# Do Prostitution Laws Affect Rape Rates? Evidence from Europe

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**Abstract**

We identify a causal effect of the liberalization and prohibition of commercial sex on rape rates, using staggered legislative changes in European countries. Liberalizing prostitution leads to a significant decrease in rape rates, while prohibiting it leads to a significant increase. The results are stronger when rape is less severely underreported and when it is more difficult for men to obtain sex via marriage or partnership. We also provide the first evidence for the asymmetric effect of prostitution regulation on rape rates: the magnitude of prostitution prohibition is much larger than that of prostitution liberalization. Placebo tests show that prostitution laws have no impact on nonsexual crimes. Overall, our results indicate that prostitution is a substitute for sexual violence and that the recent global trend of prohibiting commercial sex (especially the Nordic model) could have the unforeseen consequence of proliferating sexual violence.

> If you expel prostitutes from society, you will unsettle everything on account of lusts. (St. Augustine, in Richards 1995, p. 118)[1]

## 1. Introduction

Prostitution has long been controversial. The US government defines it as "inherently harmful and dehumanizing" (US Department of State, 2007, p. 27). The radical feminist movement maintains that prostitution, the result of an existing patriarchal societal order, is a synonym for exploitation and male domination (Weitzer 2005; see also Barry 1995; Farley 2004; Jeffreys 1997; MacKinnon

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[1] Augustine of Hippo, Christian church father, philosopher, and saint, wrote this in *De Ordine* (2.4.12). Although this translation is often quoted, a variant is "If you do away with harlots, the world will be convulsed with lust" (Aquinas [1485] 1947, 2-2.10.11.resp.).

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1989). In 1949, the United Nations (UN) asserted that prostitution "endanger[s] the welfare of the individual, the family and the community" (Convention for the Suppression of the Traffic in Persons and of the Exploitation of the Prostitution of Others, December 2, 1949, 96 U.N.T.S. 271). In opposition, many international organizations[2] have called for the liberalization of prostitution with the intention of preventing the industry from going underground, creating a safer environment, and reducing the spread of sexually transmitted infections (STIs) (Amnesty International 2015). Despite these debates, the effects of prohibiting and liberalizing commercial sex on society are understudied. In this paper, we shed light on this issue from the perspective of sex crimes.

We expect prostitution to affect rape via the substitution mechanism. We expect that liberalizing prostitution will reduce rape rates by providing greater access to consensual sex, improving the affordability of commercial sex services, and increasing the safety of contractual sex. In contrast, banning commercial sex induces its scarcity, pushes suppliers into a black market with human trafficking, drives costs up, and imposes higher health, safety, and social risks, which decreases its attractiveness as an option for consensual sex and may make it more likely that men will instead engage in sexual violence.[3]

Our tests exploit the staggered legal changes in prostitution laws in 31 European countries from 1990 to 2017. This empirical setting is appealing for four reasons. First, the driving forces behind governments' decisions to alter their prostitution policies are either to provide safer working conditions and reduce the spread of STIs (liberalizing sex work) or to enhance gender equality and social moral standards (prohibiting commercial sex). These legislative changes are not introduced to affect rape rates; thus, any effect on rape rates is likely to be an unintended consequence. Second, these staggered legal changes enable us to identify their effect on rape rates in a difference-in-differences framework (Bertrand, Duflo, and Mullainathan 2004). Because multiple shocks affect different countries at different times, we can avoid a common identification challenge faced by studies with a single shock: the potential noise coinciding with the shock that directly affects rape rates (Roberts and Whited 2013). Third, during our sample period eight countries liberalized prostitution, while six prohibited it. This allows us to examine both the treatment and the reverse treatment effects. Finally, European judicial definitions of rape are similar, and rape statistics have high conformity levels (Harrendorf 2012), which enables us to better identify the causal effects of commercial sex in a cross-country setting.

We find that liberalizing prostitution leads to a significant decrease in rape rates, whereas prohibiting it significantly increases them. This effect is asymmet-

[2] Among the groups are the World Health Organization, Amnesty International, the Joint United Nations Programme on HIV/AIDS, Human Rights Watch, and the United Nations Population Fund.

[3] We are not suggesting that rape is on average less costly to men than purchasing banned commercial sex. The criminal penalties for these crimes are similar, however. After the cost of obtaining commercial sex increases because of prostitution bans, the difference between the costs of rape and commercial sex becomes smaller. Thus, a man who sees little difference between rape and commercial sex will be more likely to rape someone.

ric: the magnitude of prohibiting commercial sex is about four times as large as that of liberalizing it. On average, countries that liberalize commercial sex experience a decline in the rape rate by approximately three cases per 100,000 people relative to countries with no legal changes in prostitution. In contrast, countries that prohibit prostitution experience an increase in the rape rate by approximately 11 cases per 100,000 people relative to countries with no legal changes in prostitution. These results are notable considering that the sample average rate is nine rapes per 100,000 people.

The identifying assumption central to a causal interpretation of the difference-in-differences specification is that the treated and control groups have parallel trends prior to a country's policy change. We show that the pretreatment trends of the two groups are indeed indistinguishable and that most of the impact of prostitution laws on rape occurs after the change, which suggests a causal effect.

Furthermore, we implement triple-differences tests to examine two possible sources of heterogeneity in the treatment effect. First, considering that rapes are likely to be underreported, we provide evidence that the effect of prostitution laws on rape rates is stronger when rapes are more likely to be reported. Second, one way for men to obtain sex is via marriage or partnership. We show that the effect of prostitution laws on rape rates is more pronounced when it is more difficult for men to obtain sex via marriage or partnership.

Next, prostitution policy models can be further classified under one of the following groups, from the most relaxed to the most restrictive: decriminalization (including abolitionism and new abolitionism), legalization, criminalization, and the Nordic model. We separately examine the effects of these prostitution models on rape rates. Among the prostitution liberalization models, decriminalization (in particular abolitionism) has a stronger effect on reducing rape than legalization does. Among the prostitution prohibition models, the Nordic model has a stronger effect on increasing rape than criminalization does.

We perform a number of robustness checks on our main findings. First, a placebo test shows that a change in a country's prostitution laws has no effect on nonsexual serious crimes (such as homicide, burglary, and robbery). This result indicates that the observed relationship between prostitution laws and rape rates is unlikely due to some confounding legal change that affects a country's levels of other overall criminal activities. Second, we match each country liberalizing prostitution to a country prohibiting it. Our main inference is unchanged according to this matched-sample analysis. Finally, we provide evidence that our main inference is largely unchanged after addressing the potential bias associated with the relatively small number of countries and the heterogeneity in the timing of treatments.

Several single-country studies in the recent economic literature indicate that prostitution decriminalization helps reduce rape and that criminalization tends to increase rape. For example, Ciacci and Sviatschi (2016) find that opening indoor prostitution venues in New York City is associated with a decrease in coercive sexual assaults. Nguyen (2016) shows that lowering the entry barriers to

massage parlors in California is associated with a significant decrease in local rape offenses. Bisschop, Kastoryano, and van der Klaauw (2017) find that opening legal street prostitution zones in 25 Dutch cities is associated with a decrease in sexual assaults. Cunningham and Shah (2018) demonstrate that Rhode Island's rape rate fell after the liberalization of indoor prostitution and that the rape rate slightly increased after Rhode Island recriminalized it. Ciacci (2020) provides evidence that Sweden's implementation of the Nordic model led to an increase in rape. Similarly, Backus and Nguyen (2021) show that the 2015 criminalization of sex work in Northern Ireland increased violence against women.

Our paper complements these studies in the following three ways. First, by examining the legal changes on not only prostitution liberalization but also prostitution prohibition, we provide the first evidence of the asymmetric effect of prostitution regulation on rape: the impact of prohibition is much larger than that of liberalization. This asymmetry implies that our previous understanding of the relationship between prostitution and sex crimes is incomplete. These findings for prostitution prohibition are particularly timely and relevant considering the recent trend of prohibiting commercial sex globally. Although this antiprostitution trend is aimed at enhancing gender equality and reducing human trafficking for sexual exploitation, we provide evidence that commercial sex bans could have the severe unforeseen consequence of proliferating sexual violence. Our findings have important policy implications, because sexual violence causes long-lasting harm to its victims and imposes weighty economic burdens on society.[4]

Second, previous studies focus on a single country's example of a statutory change. However, a country-specific setup forgoes important country-level characteristics, such as gender norms, the marriage rate, and sex imbalances. With a sample of 31 European countries, our cross-country approach enables us to provide insights on how the differences in countries' social environments influence the effects of prostitution regulation on rape rates.

Third, considering that prostitution regulation models vary, a cross-country framework allows us to gain a deeper understanding of their heterogeneous effects. For example, we provide the first empirical evidence that the Nordic model (that is, penalizing men for buying commercial sex) has a larger impact on increasing rape than models that penalize women for supplying commercial sex.

Our study also contributes to the literature on the drivers of rape. A large literature in noneconomic disciplines suggests that the power imbalance between genders is an important driver of rape. For example, Johnson (2014) points out that rape is more likely to occur in societies with a larger power imbalance between

[4] The damaging effects of sexual violence on the physical, psychological, social, and economic well-being of the assaulted and society as a whole consist of tangible costs (police and justice interventions, support services for victims, health care, lost productivity) and intangible costs (for example, loss of quality of life). In the United States, rape results in costs of more than $122,000 per victim and nearly $3.1 trillion to the economy over the lifetimes of the 25 million victims (Peterson et al. 2017). In the European Union, one in 20 women over the age of 15 has been raped (around 9 million women), and one in 10 has experienced some form of sexual violence (European Union Agency for Fundamental Rights 2014).

men and women. Scully (1988) and Kelland (2014) state that a gender power imbalance contributes to men's lack of self-control, as it encourages them to disregard women as significant beings and to see them as possessions. Brownmiller (1975) indicates that rape can be viewed as something that keeps men powerful and women powerless and therefore maintains the gender power imbalance. Complementing this strand of literature, we provide evidence that the availability of commercial sex can serve as a substitute for rape and thus has a significant effect on rape rates.

