0.00564)		(0)	(0)
Interaction Night		-0.00162***	-0.00324***		-0.00162**	-0.00324**
		(5.54e-05)	(0.000111)		(0.000788)	(0.00158)
Observations	136,532	136,532	136,532	136,532	136,532	136,532
Clustered variance at Precinct level	Y	Y	Y	Wild	Wild	Wild
Precinct FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y	Y	Y

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

TABLE A.47: Potential victims channel

	(1) Log Bordering precincts	(2) Log Bordering precincts	(3) IHS Bordering precincts	(4) Log Bordering precincts	(5) Log Bordering precincts	(6) IHS Bordering precincts
Adult Entertainment Est.	-0.00419	-0.00284	-0.00568	-0.00419	-0.00284	-0.00568
	(0.00378)	(0.00430)	(0.00859)	(0.00327)	(0.00542)	(0.0108)
Dummy Night		0.00546**	0.0109**		0.00546*	0.0109*
		(0.00249)	(0.00497)		(0.00312)	(0.00624)
Interaction Night & No IP est. in bordering precinct		-0.000384	-0.000768		-0.000384	-0.000768
		(0.00416)	(0.00831)		(1.304e+19)	(1.304e+19)
Dummy No IP est. in bordering precinct	0.00203	0.00203	0.00405	0.00203	0.00203	0.00405
	(0.00415)	(0.00415)	(0.00830)	(0.0100)	(0.0100)	(0.0201)
Interaction Night		-0.00270	-0.00540		-0.00270	-0.00540
		(0.00257)	(0.00514)		(0.00211)	(0.00421)
Interaction No IP est. in bordering precinct	0.00787	0.00806	0.0161	0.00787	0.00806	0.0161
	(0.00553)	(0.00640)	(0.0128)	(0.00670)	(0.00958)	(0.0192)
Observations	155,150	155,150	155,150	155,150	155,150	155,150
Clustered variance at Precinct level	Y	Y	Y	Wild	Wild	Wild
Precinct FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y	Y	Y
p-value joint effect		0.722	0.722		1	1
p-value		0.927	0.927		1	1

Notes: Clustered standard errors at the precinct level in parentheses. ***$p<0.01$, **$p<0.05$, *$p<0.1$

A.16 Mechanisms behind the effect of adult entertainment establishments on sex crimes: potential criminals channel

In this section we run the same analysis as in Section 6.3 but dividing the day into two equal halves: morning (6 A.M. to 6 P.M.) and night (6 P.M. to 6 A.M.). So now the time unit is a half-day. Furthermore, we create a dummy variable that takes a value of 1 at night and 0 in the morning. Finally, we saturate the main specification including the interaction between the number of establishments and this dummy.

Table A.48 presents the results of this specification for the logarithmic transformation and the IHS, respectively. The effect of the number of establishments is still negative, and the coefficient on the night/day dummy variable is positive, showing that at night there are more sex crimes, as expected. The coefficient of the interaction term is negative, but it is not statistically significant at standard levels. Yet, by comparing the size of the coefficients in Columns (1) to (2) to those in Columns (3) to (4), we can observe that most of the effect is driven by the effect of adult entertainment establishments at night. These results suggest that the effect of adult entertainment establishments is mostly driven at times when these establishments are open for business.

TABLE A.48: Potential Criminal Channel

	(1) Log	(2) Log	(3) IHS	(4) IHS
Adult Entertainment Est.	-0.00215*	-0.00133*	-0.00430*	-0.00266*
	(0.00117)	(0.000761)	(0.00233)	(0.00152)
Dummy Night		0.00183		0.00365
		(0.00115)		(0.00231)
Interaction		-0.00164		-0.00328
		(0.00100)		(0.00201)
Observations	477,862	477,862	477,862	477,862
Clustered variance at Precinct level	Y	Y	Y	Y
Precinct FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Day of the week FE	Y	Y	Y	Y
Day of the year FE	Y	Y	Y	Y
Holiday FE	Y	Y	Y	Y
Precinct Trends	Y	Y	Y	Y
p-value joint effect		0.0718		0.0718
p-value		0.106		0.106

Notes: Clustered standard errors at the precinct level in parentheses. ***p<0.01, **p<0.05, *p<0.1

Appendix B

Appendix: The Effect of Unilateral Divorce on Prostitution: Evidence from Divorce Laws in U.S. States

B.1 Nature of the effect: Inflow vs Stock

Figure B.1 shows the effect of unilateral divorce on prostitution across age groups.[1].The dependent variable is in logs as in the main regression. Moreover, as in the main regression, each regression includes county, year and month fixed effects, county-year trends and variance is clustered at state level.

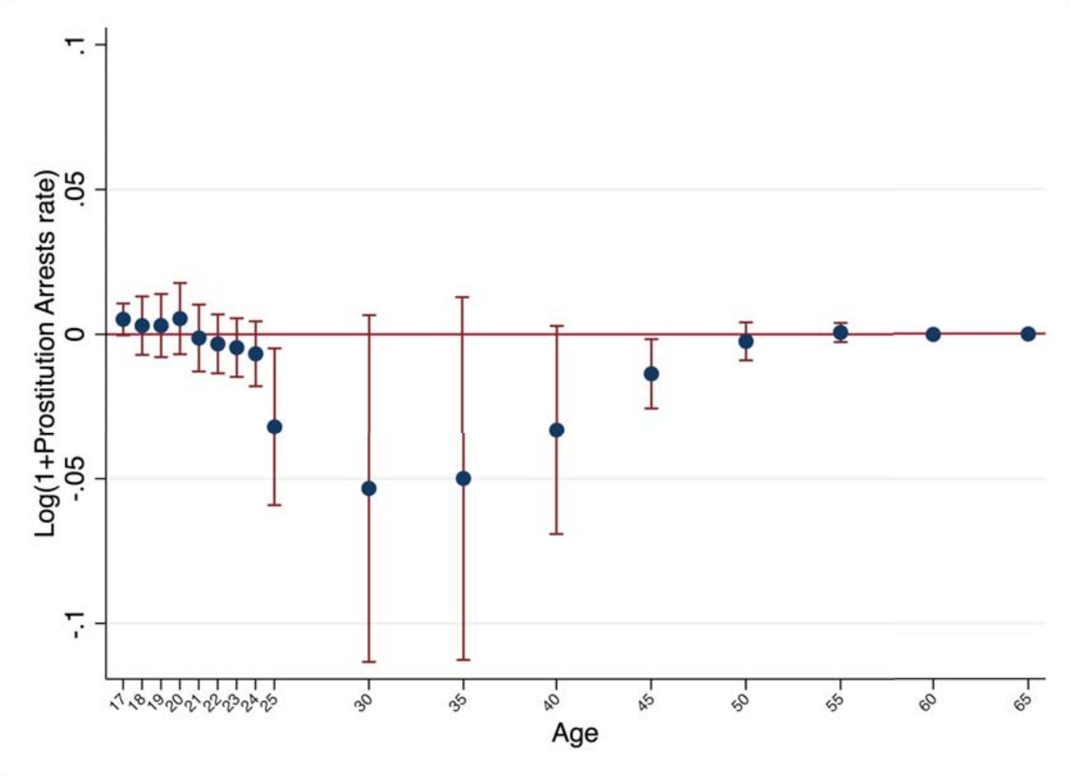
There are two ways in which unilateral divorce could affect prostitution: either by preventing women to become prostitutes (i.e. inflow effect) or by affecting prostitutes who are already inside the market (i.e. stock effect). If unilateral divorce decreases young (old) prostitutes' arrests it would support the former (latter) effect. Figure B.1 finds that unilateral divorce mainly reduces prostitution between 25 and 29 years old and prostitution between 45 and 49 years old.[2] Hence, there is evidence in favour of both effects.

