ON THE ECONOMIC DETERMINANTS OF PROSTITUTION: THE MARRIAGE COMPENSATION AND UNILATERAL DIVORCE IN U.S. STATES*

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Abstract

This paper merges two strands of the literature to study the economic determinants of prostitution. First, there is empirical evidence that unilateral divorce laws improve wives' welfare. Second, the literature has hypothesized that female prostitutes earn high wages as a compensation for forgone marriage market opportunities. Therefore, according to the literature unilateral divorce laws should decrease prostitution as a result of better wives' welfare. I build a unique panel data set 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 difference-in-difference estimation approach, I find that unilateral divorce reduces prostitution. Combining various data sets, I explore several mechanisms that could be driving this negative relationship. In line with the literature, the mechanism that fits best the evidence is one where unilateral divorce improves the option value of getting married by increasing wives' welfare. As a result, the opportunity cost of becoming a female prostitute increases, and the supply of prostitution declines. To the best of my knowledge, this is one of the first papers to show that improving prostitutes' outside option deters prostitution.

Keywords: Prostitution, unilateral divorce, difference-in-difference, marriage compensation
JEL codes: J12, J16, K14, K15, K36

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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 study the effect of a seemingly unrelated policy on prostitution activities, namely, the approval of unilateral divorce laws in several US states, to explore the economic determinants of prostitution. I find that unilateral divorce decreases arrested prostitutes. After considering numerous potential mechanisms my results support that the enforcement of unilateral divorce laws improves wives' welfare, improving as a consequence one of the main economic determinants of prostitution: prostitutes' outside option. Consequently, once prostitution is relatively less attractive, prostitution decreases.

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

[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.

laws reduces the amount of prostitution in equilibrium.

By the same token, there are reasonable 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

[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. Moreover, insofar as my identifying variation-changes of unilateral divorce laws-does not covary with changes in arrests intensity for prostitutes, my results are unaffected by changes to this intensity.

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. 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' wel-

fare) 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); 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, the empirical findings of this paper complement scholarship on the determinants of prostitution and on the relevance of several mechanisms at play in economic models of prostitution. In effect, 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; Moffatt and Peters 2004; Gertler et al. 2005; Levitt and Venkatesh 2007; Arunachalam and Shah 2008; Della Giusta et al. 2009; Edlund et al. 2009; Della Giusta 2010; de la Torre et al. 2010; Cunningham and Kendall 2010, 2011c,a; Gertler and Shah 2011; Islam and Smyth 2012; Cunningham and Kendall 2013; Arunachalam and Shah 2013; Logan and Shah 2013; Shah 2013; Immordino and Russo 2014; Bisschop et al. 2015; Immordino and Russo 2015a,b; Cunningham and Shah 2016; Sohn 2016; Cunningham and Shah 2017; Ciacci and Sviatschi 2016).

In particular, the literature analysed what is known as the *prostitution wage premium puzzle*: prostitution is low-skill, labor intensive, female, and well paid. Scholars have explained this puzzle with supply side hypotheses. On the one hand, Gertler et al. (2005) argue that prostitutes earn a wage premium due to unprotected sex. According to this hypothesis, prostitutes are willing to face the risk of contracting sexually transmitted infections since customers are willing to pay more to avoid using condoms. On the other hand, Della Giusta et al. (2009) claim that this wage premium can be explained by the low reputation that prostitution has and the social stigma it faces. Lastly, Edlund and Korn (2002) suggest that marriage compensation is the key to understanding the *prostitution wage premium puzzle*: marriage market prospects are an important source of income for women, yet by entering into prostitution women compromise such prospects. This paper

[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.

can test this thid hypothesis and find evidence in its favour.[4] In addition, a strand of the literature has focused on analyzing how policy interventions connected to prostitution regulation affects other crimes. For example, Jakobsson and Kotsadam (2013); Cho et al. (2013); Lee and Persson (2015) have studied the link between human trafficking and prostitution, while Ciacci and Sviatschi (2016); Cunningham and Shah (2017); Bisschop et al. (2015) analyze how changes in prostitution policies or business establishments connected to prostitution affect sex crimes. However, to the best of 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, 2005); Alesina and Giuliano (2007); Stevenson and Wolfers (2006, 2007); Stevenson (2008); Wickelgren (2007); Voena (2015)). Yet, none on these papers deals with the effects of these laws on prostitution.

Finally, the results of this paper also contribute to a growing line of the literature in sociology, criminology and economics that studies the effect of changing the opportunity cost of criminals on crime (see, e.g., Raphael and Weiman (2007); Raphael (2010); Beauchamp and Chan (2014); Uggen and Shannon (2014); Cook et al. (2015); Doleac and Hansen (2016); Doleac (2016); Agan and Starr (2017); Schnepel (2017); Yang (2017); Agan and Makowsky (2018); Tuttle (2019)).

The rest of the paper is organized as follows. Section 2 offers a brief overview of the prostitution market in the U.S., while Section 3 discusses the legislative context that led to the enactment of unilateral divorce laws across U.S. states. Section 4 proposes a conceptual framework explaining the main hypothesis tested throughout this paper. Section 5 describes the data sets used in this paper. Section 6 discusses the estimation approach and the main results obtained. Section 7 examines the identification assumption of the regression models. Section 8 tests the robustness of the results. In section 9, I empirically explore the numerous underlying mechanisms that might explain the findings of the paper. Finally, Section 10 concludes.

[4]More specifically, this paper also contributes to a line of research that tests the afore-mentioned mechanisms (Arunachalam and Shah 2008; Cunningham and Kendall 2011b; Immordino and Russo 2015a).

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.[5] 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 (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, 2004a); 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, 2007); Kempadoo et al. (2015)). Hence, it is criminalization of prostitution that worsens 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

[5]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.

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*.[6]

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

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 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.

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

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, 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.

4 Conceptual framework: The link between unilateral divorce and prostitution

This paper can test a specific mechanism that is a by-product of two branches of the literature. The first one studies the effect of unilateral divorce on several outcomes related to wives' welfare. This line of research finds that unilateral divorce has a positive effect on wives' welfare. The second one analyses the determinants of prostitution, namely this line of research explains the *prostitution wage premium puzzle*: prostitution is low-skill, labor intensive, female, and well paid.

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.

Following the literature this party seems to be wives. Indeed, 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, scholars have explained the *prostitution wage premium puzzle* with three supply side hypotheses. First, Gertler et al. (2005) argue that prostitutes earn a wage premium due to unprotected sex. This hypothesis states that prostitutes are willing to face the risk of contracting sexually transmitted infections since customers are eager to pay more to avoid using condoms. Second, Della Giusta et al. (2009) claim that the premium obtained by prostitutes can be explained by the low reputation that prostitution has and the social stigma it faces. Last but not least, Edlund and Korn (2002) defend that choosing to be a prostitute jeopardises one's marriage market prospects. Moreover, according to this paper, being a wife and a prostitute is largely incompatible.[7] As a result, female prostitutes earn high wages, since they are being compensated for forgone marriage opportunities; despite prostitution being low-skill and labour intensive. 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 in order to compensate the opportunity cost of forgone marriage market earnings.

Relying on the previous ideas, this paper suggests a mechanism that connects these two lines of research, in doing so, this mechanism offers an empirical test of Edlund and Korn (2002). 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.[8] 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 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

[7] This claim, as the authors write, "rests on the assumption that men prefer their wives to be faithful (for instance, from a desire to raise biological children)."

[8] 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, a wife could not afford 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.

compensation.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 A.

5 Data description

This section provides information about the data sets used throughout the paper. My econometric 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.

5.1 Arrests for prostitution

Since historical data on the number of female prostitutes is not available, I use the number 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.[9] 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.

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

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 C presents detailed descriptive statistics of this data set.

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.[10]

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 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 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

[10]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.

the problems discussed in Section 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.[11] 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).[12]

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.[13] 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.

