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ORIGINAL PAPER

Banning the purchase of sex increases cases of rape: evidence from Sweden

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Abstract
This paper leverages the timing of a ban on the purchase of sex to assess its impact on rape offenses. Relying on Swedish high-frequency data from 1997 to 2014, I find that the ban increases the number of rapes by around 44–62%. The results are robust to several econometric specifications that exploit different identification assumptions. The increase reflects a boost in completed rapes both in the short- and long-run. However, it is not accompanied by a decrease in the number of pimps. Taken together, the empirical evidence hints at the notion that the rise in rapes is not connected to the supply of prostitution but rather to changes in the demand for prostitution due to the ban. The results here have the opposite sign but larger magnitudes in absolute value than results in the literature on the decriminalization of prostitution.

Keywords Rape · Sex crimes · Prostitution · Prostitution law · Prostitution regulation · Criminalizing purchase of prostitution · Nordic model · Fuzzy difference-in-differences · Event study analysis · Instrumental variables estimation

Mathematics Subject Classification (2010) C26 · J16 · J47 · K14

1 Introduction

Regulation of prostitution is an amply debated topic in both the USA and Europe. In the USA, prostitution is illegal in every state except Nevada. In Europe, regulations vary across countries, ranging from decriminalization (e.g., Denmark) to a ban on the purchase of sex (e.g., Sweden, Norway, and Iceland), also known as the Nordic

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model. In 2014, the European parliament passed a non-binding resolution calling on other European countries to adopt the Nordic model.

Advocates of the Nordic model consider prostitution a violation of human rights and a form of violence against women. They see the Nordic model as a regime that may decrease human trafficking and both the demand and supply of prostitution, by tackling clients rather than prostitutes. In the last decade, many countries, such as Norway, Canada, Israel, France, and Ireland, among others, adopted the Nordic model. However, evidence on the effects of this model is sparse and largely qualitative.

This paper is one of the first studies to present causal evidence on the effects of criminalizing the purchase of sex. Using high-frequency regional data from Sweden between 1997 and 2014, I analyze the effect of a ban on the purchase of sex on rape offenses.[1] To address potential endogeneity issues, I use several causal inference approaches: regression discontinuity estimators leveraging exogenous variation in the time in which the law went into effect; instrumental variable techniques that exploit plausibly exogenous variation in the availability of flights (to proxy for access to sex tourism); event study analysis; and the recently developed fuzzy difference-in-differences estimation methods (de Chaisemartin & D'Haultfœuille, 2018a).

Estimating the effect of the Nordic model on rape is a relevant topic, taking into account the fact that women are drastically over-represented among victims of this crime. This is also a feature of those countries where gender violence is severely punished, as in the Scandinavian countries. For example, according to the Swedish National Council for Crime Prevention, six times as many women as men stated in 2014 that they have been victims of sex offenses in Sweden.[2]

This issue gains importance when taking into account that in the European Union around 5% of women (a group of around nine million) have been victims of rape and 11% of women have been victims of sexual violence (European Union Agency for Fundamental Rights, 2014). The main psychological consequences for the victims of such crimes are post-traumatic stress disorders, mood disorders, substance abuse, and eating and sexual disorders (Faravelli et al., 2004).

Furthermore, recent economic literature (Bisschop et al., 2017; Cunningham & Shah, 2018) has found causal evidence that decriminalizing prostitution reduces rape. In light of this evidence, a relevant question is whether criminalizing the purchase of sex has a symmetric effect in size — but opposite in sign — on rape. Research on this topic will allow policymakers to design crime policies for rape and regulations for the prostitution market according to their objectives.

The main finding of this paper is that banning the purchase of sex increases rape. The estimates are economically meaningful and suggest that criminalizing the purchase of sex increased the incidence of rape by roughly 44–62% between 1999 and 2014. This estimated effect is slightly larger in absolute value than that of decriminalizing prostitution.

An important finding of this paper is that such an effect is stable across regression discontinuity specifications, instrumental variable regressions, event study analysis,

[1] As for regions I refer to any of the self-governing local authorities, also known as regional councils. Currently and in the years analyzed in this article, there are 21 regions of this sort in Sweden.

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

and fuzzy difference-in-differences estimation methods. This paper relies on multiple econometric models and identification assumptions since estimating causal effects in this setting is challenging. Different estimators allow me to take into consideration several potential concerns that might cast doubt on the plausibility of the identification assumption. Such concerns range from changes in the legal definition of rape taking place nationally in certain years to seasonal differences occurring close to the treatment date.

I also explore whether these findings are linked to the supply or demand of prostitution. I do not find any evidence in favor of a decrease of the supply of prostitution. On the other hand, I find evidence consistent with the possibility that the effect is demand-driven and led by a substitution effect between the purchase of sex and rape. This paper contributes to a literature that addresses the effects of different prostitution laws on crime or health outcomes (see, inter alia Cho et al., 2013; Jakobsson and Kotsadam,citeyearjakobsson2013law; Berlin et al.,2019; Della Giusta et al.,2021; Stadtmann and Sonnabend, 2019; Cameron et al., 2021; Lee and Persson,citeyearlee2013human). Cunningham and Shah (2021) provide a complete review of this branch of the literature. This study is primarily connected to the strand of that literature that explores bans on selling or purchasing sex. Cameron et al. (2021) leverage an unanticipated ban on the sale of prostitution in Indonesia to examine its effect on health. Their results show an increase in sexually transmitted infections. Della Giusta et al. (2021) study the introduction of a law in 2009 in the UK targeting both soliciting and paying for sex. Their findings suggest that this law does not curb the demand for prostitution. Berlin et al. (2019) find that banning the purchase of sex in Sweden boosts cases of domestic violence, with no effect on the number of cases of violence against sex workers.

This paper makes three main contributions. First, this article evaluates criminalizing the purchase of prostitution rather than criminalizing the sale of prostitution, as is the case in Cameron et al. (2021). Second, it uses different causal inference econometric techniques to explore the effect of criminalizing the purchase of prostitution. Previous studies either provided a review and conceptual framework of the extant literature (Cunningham & Shah, 2021) or explored statistical associations and/or effects of criminalizing the purchase of prostitution, but relying on observational empirical analysis different from the one presented in this article (Della Giusta et al., 2021; Berlin et al., 2019). Third, this article analyzes the effect of criminalizing the purchase of prostitution on rape. Previous literature focused on other outcomes, ranging from preferences for paid sex to other crimes such as domestic violence (Della Giusta et al., 2021; Berlin et al., 2019). This third contribution gains importance in light of previous literature finding that decriminalization and regulation via licenses of prostitution reduce rapes (Bisschop et al., 2017; Cunningham & Shah, 2018).

Moreover, this paper contributes to a branch of the literature exploring the determinants of crime. This manuscript mainly contributes to the literature studying how variation in a certain punishment might have unintended effects on other crimes. Yang (2017) finds that the introduction of public assistance eligibility (namely, welfare and food stamps) for individuals convicted of drug-related felonies decreases one-year recidivism. Equally, Tuttle (2019) shows that a ban on food stamps access for drug traffickers boosts recidivism among such offenders. Similarly, Deshpande and

Mueller-Smith (2022) find that removing welfare benefits from disadvantaged youth triggers an increase in crimes, especially those related to income generation. In this respect, my article offers similar results, finding that a ban on the purchase of prostitution increases another type of felony, such as rape. To that effect, Doleac (2023) provides a detailed literature review on how different interventions might impinge on criminal behavior.

The rest of the paper is organized as follows. Section 2 introduces background information on rape, prostitution, and sex tourism in Sweden, as well as a simple theoretical framework useful for developing the intuition behind the empirical analysis. Section 3 describes the data sets used in this paper. Section 4 presents the empirical strategies used. In Section 5, I present the main results of the paper. Section 6 explores the robustness of the main results and the identification assumptions on which my empirical strategies rest. Section 7 explores the potential pathways leading to the main findings of the paper. After this analysis, Section 8 considers the policy implications of these results. Finally, Section 9 concludes.

2 Background information and theoretical framework

This section presents qualitative information on rape, prostitution, and sex tourism in Sweden. Furthermore, it introduces a simple theoretical framework to shed light on how fines for sex purchases might affect rape.

2.1 Rape, prostitution and sex tourism

Rape

Sweden has arguably one of the widest definitions of rape (Von Hofer, 2000). There have been three important changes to such a definition: in 1965, in 1984, and in 2005. Only the last change falls in the sample period analyzed in this paper.