In addition, Figure B.1 has two features worth mentioning. First, unilateral divorce does not affect prostitutes among 17 and 24 years old and prostitutes among 50 and 65 years old or older. In these two age groups the point estimate is close to zero and it is reassuring to find that the standard errors are narrow. Second, on the contrary, in the age group among 25 and 49 years old there seem to be a U-shape curve, but standard errors are not as precise.

[1] Age groups are classified according to UCR database as in Table B.3. Starting at 25 years old, ages are grouped in five years blocks: 25 to 29 years old, 30 to 34 years old, and so on and so forth.

[2] There could be the concern that there is no effect in 17-24 age group since data is not pooled. Yet, Section B.8.1 presents the results of running a regression pooling together arrests of female prostitutes between 17 and 24 years old and results do not change.

FIGURE B.1: Parallel trends between treated and control groups, other ages



B.2 List of crimes in UCR data set

TABLE B.1: List of offenses

Offense code	Offense
01A	Murder and non-negligent manslaughter
01B	Manslaughter by negligence
02	Forcible rape
03	Robbery
04	Aggravated assault
05	Burglary-breaking or entering
06	Larceny-theft (not motor vehicles
07	Motor vehicle theft
08	Other assaults
09	Arson
10	Forgery and counterfeiting
11	Fraud
12	Embezzlement
13	Stolen property-buy, receive, poss.
14	Vandalism
15	Weapons-carry, posses, etc.
16	Prostitution and commercialized vice
17	Sex offenses (not rape or prostitution)
18	Total drug abuse violations
180	Sale/manufacture (subtotal)
185	Possession (subtotal)
18A	Sale/mfg-Opium, coke, and their derivatives
18B	Sale/mfg-Marijuana
18C	Sale/mfg-Truly addicting synthetic narcotics
18D	Sale/mfg-Other dangerous non-narc drugs
18E	Possession-Opium, coke, and their derivatives
18F	Possession-Marijuana
18G	Possession-Truly addicting synthetic narcotics
18H	Possession-Other dangerous non-narc drugs
19	Gambling (total)
19A	Bookmaking (horse and sports)
19B	Number and lottery
19C	All other gambling
20	Offenses against family and children
21	Driving under the influence
22	Liquor laws
23	Drunkenness
24	Disorderly conduct
25	Vagrancy
26	All other non-traffic offenses
27	Suspicion
28	Curfew and loitering violations
29	Runaways

B.3 Further information on the data set

B.3.1 Descriptive statistics

Table B.2 displays summary statistics for arrests of female prostitutes per 1,000,0000 inhabitants across treated and control states.[3] Data is at county-month level and treated states are disaggregated at pre and post treatment level.

[3]Arrests of female prostitutes per 1,000,000 inhabitants is computed as the number of arrested female prostitutes divided by population and multiplied by 1,000,000. Same computations are made for data on other crimes.

TABLE B.2: Summary statistics

	(1) Never-treated	(2) Always-treated	(3)	(4) Treated	(5)
Arrests of female prostitutes per 1,000,0000 inhabitants			pre	post	all
Mean	1.87	1.80	3.19	0.88	2.29
Std. dev.	13.83	20.44	16.27	6.39	13.38
Obs.	347,712	764,554	85,642	54,374	140,016
Max	2,042	3,969	1,058.22	484	1,058.22

Table B.3 shows summary statistics for arrests of female prostitutes per 1,000,0000 inhabitants broken out by age group. Columns (1) to (4) respectively report mean, standard deviation, minimum and maximum. While, column (5) reports the share of each group, out of the total arrests of female prostitutes, without taking into account the population.[4]