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

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

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

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.[14] Replies were classified in multiple categories, among which there was prostitution.[15]

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 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.[16]

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.

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

[15]The question of the survey is: " What are the things you are least proud of as an American?". The answer connected to prostitution states: "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".

[16]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.

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 "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 had and have views both 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.[17]

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 that prostitutes, clients or police officers could move among states depending on their divorce regimes.[18]

[17]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.

[18]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 support-

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. If they had 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 particular, the following regression model is considered here

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

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.[19] 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 0 before the effective month of unilateral divorce and value 1 in the month in which the unilateral divorce law becomes effective and afterwards.[20] 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 0.

Taking the logarithmic transformation of the dependent variable is common in crime

ing this hypothesis.

[19] Arrests of female prostitutes per 1,000,000 inhabitants are 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 in the rest of this paper.

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

economics, mainly because data presents extreme values that may skew the results. In addition, since arrests might take value 0, I use $log(1 + Prostitution_{csmy})$ as the 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 (inclusion of month fixed effects), and that these patterns might differ between counties within the same state (inclusion of county fixed effects and county-year trends).

6.2 Results

Panel A of Table 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 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%.[21] 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.[22] The size of this effect could be compared to Allard and Herbon (2003)'s results, who found that prostitution arrests in 2001 caused an expense of $10.3 million only in the city of Chicago. Therefore, unilateral divorce law would help the U.S. to save around 1.5 times the cost of arrests for prostitutes in Chicago.

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

[22] 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.

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 might be the concern 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 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 changes 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 7) and discussing the robustness of the results (Section 8), I explore thoroughly each one of these mechanisms in Section 9.

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.

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.[23] 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 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 groups 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 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 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.

[23] In Appendix Section E I carefully explain the methodology followed for the event study analysis.

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.[24] 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 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 groups 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 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 A.1 and Figure A.6. This evidence is in

[24] 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).

line with the findings of the event study.

8 Robustness checks

This section deals with the robustness of the results. First, 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.

8.1 Sensitivity to changes in the definition of the dependent variable

There might be the concern that my findings rest 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 $\log\left(y+(y^2+1)^{\frac{1}{2}}\right)$. Panel B of Table 2 shows the results of running the same regression as in Section 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%.[25]

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 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 considers a specification where the dependent variable is in level form (i.e. the number of female prostitutes arrests per

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

1,000,000 inhabitants). Columns (1), (2), (3) and (4) of Panel D of Table 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.

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 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. 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 4 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 becomes effective and in the following months, and 0 before the effective date.[26] As can be inspected in Table 4, the estimated coefficients are not statistically different from the ones of the main regression.[27] This supports that no-fault divorce laws did not play an important role in the reduction of arrested female prostitutes.

Table 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 5 no-fault divorce laws do not appear to cause the reduction in arrested female prostitutes. Indeed, the estimated coefficients in columns (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.

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.[28]

[26] Exactly as the treatment variable (i.e. unilateral divorce law).

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

[28] This section does not explore any mechanism connected with migration. There might be the concern that unilateral divorce laws by making marriage more attractive to women affect the number of women living in a certain state. If this is the case the finding of this paper might be explained simply because the population in treated states increase. Moreover, this hypothesis would violate SUTVA since treatment in a certain state would affect the outcome in a different state. This mechanism seems unlikely because spouses can file a divorce in a different state from the one where they were married as long as one of the spouses

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.[29] 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 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.

meets the residency requirements of that state, so there would not be any incentive to move to a state to get married due to their unilateral divorce law. However, I investigate this issue using, as dependent variable, data on the number of men, women and the sex ratio in each state. Should this be the case, my treatment variable will affect at least one of the three dependent variables listed above. As expected, I find no empirical evidence supporting this hypothesis. Tables are available upon request.

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

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 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.

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 in which unilateral divorce law becomes effective in a certain state, the number of police officers decreases in the majority of counties of such state.[30] 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.[31]

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:

$$Officers_{sy} = \beta Unilateral_{sy} + \alpha_y + \alpha_s + \alpha_s * y + \varepsilon_{sy} \tag{2}$$

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 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.

[30] A possible explanation could be that, contemporaneously to the introduction of unilateral divorce in a certain state, police's budget reduces and so the number of officers decreases.

[31] 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 in a certain state, 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.

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.[32]

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.[33] 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

[32] Appendix Section F.1 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.

[33] 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 et al. 2017; HG.org 2017).

or women's crime behaviour are changing, these crimes would change as well.[34]

Table 7 shows the results of running my main regression using data on women arrested for such crimes. Columns (1), (3) and (5) show the results using as the dependent variable $\log(1+y)$, while columns (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.

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.

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.[35]

9.2.1 Internet searches

The first data set used is drawn from Google trends. Cunningham and Kendall (2010, 2011c, 2013) claim that "overall, online solicitation represents an augmentation of the prostitution market".[36] Indeed, according to these researchers internet allowed prosti-

[34] Appendix Section F.2 presents results for each one of the the main categories of offenses recorded by UCR. A similar approach is used in Ciacci and Sviatschi (2016).

[35] In addition, Appendix Section G explores a supplementary demand mechanism connected to Edlund and Korn (2002).

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

tutes 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 gather data about searches of different words that might be used by prostitutes' potential clients. The frequency with which these words are searched online might proxy the demand of prostitution. 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.[37] The Erotic Review is one of the most important websites that matches prostitutes and clients in the U.S.[38] 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:

$$Searches_{smy} = \beta Unilateral_{smy} + \alpha_m + \alpha_y + \alpha_s + \alpha_s * y + \varepsilon_{smy} \tag{3}$$

Where, $Searches_{smy}$ stands for the number of searches of a certain word in state s, month m and year y; α_m, α_y and α_s are respectively month, year and state fixed effects; and $\alpha_s * y$ is a state-year linear trend. 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 8 displays the estimated coefficients after running such regressions for the largest sample I have (i.e. 2004 to 2017). While, Table 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.[39] 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.

[37] 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).

[38] This website has been used in the literature to collect data on prostitutes and customers (see, among others, Cunningham and Shah (2017)).

[39] 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. A list of missing state/s for each word is available upon request.

9.2.2 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 it is possible to study how observable characteristics of divorced people change after their divorce.[40]

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:

$$Dislike\,Prostitution_{iw} = \beta_1 divorced_{iw} + \beta_2 divorced_{iw} * male_i + X_{iw}\delta + \alpha_i + \alpha_w + \varepsilon_{iw} \quad (4)$$

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 prostitution more than married men, β_2 would be positive.

Column (1) of Table 10 shows the results of regression model (4). 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 10 pools together respondents whose marital status is divorced or separated. Column (1) considers only respondents who said that were divorced, while

[40] Similarly, the same data set has been used by Edlund and Pande (2002) to show that, after getting divorced, women are more likely to support left-wing parties.

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 right after 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.[41] Consequently, as a further check, the last two columns of Table 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; 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 10 show that both β_1 and β_2 are again not statistically different from zero.

9.2.3 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:

$$Unmarried\,men_{smy} = \beta Unilateral_{smy} + \alpha_m + \alpha_y + \alpha_s + \alpha_s * y + \varepsilon_{smy} \tag{5}$$

where $Unmarried\,men_{smy}$ is either the number of unmarried men per 1,000,000 inhabitants in state s, month m and year y or its growth rate. The other variables follow the same notation of regression model (3). Column (1) and (2) of Table 11 respectively show the results using as dependent variable the number of unmarried men per 1,000,000 and its growth rate.[42] Column (3) shows the results for the logarithmic transformation of the

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

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

number of unmarried men per 1,000,000. As Table 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.[43]

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.

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, there are two supply mechanisms suggested by Edlund and Korn (2002): wives' wage and marriage compensation. In this section I test both of them.

9.3.1 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.[44]

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. I run the following

[43] 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.