In 1962, a legal definition of rape was included in the Swedish Penal Code and, since then, several revisions to this legal definition have been made to include non-consensual sexual acts comparable to sexual intercourse (Jareborg, 1994; Von Hofer, 2000). In 1965, Sweden was the first country to criminalize marital rape (Von Hofer, 2000). In 1984, both heterosexual and homosexual acts were included under the rape rubric, rendering rape gender neutral (Von Hofer, 2000). Moreover, in 2005, sexual acts with someone who is unconscious (e.g. due to intoxication or sleep) were added to the legal definition of rape,[3]

Consequently, it is not surprising that Sweden has presented the highest number of rapes committed in Europe since the Council of Europe started collecting data

[3] *Sexualbrottslagstiftningen* Uppsala University and the National Centre for Knowledge on Men's Violence against Women. Retrieved on 12-August-2023 https://www.nck.uu.se/kunskapsbanken/amnesguider/sexuellt-vald/sexualbrottslagstiftningen/

regarding this crime. According to the criminology literature, this can be explained by three important factors (Von Hofer, 2000):

1. Legal factor: as explained above, the Swedish legal definition of rape is broader compared with other European countries.
2. Statistical factor: Sweden has a system of expansive offense counts and crime data are collected when the offense in question is first reported, even if later investigations indicate that the offense must be given an alternative classification. Expansive offense counts mean that a victim who reports being abused during a period of time should provide details about the number of times the crime occurred, so the offense will not be counted as one but as the number of times reported by the victim.
3. Substantive factor: countries with high levels of sexual equality, and low police corruption, exhibit a higher propensity for rape offenses to be reported.

Prostitution

Prior to 1999, prostitution was not regulated in Sweden. Yet, pimping (i.e., procuring sexual services and/or operating a brothel) and human trafficking were illegal. In February 1998, the Swedish Parliament discussed the criminalization of the purchase of sex. This bill, also known as the *Kvinnofrid* (women's integrity) law, combined measures to prevent both sexual harassment at work and prostitution.

Two months later, the criminalization of the purchase of sex became the object of a separate provision known as *Sexköpslagen* (sex purchase act), which prohibits an individual from buying, but not selling, sexual services. Eventually, the bill passed in June 1998 and became effective on the 1st of January 1999, making Sweden the first country to introduce this type of regulation (Ekberg, 2004). More specifically, since January 1999, the customers of prostitutes in Sweden have faced the risk of receiving a fine or up to six months in prison for buying sexual services. In April 2005, the provision was transferred to the Swedish Penal Code.[4]

Banning the purchase of prostitution is linked with multiple types of crime: either crimes targeted by the policy *per se* or crimes related to prostitution. On the one hand, as Ekberg (2004) exposes, the aim of such a ban was also to reduce human trafficking and pimps, two crimes closely connected with prostitution. On the other hand, it might seem plausible to believe that the ban might affect other crimes that are connected to prostitution as well. Although, the sign of the association is a priori unclear and depends on the crime. Since this ban complicates client-prostitute connections, in view of previous literature (Cunningham et al., 2023), one could expect this ban to increase violence towards female victims. To this extent, Berlin et al. (2019) posited that this ban might boost domestic violence.

In the same way, there is qualitative evidence that prostitutes and their customers might consume drugs (Farley et al., 2004; Ekberg, 2004; Farley et al., 2009). As a result, the ban could also alter crimes related to drug usage. A reduction of the size of the market (either via demand or via supply) should reduce drug usage, but a change of choices and/or profiles of customers might increase it (Stadtmann & Sonnabend,

[4] For further information, see Svanström (2005).

2019; Della Giusta et al., 2021). Thus, without further empirical evidence, it is unclear whether drug usage might rise or fall due to the introduction of the ban. Furthermore, there is empirical evidence that a prohibition on the sale of prostitution raises sexually transmitted infections (Cameron et al., 2021). Accordingly, it might be posited that a prohibition on the purchase of sex could affect those infections as well. Insofar as it might be argued that such infections are related to a given sort of crime, it could be speculated that a ban on the purchase of sex might affect that certain crime. By the same token, in light of previous literature findings, similar analyses might be carried out for further sorts of crime.

Sex tourism

Sex tourism is a relatively recent phenomenon in which prostitutes' customers travel in order to buy sex abroad. The World Tourism Organization defines sex tourism as "trips organized from within the tourism sector, or from outside this sector but using its structures and networks, with the primary purpose of effecting a commercial sexual relationship by the tourist with residents at the destination" (Steinman, 2002).

Nowadays, sex tourism is mainly associated with the cross-boarding of tourists from "developed" to "developing" countries. In effect, according to the literature, Brazil and Thailand are two of the most popular destinations for Swedish sex tourists (Weibull, 2003; Manieri et al., 2013). In the same way, Lu et al. (2020) find that South/Central America and the Caribbean are the most likely destinations to receive sex tourists.

When the *Sexköpslagen* law was introduced sex tourism was not considered an issue (Ekberg, 2004). Yet, as time elapsed, sex tourism has become a growing phenomenon, leading the Swedish Parliament even to discuss whether to introduce a ban on the purchase of sex abroad (Pruth, 2007).

Considering that sex tourism might also take place between "developed" countries depending on how prostitution is regulated in each of the two "developed" countries, one could think that in Sweden there are two types of sex tourism intercontinental and intracontinental. Assuming that for the client intracontinental sex tourism is less costly than intercontinental sex tourism, might imply that the results of the latter are a lower bound of the former. To this extent, it is also worth mentioning the importance of clients in these encounters. Indeed, recent research found that given their traveling patterns and conditional on the number of sexual partners, customers are globally more central than sex workers (Hsieh et al., 2014).

2.2 Conceptual and theoretical framework

Conceptual framework

There might be many channels through which banning the purchase of prostitution might affect rape. This section sums up the main transmission channels relying on the results of previous articles.

Banning the purchase of prostitution might affect the supply of prostitution. The four main theoretical papers modeling prostitution in the economic literature relates the supply of prostitution to either the opportunity cost of prostitutes in the marriage market, the premium generated by the lack of condom usage by prostitutes, reputational losses of prostitutes due to stigma or health risk issues (Edlund & Korn, 2002; Gertler et al., 2005; Della Giusta et al., 2009; Immordino and Russo, 2015). Therefore, they offer a formal theory supporting this channel if one believes that the ban might affect one or more of the factors listed above.

Theoretical literature results are unclear and mixed: a priori, the supply of prostitution could move downward and/or upward due to the ban. With regard to the first channel, there is empirical evidence that this channel might trigger a decay in prostitution whenever taking into account laws that improve women's wellbeing in marriage (Ciacci, 2023). Nonetheless, since it seems implausible in this setting that the ban might affect prostitutes' marriage market opportunities, then it does not seem plausible either that this channel might be at play. If one believes that the ban could increase the premium paid to prostitutes due to the lack of usage of condoms, possibly due to hampering the connections between prostitutes and customers, then the second channel predicts that the supply of prostitution will move upward. As for the third channel, if the ban reinforces negative attitudes towards prostitutes (Kotsadam & Jakobsson, 2011), and so increases their reputational loss, the supply of prostitution will move downward. By the same token, if the ban raises the probability to lose health by prostitutes in their transactions, then the supply of prostitution will move downward.

Banning the purchase of prostitution might affect the demand of prostitution. In this case, since the ban directly impinges on the purchase of prostitution, the transmission channel is clear both theoretically (Stadtmann & Sonnabend, 2019; Lee & Persson, 2022) and empirically (Della Giusta et al., 2021). Broadly, the ban creates an obstacle for clients hence both the choices of such clients and their profile might change. Namely, both their choices and profiles might lean towards risk-seeking attitudes and activities.

Finally, it has been posited that prostitution falls as gender equality improves. If the ban improves gender equality, then prostitution might decay as a combination of both supply and demand mechanisms. On this channel so far only empirical evidence is available (Ciacci & Sansone, 2023). To this extent, further research is needed to elucidate the relative importance of demand and/or supply mechanisms at stake in this case.

Theoretical framework

Consider a modified version of the theoretical model built in Ciacci (2021). In this section, I restrict my analysis to individual rape decisions to show how an intuitive simple model can rationalize the analysis of this study. For further details on the model see Ciacci (2021).

Consider a society formed by two sexes: male and female. Let the population of both sexes be normalized to 1. In this economy, there are two goods: the consumption good c and sex s. The latter is comprised of three types of sex: mating sex s_m, prostitution sex s_p, and rape sex s_r.[5] Mating sex (s_m) is either marriages, engagements, or merely any sort of relationships with some frequency. The key component is that there is a certain regularity in the relationship. This regular component is modeled with the fixed amount of sex s_m that mated individuals receive. This amount of sex is fixed since it is not individually chosen as s_p or s_r. Sex is a weighted sum of these three types.