TABLE B.3: Summary statistics

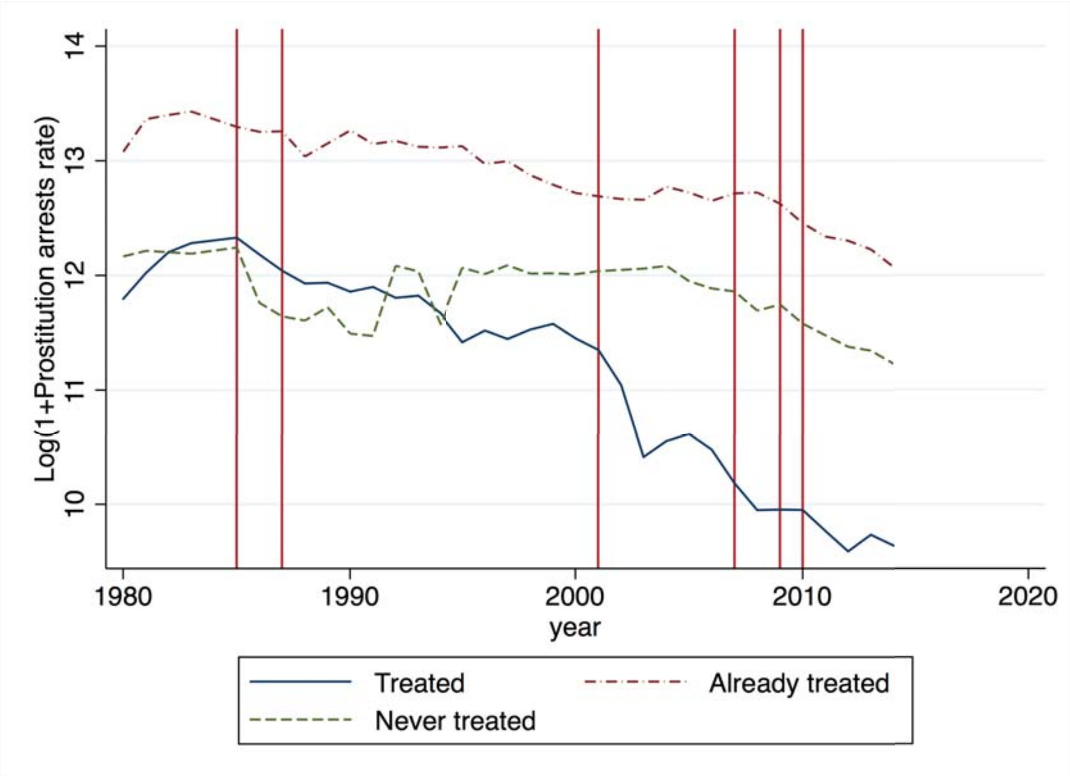
	(1)	(2)	(3)	(4)	(5)
Arrests of female prostitutes per 1,000,0000 inhabitants	Mean	Std. dev.	Min	Max	Relative share (%)
Age group					
17	.0223	1.4267	0	1225.49	0.93
18	0.0586	0.9967	0	222	3.15
19	0.0809	1.3189	0	253.23	4.65
20	0 .0885	1.6375	0	461.04	5.07
21	0.099	2.1318	0	745.86	5.7
22	0.1017	2.2021	0	563.49	5.89
23	0.0998	2.0089	0	485.63	5.69
24	0.0979	1.7881	0	370.88	5.37
25-29	0.4155	4.7445	0	889.3	22.85
30-34	0.3216	3.8326	0	2849	17.08
35-39	0.2219	2.1452	0	411.07	11.64
40-44	0.1327	1.4215	0	309.26	6.9
45-49	0.0681	1.1198	0	545.55	3.35
50-54	0.0243	0.5573	0	212.95	1.2
55-59	0.0084	0.4604	0	236.91	0.37
60-64	0.0029	0.2399	0	122.44	0.13
65 or older	0.0022	0.2487	0	134.12	0.07
Total	1.87	18.11	0	3969.04	100

Figure B.2 displays arrests of female prostitutes per 1,000,0000 inhabitants (in the same logarithmic transformation as the dependent variable) for the three groups of states: treated, never-treated and already-treated. Vertical lines represents the year in which unilateral divorce laws became effective in each of the treated states.

[4]Age groups are defined according to the UCR database.

This figure cannot be used to assess whether the trends of treated and control groups are parallel since the effective dates of unilateral divorce laws differ across states. However, it shows that, as many more states adopt unilateral divorce, treated states experience a substantial decline in arrests of female prostitutes per 1,000,0000 inhabitants in line with my findings. In other words, as treated states adopt unilateral divorce arrests of female prostitutes decrease more severely there than in control states.

FIGURE B.2: Evolution of female prostitutes arrests in treated and control states

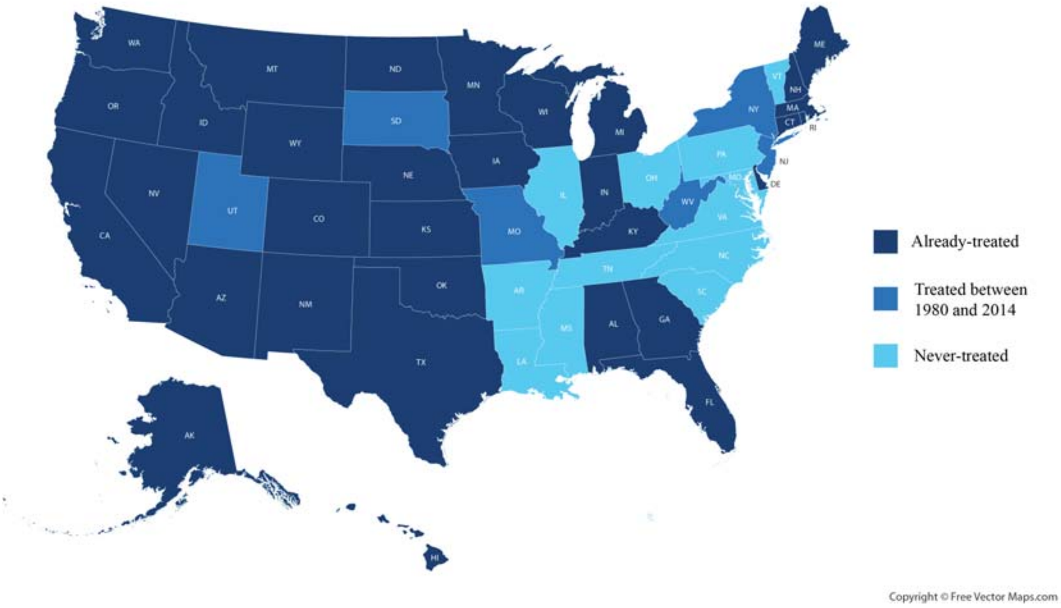


Notes: This figure plots arrests of female prostitutes per 1,000,0000 inhabitants, in the same logarithmic transformation as the dependent variable, for the three groups of states analyzed in the study: treated, never-treated and already-treated. Vertical lines represents the year in which unilateral divorce law became effective in each of the treated states.

B.4 Effective date of unilateral divorce laws across U.S. states

The effective date is established using Thomson Reuters Westlaw. In the section "Statutes and Court rules", Thomson Reuters Westlaw keeps track of different legislations and when they became effective. This procedure establishes an effective month for each state that experienced a change of divorce law during my sample period. Figure B.3 maps treated and control states (i.e. never-treated and already-treated).

FIGURE B.3: Treated and control states



Notes: This figure maps U.S. states according to their treatment status.