## 2. Background for European Prostitution Legislation

Historians agree that prostitution was first legalized in the sixth century BCE by Solon, who established state-supported brothels and taxed prostitutes. The association of prostitution with contagious disease was introduced in the 16th century and caused brothels to be outlawed in many European countries. Policies reverted in the 19th and 20th centuries: instead of trying to outlaw the practice, many governments chose to regulate the sex industry. Under the premise that it was easier to administer a legal business than control an illegal trade, sex work was decriminalized in Switzerland in 1942, in Spain in 1995, and in Denmark in 1999. In 2000 the Netherlands, the symbol of European prostitution, turned sex work into a fully legal industry (on the history and theories of prostitution, see Bullough and Brundage 1982; Otis 1985; Rossiaud 1988; MacKinnon 1989; Barry 1995; Karras 1996; Weitzer 2010; Farley 2004).

Sex work occurs in a plethora of forms including street prostitution, employment at brothels, escort services, and virtual sex acts; its legal status ranges from a regulated activity to a vaguely permissible or lawful but unregulated industry to an enforced crime. Categorization is typically based on governing the demand and/or supply side, the type of services permitted (indoor: brothels, apartments, hotels, nightclubs, pubs, parlors, and windows; outdoor: street prostitution), and third-party involvement (solicitation, operation of a brothel, and living off the profits of prostitution). Legislative models largely fall under one of the following groups, from the most relaxed to the most restrictive: decriminalization (including abolitionism and new abolitionism), legalization, criminalization, and the Nordic model.[5]

According to the decriminalization stance, as a labor just like any other, prostitution should not be subject to special regulation or laws, namely, no criminal penalties for sex work (neither indoor nor outdoor sexual services are prohibited). This model of state tolerance without intervention is often referred to as abolitionism. New abolitionism is developed on the basis of the former model: indoor and outdoor prostitution are not prohibited, but the existence of brothels

[5] A widely used classification system is decriminalization, legalization, and criminalization (West 2000; Harcourt, Egger, and Donovan 2005; Östergren 2017). Decriminalization is usually further subdivided into abolitionism and new abolitionism (Di Nicola et al. 2005), while criminalization is usually further subdivided into criminalizing of the supply side or the demand side (Bernstein 2007; Munro and Della Giusta 2008; Dewey and Kelly 2011; Skilbrei and Holmström 2013).

is explicitly outlawed. As an illustration, commercial sex in Denmark is not illegal, but keeping a brothel is an offense punishable by imprisonment for up to 4 years (Crim. Code, art. 228 [Den.]). Owning or running a brothel in Luxembourg carries the risk of up to 3 years of incarceration and a maximum fine of 50,000 EUR (Crim. Code, art. 379 [Lux.]).

Legalization (also known as regulationism) refers to the regulation and control of sex work through criminal law, labor law, or other legislation. Regulation allows prostitution within certain limits and takes the form of work permits, licensing, and tolerance zones. Prostitutes are registered by local authorities and are in some cases obliged to undergo medical checkups. The Dutch approach lets municipalities license and spatially restrict brothels, cap their number in certain areas, and close them if they have negative consequences for the community (Hubbard, Matthews, and Scoular 2008). A prerequisite for legal sex work in Greece is the certificate of profession, which can be obtained by unmarried women only with a valid residence permit. Under the legalization framework in Germany, for instance, sex workers have access to social benefits, including health, unemployment, and pension insurance.

The antipode, criminalization (also known as prohibitionism), condemns all aspects of the sex industry as violations of human rights and dignity and prosecutes both prostitutes and clients (punishments range from fines and prison sentences to the death penalty). These are the prevailing policies in the United States, Asia, and the Middle East. Current representatives of criminalization in our sample are Croatia and Lithuania.

An alternative type of criminalization was initiated by Sweden with the Act of Prohibiting the Purchase of Sexual Services, which came into force on January 1, 1999. The so-called Nordic model or end-demand model—the apotheosis of the Swedish feminists who have consistently argued since the 1980s that men who buy sex from prostitutes should face criminal penalties (Ekberg 2004)—aims to stamp out the root cause of prostitution: demand. Conviction for purchasing sex comes with a hefty fine or up to a year's imprisonment and applies to all types of sexual services, irrespective of whether they occur on the street, in brothels, or in massage parlors. Figure 1 presents our sample countries by regulation type as of 2017 (the end of the sample period).

The boundaries between these models are sometimes blurry (Barnett, Casavant, and Nicol 2011). For instance, the Danish policy falls under the new abolitionist framework because outdoor sexual labor is not prohibited, whereas working in a brothel is. However, it is legal to work in an apartment. The Greek approach is categorized as legalization, albeit an imperfect example of that type: outdoor prostitution is considered a crime, yet providing commercial sexual services in state-run regulated brothels and apartments is legal. The subtle differences are in the details, but we do not aim to place a particular country's regime under a label with unshakeable conviction. Our goal is to apply a holistic approach and track how a statutory change from overall illegal prostitution to legal prostitution (and vice versa) affects sex crimes. Therefore, we consider commercial sex to be

| | Decriminalization | | | |
|---|---|---|---|---|
| **Legalization** | **Abolitionism** | **New Abolitionism** | **Criminalization** | **Nordic** |
| Austria | Czechia | Belgium | Croatia | France |
| Germany | Portugal | Bulgaria | Lithuania | Iceland |
| Greece | Slovakia | Cyprus | | Ireland |
| Hungary | Slovenia | Denmark | | Norway |
| Latvia | Spain | Estonia | | Sweden |
| Netherlands | | Finland | | |
| Switzerland | | Italy | | |
| United Kingdom | | Luxembourg | | |
| | | Malta | | |
| | | Poland | | |
| | | Romania | | |

**Figure 1.** Legal status of prostitution in our sample countries

**Table 1**
**Legislative Changes in Prostitution Laws**

| | Law | Enactment Year |
|---|---|---|
| Liberalization: | | |
| Spain | Criminal Code, art. 188 | 1995 |
| Denmark | Criminal Code, chap. 24 | 1999 |
| Hungary | Act LXXV of 1999 on Organized Crime | 1999 |
| Netherlands | Criminal Code brothel ban lifted | 2000 |
| Germany | Prostitution Act | 2002 |
| Slovenia | Criminal Code, art. 175 | 2003 |
| Latvia | Cabinet Regulation No. 32 Regarding Restriction of Prostitution | 2008 |
| Romania | Criminal Code, law 286/2009 | 2014 |
| Prohibition: | | |
| Sweden | Sex Purchase Act | 1999 |
| Croatia | Act on Misdemeanors against Public Peace and Order, art. 12; Criminal Code, art. 175 | 2000 |
| Norway | General Civil Penal Code sec. 202a | 2009 |
| Iceland | Penal Code, chap. 22, art. 206 | 2009 |
| France | Law 2016-444 | 2016 |
| Ireland | Criminal Law (Sexual Offenses) Act 2017 | 2017 |

legal in a country if sexual services are available and accessible overall and neither party to the transaction (supply or demand) is prosecuted. That is, we classify decriminalization (abolitionism and new abolitionism) and legalization as legislative frameworks of legal prostitution. We set aside the technicalities of whether sex work is permitted, say, in an apartment but not in a club or window, as long as in general it is available, accessible, and not prosecuted. On the opposite side, we combine criminalization and the Nordic model for the set of illegal prostitution forms.

Table 1 presents the legislative changes adopted in the countries that liberalize (decriminalize or legalize) and prohibit (criminalize prostitution or clients) prostitution. During the sample period, eight countries liberalized prostitution, whereas six countries prohibited it.

After a country liberalizes prostitution, commercial sex greatly increases in value. For instance, Häggström (2016) finds that Germany's adoption of the Prostitution Act in 2002 led to a dramatic increase in the transaction value in its sex industry. In contrast, after prohibition, participation in the commercial sex industry is significantly reduced. For example, as a result of the Sex Buyer Law in Sweden in 1999, the number of female prostitutes decreased from 2,500 before the reform to no more than 1,500 in 2002 (Ekberg 2004), and the number of clients shrank by 80 percent (Danna 2007).

The main motivators for liberalizing commercial sex include protecting the human and labor rights of sex workers, ameliorating public health and safety risks, severing the links between prostitution and crime, and preventing human trafficking.[6] Additional government stimuli are the lucrative tax and tourism revenues from a commercial sex industry. For illustration, in 2002, following a 30-year political process, Germany passed the Prostitution Act, which removed the general prohibition on sex services and allows workers in the industry to obtain regular working contracts. The law's rationale is that prostitution should not be considered immoral since it is not sex work per se that promotes oppressive values but rather the production of marginalized, degraded prostitution (Zatz 1997). When the industry was condemned, prostitutes did not have a right to claim counterperformance because agreements governing sexual services were invalid (Civ. Code, sec. 138 [Ger.]). Accordingly, the legislators' intention was to eliminate this discrimination by enabling legally effective employment relationships that would ensure lawful action to pay, facilitate access to social insurance, and improve health conditions at work. In addition, the act was projected to curb human trafficking and provide sex workers with an easier way out of the industry.

The leading reasons behind the prohibition of commercial sex are that it is "incompatible with the dignity and worth of the human person," is intrinsically abusive, and incites human trafficking (Convention for Traffic in Persons and of the the Suppression of the Exploitation of the Prostitution of Others, 96 U.N.T.S. 271). For example, since the early 1980s Swedish feminists have consistently argued that prostitution should be outlawed. In 1987, the National Organization for Women's Shelters and Young Women's Shelters in Sweden presented this demand to female parliamentarians. Because of their dedicated lobbying and the

[6] Both those supporting and those opposing commercial sex believe that their approach can help reduce human trafficking. Those calling for prohibition argue that expanding the sex market into a legal trade will increase the demand for prostitutes and thus boost human trafficking to fill the demand. In contrast, those who call for liberalization claim that a legal sex trade improves the industry's conditions and allows it to legitimately recruit women, which makes resorting to trafficking less attractive. See, for example, Hughes (2000), Di Nicola et al. (2005), Cho, Dreher, and Neumayer (2013), Jakobsson and Kotsadam (2013), Akee et al. (2014), and Lee and Persson (2018).

will of Sweden's female politicians, the Sex Purchase Act was approved and came into force on January 1, 1999 (Ekberg 2004). Offenses comprise all forms of sexual services, regardless of whether they are purchased on the street, in brothels, or from escorts; procuring and operating a brothel remained illegal, too. The main catalyst for reform was the public's belief that prostitution is irreconcilable with gender equality and inseparably linked with human trafficking. The law was designed to have a normative function: to manifest that women are not commodities to be bought and to exterminate prostitution by eliminating male demand.