Assume each individual i in the population of men differs in how he values these three types of sex. Let γ_{mi} and γ_{pi} respectively denote the weights for mating and prostitution types of sex of individual i. Since I focus on the decision of potential sex offenders, I only consider individuals who value rape positively. Thus, I normalize the weight associated to rape sex s_r to 1. Therefore, using constant elasticity of substitution (hereinafter, CES) preferences, define total sex as $s \equiv (\gamma_{mi} s_m^\rho + \gamma_{pi} s_p^\rho + s_r^\rho)^{\frac{1}{\rho}}$ where $-\infty < \rho < 1$. Moreover, let $\gamma_{ji} \in \left[0,\, a_j\right]$ for some upper-bound $a_j \geq 1$ where $j = m,\ p$. Both weights are distributed in the population using the densities $h_j\left(\gamma_{ji}\right)$ s.t. $0 < h_j\left(\gamma_{ji}\right) < 1$ and $\int_0^{a_j} h_j(x)\,dx = 1$. These densities reflect the proportion of each type of man in the total population. In addition, assume that the two densities are independent.

Men's problem

At the beginning of the period the parameters γ_{mi} and γ_{pl} are drawn for each man. All men earn wage y that can be consumed as consumption goods. In this simplified version of the model developed in Ciacci (2021) I disregard mating relationships. Men who have a mating relationship get a fixed amount of sex s_m but have to pay a fixed cost k in terms of consumption good.

Men choose whether to commit rape and/or to buy sex. Rape offenders are caught with probability $q(s_r)$. If caught, offenders face a penalty $F(s_r)$ for some positive F. Hence, $q(s_r)F(s_r)$ is the expected fine for committing rape. Individuals might purchase sex at a price p for each unit purchased s_p. Likewise, consider a law that criminalizes buying prostitution, where individuals are caught with probability q_c and face a fine $F_c\left(s_p\right)$. Similarly, $q_c(s_p)F_c(s_p)$ is the expected fine of buying sex. Assume the two expected fines above are linear. In other words, $q(s_r)F(s_r) = qFs_r$ and $q_c(s_p)F_c(s_p) = q_c F_c s_p$. Then, note that q_c proxies the number of fines for buying prostitution (i.e., how likely is to get a fine for buying one unit of prostitution).

Assuming men's preferences are quasi linear in c and CES in s (i.e., $U(c, s) = c + (\gamma_{mi} s_m^\rho + \gamma_{pi} s_p^\rho + s_r^\rho)^{\frac{1}{\rho}}$) this problem boils down to:

$$\max_{s_r, s_p}\ y - \mathbb{I}k - ps_p - q_c F_c s_p - qFs_r + (\gamma_{mi} s_m^\rho + \gamma_{pi} s_p^\rho + s_r^\rho)^{\frac{1}{\rho}} \tag{1}$$

Result 1 *Increasing the number of fines for buying sex leads to a surge in rapes.*

Proof In this setting, the individual demand of rape with respect to prostitution is: $\frac{s_r}{s_p} = (\frac{p+q_c F_c}{Fq\gamma_p})^{\frac{-1}{\rho-1}}$. Hence, $\frac{\partial \frac{s_r}{s_p}}{\partial q_c} = \frac{-1}{\rho-1}(\frac{p+q_c F_c}{Fq\gamma_p})^{\frac{-\rho}{\rho-1}} \frac{F_c}{Fq\gamma_p} > 0$ □

[5] In this framework I assume these three types of sex are imperfect substitutes between them. Clearly, assuming perfect substitutability does not change the results.

Note that the mechanisms via which changes in the number of fines might affect rape are numerous. This section theoretically pins down that a substitutable relationship between prostitution and rape implies a positive effect of fines for sex purchases on rape, however, given the evidence that prostitutes customers form networks the channels at play might be more complex (Hsieh et al., 2014).

This article makes use of different econometric techniques to explore how the introduction of a ban on purchasing sex and changes in the number of fines for purchasing sex affect rape decisions. In the empirical framework, this analysis is based on data drawn from official institutions, hence it relies on both rape and prostitution decisions and their corresponding probabilities for which aggressors might be caught by police officers. Nonetheless, this section illustrates that it suffices to build a simple model, where prostitution and rape are substitutable, to yield the result that increasing the number of fines for purchasing sex drives rape up.

3 Data

This paper makes use of high-frequency (i.e., monthly) crime data drawn from *The Swedish National Council for Crime Prevention* (also known as Brå, as used hereafter). Brå is the most important institution for the collection of crime data in Sweden. Among other types of crime data, it collects data of crimes reported to police officers. Hence, it also provides detailed information on the number of sex crimes and on the number of fines for the purchase of sex since the enforcement of the ban in 1999.

Table 1 shows summary statistics for rapes, fines for the purchase of sex, fine events, pimps, and police officers. Rapes are classified according to whether the sexual intercourse was completed and whether the place where the crime occurred was outdoor or indoor. Therefore, the variable is split into two sets of two mutually exclusive categories: (i) completed versus attempted rape; (ii) outdoor versus indoor rape.

A fine event is defined as a binary variable taking the value of 1 for a month in which fines for the purchase of sex are issued (i.e., take a positive value), and 0 otherwise. This variable is the main regressor to estimate the effect of fines on rape via the event study and the Wald–DID (i.e., fuzzy difference-in-differences) estimator.

This table separates the statistics into three-time periods. Panels A, B, and C respectively display descriptive statistics for the whole sample period (i.e., from 1997 to 2014), the sample period before the introduction of the ban (i.e., 1997 and 1998), and the sample period subsequent to the introduction of the ban (i.e., from 1999 to 2014). Both rapes and pimps show higher mean, median, and standard deviation after the introduction of the ban. As for rape, completed, outdoor and indoor rape have higher statistics after the introduction of the ban. Across the three-time periods, the vast majority of rapes are comprised of completed and indoor rapes. Furthermore, for all variables, the mean is greater than the median, as illustrated — for the case of rape — by the right-skewed distribution of rape displayed in Fig. A.1.

Before exploring such summary statistics there might be the concern that the extant variation in fine events and fines for sex purchase was insufficient to carry out the analysis. This table is useful to tackle this concern since it allows to compare data on fines (i.e., linked to the demand) to historical data on pimps (i.e., linked to the supply).

Table 1 Summary statistics

Panel A: Whole period			
Rape	Mean	Median	Std.Dev.
Completed	9.99	5	16.3
Attempted	1.39	1	2.41
Outdoor	2.57	1	4.21
Indoor	8.81	4	14.53
Total	11.38	6	18.06
Fines for sex purchase	1.31	0	7.35
Fine events	0.19	0	0.39
Pimps	.28	0	.93
Police officers	831.77	446	1146.9
Observations 4,536			
Panel B: Before the introduction of the ban			
Rape	Mean	Median	Std.Dev.
Completed	4.81	2	8.06
Attempted	1.35	0	2.8
Outdoor	1.57	1	2.76
Indoor	4.59	2	7.83
Total	6.16	3	9.92
Fines for sex purchase	0	0	0
Fine events	0	0	0
Pimps	.09	0	.37
Police officers	776.4	429.	1090.37
Observations 504			
Panel C: After the introduction of the ban			
Rape	Mean	Median	Std.Dev.
Completed	10.64	5	16.94
Attempted	1.39	1	2.36
Outdoor	2.69	1	4.34
Indoor	9.34	5	15.08
Total	12.03	6	18.73
Fines for sex purchase	1.47	0	7.78
Fine events	0.21	0	0.41
Pimps	.3	0	.97
Police officers	838.69	448.5	1153.71
Observations 4,032			

Notes: Summary statistics of main variables across sample periods.

Namely, data on fine events display statistics similar to those of pimps. Whereas, data on fines for sex purchase display greater mean and volatility than data on pimps. Taken together, these results suggest that the introduction of the ban cannot be disregarded: it led to crime statistics similar to or even larger than those of pimps.

To further clarify the differences between fine events and fines for sex purchase two figures are presented. Figures 1 and 2 respectively show the number of fine events and the number of fines for the purchase of sex with respect to rape (in logs) during the sample period considered in this paper. Two features are worth highlighting. First, there is considerable variation in the number of fines and fine events for the purchase of sex. Second, both variables exhibit an upward trend during the sample period. Additionally, Fig. A.2 shows the number of fine events and fines for the purchase of sex during the sample period considered in this paper. As expected, the two variables are positively correlated and exhibit similar spikes and drops.

Having shown the extant variation across both fine events and fines for sex purchase the question of whether these fines had an effect on customers and eventually on rape is an empirical one. The two different variables (i.e., fine events and fines for sex purchase) are useful to present a comprehensive analysis. Depending on the specifical question to answer, either fine events or fines for sex purchases might be better suited. By and large, fine events are a better measure for the extensive margin, while fines for sex purchase are a better measure for the intensive margin.