B.5 Comment on the event study methodology

A growing literature makes use of event studies for treatment effects estimation. In this section I carefully explain how the event study considered in this paper was built. Event studies use variation in the treatment timing to assess the existence of pre-treatment differential trends. As a matter of fact, if such different trends occurred prior to the treatment then the outcome should experience the estimated effect before the unit is treated. Pooling all the treated units together shows whether this happens systematically. If this were the case, it would reduce credibility to my results.

Formally, event studies build a vector composed of dichotomous variables taking value 1 for each of the $t < T$ periods before and after a certain event. The researcher chooses the time window of the dichotomous vector (i.e. total number of periods earlier and after, in this case denoted by T).[5] In other words, each of these variables takes value 1 t periods away from the event: precisely, there is a variable for each of the T periods before the event occurred, and a variable for each of the T periods after. This vector can be written mathematically as $\sum_{t=-T}^{T} i_{st}$, where the first and the last variable (namely, using the same notation i_{s-T} and i_{sT}) take respectively value 1 for each period prior to $-T$ and each period posterior to T. Each of these variables captures whether the effect on the outcome took place at time t. This dichotomous vector replaces the treatment variable in the specification. The specification considered in Section 7.1 is shown in equation (A.1).

[5] Note that in the literature the time window considered before and after the event does not need to have the same length. More generally I could write T_1 as the length of the time window prior to the event and T_2 as the length of the time window posterior to the event.

$$log(1 + Prostitution_{csmy}) = \sum_{t=-3}^{5} \beta_t Unilateral_{sm,y+t} + \alpha_m + \alpha_y + \alpha_c + \alpha_c * y + \varepsilon_{csmy} \tag{B.1}$$

Contrary to a standard DiD, in an event study only treated units are left in the sample. In addition, one of the dichotomous variables is excluded (to avoid collinearity), so such excluded indicator takes value zero by construction and is the benchmark to compare the estimated coefficients. Usually, a dichotomous variable measuring if the treatment had an effect prior to its occurrence (i.e. an i_{st} with $-T \leq t < 0$) is chosen as the excluded indicator on the presumption that there was no effect in the past. In the literature it is common to choose $t = -1$.

Table B.4 explores the robustness of the event study, it presents results of running equation (A.1) with different dependent variables: column (1) uses $\log(1 + y)$ and column (2) uses the IHS transformation. The F-test shows that only estimated coefficients posterior to the entry into force of unilateral divorce laws (i.e. lags) are statistically significantly different from zero, while estimated coefficients prior the entry into force of unilateral divorce laws (i.e. leads) are not.

TABLE B.4: Event study

VARIABLES	(1) Log(1+y)	(2) IHS
3 Years Prior	0.0433	0.0520
	(0.0228)	(0.0278)
2 Years Prior	0.00827	0.0101
	(0.00802)	(0.00969)
0	-0.0236**	-0.0284*
	(0.00869)	(0.0111)
1 Years After	-0.0162*	-0.0185*
	(0.00786)	(0.00852)
2 Years After	-0.00967	-0.0112
	(0.0114)	(0.0138)
3 Years After	-0.0193	-0.0236
	(0.0179)	(0.0212)
4 Years After	-0.0321	-0.0373
	(0.0305)	(0.0354)
5 Years After	-0.0690	-0.0819
	(0.0634)	(0.0743)
Observations	140,016	140,016
Clustered variance at State level	✓	✓
County FE	✓	✓
County Year Trends	✓	✓
Year FE	✓	✓
Month FE	✓	✓

Clustered standard errors at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table displays the estimated coefficients of the event study analysis 4 years prior and posterior to the enter into force of unilateral divorce law (i.e. specification (A.1)). Data is at county-month level. Standard errors are clustered at state level. Each column of the table uses a different dependent variable. Column (1) uses $\log(1+y)$, column (2) uses the IHS transformation. Each column includes county fixed-effects, county-year trends, year fixed-effects and month fixed-effects.

B.6 Comment on potential mechanisms: fight against crime mechanism

B.6.1 Officers

There could be the concern that hired officers do not vary considerably over years and that this lack of variation is driving the results of the police mechanism.

To address this issue, this section considers equation (2) but it makes use of two different transformations of the dependent variable. First, I use the first difference of officers per 1,000 inhabitants. In other words, I use the variation (i.e. increase/decrease) of hired officers normalised by a state's population. Second, I use the growth rate of officers per 1,000 inhabitants. Results are presented in the same fashion as in the police mechanism analysis.

I find no empirical evidence supporting that unilateral divorce correlates with a reduction of officers.

TABLE B.5: Potential mechanisms: fight against crime mechanism

VARIABLES	(1) First Difference Officers	(2) First Difference Officers	(3) First Difference Officers	(4) First Difference Officers	(5) Growth rate Officers	(6) Growth rate Officers	(7) Growth rate Officers	(8) Growth rate Officers
Unilateral	0.00535	-0.00554	-0.00758	-0.0166	-0.000861	-0.00181	-0.00607	-0.00792
	(0.00665)	(0.00753)	(0.00916)	(0.0160)	(0.00418)	(0.00437)	(0.00439)	(0.00748)
Observations	2,150	2,150	1,750	1,750	2,150	2,150	1,750	1,750
Clustered variance at State level	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
State Year Trends		✓		✓		✓		✓
Sample	1971-2016	1971-2016	1980-2014	1980-2014	1971-2016	1971-2016	1980-2014	1980-2014

Clustered standard errors at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table displays the estimated coefficients of running specification (2) for two different transformations of the dependent variable. Data is at state-year level. Standard errors are clustered at state level. Columns (1) to (4) use the dependent variable in levels, columns (5) to (8) use the dependent variable in logs.

B.6.2 Other crimes

This section presents results of running my main specification using as dependent variable each one of the the main categories of offenses recorded by UCR (28 main categories of offenses excluding prostitution).[6] Such offenses are recorded in two panels depending on whether there is evidence in the literature they are connected to prostitution. Namely, Panel A shows offenses not connected to prostitution while Panel B displays offenses connected to prostitution.

There is evidence in the literature (Urban Justice Center, 2005; Dank et al., 2014; Cunningham, DeAngelo, and Tripp, 2017; HG.org, 2017) that prostitution is connected to different crimes. Using such literature I divided offenses in two groups: connected and not connected to prostitution as showed in Table B.6.[7]

Each cell in the column shows the estimated coefficient, and its standard error, associated to unilateral divorce using the corresponding offense in the row as dependent variable transformed according to the corresponding column. In fact, each column shows the results of running the above-mentioned regression with a different functional form of the dependent variable. Columns (1), (2) and(3) respectively use the dependent variable in logs, IHS and levels. Each regression includes month and year fixed effects, county fixed effects and linear trends and variance is clustered at state level.