[44] An alternative mechanism, not supported by the literature, is that unilateral divorce increases women's wages (not only wives' wages). This increment could in turn decrease prostitution insofar as legal jobs become more attractive to women and deter them from prostitution. I have explored this hypothesis as well and found no evidence in its favour. Tables are available upon request.

specification:

$$W_{smy} = \beta Unilateral_{smy} + \alpha_m + \alpha_y + \alpha_s + \alpha_s * y + \varepsilon_{smy} \tag{6}$$

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 (3) and (5).

Column (1) of Table 12 shows the result of this specification using as dependent variable wives' average real wage in logs, while column (2) reports results for wages in levels.

Table 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.[45]

9.3.2 Marriage compensation

As discussed in Section 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. In my sample period the median marriage age in the U.S. for women is 24.8 years old.[46] In addition, Alesina and Giuliano (2007) studied the effect of unilateral divorce on fertility and used 49 years 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.[47]

[45]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.

[46]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.

[47]The relative size of the two samples is fairly balanced since around 60% of my sample falls in the

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.[48] 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 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 not statistically significant in either regression, equation (6) presents a regression model that separates the number of arrested prostitutes according to the two previously defined age groups.

$$log(1+Prostitution_{acsmy}) = \beta_1 Unilateral_{smy} + \beta_2 \alpha_a * Unilateral_{smy} + \alpha_a + \alpha_m + \alpha_y + \alpha_c + \alpha_c * y + \varepsilon_{acsmy} \tag{7}$$

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.

marrying-fertile age range (Table A.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 et al. (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.

[48] 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.

Columns (5) and (6) run this regression model respectively using $\log(1+y)$ and the IHS transformation as dependent variable. 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.[49] In addition, Appendix Section H.3 replicates this analysis for indoor prostitution. Results do not change. 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.

As a double-check I run the same event study as in Section 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 4 and 5 respectively show the results of the event study for arrested female prostitutes in marrying-fertile age and in other ages. As Figure 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.[50] 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.[51] Overall, this evidence provides additional support on unilateral divorce law increasing p_m.

[49] 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 H.1 replicates the analysis using only arrested prostitutes between 17 and 24 years old in the comparison group. Results do not change.

[50] 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.

[51] Likewise, in Appendix Section H.2 I compare the parallel trends graphs of the two restricted samples (i.e. marrying-fertile age vs other ages).

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.

10 Concluding remarks

This paper empirically explores the economic determinants of female prostitution using a quasi-natural experiment setting provided by differences in the timing of entry into force of unilateral divorce laws across U.S. states. Female prostitution is proxied by the arrests 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%. 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 of 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.

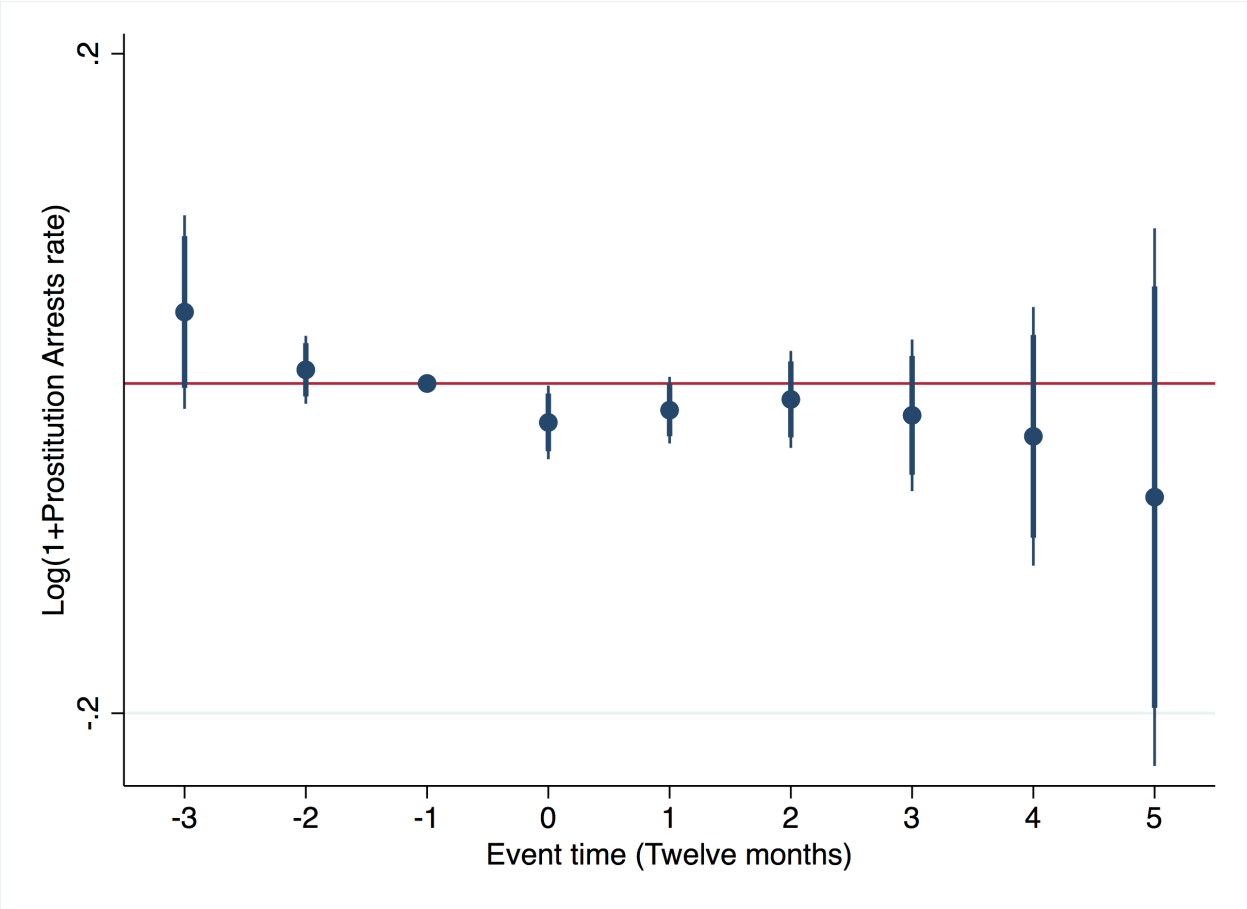
Next, I consider each of the underlying channels that could be driving the results. The explored mechanisms range from changes in police officers' effectiveness in fighting crimes to shifts in the demand and supply of prostitution. To identify the latter, I rely on the well-known model of the link between 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 web 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 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.