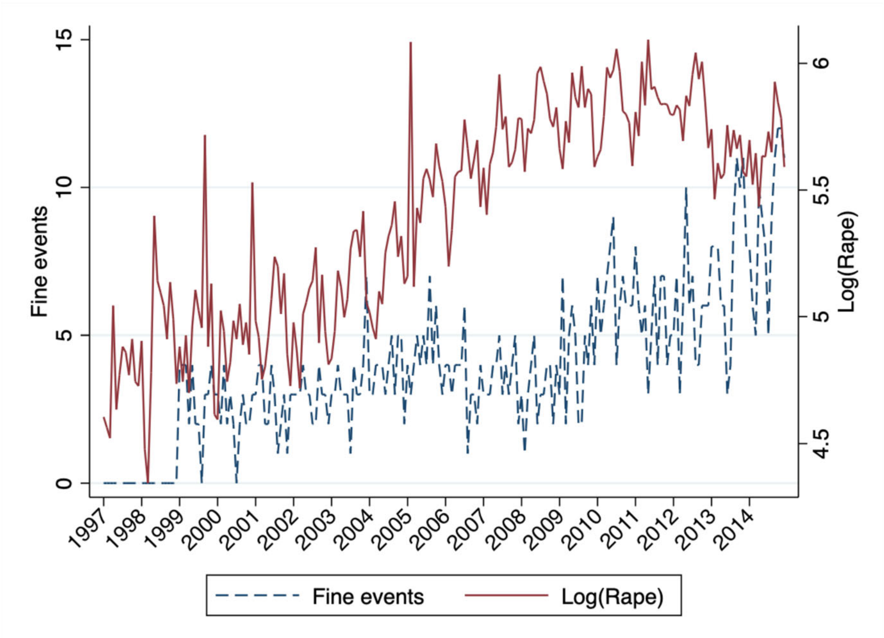

Fig. 1 Evolution of fine events for the purchase of sex and numbers of rapes in Sweden. Notes: This figure shows the number of fine events for the purchase of sex and (log) rapes in Sweden according to Brå during the period 1997–2014

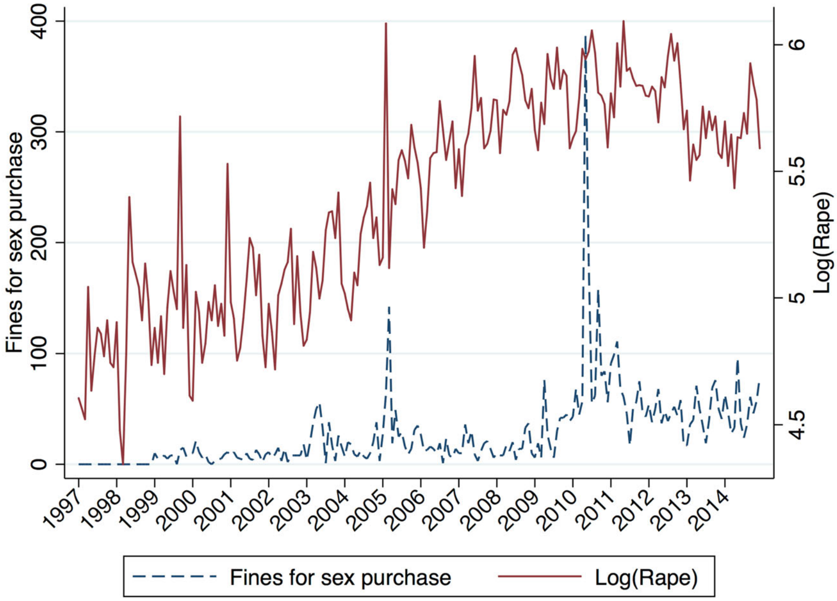

Fig. 2 Evolution of fines issued for the purchase of sex and the number of rapes reported in Sweden. Notes: This figure shows the number of fines for the purchase of sex and (log) rapes in Sweden according to Brå during the period 1997–2014

In addition, this paper also makes use of data on the number of police officers hired by each region from 1997 to 2014 to account for the degree of enforcement of the law. These data are drawn from the *Swedish Police*. Because police recruitment takes place each year, this variable does not exhibit monthly variation within a given year. As shown in Table 1, the descriptive statistics for this variable are substantially similar across the three-time windows. This paper also makes use of meteorologic, economic, and demographic variables, such as data on precipitations (measured as precipitation deviation from the average in %), male and female employment, male and female population, male and female immigration, and male and female civil status. These datasets are respectively drawn from the *Swedish Meteorological and Hydrological Institute* and *Statistics Sweden*.

Finally, I use data drawn from Google and the *Swedish Transport Agency*. In particular, from Google Maps, I collect data on the distances from each region to the closest the airport in a radius of 60 km. Figure 3 shows an example of how such distances are computed. Lastly, data on the number of flights from Swedish airports are drawn from the *Swedish Transport Agency*.[6] Descriptive statistics for these variables are reported in Appendix B.

[6] In this data base, data in 2005 for a few airports are missing.

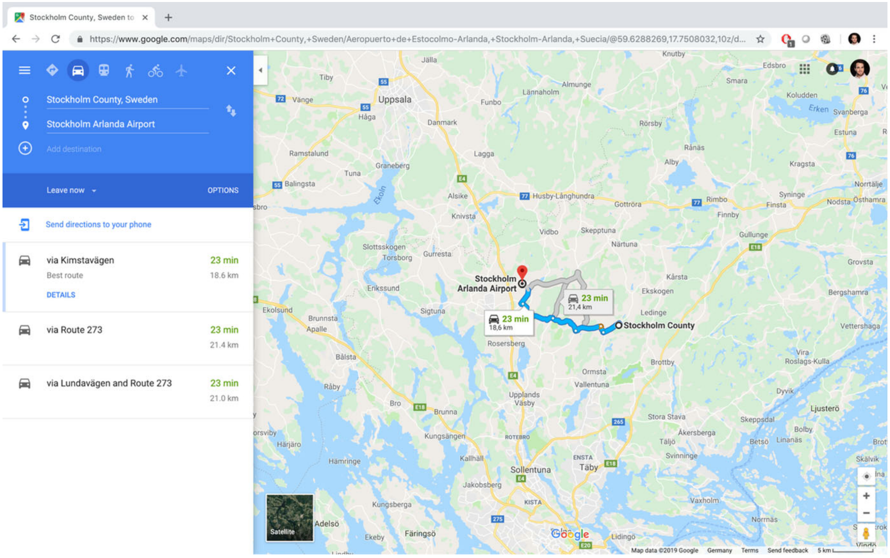

Fig. 3 An example of the airport–region distance by car using Google maps. Notes: Distance from the closest airport to the region, computed via Google maps and using the option of car transport. The example shown is Stockholm county

4 Empirical strategy

Several pieces of evidence find that rape more than doubled after the introduction of the ban. First, Table 1 finds that the average before the ban is around 6 rapes per region and month, while after the introduction is roughly 12. Second, Table 2 presents the results of the naive analysis of regressing rape on a binary variable taking value 0 before the ban and 1 after, controlling for year, month, and region fixed effects. Results show that the post ban period is associated with an increase of around 100% of cases of rape in logs and 125% of cases of rape in the inverse hyperbolic sine transformation (IHS, hereafter). Third, a simple descriptive exercise –plotting rape normalized before the ban around zero by removing pre-treatment fixed effects– encounters that rape boosted around 110% during the sample period (Fig. 4).

Table 2 OLS

	Logs (1)	IHS (2)
Post ban	1.029***	1.255***
	(0.065)	(0.076)
Observations	4,536	4,536

Notes: OLS regression including region, year and month fixed effects. Post ban is a binary variable taking value 1 after the ban and 0 before Standard errors clustered at region level are in parentheses

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

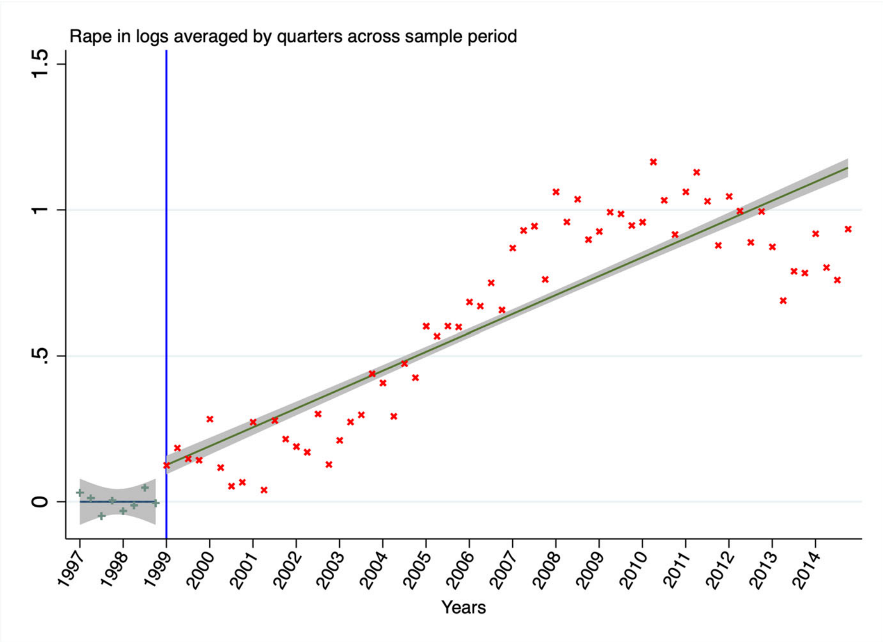

Fig. 4 Raw data on rapes around the treatment date. Notes: This figure shows the number of rapes. The solid line depicts a linear average of rapes across regions and time (quarters). Shadowed areas plot a 95% confidence interval. Pre-treatment fixed effects are removed from the data to normalize the number of rapes around zero before the ban

Taken together, these estimates suggest that the ban is associated to more than doubling the cases of rape. This section explores whether part of this jump might be ascribed to the ban.