[6]All the categories are reported in Appendix Section B.2

[7]Two crimes in Panel A could have been in Panel B. First, "total drug abuse" (i.e drugs crimes/use) there is evidence in the literature that both prostitutes and prostitutes' clients make use of drugs. Yet, it is unclear their relative percentage with respect to the whole "drugs market". This is why such regressions' results also appear in the Section 2.9. Second, "vagrancy" there is evidence in the literature that prostitutes arrests are seldom reported as "loitering" (for example, the the New York State Division of Criminal Justice Services classifies "loitering" as including "loitering for prostitution"). Given the close connection between "vagrancy" and "loitering", the former could also be considered as an offense connected to prostitution.

TABLE B.6: Potential mechanisms: fight against crime mechanism

DEPENDENT VARIABLE	(1) Log(1+y)	(2) IHS	(3) Levels
Panel A: Crimes not connected to prostitution			
Robbery	-0.001721	-0.00221	-0.00031
	(0.00836)	(0.0102)	(0.08983)
Burglary	0.08697**	0.10148**	1.81443***
	(0.03777)	(0.04509)	(0.58084)
Larceny	0.03422	0.02712	9.46527*
	(0.08818)	(0.09835)	(4.78697)
Motor Theft	0.02040	0.02336	-0.60396
	(0.02898)	(0.034473)	(1.49761)
Other Assault	-0.04920	-0.05902	0.98405
	(0.09551)	(0.10851)	(4.30007)
Arson	0.00079	0.00079	0.03033
	(0.00734)	(0.00891)	(0.09112)
Forgery	-0.04906	-0.06002	0.39481
	(0.05031)	(0.05987)	(0.64869)
Fraud	-0.24433	-0.27693	-1.49883
	(0.14994)	(0.16957)	(6.56632)
Embezzlement	0.00188	0.00162	0.09943
	(0.03516)	(0.04353)	(0.22858)
Stolen Property	-0.00154	-0.00236	-0.21224
	(0.01479)	(0.01728)	(0.36632)
Vandalism	0.0256	0.0277	1.13909
	(0.0589)	(0.0681)	(1.13533)
Total Drug abuse	-0.0655	-0.0809	-1.02097
	(0.0906)	(0.102)	(6.01042)
Gambling	0.00523	0.00664	-0.05416
	(0.01352)	(0.01642)	(0.15739)
Offences against family and children	-0.27179	-0.32726	-1.91609
	(0.1766)	(0.21361)	(1.65182)
Driving under alcohol influence	-0.33186	-0.38589	-7.97683
	(0.23374)	(0.26046)	(10.0430)
Liquor laws	-0.06766	-0.09378	9.06771
	(0.12086)	(0 .14263)	(10.6131)
Drunkeness	-0.02130	-0.02631	-2.41075
	(0.07916)	(0.09107)	(3.63117)
Disorder Conduct	-0.01541	-0.01903	0.04367
	(0.06861)	(0.07877)	(2.56150)
Vagrancy	-0.04257**	-0.05104**	-0.59007*
	(0.01704)	(0.02017)	(0.33096)
Other Non Traffic Offences	-0.09939	-0.10798	-10.1071
	(0.1476)	(0.16343)	(17.5948)
Suspicion	0.00266	0.00378	-0.03259
	(0.00336)	(0.00387)	(0.15955)
Runaways	-0.14292	-0.16488	-2.64762
	(0.09808)	(0.11373)	(2.17062)
Panel B: Crimes connected to prostitution			
Homicide	-0.00891	-0.01068	-0.16131*
	(0.00541)	(0.00647)	(0.08153)
Rape	-0.00333	-0.00412	0.01808
	(0.00453)	(0.00563)	(0.03788)
Assault	-0.09301*	-0.10923*	-1.24446
	(0.05274)	(0.06289)	(0.81679)
Weapon	-0.02623*	-0.03184*	-0.11522
	(0.01409)	(0.01687)	(0.14296)
Sex Offences	-0.02103	-0.02563	0.0069
	(0.03223)	(0.03965)	(0.27205)
Curfew and Loitering violations	-0.00365	-0.00546	-0.08268
	(0.04229)	(0.04943)	(0.95489)
Observations	1,252,282	1,252,282	1,252,282

Clustered standard errors at state level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

B.7 Comment on demand mechanisms

The demand function considered in Section 2.9 is a simplified version of the original one discussed in Edlund and Korn (2002). In fact in Edlund and Korn (2002), the demand of prostitution is a weighted average of the demand of prostitution by unmarried men and of the demand of prostitution by married men. Both demands are an increasing function of men's earnings. In addition, the demand of prostitution by married men is also a decreasing function of p_m.

As for the former, I run a regression using CPS data where the dependent variable is the average wage of men. The specification has the same structure as the specification shown in equation (5). Yet, I do not find any evidence that unilateral divorce law has any effect on men's earnings (Table B.7). As for the latter, it implies that an increment in p_m could decrease the demand of prostitution by married men as well as reduce the supply of prostitution. In order to study this channel I would need data on the demand of prostitution by married men which I do not have. Hence, it is important to note that finding that unilateral divorce reduces the demand of prostitution by married men would not be inconsistent with the marriage compensation channel.

TABLE B.7: Potential mechanisms: men's wage

VARIABLES	(1) Log Average Men's Real Wage	(2) Average Men's Real Wage
Unilateral	-0.0127	-0.257
	(0.0145)	(0.161)
Observations	20,400	20,400
Clustered variance at State level	✓	✓
State FE	✓	✓
Year FE	✓	✓
Month FE	✓	✓
State Year Trends	✓	✓

Clustered standard errors at state level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: This table displays the estimated coefficients of running the specification described in Section A. Data is at state-month level. Standard errors are clustered at state level. Each column of the table uses a different dependent variable. Column (1) uses average men's real wage in logs, column (2) uses average men's real wage in levels. Each column includes state fixed-effects, state-year trends, year fixed-effects and month fixed-effects.

B.8 Comment on potential mechanisms: marriage compensation mechanism

B.8.1 Comparison group

There could be the concern that the finding that unilateral divorce has a greater impact on arrested prostitutes in marrying-fertile age is due to the choice of using arrested prostitutes in other ages as the comparison group. This latter group is composed of arrested prostitutes either between 17 and 24 years old or strictly older than 49 years old, since the marrying-fertile age group is formed by prostitutes between 25 and 49 years old. The potential concern is that results are driven by the inclusion of prostitutes strictly older than 49 years old that might seem less frequent than their younger counterparts.