## 3. Development of the Hypothesis

Suppose that a man could be a purchaser of commercial sex or be a rapist and that he makes the choice on the basis of the costs and benefits of these two options.[7] We expect prostitution liberalization to reduce rape via the substitution mechanism: men who view commercial sex and rape as having similar costs may choose prostitution over rape if prostitution becomes cheaper and more easily available, and they may choose rape if prostitution becomes costlier and less accessible.

From the cost perspective, liberalization decreases the cost of commercial sex. Lee and Persson (2018) show that liberalization expands the size of the sex market, cuts entry costs for sex workers, and lowers prices of sex services. Cunningham and Shah (2018) find that transaction prices for sex services decrease by 33 percent after legalization. Policies that recognize prostitution as a legal job also reduce the stigma associated with it, which increases the marginal willingness to pay for sex (Della Giusta, Di Tommaso, and Strøm 2009; Della Giusta 2010).

In terms of benefits, liberalization increases the accessibility and quality of commercial sex services. Legalized prostitution expands the overall commercial sex market, attracts more sex workers to the industry, and increases the variety of choices for clients. In Germany, for instance, since the legalization of commercial sex in 2002, the number of sex workers has more than tripled: at least 400,000 prostitutes are now working in a multitude of venues, ranging from flat-rate sex clubs and sex boxes in street-walking zones to megabrothels and a large eBay-style sex auction website. Furthermore, liberalization mitigates health risks by instituting regular medical examinations, enforcing licensing, and promoting safer sex (Gertler and Shah 2011). Loff, Gaze, and Fairley (2000) find an 80-fold greater prevalence of bacterial STIs among illegal street workers than among legal sex laborers. Cameron, Seager, and Shah (2021) show that legalized sex work is associated with fewer STIs and more condom use. Cunningham and Shah (2018) document that the incidence of female gonorrhea declined by 47 percent after prostitution was decriminalized.

Prostitution liberalization thus decreases the costs and increases the benefits

[7] Rape and commercial sex are not perfect substitutes, as rape involves using violence, coercion, and control to sexually penetrate the victim without consent.

for men of using commercial sex services. Therefore, we expect that liberalization will reduce rape rates for both sex workers and the general population. Farley (2005) points out that a black market for prostitution increases sexual violence against sex workers. After prostitution is liberalized, more sex workers in illegal markets switch to the relatively safer legal sector and gain more legal protection, and the incidence of rape among all sex workers decreases. The increased access to legal prostitution may also allow for better matching of buyers and sellers of commercial sex. Potential sex offenders who would normally commit acts of sexual assault in the general population may choose consensual acts with voluntary sex workers (Bhuller et al. 2013; Ciacci and Sviatschi 2016). As a result, the incidence of rape in the general population may fall after prostitution is liberalized.[8]

Using the same rationale, we expect that prostitution prohibition will increase rape rates, because prohibiting prostitution decreases the availability of commercial sex and increases the cost for men of obtaining sex via prostitution. Broadly consistent with this prediction, Thornhill and Thornhill (1983) and Thornhill and Palmer (2000a, 2000b) assert that when consensual sex becomes more difficult, rape increases, as it is an adaptive strategy in the human evolutionary environment. In a survey of men who purchased sex from women reported in Farley et al. (2011), more than 50 percent of participants stated that if prostitution did not exist then they would be more likely to rape women.

Moreover, we expect the magnitude of the effect of prohibition to be larger than that of liberalization for three reasons. First, the existing literature well documents asymmetry in crime (Glaeser, Sacerdote, and Scheinkman 1996; Calvó-Armengol, Verdier, and Zenou 2007; Mocan and Bali 2010). That is, once an individual engages in criminal activity (for example, rape), his legal human capital depreciates and his criminal human capital appreciates, which makes it difficult to switch back to the legal sector. In other words, it is naturally more difficult for a policy to reduce a certain crime than for an opposite policy to increase that crime.

The second reason could be related to persistent habits of consumption. Individuals derive utility not only from their level of current consumption but also from how it compares with their past consumption: once they are used to a certain level of consumption, it drives their future demand (Ravina 2007). This applies to goods deemed harmful to society and individuals, such as alcohol, tobacco, drugs, gambling, and pornography. For example, men may have already formed the habit of obtaining sex via prostitution. A ban on commercial sex breaks such a habit and induces men to seek alternative outlets for consensual sex or to use coercion or force and sexually assault women. However, when commercial sex was previously banned, people had no preexisting habit of purchasing sex. That is, prohibition could have a larger effect on changing people's behavior

[8] Given that our data report rape rates only for the general population, which includes sex workers, we cannot separately examine the effect of prostitution regulation on sex workers and everyone else. This could be an interesting question for future research.

than liberalization, because the former likely breaks people's preexisting habit of obtaining sex through prostitution.

Third, after prostitution is liberalized, it may take time and effort to complete the administrative procedures, set up facilities, and recruit and train sex workers. Generally, such start-up costs play an important role in delaying the creation of a new business (Ciccone and Matsuyama 1996; Fonseca, Lopez-Garcia, and Pissarides 2001). In contrast, when commercial sex is banned, the effect is immediate. In Norway, for example, the results were instant and dramatic 1 year after purchasing sex was outlawed: in 2010 all known brothels in Oslo were closed (Raymond 2013). In other words, it is usually easier to shut down a business than to develop a new one, which may further contribute to a stronger effect of the prohibition than the liberalization of prostitution.

In summary, we expect that liberalization will lead to a decrease in the rape rate and that prohibition will lead to an increase in the rape rate. We also expect an asymmetric effect: the magnitude of prostitution prohibition on the rape rate will be larger than that of prostitution liberalization.

## 4. Data and Variable Construction

We analyze how the legal changes in prostitution policies affect rape rates in 31 European countries (the 28 European Union members plus the European Free Trade Association countries of Iceland, Norway, and Switzerland).[9] We track the legal status of prostitution and the corresponding reforms by examining the sex work laws provided by the Institute of Development Studies, US human rights reports, and numerous news articles from each country. In the time span covered, commercial sex was liberalized (decriminalized or legalized) in eight countries (henceforth, liberalized countries) and outlawed (criminalized) in six countries (henceforth, prohibited countries) (see Table 1).[10] We collect the number of police-recorded rape offenses from Eurostat.[11] As described by the data vendor, the full definition of rape is "[s]exual penetration without valid consent or with consent as a result of intimidation, force, fraud, coercion, threat, deception, use of drugs or alcohol, abuse of power or of a position of vulnerability, or the giving or receiving of benefits."[12] Where rape rates are missing in Eurostat, we collect

[9] Liechtenstein, the fourth European Free Trade Association member, is not included because of its very small population and lack of data.

[10] The control countries are Austria, Belgium, Bulgaria, Cyprus, Czechia, Estonia, Finland, Greece, Italy, Lithuania, Luxembourg, Malta, Poland, Portugal, Slovakia, Switzerland, and the United Kingdom. Although the United Kingdom voluntarily ended its EU membership in 2020, it was a member throughout our sample period of 1990–2007.

[11] Eurostat, Recorded Offences by Offence Category (https://ec.europa.eu/eurostat/cache/metadata/en/crim_off_cat_esms.htm). Eurostat is the statistical office of the European Union, and its mission is to provide comparable and high-quality statistics for European countries to provide a basis for deciding on, planning, and implementing policies. Eurostat collects data from member states but also works with them to refine and harmonize European statistics.

[12] This definition of rape is generally gender neutral and includes males as victims. However, the overwhelming majority of rape victims are women (Rennison 2002).

**Table 2**
**Summary Statistics: Full Sample**

| Variable | Mean | SD | 25th Percentile | Median | 75th Percentile |
|---|---|---|---|---|---|
| Rape Rate | 9.21 | 9.79 | 3.83 | 6.36 | 10.20 |
| GDP Per Capita | 26,023 | 20,789 | 10,691 | 22,407 | 36,628 |
| Population (millions) | 15.21 | 20.52 | .49 | .59 | 11.09 |
| Unemployment Rate | 8.38 | 4.51 | 5.00 | 7.53 | 10.43 |
| Women per 100 Men | 105.36 | 4.23 | 102.40 | 104.70 | 106.50 |
| Police Officers | .33 | .14 | .25 | .33 | .42 |
| Gender Inequality Index | .18 | .09 | .11 | .17 | .25 |
| Immigrants | 9.24 | 7.65 | 3.64 | 8.08 | 11.88 |
| Homicide | 2.43 | 3.22 | 1.00 | 1.39 | 2.27 |
| Burglary | 294.63 | 258.37 | 131.20 | 211.50 | 408.50 |
| Robbery | 100.29 | 258.37 | 29.60 | 54.65 | 100.70 |
| Total Crime | 397.68 | 388.43 | 168.88 | 294.87 | 501.00 |

such information from the UN Office on Drugs and Crime and national statistics. Country characteristics are obtained from national accounts data collected by World Bank and the Organisation for Economic Co-operation and Development; the number of police officers and the ratio of women to men are from Eurostat. The data on migration and the marital status of the population are from the Population Division of the UN Department of Economic and Social Affairs and Eurostat. The gender inequality index is from the UN Development Programme.[13] Our final sample consists of 841 country-year observations from 1990 to 2017. We start the sample with 1990 because crime statistics became widely available at that time. Definitions of the variables are provided in the Appendix.

Table 2 provides summary statistics for the full sample. Table 3 compares liberalized countries and control countries (which had no changes in prostitution regulation). Liberalized countries (relative to control countries) have a larger population, a slightly lower percentage of police officers, and fewer incidences of robbery. Table 4 compares prohibited countries and control countries. Relative to control countries, prohibited countries have a higher rape rate, a higher GDP per capita, fewer women per 100 men, a lower percentage of police officers, a higher percentage of immigrants, less gender inequality, and a lower homicide rate. It is ideal for the treatment group and control group to be relatively similar along observable dimensions. However, if not, one can directly include control variables in the regression specification (Roberts and Whited 2013).

[13] United Nations Development Programme, Human Development Index (HDI) (https://hdr.undp.org/data-center/human-development-index#/indicies/HDI).