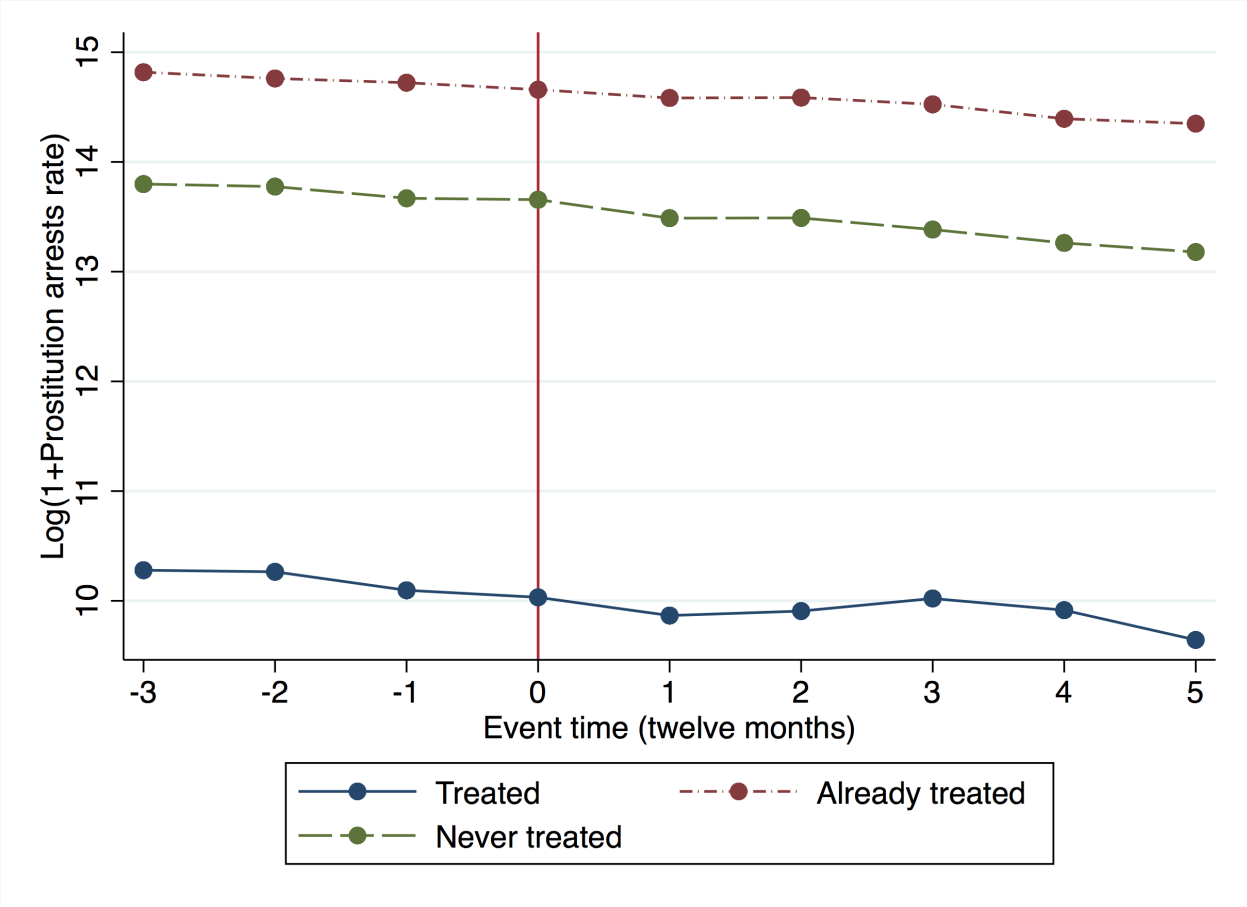
Figures & Tables

Figure 1: Event study



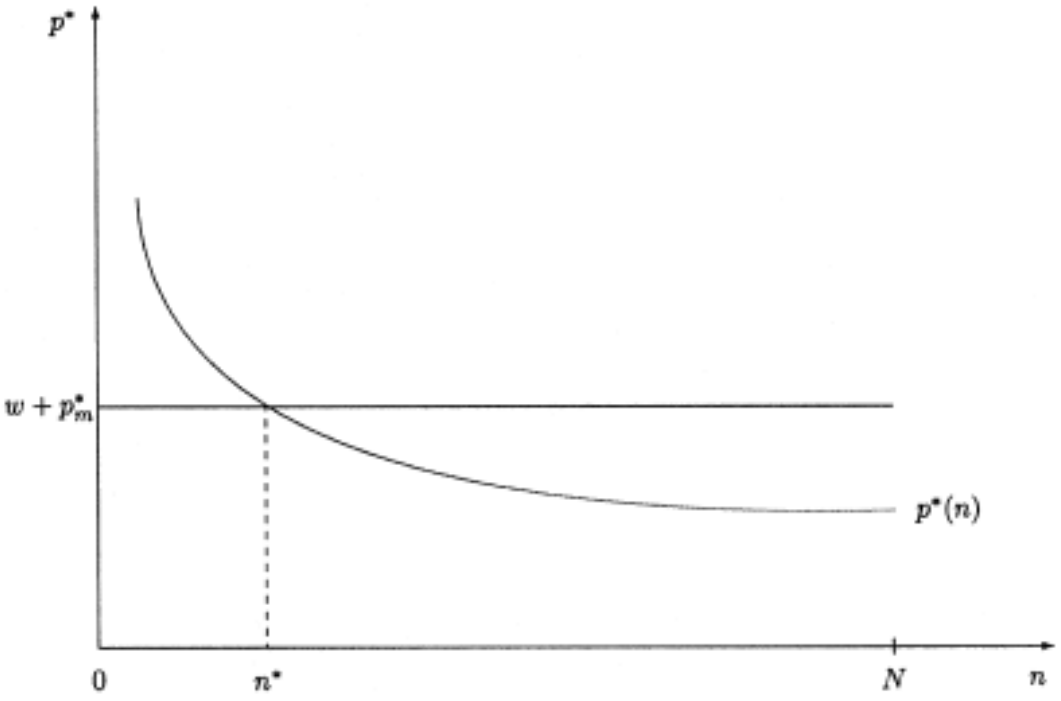
Notes: This figure plots the estimated coefficients of the event study analysis three years prior and five posterior to the enter into force of unilateral divorce law. On the horizontal axis there is the event time, each period lasts twelve months (e.g. period 0 comprises the month in which unilateral 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: Parallel trends between treated and control groups



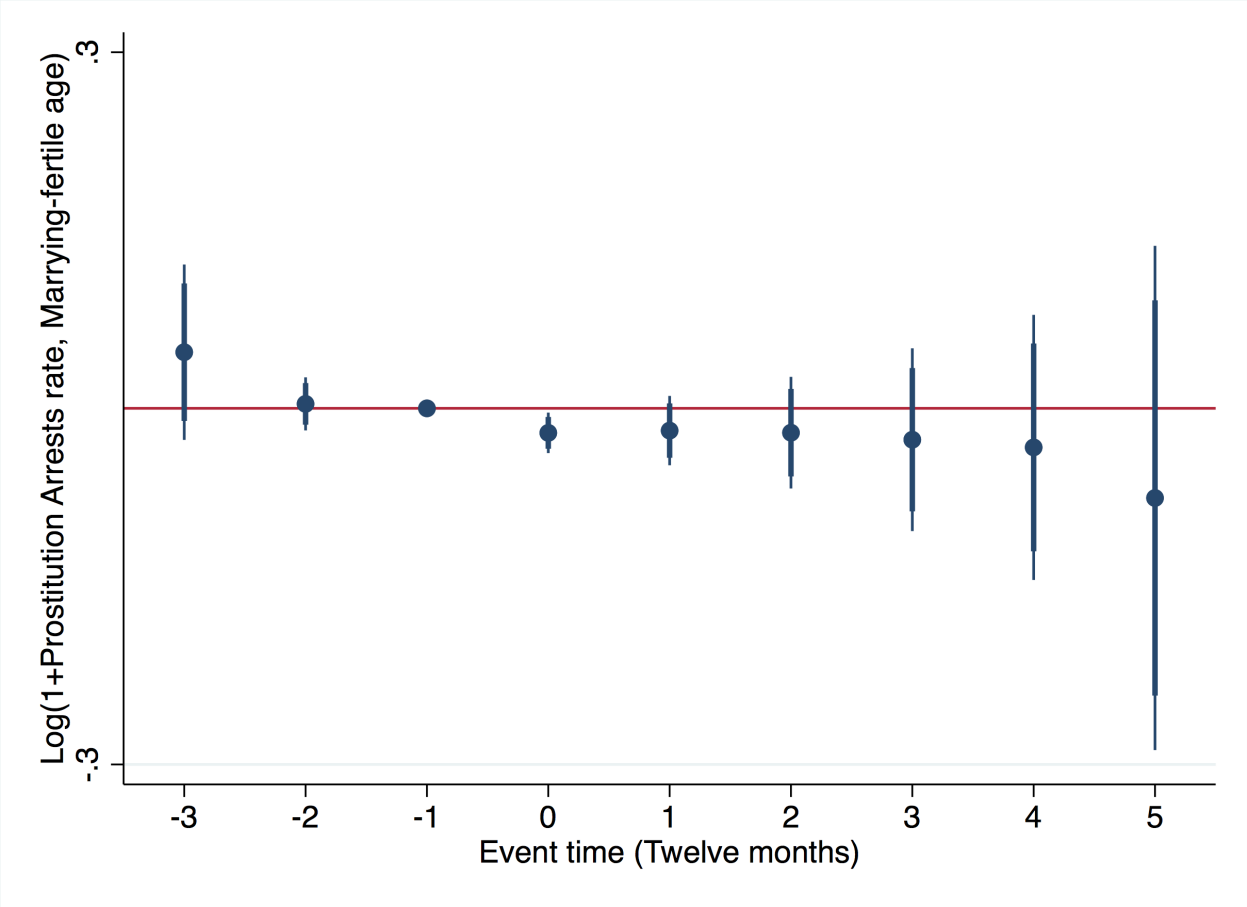
Notes: This figure plots the trends of the treated and control groups three years prior and five after the enter into force of unilateral divorce law. On the horizontal axis there is the event time, each period lasts twelve months (e.g. period 0 comprises the month in which unilateral divorce law becomes 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 3: Marriage and prostitution market equilibrium