To address this issue, this section presents the results of running equation (6) but using arrested prostitutes between 17 and 24 years old only (i.e. arrested prostitutes older than 49 years old are discarded). Using only prostitutes between 17 and 24 years old signifies using only prostitutes in fertile age but too young to get married.

The table below shows the results of running the same analysis as before but for this age group. Findings are qualitatively similar: there is evidence that unilateral divorce law has a larger impact on arrested prostitutes in marrying-fertile age than on arrested prostitutes of other ages. This evidence supports the marriage compensation mechanism.

TABLE B.8: Potential mechanisms: marriage compensation

VARIABLES	(1) Log(1+y) Marrying-Fertile age	(2) IHS Marrying-Fertile age	(3) Log(1+y) 17-24 y.o.	(4) IHS 17-24 y.o.	(5) Log(1+y) Joint regression	(6) IHS Joint regression
Unilateral	-0.0800	-0.0945	-0.0176	-0.0230	-0.0282	-0.0345
	(0.0521)	(0.0615)	(0.0155)	(0.0186)	(0.0232)	(0.0279)
Dummy Marrying -Fertile age					0.0774***	0.0929***
					(0.0163)	(0.0197)
Unilateral*Dummy Marrying-Fertile age					-0.0352**	-0.0422**
					(0.0172)	(0.0208)
Observations	1,252,282	1,252,282	1,252,282	1,252,282	2,504,564	2,504,564
Clustered variance at State level	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
County Year Trends	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓

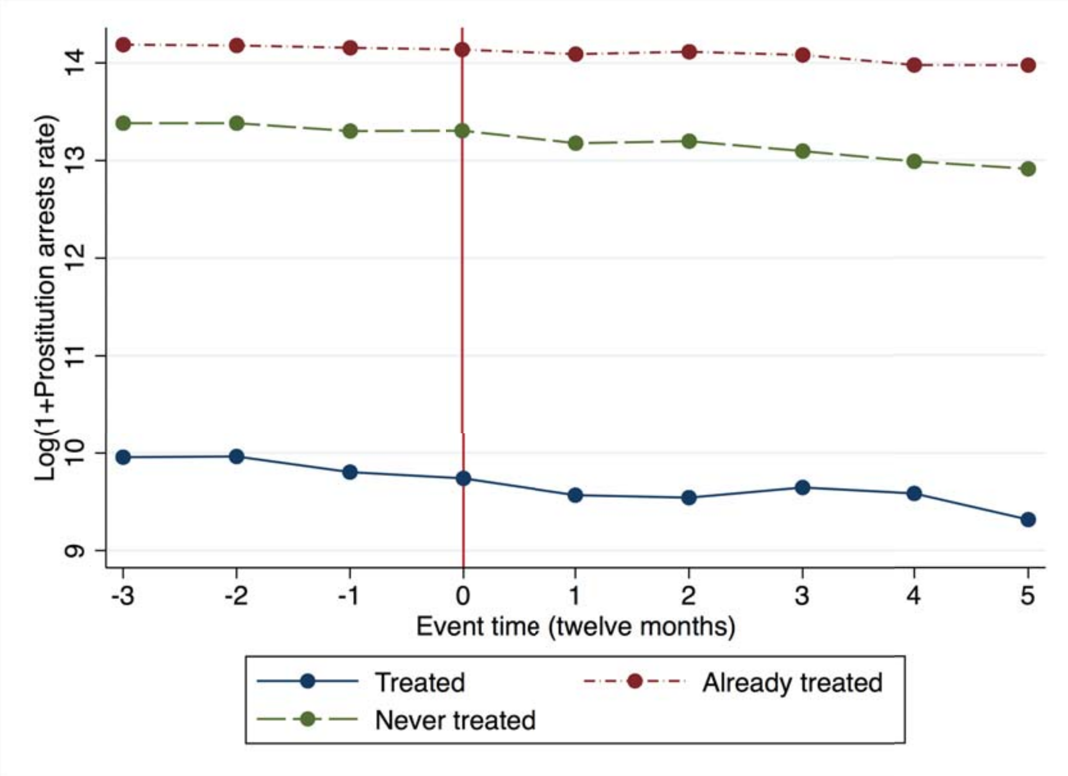
Clustered standard errors at state level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table displays the estimated coefficients of running specification (1) for marrying-fertile age sample and for "17-24 years old" sample . Data is at county-month level. Standard errors are clustered at state level. Each column of the table uses a different dependent variable. Column (1) uses $\log(1+y)$ of the marrying-fertile age group, column (2) uses the IHS transformation of the marrying-fertile age group, column (3) uses $\log(1+y)$ of "17-24 years old" group and column (4) uses the IHS transformation of "17-24 years old" group. Column (5) and (6) show the results of running equation (6).