4.1 Regression discontinuity in time

This section studies the effect of criminalizing the purchase of prostitution on rape using a regression discontinuity in time (RDiT, hereafter) research design that exploits the cut-off date on which the ban went into force. Specifically, I consider:

$$\log(rape_{rmy}) = \beta_1 \mathbb{I}\{y \geq Jan99\} + \beta_2 F(y \geq Jan99) + \gamma officers_{ry} + \alpha_r + \alpha_m + \alpha_y + \varepsilon_{rmy} \tag{2}$$

where $rape_{rmy}$ is the number of reported cases of rape in region r and month m of year y.[7] $F(y \geq Jan99)$ is the usual polynomial control function included in regression discontinuity frameworks. α_r, α_m, and α_y are fixed effects for region, month, and year, respectively. Specification Eq. 2 clarifies that each observation corresponds to a

[7] Namely, $\log(rape_{rmy})$ is either the number of reported cases of rape in logs $+1$ (i.e., $\log(1 + rape_{rmy})$) or the inverse hyperbolic sine transformation of rapes, since rape might take value 0.

certain region during a given month of a fixed year. The usage of observations at the region-month level with respect to national-year level data improves the precision of the estimates.

Including such fixed effects it is paramount to take into consideration different potential concerns. Specifically, α_r takes into account that regions might be different, and any difference varying at the regional level but constant over time (e.g., some regions might be historically more traditional than others) is captured by such fixed effects. Likewise, α_m takes into account that rape might occur seasonally (e.g., some months might experience higher amounts of cases of rape due to weather conditions), changes varying at a monthly level but constant geographically and over years are captured by these fixed effects. Finally, α_y takes into consideration that rape might experience some differences across years (e.g., in 2005 the definition of rape was changed nationally as mentioned in Section 2). To this extent, any change that does not vary across regions and months, but only over years, is captured by such fixed effects.

The control variable $officers_{ry}$ is the number of police officers in the region r in year y; this variable does not vary at the monthly level m because police officers are hired by regions on a yearly basis. I control for the number of officers hired in each region following a strand of the literature that found that increasing officers decreases the crime rate (see, inter alia,Di Tella and Schargrodsky, 2004; Draca et al., 2011). In light of these results, there might be the concern that the number of fines correlates with the number of officers. Since officers are hired yearly and the hired amount is decided in advance, it is straightforward to dismiss concerns about this variable being affected by crimes taking place during the year.

$\mathbb{I}\{y \geq Jan99\}$ is the treatment variable, taking value 1 for observations after the entry into force of criminalization of the purchase of prostitution and 0 otherwise. Hence, β_1 is the coefficient of interest that under the identification assumption captures the effect of the ban of sex purchases on rape. I use the optimal bandwidth as described by Calonico et al. (2014), and then test the robustness of the results to alternative bandwidths equal to 0.75 and 1.5 times the optimal bandwidth. Furthermore, following Gelman and Imbens (2019) I estimate the results using a first-order and a second order polynomial for the running variable allowing a different polynomial on both sides of the discontinuity.

4.2 Identification assumption

The identification assumption of the RDiT framework is that from December 1998 to January 1999, the month in which the policy is implemented, the only variable that affects rape and it is not controlled in equation Eq. 2 is the entry into force of the ban. Put it differently, the error term ε_{rmy} of equation Eq. 2 is not correlated to the entry into force of the ban given the control function and covariates.

The main concern that could threat the identification assumption of my estimates is that there might be an omitted variable occurring at the same time as the treatment variation and not addressed by the RDiT estimator. Given the time component of the identification assumption in this setting, this omitted variable boils down to seasonal differences between rapes taking place in December and those occurring in January. In other words, one might believe that the policy effect might be confounded by seasonal changes taking place between December and January. Additionally, since the ban entries into force in January 1999, any specific change between December 1998 and January 1999 might also bias the RDiT estimates. These two potential sources of bias could be tackled by adding control variables that measure this variation (such as month fixed effects) or by using a different empirical strategy, as it is the case for difference in regression discontinuity in time. I address such concerns further in Section 6.2.

Given the structure of RDiT research design, density tests are irrelevant (Hausman and Rapson, 2017). To offer evidence on the identification assumption I run equation Eq. 2 using as treatment date the first month in which the ban was known to become effective. During the month of June 1998, the Swedish parliament approved the ban to purchase prostitution, even if the effective date was the 1st of January of 1999 it might be debated that there could have been sorting around the date: customers of prostitution could have anticipated the entry into force of the law. Thereby, there might be the doubt that estimating an RDiT with such a date could lead to finding a statistical significant coefficient for the treatment variable.

Panel A of Table C.3 presents the results of running specification Eq. 2 with this placebo date for the whole sample across functional forms and different degree of the polynomial control function. Estimated coefficients are statistically insignificant in all specifications and close to zero in point estimate or negative. Overall, these findings suggest the effect is not anticipated.

5 Main results

Column (1) of Table 3 shows the results of estimating specification Eq. 2. Panel A uses the whole sample. Results using this sample might be seen as the least conservative since the optimal bandwidth spans 29 months and in the sample there are only 24 months (i.e., 2 years) prior to the ban. Then it is heartening to find that, even for this specification, estimates suggest that the ban led to an increment in rape of about 55%.

There might be the concern that results are statistically significant merely because the post ban period has more observations than the pre ban period. To have the same amount of observations before and after the policy intervention, Panel B restricts the sample to 2 years prior and posterior to the introduction of the ban. In this case, the optimal bandwidth decreases to 9 months. This reduces the number of observations too and, as a consequence, the precision of the estimates. Yet, in both cases, estimates are statistically positive. In addition, even if the 1st order polynomial results of Panel B are slightly lower in point size, taking into account the confidence intervals of the other estimates points out that they are not statistically different.

The preferred specification is the 2nd order polynomial of the restricted sample since ex ante is the most conservative choice and relies on the same number of observations prior and posterior to the cut-off. Results of this specification suggest that the ban raised rape by roughly 60%. Taking into account that prior to the ban there were on average 6 rapes per region and month and 213,200 women per region, this jump means

Table 3 RDiT

	Logs (1)	IHS (2)	Logs (3)	IHS (4)	Logs (5)	IHS (6)
Panel A: Whole sample						
1st order polynomial						
After January 1999	0.555*	0.720*	0.666*	0.860*	0.571**	0.734**
	(0.310)	(0.391)	(0.369)	(0.470)	(0.266)	(0.340)
2nd order polynomial						
After January 1999	0.548*	0.709*	0.641*	0.830*	0.574**	0.738**
	(0.306)	(0.385)	(0.364)	(0.463)	(0.266)	(0.341)
N	1113	1113	1428	1407	903	903
Bandwidth	29	29	43	43	22	21
Panel B: Restricted sample						
1st order polynomial						
After January 1999	0.294*	0.392*	0.590*	0.753*	0.601*	0.753*
	(0.170)	(0.223)	(0.256)	(0.324)	(0.264)	(0.335)
2nd order polynomial						
After January 1999	0.643**	0.835**	0.404	0.526	0.899**	1.134**
	(0.280)	(0.363)	(0.266)	(0.337)	(0.443)	(0.563)
N	399	357	567	567	273	273
Bandwidth	9	9	14	13	7	7

Notes: Each coefficient provided in the table is estimated using a separate regression and measures the discontinuity at the cut-off, that is, after the enforcement of the regulation in January 1999. Panel A considers the whole sample period 1997–2014, while Panel B considers the restricted sample of the same length 1997–2001. Following Gelman and Imbens (2019), results are computed using polynomials of order 1 and 2. The bandwidth used is the optimal bandwidth as defined in Calonico et al. (2014). Standard errors clustered at the region-month level are in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

that before the ban there were on average 2.8 rapes per 100,000 women while, after the ban, this amount rose to 4.5 rapes per 100,000 women.