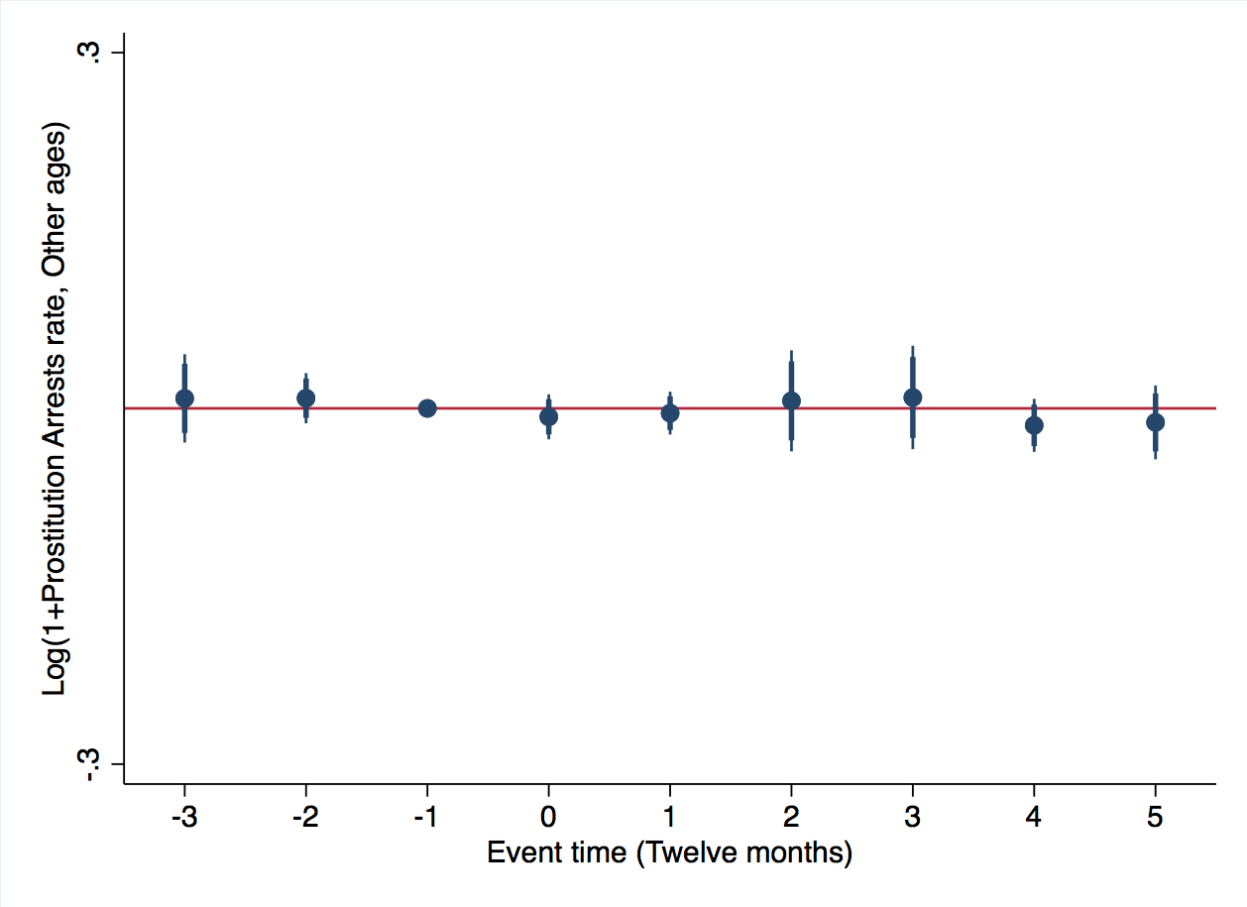
Source: Edlund and Korn (2002).

Figure 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 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 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: 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 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 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				✓

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) 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 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 6: Potential mechanisms: fight against crime mechanism

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Officers	Officers	Officers	Officers	Log Officers	Log Officers	Log Officers	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 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 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: IHS																
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 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: IHS																
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 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 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 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 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 (7) 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).

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Appendix

A Nature of the effect: Inflow vs Stock

Figure A.1 shows the effect of unilateral divorce on prostitution across age groups.[52]. Likewise the main regression, the dependent variable is in logs and each regression includes county, year and month fixed effects, county-year trends and variance is clustered at state level.

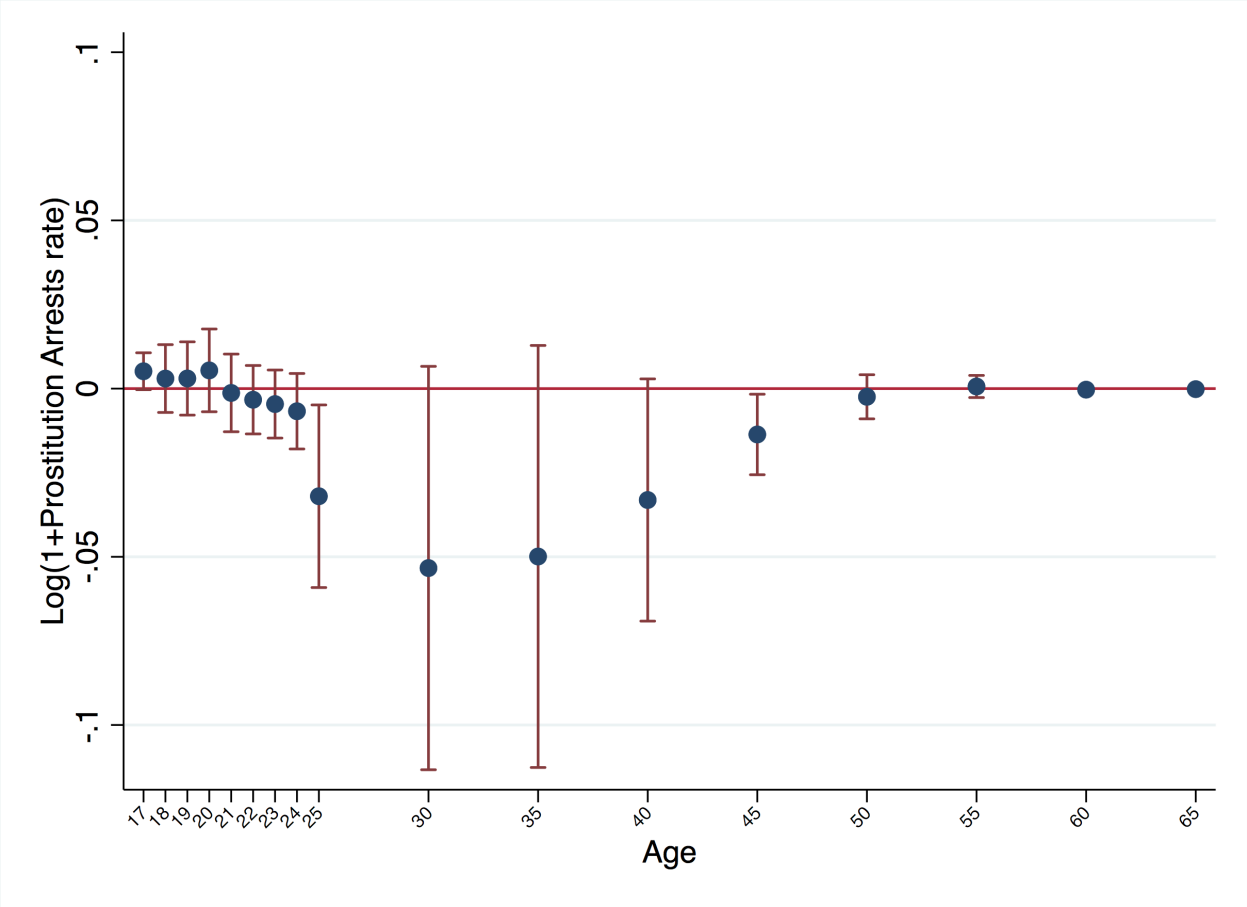
There are two ways in which unilateral divorce could affect prostitution: either by preventing women to become prostitutes (i.e. inflow effect) or by affecting prostitutes who are already inside the market (i.e. stock effect). If unilateral divorce decreases young (old) prostitutes' arrests it would support the former (latter) effect. Figure A.1 finds that unilateral divorce mainly reduces prostitution between 25 and 29 years old and prostitution between 45 and 49 years old.[53] Hence, there is evidence in favour of both effects.