**Table 3**

**Summary Statistics: Liberalized Countries versus Control Countries**

| | Liberalized Countries | | Control Countries | | Test of Difference | |
|---|---|---|---|---|---|---|
| | Mean (1) | Median (2) | Mean (3) | Median (4) | *t*-Test (1) − (3) | Wilcoxon Test (2) − (4) |
| Rape Rate | 6.65 | 5.62 | 7.05 | 5.62 | −.40 | .00 |
| GDP Per Capita | 22,030 | 17,715 | 24,303 | 19,375 | −2,273 | −1,660 |
| Population (millions) | 22.03 | 10.37 | 12.24 | 7.63 | 9.79** | 2.74** |
| Unemployment Rate | 8.71 | 7.36 | 8.40 | 7.65 | .31 | −.29 |
| Women per 100 Men | 106.07 | 104.25 | 105.94 | 105.30 | .13 | −1.05** |
| Police Officers | .31 | .30 | .37 | .35 | −.06** | −.05** |
| Immigrants | 8.47 | 8.77 | 9.21 | 6.15 | −.74 | 2.62+ |
| Gender Inequality Index | .20 | .16 | .19 | .19 | .01 | −.03 |
| Homicide | 2.73 | 1.31 | 2.66 | 1.54 | .07 | −.23 |
| Burglary | 295.63 | 196.80 | 294.50 | 210.20 | 1.13 | −13.40 |
| Robbery | 71.54 | 59.15 | 125.98 | 54.35 | −54.44+ | 4.80 |
| Total Crime | 291.78 | 208.05 | 332.97 | 231.90 | −41.19 | −23.85 |

+ Significant at the 10% level.
** Significant at the 1% level.

**Table 4**

**Summary Statistics: Prohibited Countries versus Control Countries**

| | Prohibited Countries | | Control Countries | | Test of Difference | |
|---|---|---|---|---|---|---|
| | Mean (1) | Median (2) | Mean (3) | Median (4) | *t*-Test (1) − (3) | Wilcoxon Test (2) − (4) |
| Rape Rate | 18.53 | 15.46 | 7.05 | 5.62 | 11.48** | 9.84** |
| GDP Per Capita | 35,928 | 32,563 | 24,303 | 19,375 | 11,625** | 13,188** |
| Population (millions) | 14.29 | 4.56 | 12.24 | 7.63 | 2.05 | −3.07* |
| Unemployment Rate | 8.02 | 7.60 | 8.40 | 7.65 | −.38 | −.05 |
| Women per 100 Men | 102.81 | 102.00 | 105.94 | 105.30 | −3.13** | −3.30** |
| Police Officers | .29 | .27 | .37 | .35 | −.08** | −.08** |
| Immigrants | 10.43 | 10.57 | 9.21 | 6.15 | 1.22+ | 4.42** |
| Gender Inequality Index | .14 | .13 | .19 | .19 | −.05** | −.06** |
| Homicide | 1.39 | 1.10 | 2.66 | 1.54 | −1.27** | −.44** |
| Burglary | 293.60 | 296.40 | 294.50 | 210.20 | −.90 | 86.20 |
| Robbery | 66.78 | 37.00 | 125.98 | 54.35 | −59.20 | −17.35* |
| Total Crime | 271.55 | 256.61 | 332.97 | 231.90 | −61.42 | 24.71 |

+ Significant at the 10% level.
* Significant at the 5% level.
** Significant at the 1% level.

## 5. Empirical Results

### 5.1. Visual Trends

Figure 2 plots the trend of the average rape rate per 100,000 people in prohibited, liberalized, and control countries. The rape rate increased over time across all three groups, but the increase in prohibited countries is clearly greater than in the other two groups. From 1990 to 2017, the average rape rate in prohibited countries increased from 7.70 to 36.81 (380 percent). In contrast, the rape rate increased from 6.49 to 9.71 (50 percent) in liberalized countries and from 4.67 to 8.48 in control countries (82 percent).

Figure 3 shows the difference-in-differences effect of banning commercial sex on rape rates in prohibited countries relative to control countries. We follow Autor, Donohue, and Schwab (2006) in constructing the graph. The point estimates are of the coefficients $\beta_n$ from the following regression:

$$\text{Rape Rate}_{i,t} = \alpha + \sum_{n=-10}^{10} \beta_n \times \text{Ban\_Year}_{i,t+n} + \text{Year Fixed Effects} + \varepsilon_{i,t}, \quad (1)$$

where $i$ indexes country, $t$ indexes year, and $\text{Ban\_Year}_{t+n}$ is a dummy variable indicating the year relative to the legislative reform in country $i$ and year $t$.[14] The sample includes countries that prohibit commercial sex and those that made no policy changes to the legal status of prostitution during our sample period. We excludes countries in which commercial sex is liberalized to ensure that liberalization does not confound the estimated impact of banning it. The coefficient of interest, $\beta_n$, is an estimate of the before/after change in the outcome variable in countries that prohibit commercial sex relative to that in countries that made no policy changes with regard to prostitution. As shown in Figure 3, rape rates increase gradually and consistently after restricting sex work. The coefficient on $\beta_{-1}$ is approximately 5.5; in the year after the prohibition, the corresponding coefficient, $\beta_1$, is approximately 13 (more than twice the size of $\beta_{-1}$). Ten years after the ban, the coefficient of $\beta_{10}$ is 25 (almost 4.5 times the size of $\beta_{-1}$).[15]

Figure 4 captures the difference-in-differences impact of liberalizing prostitution on rape rates in liberalized countries relative to control countries. The estimates are of the coefficients $\beta_n$ from the following regression:

$$\text{Rape Rate}_{i,t} = \alpha + \sum_{n=-10}^{10} \beta_n \times \text{Liberalize\_Year}_{i,t+n} + \text{Year Fixed Effects} + \varepsilon_{i,t}, \quad (2)$$

where Liberalize_Year is a dummy variable denoting the year relative to liberalizing prostitution in country $i$ and year $t$. To avoid any confounding effects of prohibition, we drop all countries that banned commercial sex in the sample period. In other words, this time we estimate the before/after effect on rape rates only in countries that liberalized commercial sex. Figure 4 shows that rape rates decrease

[14] Taking Sweden, which banned prostitution in 1999, as an example, $\text{Ban\_Year}_{t+2}$ equals one for Sweden in 2001 and zero otherwise.

[15] In Figures 3 and 4, the 90 percent confidence intervals are based on robust standard errors clustered by country.

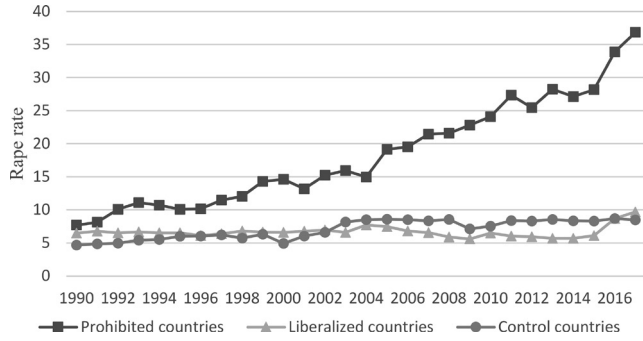


**Figure 2.** Trend in the average rape rate

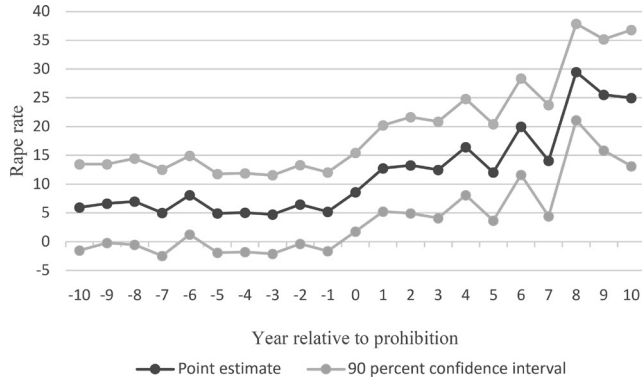


**Figure 3.** Effect of prohibiting prostitution on rape

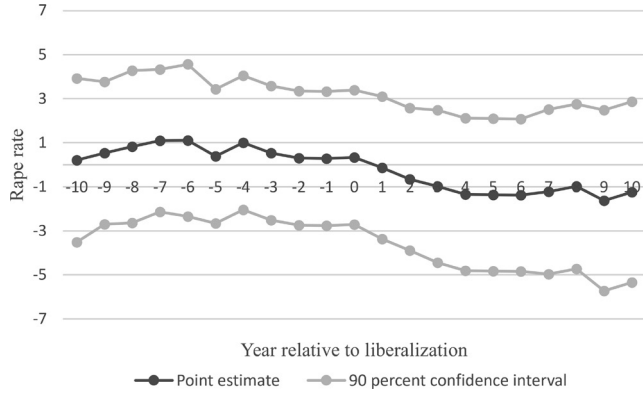


**Figure 4.** Effect of liberalizing prostitution on rape

gradually and consistently after liberalizing sex work. The $\beta_{-1}$ coefficient is approximately .28; the coefficient on $\beta_{10}$ is roughly $-1.3$.

In summary, Figures 2–4 provide preliminary evidence that prohibiting prostitution increases a country's rape rate, while liberalizing prostitution decreases it.[16] Moreover, the magnitude of prohibition's effect seems much larger than that of liberalization.

### 5.2. Baseline Regression

During our sample period, changes in the legal status of prostitution occurred in several countries in various years, which enables us to examine the before/after effect of the regulatory reforms in affected countries (the treatment group) compared with that in countries where no legislative amendments took place (the control group). This is a difference-in-differences test design in multiple treatment groups and multiple time periods, as employed in Imbens and Wooldridge (2009). We implement this test through the following regression:

$$\begin{aligned}\text{Rape Rate}_{i,t} = \alpha + \beta_1 \text{Legal Prostitution}_{i,t} + \beta_2 \text{County Characteristics}_{i,t-1} \\ + \text{Country Fixed Effects} + \text{Year Fixed Effects} + \varepsilon_{i,t}.\end{aligned} \tag{3}$$

The dependent variable measures the number of rape cases per 100,000 people recorded at the national level. The indicator Legal Prostitution equals one if prostitution is legal (decriminalized or legalized) in country *i* in a given year and zero otherwise. The value of Legal Prostitution can change either from zero to one (commercial sex becomes liberalized in a country) or from one to zero (commercial sex becomes prohibited in a country). The year fixed effects enable us to control for intertemporal trends of sex crimes, and the country fixed effects allow us to control for time-invariant differences in sex crimes across countries.