Panel A of Fig. C.1 provides a graphical analysis of the RDiT. First, I net out pre-treatment region and month fixed effects to account for pre-treatment regional and seasonal differences. Then, I use the restricted sample to estimate local means binning the data for periods of two months and I accompany them with a quadratic polynomial and its corresponding confidence interval at 90% level.

It is remarkable to find a neat jump in rape levels in a tight window around the cutoff. The width of the depicted window is simply given by the optimal bandwidth selected in the RDiT framework (i.e., 9 months prior and posterior to the cutoff).

5.1 Size of the effect of criminalizing the purchase of sex across specifications

This section explores the size of the effect of criminalizing the purchase of prostitution on rape. With this aim in mind, this section relies on four further specifications. First, Difference in RDiT, labelled hereafter Diff in RDiT. Second, fuzzy difference-in-differences using the Wald-DID presented in de Chaisemartin and D'Haultfœuille (2018a). Third, event study analysis. Fourth, instrumental variables (hereafter, IV). I briefly introduce these four specifications and corresponding results in this section, yet Supplementary materials S.1 and S.2 respectively present the estimators and their results in detail. This section leverages such results to make a comparison across regression models.

There might be concerns that the found effect using the RDiT specification is due to seasonal changes in rape due to the fact that the ban went into effect in January. To address this issue, the Diff in RDiT specification solves this problem by differencing the data across time to remove seasonal changes. Its identification assumption relies on the notion that previous months might be used to net out seasonal changes. The second column of Fig. 5 presents these estimates, they suggest that rape increased by about 50% due to the ban.

Nonetheless, there might be further concerns that so far the identifying used variation stems from the timing of the introduction of the ban. If other factors were at play at the same time, demanding readers might think that the estimated effects are compound effects of those factors and the ban.

To tackle such concerns, I make use of three different specifications leveraging identifying variations from fines issued for the purchase of sex. Namely, fuzzy difference-in-differences, event study analysis, and IV. The fuzzy difference-in-differences framework makes use of when fines are issued. I refer to such events

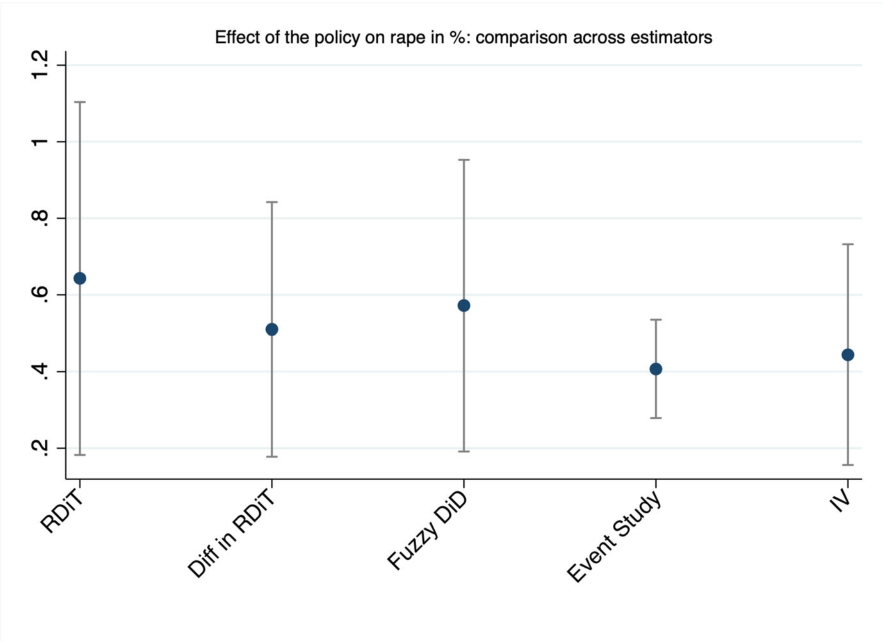

Fig. 5 Comparison of policy effect across techniques. Notes: This figure shows the estimated effect of the policy, and the respective 90% confidence intervals, of the different econometrics techniques used in this paper. Confidence intervals overlap across specifications (i.e., estimates are statically equal)

as fine events. Treatment is defined by cumulating fine events across time in each region. This analysis rests on the assumption that taking into account the number of officers working in each region, the timing of such fines is exogenous to rape. Since all regions are treated during the sample period and treatment is not sharp estimates computed via the classical difference-in-differences techniques are biased.

Fuzzy difference-in-differences techniques find that a fine event boosts rape by about 4.8%. Since between 1999 and 2014, there have been on average 13 fine events for the purchase of sex per month in each region, such estimates suggest that the ban boosted rapes by around 62.4% (column 3 of Fig. 5).

The event study analysis makes use of fine events in a different fashion. It considers a binary variable taking value 1 when fines are issued and 0 otherwise. It augments the model considering the lags and leads of such a variable. The identification assumption is the same as the one of the fuzzy difference-in-differences framework. Yet, its results might be viewed as a further robustness check since they make use only of the timing of fine events and not on the amount of fines issued.

In this case, results suggest that a fine event raises rape by around 3.4%. Using the afore-mentioned average number of fine events in the sample period, these estimates state that the ban boosted rapes by around 44.2% (column 4 of Fig. 5).

The IV analysis exploits variation given by the exact number of fines issued each month. There might be concerns that the number of fines correlates to unobservable factors linked to rape. After the introduction of the ban, destinations outside the European continent gained importance in Sweden as destinations for sex tourism (Weibull, 2003; Pruth, 2007; Manieri et al., 2013; Lu et al., 2020). To circumvent concerns on the endogeneity of fines, this variable is instrumented using variation in the number of intercontinental flights.

IV estimates point that a fine increases rape by approximately 1.9%. Since between 1999 and 2014, there have been on average 23.5 fines for the purchase of sex per month in each region, then the IV estimates suggest that the ban raised rapes by roughly 47% (column 5 of Fig. 5).

These results quantify the effect of the ban so they can be compared to those of the regression discontinuity designs. Estimates from RDiT, Diff in RDiT, IV techniques, event study analysis, and Wald–DID estimator in the fuzzy differences-in-differences setting are presented in Fig. 5. It is worth to mention that encountering that confidence intervals of each estimate overlap, and so that results are statistically equal, supports the robustness of such estimates.

Precision varies across specifications, regression discontinuity, as it might be expected, are the least precise, probably because these estimates depend on fewer observations. In addition, the considered estimators estimate a LATE, but finding that such estimates are stable across specifications that rely on different identifying variations, suggests the complier population does not differ from other subpopulations. Hence, this finding suggests that in this specifical case, these estimates are ATE.[8]

Overall, these results are in line with those encountered by other studies. Cunningham and Shah (2018) find that decriminalizing prostitution reduces reported rape offenses by 30%, whereas Bisschop et al. (2017) find that the introduction of

[8] Supplementary material S.2.5 offers further information on the size of these estimators.

street prostitution zones decreases rape offenses by 30–40%. Finally, as highlighted by Cameron et al. (2021), these findings also suggest that criminalizing prostitution might lead to larger effects in absolute value than decriminalizing prostitution.

6 Robustness of main results and plausibility of identification assumptions

This section delves into how robust RDiT results are. Supplementary material S.5 presents robustness of results for the four further estimators considered.

6.1 RDiT

6.1.1 Model specification and functional forms of the dependent variable

This section explores the robustness of the results of the RDiT specification. Column (2) of Table 3 shows the results of the main specification but takes the IHS transformation of rape instead of logs. Results are slightly larger in point size but unaltered overall considering the confidence intervals. Columns (3) and (4) of Table 3 respectively show the results for 1.5 times the optimal bandwidth in logs and IHS. Estimates are statistically positive except for the 2nd order polynomial of the restricted sample for which the point estimates are positive but marginally significant (at 12%). Columns (5) and (6) of Table 3 respectively show the results for 0.75 times the optimal bandwidth in logs and IHS. Estimates are relatively larger in point estimate and gain statistical significance across all specifications.

6.2 Identification assumption

There might be concerns that the set of control variables of the main analysis are not sufficient to take into account the variation, unrelated to the ban, but occurring when the ban is introduced. Namely, this variation is comprised of changes taking place between December 1998 and January 1999, such as alterations in meteorologic, economic, or demographic conditions. On this regard, it is important to note two issues. First, this variation might change my results only if it is correlated to the treatment variable (i.e., the introduction of the ban) and not measured by the set of controls included in specification Eq. 2. Second, economic or demographic conditions might be influenced by the ban, rendering some of such control variables as *bad controls*.[9] Hence, it is paramount to interpret these results carefully.