In addition, Figure A.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.

[52] Age groups are classified according to UCR database as in Table A.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.

[53] There could be the concern that there is no effect in 17-24 age group since data is not pooled. Yet, Section H.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 A.1: The effect of unilateral divorce on prostitution across age groups



Notes: This figure shows the effect of unilateral divorce on prostitution across age groups. Each coefficient and standard errors come from a regression, with the same structure of the main specification, where the dependent variable was computed using the age group indicated. Confidence intervals at 90% level. These results suggest unilateral divorce both prevents women from entering prostitution and affects women who are already prostitutes.

B List of crimes in UCR data set

Table A.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

C Further information on the data set

C.1 Descriptive statistics

Table A.2 displays summary statistics for arrests of female prostitutes per 1,000,0000 inhabitants across treated and control states.[54] Data is at county-month level and treated states are disaggregated at pre and post treatment level.

Table A.2: Summary statistics

	(1)	(2)	(3)	(4)	(5)
	Never-treated	Always-treated		Treated	
Arrests of female prostitutes per 1,000,000 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 A.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.[55]

[54] 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.

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

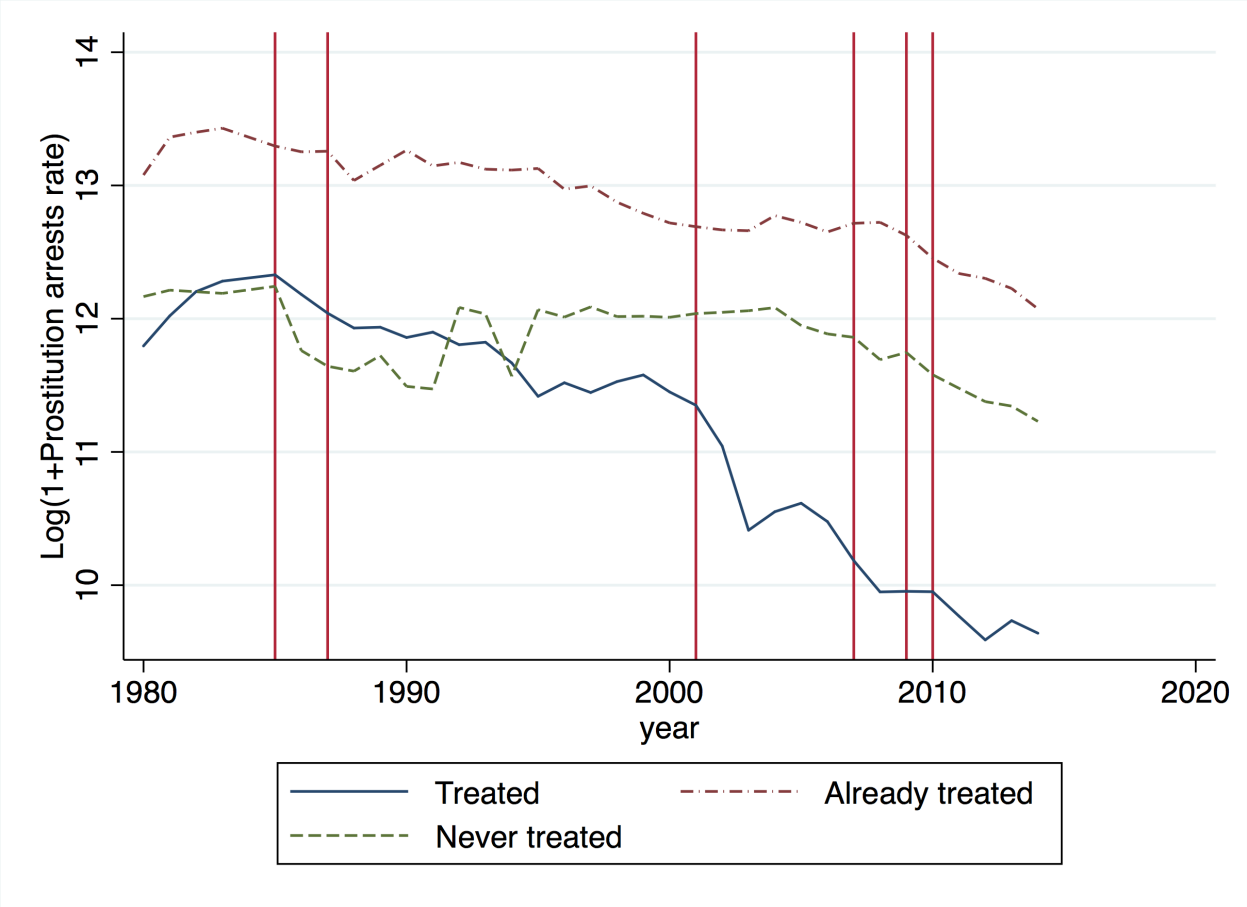
Table A.3: Summary statistics

	(1)	(2)	(3)	(4)	(5)
Arrests of female prostitutes per 1,000,000 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 A.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.