Existing literature shows that when the number of clusters is small, the failure to control for within-cluster error correlation can lead to misleadingly small standard errors and consequently deceptively large *t*-statistics and low *p*-values (Angrist and Pischke 2008; Conley and Taber 2011; Cameron and Miller 2015). Because our sample has 31 countries, we apply the correction for the small number of clusters using wild cluster bootstrapping by country (Roodman et al. 2019) and report the corresponding *p*-values throughout the paper.[17]

[16] It is worth pointing out that the liberalization effect in Figure 4 has a large error band, while the baseline regression reported in column 3 of Table 3 indicates a significant liberalization effect. Unlike in the baseline regression, in Figure 4 we decompose Prostitution Liberalization into 21 indicators (from $\text{Liberalize\_Year}_{i,-10}$ to $\text{Liberalize\_Year}_{i,10}$) and plot the coefficients on them. Possibly because having more parameters causes losses of degrees of freedom, the corresponding standard error increases.

[17] The underlying idea is to generate a large number of bootstrap samples that mimic the distribution from which the actual sample was obtained. Then, using the same test procedure as for the original sample, each bootstrap sample is used to compute a bootstrap test statistic. The bootstrap *p*-value is then calculated as the proportion of the bootstrap statistics that are more extreme than the one from the original sample. For more details on the procedure, see Roodman et al. (2019, sec. 8.3). The Online Appendix (Tables OA1–OA6) reports *p*-values based on robust standard errors clustered by country, and our inference is unchanged.

The country fixed effects lead to $\beta_1$ being estimated as the within-country differences before and after the policy change as opposed to similar before/after differences in countries that did not make a policy change during the same period (Imbens and Wooldridge 2009). It is helpful to consider an example. Suppose we want to estimate the impact of prostitution liberalization on rape in Germany in 2002. We can subtract Germany's rape rate before liberalization from the rate after liberalization. However, economy-wide shocks may occur at the same time and affect sex crimes. Choosing a control country such as Austria, which did not alter its prostitution policy, would help cancel out such factors. We calculate the same difference for Austria. Finally, we compute the difference between the two differences, which represents the incremental effect of the policy change on Germany's rape rate compared with Austria's. One important difference between this example and our regression framework is that our regression accounts for many changes in law over time. The staggered changes in law mean that our control country is not restricted to countries that never changed prostitution laws. As explained in Bertrand and Mullainathan (2003), the control group includes all countries not altering their prostitution regulations at time *t*, even if they had already altered the regulations or will alter them later.

In Table 5, the coefficients on Legal Prostitution are negative and significant in both specifications. Column 1 includes only country and year fixed effects, while column 2 additionally controls for various country characteristics. The coefficients on Legal Prostitution are similar, and both are significant at the 1 percent level. The Legal Prostitution indicator captures both the liberalization and the prohibition of commercial sex. Because we are interested in identifying any asymmetric effect of prostitution laws on rape, we next conduct our difference-in-differences tests separately for liberalization and prohibition of commercial sex.

Column 3 excludes countries that prohibit prostitution. The coefficient on the dummy variable Prostitution Liberalization, which equals one when a country liberalizes prostitution and zero otherwise, is −2.729 and significant at the 1 percent level. This result indicates that, compared with countries that made no changes in prostitution laws, the rape rate in countries that liberalize prostitution decreases by about three cases per 100,000 people. This effect is consequential given that the average rape rate in the sample is 9.21 cases per 100,000 (a decrease of 30 percent). Bisschop, Kastoryano, and van der Klaauw (2017) find that legal street prostitution zones in the Netherlands are associated with a 30–40 percent decrease in sexual abuse and rape. Nguyen (2016) finds that reducing costs to open massage parlors led to a decrease in rape offenses in California by approximately 28 percent. Our estimate of the impact of prostitution liberalization is comparable to those results.

Column 4 examines the effects of prostitution prohibition on rape rates and excludes countries that liberalize prostitution. The key independent variable is the indicator Prostitution Prohibition, which equals one beginning in the year when a country prohibits commercial sex and zero otherwise. There is a significant increase in rape rates in countries that prohibit prostitution relative to countries that made no changes in prostitution laws: banning prostitution leads to an

**Table 5**
**Effects of Prostitution Laws on the Rape Rate**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Legal Prostitution | −7.087** | −7.070** | | |
| | (.009) | (.001) | | |
| Prostitution Liberalization | | | −2.729* | |
| | | | (.011) | |
| Prostitution Prohibition | | | | 11.451** |
| | | | | (.002) |
| Ln(GDP Per Capita) | | −5.165** | −3.744** | −6.225** |
| | | (.001) | (.008) | (.003) |
| Ln(Population) | | 2.532 | .776 | 4.924 |
| | | (.595) | (.836) | (.485) |
| Unemployment Rate | | −.167* | −.126+ | −.172 |
| | | (.043) | (.059) | (.122) |
| Women per 100 Men | | −.682* | −.456 | −.602 |
| | | (.083) | (.184) | (.334) |
| Police Officers | | −.788 | −1.572 | 1.789 |
| | | (.891) | (.689) | (.799) |
| Immigrants | | −.154 | −.107 | −.125 |
| | | (.599) | (.657) | (.752) |
| Gender Inequality Index | | 33.343 | 11.210 | 38.419 |
| | | (.145) | (.420) | (.341) |
| Constant | 13.370** | 92.253 | 80.428 | 47.497 |
| | (.000) | (.360) | (.269) | (.691) |
| *N* | 841 | 841 | 675 | 621 |
| Adjusted $R^2$ | .776 | .815 | .792 | .838 |
| Mean of dependent variable | 9.21 | 9.21 | 6.97 | 10.11 |
| Treated countries | 14 | 14 | 8 | 6 |

**Note.** Results are from difference-in-differences tests that examine the impact of changes in prostitution laws on rape rates. The *p*-values based on wild cluster bootstrapping by country are reported in parentheses. All regressions include year and country fixed effects. $N = 17$ control countries.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

increase in rape rates by about 11 cases per 100,000 people. This increase is about four times that from liberalizing prostitution, which is consistent with our conjecture about the asymmetric effect of prostitution regulation on sex crimes.[18]

Taken together, the results indicate that a country's rape rate significantly increases (decreases) when it bans (liberalizes) commercial sex. These findings pro-

[18] Results for the magnitude of the effects of prohibiting prostitution on rape are mixed. For example, Cunningham and Shah (2018) show that the recriminalization of indoor prostitution in Rhode Island had an insignificant effect on rape. Backus and Nguyen (2021) find that the criminalization of purchasing sexual services in Northern Ireland increased sexual assaults by 15–20 percent. Ciacci (2020) finds that banning the purchase of commercial sex in Sweden is associated with an increase in reported rapes of 47 percent.

vide support for a causal effect of commercial sex on sex crimes.[19] Moreover, we provide the first evidence on the asymmetric effect of prostitution regulation: the effect of prohibition is much larger in magnitude than that of liberalization.

### *5.3. The Pretreatment Trends*

The validity of difference-in-differences estimation depends on the parallel-trends assumption: absent the prostitution laws, sex crimes would have evolved in the same way in both the treatment and control groups. Table 6 presents results that investigate the pretreatment trends. Five dummy variables designate each year relative to the enactment of the prostitution law.

In column 1 of Table 6, we reestimate column 3 of Table 5 by replacing Prostitution Liberalization with these five indicator variables. The coefficients on the Year −2 and Year −1 indicators are especially important because their significance and magnitude indicate whether there is any difference between the treatment and the control groups prior to the policy change. The coefficients are close to 0 and not statistically significant, which suggests that the parallel-trends assumption is not violated. Moreover, the impact of prostitution liberalization shows up after the law's enactment: the coefficient on Year 2+ is significantly negative.

In column 2 of Table 6, we focus on the prohibition of commercial sex and reestimate column 4 of Table 5 by replacing Prostitution Prohibition with the five indicator variables. The treated and control groups have similar trends prior to the policy change: The coefficients of Year −2 and Year −1 are not significantly different from 0. The positive effect of prostitution prohibition on a country's rape rate shows up after the policy change: the coefficients on Year +1 and Year 2+ are significantly positive.

Overall, Table 6 confirms that the treated and control groups have a similar trend in rape rates prior to the changes in law, which supports the parallel-trends assumption. Moreover, Table 6 indicates that most of the impact of prostitution laws on rape rates occurs after the laws are enacted, which suggests a causal effect.

### *5.4. Heterogeneous Treatment Effects*

#### 5.4.1. Underreporting Rapes

It is widely documented that reported rapes are likely to be an underestimate of the number of offenses (Cunningham and Shah 2018). Various barriers stop victims from disclosing sex crimes: shame, denial, depression, fear of retaliation,

[19] Among the control variables, the coefficient on the unemployment rate is significantly negative, which indicates that higher unemployment is associated with lower rape rates. This finding is consistent with the literature showing that unemployment is positively associated with property crime but negatively associated with violent crime (Cantor and Land 1985; Raphael and Winter-Ebmer 2001). The reasoning is that the unemployed are less involved in public interactions, and thus their opportunities for person-focused crime are reduced.

**Table 6**
**Testing for Pretreatment Trends and Reversals**

| | Prostitution Liberalization (1) | Prostitution Prohibition (2) |
|---|---|---|
| Year −2 | −.490 | .995 |
| | (.256) | (.700) |
| Year −1 | −.283 | −1.127 |
| | (.624) | (.670) |
| Year 0 | −.659 | 1.900 |
| | (.323) | (.147) |
| Year +1 | −.794 | 4.637** |
| | (.263) | (.000) |
| Year 2+ | −2.929* | 14.354* |
| | (.015) | (.018) |
| Ln(GDP Per Capita) | −3.852** | −6.366** |
| | (.008) | (.001) |
| Ln(Population) | .836 | 7.001 |
| | (.802) | (.335) |
| Unemployment Rate | −.131* | −.171 |
| | (.065) | (.127) |
| Women per 100 Men | −.434 | −.472 |
| | (.209) | (.448) |
| Police Officers | −1.533 | 3.642 |
| | (.681) | (.664) |
| Immigrants | −.106 | −.144 |
| | (.656) | (.670) |
| Gender Inequality Index | 12.112 | 36.331 |
| | (.394) | (.333) |
| Constant | 77.926 | 2.241 |
| | (.308) | (.985) |
| *N* | 675 | 621 |
| Adjusted $R^2$ | .791 | .852 |

**Note.** Results are from difference-in-differences tests that investigate the pretreatment trends of the treated and control groups. The *p*-values based on wild cluster bootstrapping by country are in parentheses. All regressions include year and country fixed effects.

* Significant at the 5% level.