Thereupon, I present results using a vast set of controls measuring the above-mentioned conditions. Specifically, I add to the main specification as control variables: precipitations, female employment, male employment, female population, male population, female immigration, male immigration, married women, married men, single women, and single men. Results are presented in Table 4. Findings slightly differ

[9] Accordingly, I also present results only adding precipitations as a control variable to specification Eq. 2 Results are shown in Table C.2. Findings do not change.

Table 4 RDiT with control variables

	Logs (1)	IHS (2)	Logs (3)	IHS (4)	Logs (5)	IHS (6)
Panel A: Whole sample						
1st order polynomial						
After January 1999	0.699**	0.887**	0.814**	1.044**	0.665**	0.857**
	(0.318)	(0.401)	(0.372)	(0.474)	(0.273)	(0.349)
2nd order polynomial						
After January 1999	0.691**	0.876**	0.794**	1.018**	0.663**	0.856**
	(0.313)	(0.395)	(0.367)	(0.467)	(0.274)	(0.350)
N	1113	1113	1428	1407	903	903
Bandwidth	29	29	43	43	22	21
Panel B: Restricted sample						
1st order polynomial						
After January 1999	0.294	0.377	0.717	0.919	0.671	0.867
	(0.217)	(0.287)	(0.294)	(0.370)	(0.322)	(0.409)
2nd order polynomial						
After January 1999	0.643**	0.813*	0.537*	0.700*	0.951*	1.228*
	(0.318)	(0.415)	(0.293)	(0.368)	(0.498)	(0.632)
N	399	357	567	567	273	273
Bandwidth	9	9	14	13	7	7

Notes: Each coefficient provided in the table is estimated using a separate regression and measures the discontinuity at the cut-off, that is, after the enforcement of the regulation in January 1999. Panel A considers the whole sample period 1997–2014, while Panel B considers the restricted sample of the same length 1997–2001. Following Gelman and Imbens, results are computed using polynomials of order 1 and 2. The bandwidth used is the optimal bandwidth as defined in Calonico et al. (2014). This table adds to the main analysis the following control variables: precipitation, female employment, male employment, female population, male population, female immigration, male immigration, married women, married men, single women, and single men. Standard errors clustered at the region-month level are in parentheses. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

in size and are statistically equal to Table 3. All in all, this piece of empirical evidence backs up that results are not driven by alterations in meteorologic, economic, or demographic conditions close to the cut-off date.

As exposed in Section Supplementary material S.3 comparing the Time Corrected RDiT (hereafter, TCRDiT) to both the RDiT and Diff in RDiT estimators might be useful to check the plausibility of the identification assumptions on which these two estimators rely on. Given the regression discontinuity design, both estimators require continuity of the polynomial function, controls, and errors at the cutoff. Yet, they differ on an identification assumption that is crucial considering in this setting in which the running variable is time: evolution of time-dependent fixed effects at the cutoff. Relaxing this last assumption, it is possible to show evidence on its validity.

To this end, identification for the TCRDiT estimator does not depend on the assumption of continuity of time-dependent fixed effects at the cutoff, while it relies on the assumption that pre-cutoff time-dependent fixed effects might be used to compute the time-dependent fixed effects of the whole sample. Insofar as this last assumption seems more plausible than the former one, a comparison of TCRDiT and RDiT estimators might be used to shed light on the validity of the RDiT estimator.

In the setting of this paper, there are two time-dependent fixed effects: month fixed effects and year fixed effects. In this case, the difference between the RDiT and TCRDiT estimators is that the former assumes that both month fixed effects, in December and January, and year fixed effects, in 1999 and 1998, are equal. While, the latter merely assumes that December and January fixed effects might be estimated using the pre-intervention sample and that the 1998 year fixed effect of the sample pre-cutoff might be used to estimate the 1998 year fixed effect for the whole sample, where the 1999 year fixed effect might be normalized to zero.

To this extent, if it seems more plausible to rely on the assumptions to identify β^{TCRDiT}, then the validity of the RDiT estimator might be tested by comparing it to the TCRDiT estimator. If the time-dependent fixed effects are continuous at the cutoff β^{TCRDiT} should be close to β^{RDiT} since the change given by time-dependent fixed effect between the two estimators would be close to zero. Figure S.4 compares β^{RDiT} and β^{TCRDiT} using rape in logs and IHS (labelled *asinh*) for the main specification the 2nd order polynomial of the restricted sample. β^{TCRDiT} is always lower in point estimate and narrower in standard error than β^{RDiT}, suggesting that at the cutoff either $\alpha_m > \alpha_{m-1}$ or $\alpha_y > \alpha_{y-1}$ or both.

7 Underlying mechanisms

In this section, I use secondary data to explore the underlying mechanisms that could drive the findings of the paper. There are three mechanisms that could lead to such findings: changes in the supply of prostitution, in the demand of prostitution or a combination of both.[10]

[10] Supplementary material S.9 considers additional specifications that might be useful to shed further light on the mechanisms analyzed here.

7.1 Supply of prostitution

The introduction of the ban on the purchase of sex and, as a consequence, the fines for the purchase of sex might have affected the supply of prostitution. Given the causal evidence that decriminalizing prostitution reduces the number of rapes (Bisschop et al., 2017; Cunningham & Shah, 2018), if the introduction of such fines shifts the supply of prostitution downward, this might lead to an increment in the number of sexual offenses.

However, a priori, the effect of fines for the purchase of sex on the supply of prostitution is ambiguous. On the one hand, it could be that such fines disincentivize the sale of sex and so reduce prostitution. On the other hand, it could be that such fines incentivize the sale of sex because this law makes it clear that prostitutes will not be prosecuted.[11]

To this extent, I gather data about the number of *pimps* to proxy the supply of prostitution.[12] There are two issues worth mentioning. First, because each pimp controls many prostitutes, the number of pimps might be seen as a lower bound on the supply of prostitution. Second, I make use of the number of arrested pimps. As usual with crime variables, this variable is the outcome of an equilibrium between arrests and the offense (in this case, prostitutes/pimps). Compositional changes in such two variables, that is, changes in the number of prostitutes that work without a pimp or changes in the behavior of officers choosing whether to search for pimps, might affect the results when using this proxy insofar as they are correlated with the introduction of the ban.

Columns (5) in Tables 5 and 6 show the results of estimating respectively the RDiT and Diff in RDiT specification using as dependent variable the data on convicted pimps in logs. Results across these regression models differ in point estimate since they flip sign. Results of the RDiT specification suggest that the effect is close to zero in size. Note indeed that point estimates of the estimated coefficient in Column (5) of Table 5 are closer to zero than those in Columns (1) - (4) but are estimated with more precision (i.e., narrower standard errors). Thus, this suggests the lack of significance is not due to precision but rather to the absence of the effect. A similar reasoning might be applied to estimates from the Diff in RDiT approach, column (5) of Table 6. As a whole, regression discontinuity estimates suggest that, at least in the short-run, the ban on sex purchase did not affect the supply of prostitution proxied by the cases of arrested pimps. Supplementary material S.8.1 makes use of the different econometric techniques used in this paper to explore the effect of the ban on pimps. All in all, results suggest the ban did not impact pimps in the short-run but boosted pimps in the long-run.

It is worth noting that insofar pimps are a better proxy of coercive prostitution than of non-coercive prostitution, these results might be seen as a lower bound for the effect

[11] In particular, with respect to the years before 1999, at that time prostitution was not regulated in Sweden (Ciacci, 2021).

[12] A pimp (or procurer) denotes a person, especially a man, who controls prostitutes and arrange customers for them, usually in return for a share of the earnings. In Sweden, even if selling sex is not penalized, making money out of prostitutes (e.g., pimping) is a crime. For this reason, Brå also collects data on the number of convicted pimps. Pimping is also closely related to coercive exploitation of prostitution and human trafficking (Elrod, 2015).

of the ban on non-coercive prostitution. Namely, given that non-coercive prostitution is legal in Sweden, there is no reason to believe that the introduction of the ban on the purchase of sex discouraged this activity. In fact, because the introduction of such a ban raises awareness of prostitution, it could discourage the procuring of prostitution and, as a consequence, coercive prostitution. Thus, coercive prostitution seems to be a lower bound to non-coercive prostitution. This reasoning rules out the hypothesis that non-coercive prostitution decreased.