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

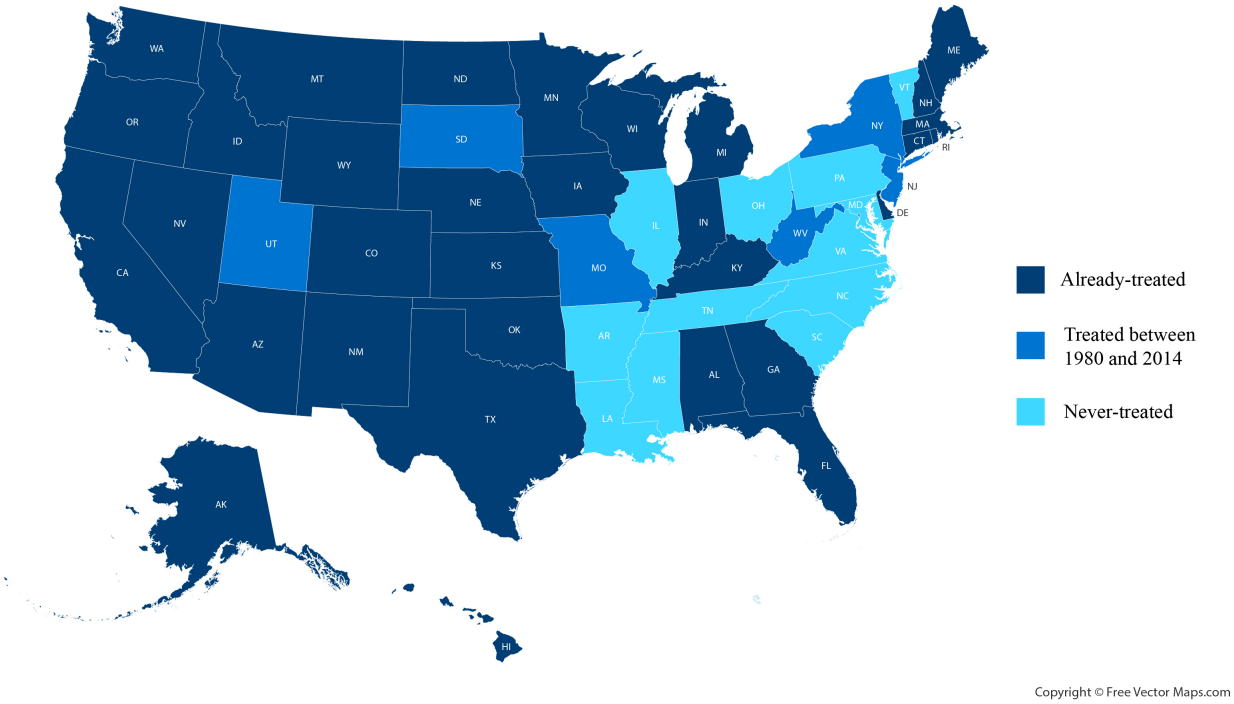


Notes: This figure plots arrests of female prostitutes per 1,000,0000 inhabitants, in the same logarithmic transformation as the dependent variable, for the three groups of states analyzed in the study: treated, never-treated and already-treated. Vertical lines represents the year in which unilateral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

D 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 A.3 maps treated and control states (i.e. never-treated and already-treated).

Figure A.3: Treated and control states



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

E 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).[56] In other words, each of these variables takes value

[56]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

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).

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

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 A.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.

and T_2 as the length of the time window posterior to the event.

Table A.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.

F Comment on potential mechanisms: fight against crime mechanism

F.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 A.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.

F.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 cate-

gories of offenses excluding prostitution).[57] 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 et al. 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 A.6.[58]

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.

[57] All the categories are reported in Appendix Section B

[58] 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 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 A.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

Notes: This table displays the estimated coefficients of running specification (1) for each crime of my data set (row) and functional form of the dependent variable (column).

G Comment on demand mechanisms

The demand function considered in Section 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 A.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 A.7: Potential mechanisms: men's wage

	(1)	(2)
	Log	
VARIABLES	Average Men's Real Wage	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 (6) for men. 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.

H Comment on potential mechanisms: marriage compensation mechanism

H.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 A.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

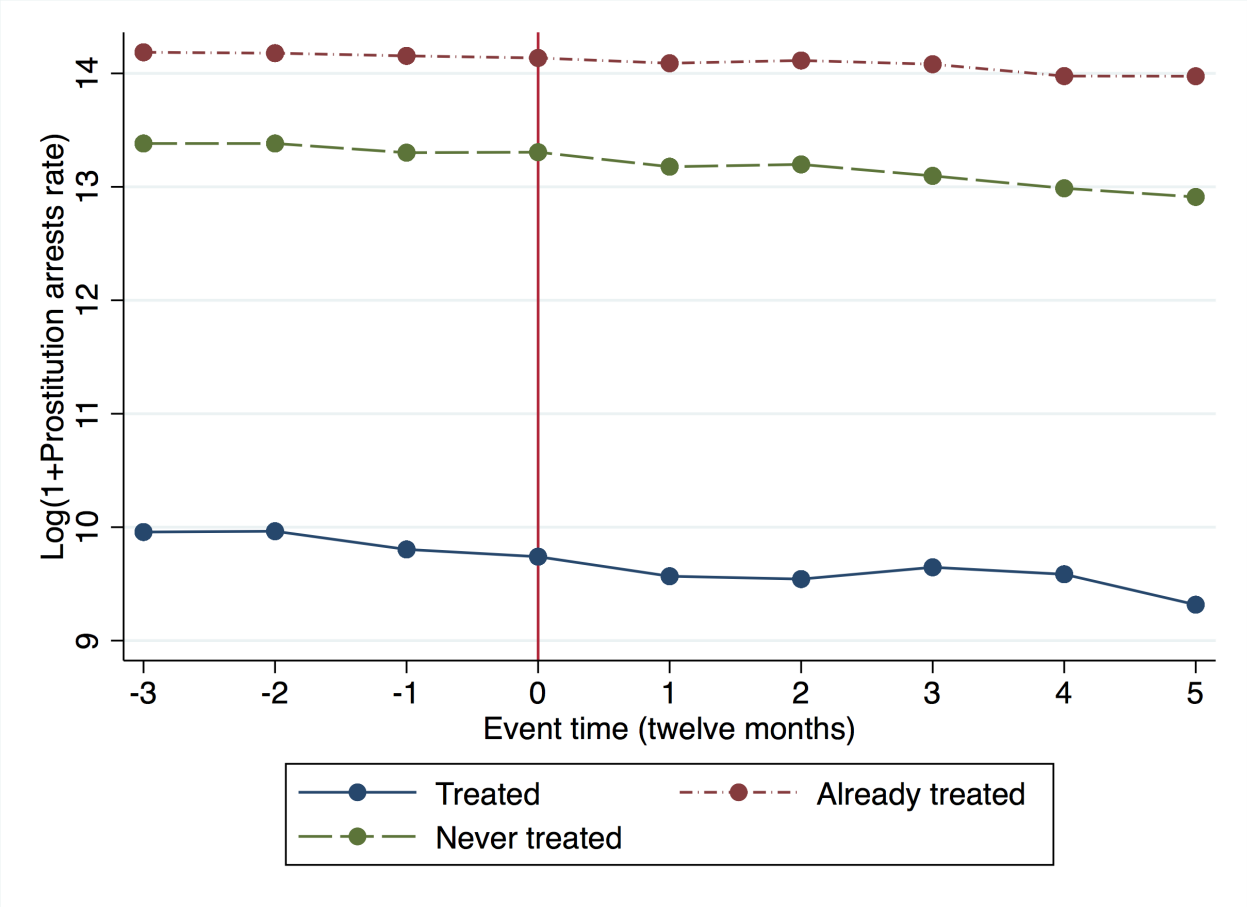
Notes: This table displays the estimated coefficients of running specification (7) for marrying-fertile age sample and for "17-24 years old" sample . Data is at county-month level. Standard errors are clustered at state level. Each column of the table uses a different dependent variable. Column (1) uses $\log(1+y)$ 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).

H.2 Parallel trends

Figure A.4 and A.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 6. Yet, visual inspection of