** Significant at the 1% level.

uncertainty about how to report, lack of information, and so on. References at the European level generally indicate that somewhere between 2 percent and 10 percent of rapes are reported.[20] It is worth noting that underreporting rapes could make us underestimate the effect of prostitution regulation on sex crimes for two reasons.

First, sex workers, who are often victims of physical and sexual abuse (Bisschop, Kastoryano, and van der Klaauw 2017), are more likely to report rapes after prostitution is liberalized, as they are no longer engaging in illegal activities (World Health Organization 2005; Cunningham and Shah 2018). This should

[20] According to the European Union Agency for Fundamental Rights (2014) survey on violence against women, fewer than 15 percent of victims reported their most serious incident of sexual violence.

work against finding a negative effect of prostitution liberalization on a country's rape rate. Similarly, prohibiting commercial sex marginalizes prostitutes and thus discourages them from reporting sex crimes (Bridgett and Robinson 1999), which likely works against finding a positive effect of prohibition on a country's rape rate.

Second, although the tendency in the general population to report rapes may not be correlated with the legislative changes in prostitution laws, underreporting rapes may still lead to an underestimate of the effect of prostitution laws. Suppose that only $\beta$ percent of rapes among the general population is reported ($0 < \beta < 1$) and that prostitution laws change the rape rate from $M$ cases to $N$ cases per 100,000 people. In this situation, although the effect of prostitution laws on rape rates is $(N - M)$, the estimated effect based on reported rape data is only $\beta \times (N - M)$. When $\beta \to 0$ (that is, rapes are severely underreported), the estimated effect of prostitution laws will also be biased toward 0. When $\beta$ increases from 0 to 1, the estimated effect from our regression analysis would also increase accordingly.

On the basis of this discussion, we expect our results to be stronger when underreporting becomes less severe. To empirically examine this conjecture, we focus on three proxies to measure the extent of underreporting of rape.

First, over the last few decades, women have become more likely to report sexual assaults (Lovett and Kelly 2009; Amnesty International 2018).[21] Therefore, in the latter period of our sample, the problem of underreporting should be mitigated to some extent, and thus our treatment effects should be larger. To test this implication, Table 7 includes the indicator variable Latter Period, which equals one for the period from 2003 onward (the midpoint of our sample period) and zero otherwise. We then reestimate Table 5, column 2, by adding Legal Prostitution $\times$ Latter Period (and dropping the year fixed effects). The coefficient on the interaction is significantly negative, which indicates that the treatment effect is stronger in the latter period (when rapes are likely to be less severely underreported).

Second, an important factor affecting the propensity to report sexual violence is gender equality: women are less likely to report sex crimes when they are in a more disadvantaged position relative to men (García-Moreno et al. 2005; Lovett and Kelly 2009; Heise 2011). To measure a country's gender inequality, we use the UN Development Programme's gender inequality index (GII), which is a composite of indices of reproductive health, empowerment, and economic status that ranges from 0 (women and men are equal) to 1 (where one gender scores poorly on all indices).[22] The indicator variable High Gender Inequality equals one

[21] There are a variety of reasons for this trend, including the women's movement challenging gender stereotypes and breaking taboos about discussing sexual violence, increased media attention and social awareness, the emergence of support services (sexual assault centers, rape crisis lines, self-help groups), new guidelines and training in some countries for police and prosecutors, and so on.

[22] The gender inequality index reflects gender-based disadvantages in reproductive health, measured by maternal mortality ratio and adolescent birth rates; empowerment, measured by the proportion of parliamentary seats occupied by women and the proportion of adult women and men with at least some secondary education; and economic status, measured by women's labor market participation rate.

**Table 7**
**Heterogeneous Treatment Effects: Underreporting Rape**

| | (1) | (2) | (3) |
|---|---|---|---|
| Legal Prostitution | −3.177* | −9.089** | −8.691** |
| | (.021) | (.002) | (.003) |
| Legal Prostitution × Latter Period | −7.767* | | |
| | (.039) | | |
| Legal Prostitution × High Gender Inequality | | 5.592* | |
| | | (.025) | |
| Legal Prostitution × Eastern Europe | | | 7.135* |
| | | | (.049) |
| Latter Period | 11.259** | | |
| | (.002) | | |
| High Gender Inequality | | −1.732 | |
| | | (.350) | |
| Eastern Europe | | | −24.192** |
| | | | (.000) |
| Ln(GDP Per Capita) | −2.171* | −7.196** | −5.928** |
| | (.023) | (.000) | (.000) |
| Ln(Population) | 12.177** | 6.912 | .361 |
| | (.028) | (.139) | (.933) |
| Unemployment Rate | −.053 | −.201** | −.174* |
| | (.566) | (.001) | (.025) |
| Women per 100 Men | −.984* | −.568 | −.775* |
| | (.033) | (.114) | (.027) |
| Police Officers | 8.535 | 3.056 | −2.628 |
| | (.242) | (.603) | (.618) |
| Immigrants | .121 | .031 | −.137 |
| | (.506) | (.864) | (.622) |
| Gender Inequality Index | −.981 | | 26.651 |
| | (.948) | | (.214) |
| Constant | −59.929 | 33.669 | 147.344 |
| | (.531) | (.689) | (.101) |
| Year fixed effects | No | Yes | Yes |
| Adjusted $R^2$ | .813 | .824 | .823 |

**Note.** The *p*-values based on wild cluster bootstrapping by country are in parentheses. All regressions include country fixed effects. $N = 841$.
* Significant at the 5% level.
** Significant at the 1% level.

if the GII is higher than or equal to its sample average and zero otherwise. We then reestimate Table 5, column 2, by adding Legal Prostitution × High Gender Inequality. The coefficient on the interaction is significantly positive, which indicates that our treatment effect is weaker in countries with greater gender inequality (where rapes are likely to be severely underreported).

Third, considering that Eastern Europe (relative to other parts of Europe) has a weaker rule of law and less public confidence in the criminal justice system, and thus a more severe problem with underreporting rapes (Von Hofer 2000), we expect our treatment effect to be weaker there. The indicator variable Eastern Eu-

rope equals one if the country is located in Eastern Europe and zero otherwise.[23] We reestimate Table 5, column 2, by adding Legal Prostitution × Eastern Europe. The coefficient on the interaction is significantly positive, which indicates that the treatment effect is weaker in Eastern Europe.

Overall, the results in Table 7 indicate that our treatment effect is stronger when rapes are less severely underreported. The results are also consistent with our conjecture that these findings are underestimates.

#### 5.4.2. Obtaining Sex via Marriage or Partnership

Men in relationships may have sex via these marriages or partnerships. Thus, we expect our treatment effect to be stronger in countries with low marriage and partnership rates. To investigate this matter, we calculate the number of married persons as a percentage of a country's population. In Table 8, the indicator Low Marriage equals one if this percentage is below the sample mean and zero otherwise. We reestimate Table 5, column 2, by adding Legal Prostitution × Low Marriage. The coefficient on the interaction is significantly negative (−4.771), which indicates that the treatment effect is stronger in countries with low marriage rates.

Family patterns in Europe have changed substantially over recent decades: marriage rates have declined, while rates of nonmarital cohabitation, or consensual union, have increased (Thomson, Winkler-Dworak, and Beaujouan 2019). Considering that the indicator Low Marriage captures information only about legal marriage while many couples may live in consensual unions, we focus on the percentage of single adults. To this end, we obtain data on single persons in a given year and compute the number of single persons as a percentage of the population. The indicator High Single equals one if the percentage of the single population is greater than or equal to the sample mean and zero otherwise. We reestimate Table 5, column 2, by adding Legal Prostitution × High Single. The coefficient on the interaction is significantly negative (−6.455), which indicates that our treatment effect is stronger in countries with a high single rate.

As another robustness check, we use the number of women per 100 men to capture the gender imbalance in a country. Marriage squeeze—the effect on marriage of an imbalance in the numbers of males and females in a society—has long been recognized as a significant factor influencing contemporary marriage behavior (Akers 1967). Prior research (for example, Guttentag and Secord 1983; Pedersen 1991; South and Lloyd 1992) shows that it is more difficult for men to find long-term partners (and thus obtain sex via a romantic relationship) in countries with a greater gender imbalance. For that reason, we define the indicator Low Sex Ratio to equal one if the number of women per 100 men (ages 15–64) is be-

[23] In accordance with the UN M49 standard classification, the Eastern European countries in our sample are Bulgaria, Czechia, Hungary, Poland, Romania, and Slovakia (UN Statistics Division, Methodology [https://unstats.un.org/unsd/methodology/m49]). Croatia, Cyprus, and Greece, which have similarly low levels of public confidence or weaker rules of law and are often classified as southeastern European countries, are also in this sample.

**Table 8**
**Heterogeneous Treatment Effects: Marriage/Partnership**

| | (1) | (2) | (3) |
|---|---|---|---|
| Legal Prostitution | −4.325** | −2.645 | −2.577 |
| | (.003) | (.104) | (.154) |
| Legal Prostitution × Low Marriage | −4.771+ | | |
| | (.052) | | |
| Legal Prostitution × High Single | | −6.455* | |
| | | (.040) | |
| Legal Prostitution × Low Sex Ratio | | | −6.023+ |
| | | | (.052) |
| Low Marriage | 3.109 | | |
| | (.214) | | |
| High Single | | 5.081* | |
| | | (.048) | |
| Low Sex Ratio | | | 2.114 |
| | | | (.485) |
| Ln(GDP Per Capita) | −5.284** | −5.222** | −5.641** |
| | (.000) | (.000) | (.000) |
| Ln(Population) | .968 | 3.425 | 5.669 |
| | (.833) | (.382) | .178 |
| Unemployment Rate | −.167* | −.174* | −.239** |
| | (.038) | (.031) | (.008) |
| Women per 100 Men | −.760* | −.772* | |
| | (.026) | (.024) | |
| Police Officers | −2.696 | .419 | 1.817 |
| | (.624) | (.935) | (.748) |
| Immigrants | −.114 | −.142 | −.068 |
| | (.675) | (.603) | (.747) |
| Gender Inequality Index | 25.387 | 27.318 | 35.108* |
| | (.228) | (.166) | (.092) |
| Constant | 126.552 | 84.711 | −28.476 |
| | (.176) | (.292) | (.705) |
| Adjusted $R^2$ | .822 | .824 | .822 |

**Note.** The *p*-values based on wild cluster bootstrapping by country are in parentheses. All regressions include year and country fixed effects. $N = 841$.
+ Significant at the 10% level.
* Significant at the 5% level.
** Significant at the 1% level.

low the sample mean and zero otherwise. We then reestimate Table 5, column 2, by adding Legal Prostitution × Low Sex Ratio. The coefficient on the interaction is significantly negative, which indicates that our treatment effect is stronger in countries with fewer women per 100 men. Overall, the results in Table 8 indicate that the effect of prostitution laws on rape rates is more pronounced when it is more difficult for men to obtain sex via marriage or partnership.