7.2 Demand for prostitution

Given the afore-mentioned literature finding causal evidence supporting that decriminalizing prostitution reduces cases of rape, it might be thought that since some prostitutes' customers view and use rape as an alternative to prostitution (see, among others, Farley et al. (2009); Ciacci and Sviatschi (2022); Cunningham and Shah (2021)), changes in the demand of prostitution imply changes in the sort of committed rapes. Likewise, there is empirical evidence that services connecting clients and prostitutes reduce female homicide and rape rates (Cunningham et al., 2023). Since banning the purchase of prostitution hinders the connection between clients and prostitutes, it might be posited that changes in the demand of prostitution might affect the types of committed rapes. Moreover, economic theory predicts that banning the purchase of sex changes the composition of clients towards more risk-seeking individuals (Stadtmann & Sonnabend, 2019). As a consequence, this might affect the types of committed rapes via a substitution effect. To this end, in this section to explore the effect of the ban on the demand of prostitution I rely on the following categories of rape: attempted versus completed, and indoor versus outdoor rapes.

Columns (1) to (4) of Tables 5 and 6 show the results of estimating respectively the RDiT and Diff in RDiT using data on attempted, completed, outdoor and indoor rapes. Results are not robust. They differ across specifications and samples for both RDiT and Diff in RDiT. Yet, coefficients are always positive in point size and even statistically significant in the restricted sample for the main specification (i.e., 2nd-order polynomial). These findings suggest that the demand of prostitution changed due to the policy, this is no surprise given qualitative and quantitative evidence suggesting that demand is one of the main causes behind sex work and trafficking (see, among others, Ekberg (2004); Farley et al. (2011); Jirjahn and Ottenbacher (2023)).

Nevertheless, this analysis hints at the possibility that all categories of rape rose at different times due to the policy. To this effect, it might be posited that banning the purchase of prostitution makes victims more prone to report certain categories of sex crimes. Currently and to the best of my knowledge, there is no empirical evidence supporting this notion. Assuming there is a jump in victims' reporting due to the policy, it is unclear why different categories increase in the short vs the long-run, still, given clear data limitations to test this proposition, data collection on victims' reporting behavior and further research are needed.

Supplementary material S.8.2 explores the effect of the ban using the further econometric techniques used in this paper. Taken together, results suggest that the ban

Table 5 RDiT: Mechanisms

	Attempted (1)	Completed (2)	Outside (3)	Inside (4)	Pimps (5)
Panel A: Whole sample					
1st order polynomial					
After January 1999	0.250	0.404	0.015	0.547*	0.007
	(0.253)	(0.317)	(0.278)	(0.302)	(0.130)
2nd order polynomial					
After January 1999	0.295	0.388	0.050	0.522*	−0.002
	(0.259)	(0.313)	(0.273)	(0.304)	(0.137)
N	1071	1155	1050	1155	1176
Bandwidth	26	30	26	30	32
Panel B: Prior to January 1999					
1st order polynomial					
After January 1999	0.083	0.221	0.056	0.519**	0.088
	(0.138)	(0.164)	(0.150)	(0.251)	(0.103)
2nd order polynomial					
After January 1999	0.509*	0.541**	0.232	0.453*	0.120
	(0.261)	(0.244)	(0.266)	(0.246)	(0.118)
N	315	483	315	609	567
Bandwidth	7	11	7	14	14

Notes: Each coefficient provided in the table is estimated using a separate regression and measures the discontinuity at the cut-off, that is, after the enforcement of the regulation in January 1999. Panel A considers the whole sample period 1997–2014, while Panel B considers the restricted sample of the same length 1997–2001. Following Gelman and Imbens, results are computed using polynomials of order 1 and 2. The bandwidth used is the optimal bandwidth as defined in Calonico et al. (2014). Standard errors clustered at the region-month level are in parentheses. *$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

boosted attempted, completed, and indoor rapes in the short-run, but increased completed and outdoor rapes in the long-run.

8 Policy implications

An analysis of the results of this paper as a whole offers important conclusions about the effects of the Nordic model. The introduction of a ban on the purchase of sex has the unwanted effect of changing the behavior of prostitutes' customers. Specifically, it shifts their demand outside and towards other crimes, such as rape — in particular, completed rapes. This mechanism is also aligned with considering prostitution as paid rape, if this is the case, it is no surprise that restricting the purchase of sex raises rapes and specifically, completed rapes. To this extent, it might be that customers rape prostitutes now that prostitution is more expensive. A potential channel in this case might be even not paying for sex.

Accordingly, policy implications are numerous. First, it might be debated that these results suggest that the purchase of sex should not be criminalized. This current of

Table 6 Diff in RDiT: Mechanisms

	Attempted (1)	Completed (2)	Outside (3)	Inside (4)	Pimps (5)
Panel A: Whole sample					
1st order polynomial					
After January 1999	0.556**	0.341	0.538**	0.319	−0.132
	(0.229)	(0.292)	(0.240)	(0.314)	(0.124)
2nd order polynomial					
After January 1999	0.625**	0.372	0.558**	0.323	−0.153
	(0.241)	(0.291)	(0.241)	(0.312)	(0.126)
N	1050	1155	1113	1113	1155
Bandwidth	27	31	30	30	31
Panel B: Prior to January 1999					
1st order polynomial					
After January 1999	0.269**	0.889***	0.256**	0.745***	−0.087
	(0.110)	(0.263)	(0.106)	(0.275)	(0.106)
2nd order polynomial					
After January 1999	0.582***	0.944***	0.334*	0.759***	−0.067
	(0.175)	(0.276)	(0.174)	(0.290)	(0.098)
N	441	567	483	567	567
Bandwidth	10	14	12	13	13

Notes: Each coefficient provided in the table is estimated using a separate regression and measures the discontinuity at the cut-off, that is, after the enforcement of the regulation in January 1999. Panel A considers the whole sample period 1997–2014, while Panel B considers the restricted sample of the same length 1997–2001. Following Gelman and Imbens, results are computed using polynomials of order 1 and 2. The bandwidth used is the optimal bandwidth as defined in Calonico et al. (2014). Standard errors clustered at the region-month level are in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$

thought might be motivated on the basis that if purchasing of sex is not criminalized there will be no increase in rapes. Second, it might be also debated that, to the extent that prostitution is paid rape, these results tell us that society might alter human behavior and thus, this policy needs to be accompanied by further measures targeting a potential boost in rape to prevent it. In other words, one might suspect that had this policy been accompanied by policies targeting rape as well, the results might have been different. Third, it might be posited that the rise in rape might be driven by a change in reporting by victims as well. If banning the purchase of prostitution makes victims more prone to report certain categories of sex crimes, results might be due to this behavior too. On this aspect, it is important to state that there is no empirical evidence supporting that the report of a certain sort of rape might change due to the ban. As a matter of fact, assuming that there is a rise in victim reporting due to the ban does not seem aligned with categories of rape increasing at different times. Put it differently, if the effect is led by increases in rape reporting, one would expect all categories of rape to go up both in the short and long-run. However, only completed rapes increase both in the short and long-run. Yet, given clear data limitations to test this notion, data collection on victims' reporting behavior and further research are necessary.

Additionally, this ban does not seem to reduce the supply of prostitution. In fact, I find that such a ban had no effect on pimps in the short-run, but led to a surge in the number of pimps arrested in the long-run. This result is robust across techniques and suggests that, with the introduction of the ban, prostitutes might prefer to work with pimps. A potential explanation might be that they help them to find new matches and to avoid being caught by the police. To this end, further research on these potential mechanisms is needed.

9 Conclusion

In this paper, I offer novel causal evidence on the effects of criminalizing the purchase of sex in high-income countries. Recently many countries — such as Sweden, Norway, Iceland, Canada, Israel, France, and Ireland, among others — criminalized the purchase of prostitution. Notably, I have used different causal inference techniques to estimate such effects: regression discontinuity, fuzzy difference-in-differences approaches, event study analysis, and instrumental variables. I find that criminalizing the purchase of sex increases rape and that this surge is driven by increases in attempted, completed, and indoor rapes in the short-run and in completed and outdoor rapes in the long-run.

The findings of this paper posit that this regulation increases the number of rapes by about 44–62%. The size of this effect is aligned with the findings of scholars on the effect of decriminalization of prostitution on rape (Bisschop et al., 2017; Cunningham & Shah, 2018). Furthermore, this study is one of the first to offer supporting evidence that the effects of criminalizing prostitution seem to be larger in absolute value than those of decriminalizing it Cameron et al. (2021); Cunningham and Shah (2021).

The results also suggest that this regulation did not reduce the supply of prostitution but affected the types of rapes committed. This evidence seems to support the hypothesis that the found effect is demand-driven. To this extent, further research on whether bans of this sort might quash demand is needed (Della Giusta et al., 2021). Taken together, these findings are coherent with economic theory: the introduction of a ban on the purchase of sex alters the composition of buyers towards more risk-seeking individuals (Stadtmann & Sonnabend, 2019).

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Data availibility All the data sets used in this article might be accessed publicly contacting the corresponding websites mentioned in Section 3.

Declarations

Conflict of interest The author declares no competing interests.

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