B.8.2 Parallel trends

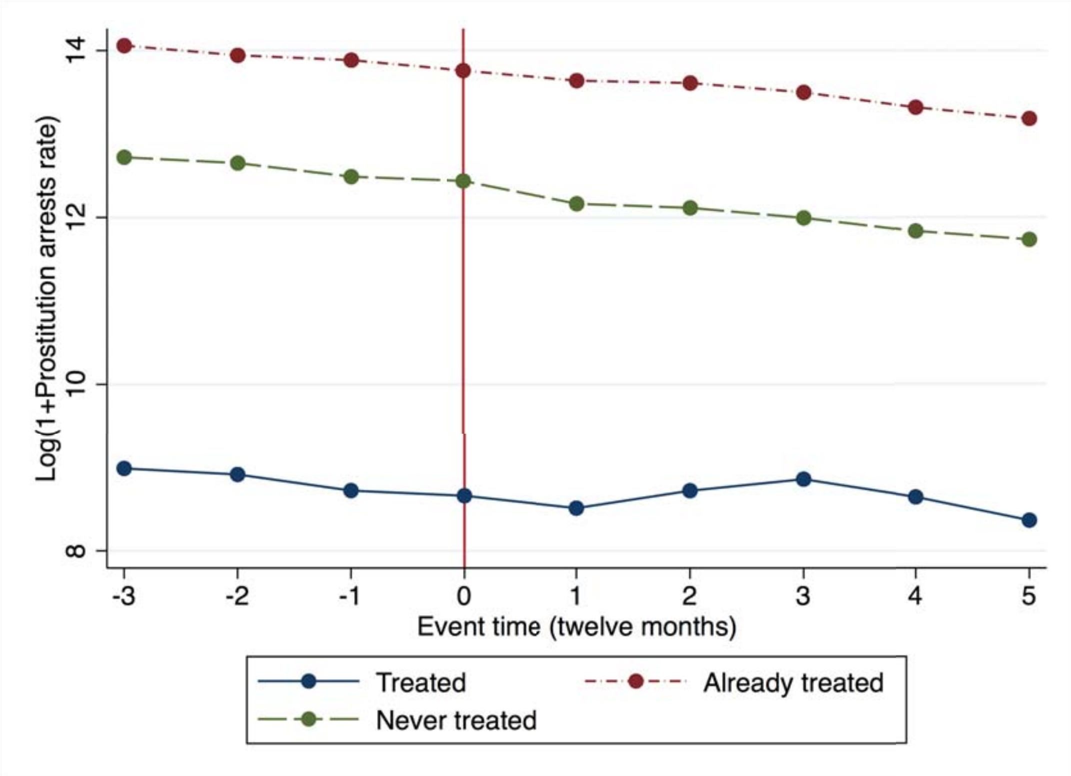
Figure B.4 and B.5 respectively show the trends of treated and control counties for arrested female prostitutes in marrying-fertile age and in other ages. The graph is in the same format than the one for the main regression in Section 2.6. Yet, visual inspection of both graphs does not clarify whether the two age groups exhibit different patterns.

FIGURE B.4: Parallel trends between treated and control groups, marrying-fertile age



Notes: This figure plots the trends of the treated and control groups three years prior and five after the enter into force of unilateral divorce law for the sample in marrying-fertile age. On the horizontal axis there is the event time, each period lasts twelve months (e.g. period 0 comprises the month in which unilateral divorce law becomes effective and eleven moths after that). On the vertical axis there is the average value of the dependent variable in that period of time. The treated group's trend is an average for each treated county. Details on the computations of the control groups' trend can be found in the paper.

FIGURE B.5: Parallel trends between treated and control groups, other ages



Notes: This figure plots the trends of the treated and control groups three years prior and five after the enter into force of unilateral divorce law for the sample in marrying-fertile age. On the horizontal axis there is the event time, each period lasts twelve months (e.g. period 0 comprises the month in which unilateral divorce law becomes effective and eleven moths after that). On the vertical axis there is the average value of the dependent variable in that period of time. The treated group's trend is an average for each treated county. Details on the computations of the control groups' trend can be found in the paper.

B.8.3 Indoor Prostitution

A potential concern could be that female prostitutes in marrying and fertile age became more difficult to arrest for reasons disconnected to their opportunity cost of getting married. As far as I am concerned, there is no clear plausible mechanism that could support this explanation.[8]

CPS data provides information on the occupational code, this allows me to restrict the sample to potential indoor prostitutes. Using the occupational code I can restrict the sample to female respondents working in industrial sectors connected to indoor prostitution. Hence, I get a reasonable proxy for potential indoor prostitutes.[9]

Namely, I consider the following regression model similar to regression model (4):

$$log(1+Indoor\ Prostitution_{smy}) = \beta Unilateral_{smy}+\alpha_m+\alpha_y+\alpha_s+\alpha_s*y+\varepsilon_{smy} \quad \text{(B.2)}$$

[8] Cunningham and Kendall (2011a) hypothesized that "the Internet and other modern technologies are drawing prime-aged (street) prostitutes into indoor work". There could be the concern that this hypothesis is driving my findings. For this to happen, internet needs to be introduced simultaneously to unilateral divorce laws. Using data on indoor prostitutes would shed light on this mechanism too.

[9] In Appendix Section B.9 there is the exact list of the occupational codes used.

where $Indoor\,prostitutes_{smy}$ is the number of women in occupational sectors that contain indoor prostitution businesses per 1,000,000 inhabitants. As in the previous analysis, I split the sample depending on the age of female respondents. In particular, I split the sample in two groups indoor prostitutes in marrying-fertile age and indoor prostitutes of other ages.

Columns (1) and (4) of Table B.9 show the results of running equation (8) for marrying-fertile age and other ages. Results show that unilateral divorce decrease potential indoor prostitutes in marrying-fertile age but dot not affect potential indoor prostitutes in other ages. Columns (2) and (5) report results using IHS, while columns (3) and (6) in levels. Results are stable across functional forms.

TABLE B.9: Potential mechanisms: marriage compensation, CPS data

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(1+y)	IHS	Levels	Log(1+y)	IHS	Levels
VARIABLES	Marrying-fertile age	Marrying-fertile age	Marrying-fertile age	Other ages	Other ages	Other ages
Unilateral	-0.317**	-0.358**	-13.01*	0.105	0.115	9.480
	(0.141)	(0.159)	(7.342)	(0.117)	(0.131)	(8.799)
Observations	20,400	20,400	20,400	20,400	20,400	20,400
Clustered variance at State level	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓
State Year Trends	✓	✓	✓	✓	✓	✓

Clustered standard errors at state level in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Notes: This table displays the estimated coefficients of running specification(8). Data is at state-month level. Standard errors are clustered at state level. Each column of the table uses a different dependent variable. Columns (1), (2) and (3) respectively use number of potential indoor prostitutes in marrying-fertile age in logs, IHS and levels. While, columns (4), (5) and (6) use the same variable but for potential indoor prostitutes in other ages. Each column includes state fixed-effects, state-year trends, year fixed-effects and month fixed-effects.

B.9 Industry sectors used to measure indoor prostitution

In order to measure potential indoor prostitutes I restrict CPS data to the following occupational codes in the table below. The names of the variables are drawn from the monthly extracts of the CPS Uniform database of the Centre of Economic Policy Research (CEPR).[10] In order to code such variables it is useful to use both SIC and NAICS systems.

Specifically I restrict my sample to women working in industry sectors composed by strip-clubs and escort-girls services (i.e. sectors that comprise indoor prostitution establishments). Note that these industry sectors are composed by various occupations, among which there are strip-clubs, massage parlours and escort-girls services. Hence, women in this sample might be working in other occupations too. However, this sample is more likely to be formed by prostitutes. Recall that in the U.S. the prostitution market is highly stratified. Women arrested for prostitution are very likely street prostitutes, who make up the low segment of the market. While, the sample I extract from CPS data is composed by strip-clubs, massage parlours and escort-girls services, who form the medium and high segment of the market. According to the theory, indoor prostitutes are as likely to respond to an increase in p_m as outdoor prostitutes.