### *5.5. Different Prostitution Policy Models*

As detailed in Section 2, prostitution policy models vary from willful ignorance to accepting sex work as a valid form of labor (decriminalization) to regulating

it as a licensed business (legalization) to prohibiting it as an illicit, degrading activity (criminalization) to restraining it as an expression of male dominance and sexual exploitation (the Nordic model). In this section, we separately analyze the prostitution models and identify their effects on rape.

We first examine the two types of liberalized prostitution: decriminalization and legalization. Decriminalization implies the removal of criminal penalties associated with all or some forms of sex work, which is generally treated like any other legitimate occupation. Under legalization, prostitution is allowed only within certain specified limits and is subject to some mandatory requirements, such as regular health checks, licensing, work permits, and adherence to tolerance zones. The primary distinction between the two models is that decriminalization provides a more tolerant environment for commercial sex. Thus, we expect that the effect of decriminalization on reducing rape is stronger than that of legalization. To examine this notion, we define the indicator Decriminalization to equal one beginning in the year when a country decriminalizes prostitution and zero otherwise and the indicator Legalization to equal one beginning in the year when a country starts regulating its sex trade and zero otherwise. The regression specification in Table 9, column 1, is the same as that in column 2 of Table 5, except that we replace Legal Prostitution with the four indicators: Legalization, Decriminalization, Criminalization, and Nordic Model. The coefficient on Decriminalization is −3.969, and the coefficient on Legalization is −3.458; both coefficients are statistically significant. This result reveals that, while both types of liberalization reduce rape rates, the effect of decriminalization (the more lenient environment for commercial sex) is (slightly) stronger than that of legalization.

It is worth noting that decriminalization can be further separated into the abolitionism model and the new abolitionism model (Di Nicola et al. 2005). Under abolitionism, the state resolves to tolerate the sex industry and not to intervene; both outdoor prostitution and indoor prostitution are permitted. Under the model of new abolitionism, outdoor and indoor prostitution are permitted too, but brothels are explicitly banned. We define two additional indicator variables, Abolitionism and New Abolitionism, which equal one beginning in the year when a country adopts the respective policy approach. Column 2 of Table 9 replaces Decriminalization with Abolitionism and New Abolitionism. The estimated coefficient on Legalization is −3.370 (significant at the 5 percent level), whereas the coefficient on Abolitionism—the model providing the most liberal sex market—is almost double that (significant at the 5 percent level). In contrast, the coefficient on New Abolitionism is only −1.397 and is not statistically significant, which indicates that permitting outdoor and indoor prostitution while keeping brothels banned has little effect on rape. This result is understandable considering that brothels play an important role in the commercial sex industry by reducing the fixed cost of location, providing security, and mitigating information asymmetry between buyers and sellers (Farmer and Horowitz 2013).

Table 9 also shows the effects of criminalization and the Nordic model. Unlike criminalization, which prohibits prostitution and makes selling sex, organizing it,

**Table 9**
**Prostitution Policy Models**

| | (1) | (2) |
|---|---|---|
| Legalization | −3.458+ | −3.370+ |
| | (.052) | (.057) |
| Decriminalization | −3.969+ | |
| | (.068) | |
| Abolitionism | | −6.510* |
| | | (.031) |
| New Abolitionism | | −1.397 |
| | | (.458) |
| Criminalization | .956 | .829 |
| | (.538) | (.525) |
| Nordic Model | 15.601** | 15.554** |
| | (.000) | (.000) |
| Ln(GDP Per Capita) | −3.898** | −4.214** |
| | (.008) | (.004) |
| Ln(Population) | 1.870 | 1.683 |
| | (.684) | (.682) |
| Unemployment Rate | −.110 | −.121+ |
| | (.106) | (.074) |
| Women per 100 Men | −.586 | −.628+ |
| | (.110) | (.087) |
| Police Officers | −.715 | −1.048 |
| | (.908) | (.850) |
| Immigrants | −.139 | −.113 |
| | (.601) | (.665) |
| Gender Inequality Index | 28.102 | 23.344 |
| | (.238) | (.345) |
| Constant | 74.473 | 85.732 |
| | (.372) | (.296) |
| Adjusted $R^2$ | .839 | .841 |

**Note.** Results are from difference-in-differences tests that examine the impact of prostitution policy models on rape rates. The *p*-values based on wild cluster bootstrapping by country are in parentheses. All regressions include year and country fixed effects. $N = 841$.

+ Significant at the 10% level.
* Significant at the 5% level.
** Significant at the 1% level.

buying it, or all of these illegal, the Nordic model punishes the purchase of sexual services. The coefficient on Nordic Model is much larger in magnitude than that on Criminalization: 15.554 (significant at the 1 percent level) versus .829 (not statistically significant). These results are understandable considering that men usually face a lower legal risk for purchasing commercial sex than for committing rape. The Nordic model (as compared with the criminalization model) increases men's legal risk for purchasing commercial sex, makes prostitution less attractive, and thus increases men's propensity to commit a sex crime.

Overall, among the prostitution liberalization models, decriminalization (in particular abolitionism) has a stronger effect on reducing rape than legalization

does. Among the prostitution prohibition models, the Nordic model has a stronger effect on increasing rape than criminalization does.

### *5.6. Robustness Checks*

#### 5.6.1. Placebo Tests: Evidence for Other Crimes

It is possible that prostitution laws are confounded with other legal changes that affect a country's levels of other criminal activities. To investigate this possibility, we implement a placebo test to examine whether prostitution laws affect other serious nonsexual crimes such as homicide, burglary, and robbery.

Table 10 first focuses on prostitution liberalization and reestimates the regression in Table 5, column 3. The variable Homicide measures the number of intentional homicide cases per 100,000 people. The variable Burglary is the number of burglary and housebreaking cases per 100,000 people. Robbery is the number of robbery cases per 100,000 people. The variable Total Crime is the sum of Homicide, Burglary, and Robbery. None of the coefficients on Prostitution Liberalization are significantly different from 0, and the magnitudes are also small. For Total Crime, the coefficient on Prostitution Liberalization is 30.906 and not significant from 0 ($p$-value = .655). Considering that the sample average of Total Crime in liberalized countries is 291.78, the magnitude of the coefficient on Prostitution Liberalization is small. Similarly, in Table 11 shows that none of the coefficients on Prostitution Prohibition are significantly different from 0 either.

In summary, Table 10 indicates that prostitution laws affect only sex crimes and have no impact on other criminal activities. This result suggests that the observed relationship between prostitution laws and a country's rape rate is unlikely to be driven by some confounding event that affects that country's levels of other criminal activities.[24]

#### 5.6.2. Matched-Sample Analysis

In this section, we perform a robustness check of our main results by matching each country prohibiting prostitution to a country liberalizing it and reestimating equation (3). With six prohibited countries and eight liberalized countries (see Table 1), we form 28 matched samples.[25] We then reestimate the baseline regression in Table 5, column 2, and save the corresponding 28 coefficients on Prostitution Prohibition.[26] By doing so, we can avoid any possible bias associated with matching samples on the basis of particular criteria.

[24] In Table OA7 in the Online Appendix, we control for Total Crime in the baseline regression, and our inference is largely the same.

[25] The number of samples that can be drawn is the number of possible combinations that can be obtained from taking a sample of six countries from a set of eight countries (each chosen sample consists of six liberalized and six prohibited countries). That is, $C(8, 6) = 8!/[6!(8 - 6)!] = 28$.

[26] Given that there are only two types of countries in this matched analysis (prohibited countries and liberalized countries), we include only Prostitution Prohibition in the regression. We could include Prostitution Liberalization instead; in that case, the coefficient on Prostitution Liberalization would be the same in magnitude but opposite in sign to that of Prostitution Prohibition.

**Table 10**

**Placebo Tests: Effects of Prostitution Laws on Nonsexual Crimes**

| | Homicide (1) | Burglary (2) | Robbery (3) | Total Crime (4) | Homicide (5) | Burglary (6) | Robbery (7) | Total Crime (8) |
|---|---|---|---|---|---|---|---|---|
| Prostitution Liberalization | .399 | 80.989 | −35.917 | 30.906 | | | | |
| | (.667) | (.140) | (.417) | (.655) | | | | |
| Prostitution Prohibition | | | | | .785 | 91.599 | 27.937 | 41.814 |
| | | | | | (.269) | (.364) | (.411) | (.464) |
| Ln(GDP Per Capita) | .217 | −94.825 | −121.848 | −222.467** | .049 | −85.715 | −136.811 | −292.151** |
| | (.832) | (.172) | (.314) | (.008) | (.981) | (.430) | (.273) | (.008) |
| Ln(Population) | 12.643* | 103.018 | −543.504 | −56.964 | 9.419* | 394.421 | −314.620 | 173.711 |
| | (.043) | (.813) | (.631) | (.726) | (.036) | (.378) | (.539) | (.395) |
| Unemployment Rate | −.047 | −.449 | 2.274 | −4.042 | .025 | −.240 | .227 | −5.966 |
| | (.499) | (.845) | (.224) | (.154) | (.427) | (.949) | (.903) | (.244) |
| Women per 100 Men | .458 | 16.823 | 7.979 | 5.996 | .533 | 24.548 | 11.973 | 18.961 |
| | (.202) | (.219) | (.469) | (.736) | (.280) | (.178) | (.246) | (.304) |
| Police Officers | 10.644+ | −116.045 | 472.790 | 67.677 | 8.348+ | −128.515 | 373.612 | 327.856 |
| | (.059) | (.711) | (.260) | (.742) | (.060) | (.801) | (.450) | (.259) |
| Immigrants | .235 | −.863 | 6.348 | −7.833 | .168 | −2.615 | 5.807 | −11.477 |
| | (.282) | (.896) | (.246) | (.457) | (.286) | (.587) | (.264) | (.209) |
| Gender Inequality Index | 20.139+ | −117.867 | −242.862 | 147.738 | 24.961 | 386.814 | −231.224 | 535.068 |
| | (.088) | (.818) | (.557) | (.769) | (.208) | (.535) | (.762) | (.424) |
| Constant | −260.612** | −2,193.300 | 8,834.510 | 2,397.788 | −214.945* | −7,804.647 | 4,960.066 | −2,083.180 |
| | (.010) | (.716) | (.440) | (.468) | (.035) | (.246) | (.456) | (.631) |
| Mean of dependent variable | 2.73 | 295.63 | 71.54 | 291.78 | 1.39 | 293.60 | 66.78 | 271.55 |
| *N* | 664 | 532 | 528 | 675 | 609 | 484 | 476 | 621 |
| Adjusted $R^2$ | .780 | .757 | .497 | .506 | .834 | .700 | .492 | .486 |

**Note.** The *p*-values based on wild cluster bootstrapping by country are in parentheses. All regressions include year and country fixed effects.