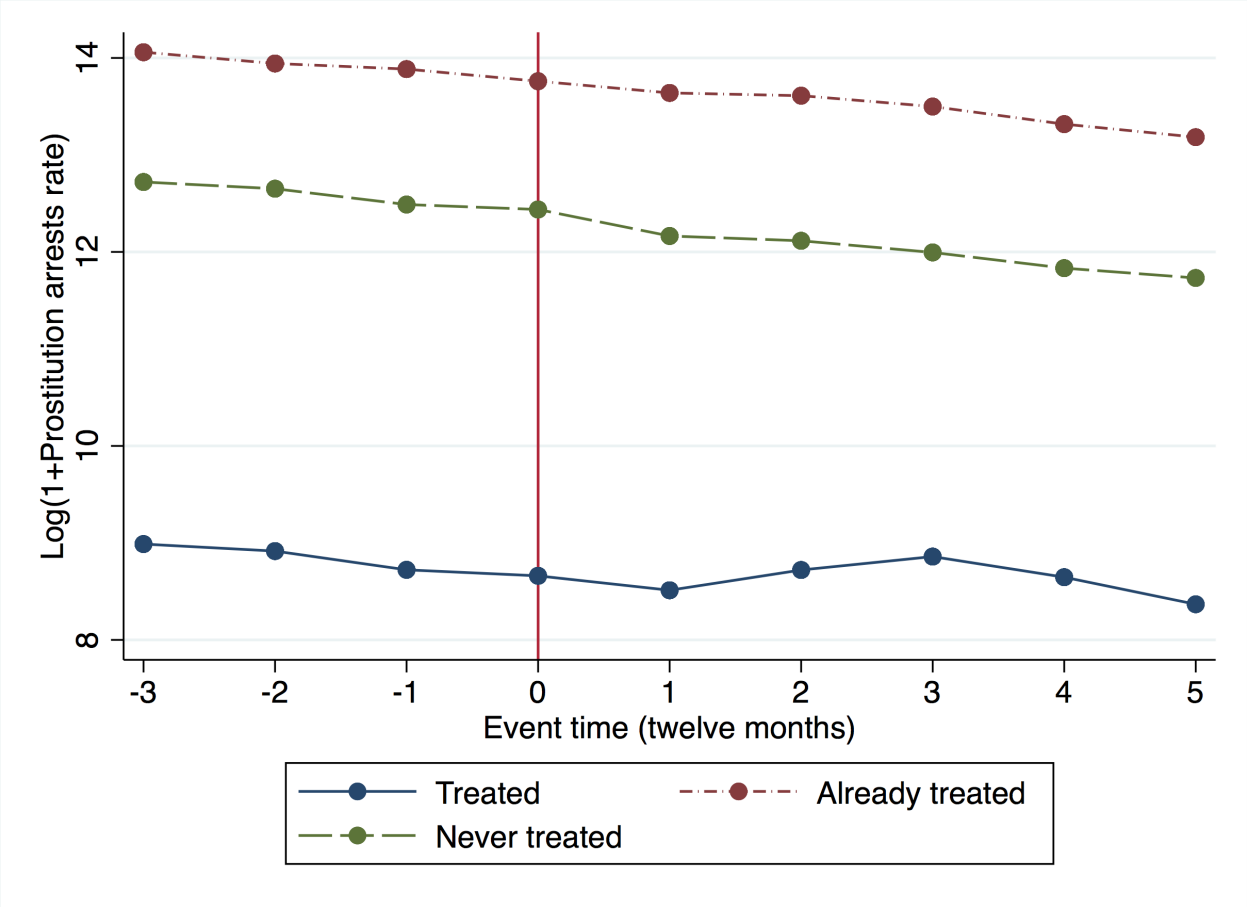
both graphs does not clarify whether the two age groups exhibit different patterns.

Figure A.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 A.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.

H.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.[59]

[59] 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.

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.[60]

Namely, I consider the following regression model similar to regression model (4):

$$log(1 + Indoor\ Prostitution_{smy}) = \beta Unilateral_{smy} + \alpha_m + \alpha_y + \alpha_s + \alpha_s * y + \varepsilon_{smy} \quad \text{(A.2)}$$

where $Indoor\ prostitutes_{smy}$ is the number of women in occupational sectors that contain indoor prostitution businesses per 1,000,000 inhabitants in state s, month m and year y; α_m, α_y and α_s are respectively month, year and state fixed effects; and $\alpha_s * y$ are state-year linear trends. 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 A.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.

[60] In Appendix Section I there is the exact list of the occupational codes used.

Table A.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 (A.2). 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.

I 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).[61] 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 A.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". [62] This sample spans from 1980 to 2014. Sectors for variables occ70 and occ80 are labelled

[61] http://ceprdata.org/cps-uniform-data-extracts/

[62] An example of the SIC code classification is `https://www.osha.gov/pls/imis/sic_manual.display?id=267&tab=description`

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".

J Normalized parallel trends

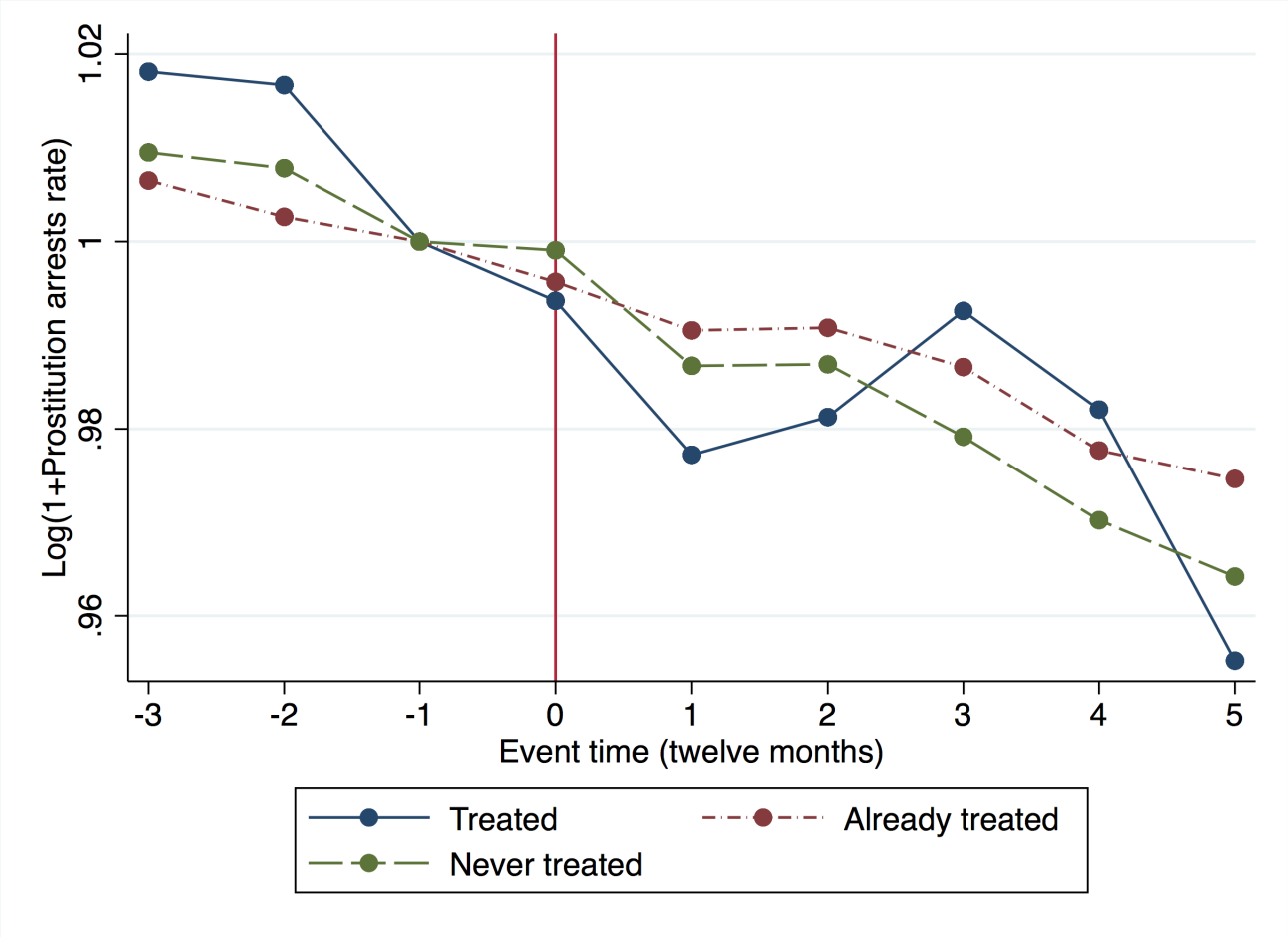
This section presents the trends of treated and control groups respectively normalised at $t = -1$ to facilitate visual examination of the decrease in prostitution after entry into force of unilateral divorce.[63] In other words, the values of each group are divided by their corresponding value at $t = -1$, 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.[64] 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).

[63] I chose $t = -1$ as in the event study to ease comparison across the two graphs.

[64] 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 A.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.