TABLE B.10: Occupational codes used

Occupational code	Strip-clubs	Escort services
ind70	798	809
ind80	791	810
ind03, ind09, ind12, ind14	8590	9090
occ70	933	
occ80	469	
occ03, occ11, occ12	4520, 4650	

For variables ind70 and ind80, strip-clubs belong to an occupational sector named "Miscellaneous entertainment and recreative services", while escort services to "Miscellaneous personal services". In the last three variables these names respectively change to "Other amusement, gambling, and recreative services" and "Other personal services". [11] This sample spans from 1980 to 2014. Sectors for variables occ70 and occ80 are labelled as "Personal service occupations, not elsewhere classified". Finally, Sectors for variables occ03, occ11 and occ12 as "Miscellaneous personal appearance workers" and "Personal care and service workers, all other".

B.10 Normalized parallel trends

This section presents the trends of treated and control groups respectively normalised at $t = -1$ to facilitate visual examination of the decrease in prostitution after entry

[10] http://ceprdata.org/cps-uniform-data-extracts/

[11] An example of the SIC code classification is https://www.osha.gov/pls/imis/sic_manual.display?id=267&tab=description

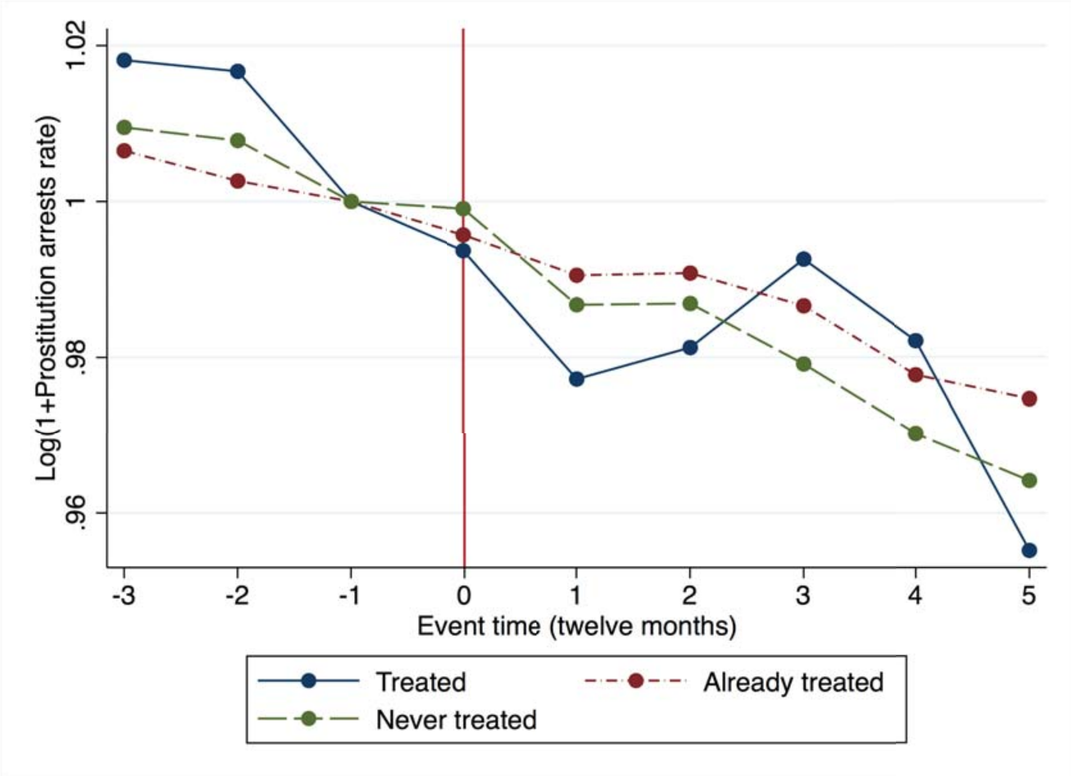
into force of unilateral divorce.[12] In other words, the values of each group are divided by their corresponding value at $t = -1$, setting by construction the latter to 1. As expected, the graph shows that the treated group presents an evident decrease compared to control groups. Such decrease starts at the treatment date (i.e. $t = 0$), peaks at $t = 1$ and then fades away. Unlike the event study, this graph finds that most of the effect takes place between the first and second year of entry into force of unilateral divorce law, while according to the event study the larger share of the decrease took place in the first year right after the entry into force of the law.[13] In addition, the size of the decrease in this figure seems much smaller than the one estimated using regression analysis. Yet, it is difficult and inaccurate to assess the size of the effect by visual inspection of graphs of this sort.

Lastly, it is important to highlight that the parallel trends assumption merely states that treatment and control groups would have had the same trend in absence of treatment. This is carried out by observing the trends of treated and control groups prior to the treatment date (i.e. event time) since we do not observe counterfactual outcomes. However, normalising an event study, such in this case, might be useful to observe the *post-treatment* change in trends among treated and control groups more evidently than in the regular graph. As a matter of fact, it is clearer to assess the common trends of treated and control groups prior to the treatment using the regular graph (i.e. Figure 2.2).

[12] I chose $t = -1$ as in the event study to ease comparison across the two graphs.

[13] Furthermore, in the event study the coefficients after the entry into force of unilateral divorce law were jointly different from zero suggesting that the effect was not temporal, while in this graph the effect disappears after period 2.

FIGURE B.6: Treated and control states



Notes: This figure plots the trends of the treated and control groups three years prior and five after the enter into force of unilateral divorce law. On the horizontal axis there is the event time, each period lasts twelve months (e.g. period 0 comprises the month in which unilateral divorce law becomes effective and eleven moths after that). On the vertical axis there is the average value of the dependent variable in that period of time normalised by the value at $t = -1$. Details on the computations of the control groups' trend can be found in the paper. This figure shows that treated and control groups seem to be on the same trend prior to the enter into force of unilateral divorce law. However, the treated group experiences a slight decay after the introduction of such law (between periods 0 and 2). This evidence is consistent with the identification assumption, with the results of the event study analysis and with former graphs analysed in the article.

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