+ Significant at the 10% level.

* Significant at the 5% level.

** Significant at the 1% level.

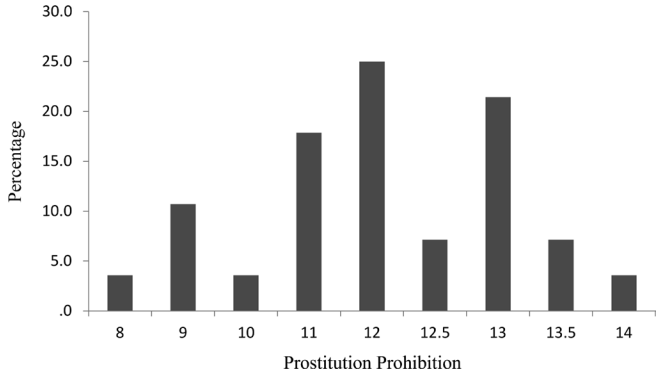


**Figure 5.** Matched-sample coefficients

Figure 5 plots the distribution of these coefficients, which range from 7.88 to 13.84 with a mean of 11.33. These results indicate that rape rates in countries prohibiting prostitution increased by 7.88–13.84 cases per 100,000 people, more than the rates in countries liberalizing prostitution. Overall, our main inference is unchanged.

### 5.6.3. Bacon Decomposition

Goodman-Bacon (2021) shows that standard difference-in-differences estimates can be biased when multiple treatments occur at different times, partially because earlier treatment cohorts serve as controls for later treatment groups. Given that we exploit 14 staggered legal changes in different years, we follow Goodman-Bacon (2021) and Goodman-Bacon, Goldring, and Nichols (2019) to perform a Bacon decomposition of difference-in-differences estimation with variation in treatment timing. The two-way fixed-effects difference-in-differences model is a weighted average of all possible $2 \times 2$ difference-in-differences estimators in the data. The results are presented in Table 11. The decomposition shows comparisons among timing groups (earlier treated versus later treated; later treated versus earlier treated), comparisons of timing groups with units never receiving treatment (treated versus never treated), and the component resulting from within-group variation in controls. The findings attest that only around 10 percent (11.2 percent in the Prostitution Liberalization sample and 12.6 percent in the Prostitution Prohibition sample) of the difference-in-differences estimates are derived from comparisons of countries with heterogeneity in treatment timing. What matters is that the major part of the difference-in-differences estimates comes solely from the comparisons of treated and untreated units. Moreover, both estimates are very similar to our baseline regression results: the coefficient on Prostitution Liberalization is −2.155 (compared with −2.729 in Table 5), and the coefficient on Prostitution Prohibition is 13.517 (compared with 11.451 in

**Table 11**
**Bacon Decomposition**

| | Coefficient | Total Weight |
|---|---|---|
| Prostitution Liberalization: | | |
| Timing-group comparisons | −.764 | .112 |
| Never treated versus timing-group comparisons | −2.155 | .849 |
| Within-group variation from covariates | −15.913 | .040 |
| Prostitution Prohibition: | | |
| Timing-group comparisons | 5.263 | .126 |
| Never-treated versus timing-group comparisons | 13.517 | .827 |
| Within-group variation from covariates | −19.624 | .047 |

Table 5). In summary, our main inference is largely unchanged after addressing the potential bias associated with the heterogeneity in the timing of treatments.

### 5.6.4. Alternative Difference-in-Differences Methods

To further address the heterogeneity in the timing of treatments, we apply three alternative difference-in-differences methods: the method proposed in Callaway and Sant'Anna (2021), the method proposed in Sun and Abraham (2021), and the stacked difference-in-differences method proposed in Cengiz et al. (2019). The estimators in Callaway and Sant'Anna (2021) and Sun and Abraham (2021) are closely related. The individual cohort-time-specific treatment effects are estimated first, which allows for treatment-effect heterogeneity; these treatment effects are aggregated to produce the overall treatment effects. However, the two methods differ methodologically regarding flexibility, accommodation of covariates, choice of control groups, and inference (Baker, Larcker, and Wang 2022). As described in Cengiz et al. (2019), the idea for stacked difference-in-differences is to create event-specific never-treated $2 \times 2$ data sets for the treated groups and never-treated control groups in the treatment window. We then stack the $2 \times 2$ data sets and estimate a two-way fixed-effects difference-in-differences regression with data-set-specific unit and time fixed effects.[27]

Table 12 reports the static effect estimates. The sample includes countries that were treated during the sample period over the years −5 to 15 relative to the treatment year (denoted year 0) and clean control countries (never-treated observations) for all sample years with available data. The coefficients on Prostitution Liberalization are negative and significant at the 5 percent level. The magnitude of these coefficients is comparable to that for our baseline regression in column 3 of Table 5 (−2.729). The coefficients on Prostitution Prohibition are significant at or below the 5 percent level. The magnitude of these coefficients is comparable to that for our baseline regression in column 4 of Table 5 (11.451). Overall, these

[27] The Stata commands for the three estimation methods are csdid, eventstudyinteract, and stackedev.

**Table 12**
**Alternative Difference-in-Differences Methods**

| | Callaway and Sant'Anna (2021) | Sun and Abraham (2021) | Stacked Difference-in-Differences |
|---|---|---|---|
| Prostitution Liberalization | −2.051* | −1.874* | −1.779* |
| | (.034) | (.049) | (.013) |
| Prostitution Prohibition | 13.487* | 12.790** | 12.824* |
| | (.038) | (.000) | (.017) |

**Note.** The *p*-values based on standard errors clustered by country are in parentheses.
* Significant at the 5% level.
** Significant at the 1% level.

results indicate that our main inference is largely unchanged (both statistically and in terms of magnitude) under alternative difference-in-differences methods.

## 6. Conclusions

In this paper, we investigate the effect of legislative changes in prostitution policies on rape rates. Using staggered legal changes in European countries over the last 3 decades and a difference-in-differences framework, we find that prohibiting commercial sex leads to a significant increase in rape rates and that liberalizing it results in a significant decrease in rape rates. We also provide the first evidence on the asymmetric effect of prostitution regulation on rape rates: the magnitude of prohibiting prostitution is significantly larger than that of liberalizing prostitution. The parallel-trends tests show that there are no pretreatment differences between treatment and control countries and that the change in rape rates occurs after the legal changes, which suggests a causal effect. The heterogeneous test shows that the treatment effect is stronger when rapes are reported more often and when men are less likely to be in marriages or partnerships. We further examine the effects of decriminalization, legalization, criminalization, and the Nordic model and find that decriminalization (in particular abolitionism) has a stronger effect on reducing rape than other liberalization models, while the Nordic model has a stronger effect on increasing rape than other prohibition models. Placebo tests show that prostitution laws have no impact on nonsexual crimes, which indicates that our findings are unlikely to be driven by some confounding event that affects a country's levels of other criminal activities. Finally, our main inference is unchanged in the matched-sample analysis and is robust to addressing the potential bias associated with the small number of clusters and the heterogeneity in the timing of treatments.

In recent years, the antiprostitution movement, fueled by ideological concerns about gender inequality and human trafficking, has gained momentum. The classification of prostitution as patriarchal oppression and the outlawing of commercial sex have been spreading: in 2014 the European Parliament adopted a non-

binding resolution in favor of prostitution prohibition. Antiprostitution policies were implemented in South Korea (2004), South Africa (2007), Canada (2014), and Israel (2018). Lawmakers in Nevada, the only US state with legal brothels, have revived the debate to have them banned (Joseph 2019). Our results suggest that policies aimed at prohibiting prostitution can have the severe unintended consequence of proliferating sexual violence.

It is worth noting that the changes in prostitution laws might not be random. It is possible that a country changes the laws as part of a general program to improve women's social status and is thus instituting other policies that may affect rape rates. Although we implement several techniques to address this concern (such as controlling for various country characteristics, using a matched-sample analysis, and placebo tests), we acknowledge that these may not fully address the possible nonrandomness of prostitution laws. Readers should be aware of this limitation when deciding how our findings might be generalized.

Finally, our paper mainly focuses on rich industrialized nations. Prostitution markets and the corresponding legal institutions work differently in developing and developed countries (Farley et al. 2004), and thus some of our findings may not apply generally to developing countries. A fruitful area for future research is to explore the effect of prostitution regulation on rape in developing countries.

## Appendix

**Table A1**

**Definitions of the Variables**

| Variable | Definition |
|---|---|
| Burglary | Burglary and domestic housebreaking cases per 100,000 people |
| Gender Inequality Index | Composite measure ranging from 0 to 1 that reflects inequality between women and men in three dimensions: reproductive health, empowerment, and the labor market |
| Homicide | Intentional homicide cases per 100,000 people |
| Immigrants | Immigrants as a percentage of the national population |
| Legal Prostitution | Indicator variable that equals one if prostitution is legal (decriminalized or legalized) in a given country in a given year and zero otherwise |
| Ln(GDP Per Capita) | Natural logarithm of gross domestic product per capita in US dollars |
| Ln(Population) | Natural logarithm of the national population |
| Police Officers | Police officers as a percentage of the national population |
| Prostitution Liberalization | Indicator variable that equals one beginning in the year when a country legalizes prostitution and zero otherwise |
| Prostitution Prohibition | Indicator variable that equals one beginning in the year when a country prohibits prostitution and zero otherwise |
| Rape Rate | Police-recorded rape offenses per 100,000 people |
| Robbery | Robbery cases per 100,000 people |
| Total Crime | The sum of intentional homicides, burglaries, and robberies per 100,000 people |
| Unemployment Rate | Unemployed persons as a percentage of the labor force |
| Women per 100 Men | Women per 100 men in the population |

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