Banning the purchase of prostitution increases rape: evidence from Sweden

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Abstract

In this paper I exploit IV techniques to study the effect of banning the purchase of prostitution on rape using Swedish regional data from 1997 to 2014. Recent economic literature reported evidence on the effect of decriminalizing prostitution on rape. Yet, little is known on the effect of criminalizing prostitution on rape. This paper exploits plausibly exogenous within and across regions variation in access to sex tourism to assess the impact of banning the purchase of prostitution on rape. I find that this regulation raises rape temporarily. In particular, this regulation increased reported rape by 47% between 1999 and 2014. Moreover, my findings show that this regulation also changes the composition of rapes committed: increasing completed and outdoor rapes, and reducing attempted rapes. This empirical evidence suggests that the increment in rapes is due to a shift of the demand of prostitution, while I find no evidence supporting that such an increment is supply driven.

Keywords: Rape, sex crimes, prostitution, prostitution law, prostitution regulation, criminalizing purchase of prostitution, Nordic model, instrumental variables estimation
JEL codes: C26, J16, J47, K14

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1 Introduction

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

This paper empirically explores the effect of criminalizing the purchase of prostitution on rape using regional data from Sweden from 1997 to 2014. In particular, I estimate the effect of issued fines for sex purchase on rape. To address endogeneity issues I use an instrument exploiting variation in availability of flights (to proxy access to sex tourism).

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

Recent economic literature (Cunningham and Shah 2018; Bisschop et al. 2017) has found causal evidence that decriminalizing prostitution reduces rape. In light of this evidence, a relevant question is whether criminalizing the purchase of prostitution affects rape. Research on this topic will allow social politicians to design crime policies for rape and regulations for the prostitution market according to their objectives.

The main finding of this paper is that fines for sex purchase increase rape. These estimates are economically meaningful, and suggest that an increase of one standard deviation in fines for sex purchase boosts rape by around 15%. These estimates also suggest that criminalizing the purchase of prostitution increased reported rape by 47% between 1999 and 2014.

Next, I explore whether supply or demand are driving these results. I find evidence supporting that the effect is demand driven. I do not find any evidence in favor of a decrease of the supply of prostitution (proxied by *pimps*). While, I find important changes in the sort of rapes committed by aggressors. My findings show that banning the purchase

[1] European Union Agency for Fundamental Rights (2014)
[2] The precise figures are 1.8% of women and 0.3% of men.

of prostitution reduces attempted rape, at the expense of raising completed and outdoor rape.

This paper contributes to a growing line of research in economics that studies prostitution either theoretically (Edlund and Korn 2002; Cameron 2002; Cameron and Collins 2003; Della Giusta et al. 2009) or empirically (Cameron et al. 1999; Moffatt and Peters 2001; Gertler et al. 2005; Gertler and Shah 2007; Arunachalam and Shah 2008; Della Giusta et al. 2009; Edlund et al. 2009; Della Giusta 2010; Cunningham and Kendall 2011a,b,c; Bisschop et al. 2017; Ciacci and Sviatschi 2016; Ciacci 2017). In particular, it contributes to a strand of the literature addressing the effects of different prostitution laws on crime or health outcomes (see, inter alias, Lee and Persson (2013); Cho et al. (2013); Jakobsson and Kotsadam (2013); Berlin et al. (2019); Cameron et al. (2019)). Finally, this paper contributes to instrumental variable literature suggesting to use Oster (2017) methodology as a benchmark to compare OLS and IV estimates.

The rest of the paper is organized as follows. Section 2 presents rape, prostitution and sex tourism in Sweden. Section 3 describes the data sets used in this paper. Section 4 presents the empirical strategy. In Section 5, I present the main results of the paper. Section 6 explores the potential pathways leading to the main findings of the paper. Finally, Section 7 concludes.

2 Rape, prostitution and sex tourism in Sweden

2.1 Rape

It might be argued that Sweden has one of the widest definitions of rape (Von Hofer 2000). In 1962, a legal definition of rape was included in the Swedish Penal Code and since then, several revisions to the legal definition of rape have been made to include non-consensual sexual acts comparable to sexual intercourse. In 1965 Sweden was the first country to criminalize marital rape. While, in 2005 sexual acts with someone who is unconscious (e.g. due to intoxication or sleep) were added to the legal definition of rape.

Consequently, it is not surprising that Sweden has presented the highest number of rapes committed in Europe since the Council of Europe started the data collection of this crime. According to the criminology literature three important factors explain this feature (Von Hofer 2000). First, as explained above, legal factors: the Swedish legal definition of rape is broader compared to other European countries. Second, statistical factors: Sweden has a system of expansive offense counts and crime data is 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 means that a victim that 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. Third, substantive factors: countries with high levels of sexual equality, and low police corruption, exhibit higher propensity to report rape offenses.

2.2 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 of 1998 the Swedish Parliament discussed to criminalize the purchase of prostitution. This bill, also known as *Kvinoffrid* (women's integrity) law, combined measures to prevent both sexual harassment at work and prostitution.

Two months later criminalization of the purchase of sex became the object of a separate provision known as *Sexköpslagen* (sex purchase act) that prohibits to buy sexual services, but not to sell them. The ban became effective in January 1999 making Sweden the first country to introduce this type of regulation. More specifically, since January 1999 prostitutes' customers in Sweden face the risk of receiving a fine or up to 6 months of prison for buying sexual services. In April 2005 the provision was transferred to the Swedish Penal Code.[3]

2.3 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 Swede sex tourists (Weibull 2003; Manieri et al. 2013). Furthermore, sex tourism became a growing phenomenon in Sweden and the Parliament even discussed to ban the purchase of sex abroad (Pruth 2007).

[3]For further information see Svanström (2005).

3 Data

In this paper, I use data on the number of fines for sex purchase and rapes in short time windows (months). The data used in this paper comes from "The Swedish National Council for Crime Prevention" (also known as and hereafter, Brå). Brå is the most important institution for crime data collection in Sweden. Among other types of crime data, it collects data of crimes reported to police officers. Hence, it provides detailed information on the number of sex crimes and on the number of fines for sex purchase since the enforcement of the ban in 1999.[4]

For each of the 21 regions of Sweden, I have collected data about reported rapes and issued fines for sex purchase at monthly level between 1997 and 2014 . Figure 1 shows the number of rapes and fines for sex purchase during the sample period considered in this paper. Two features are worth highlighting. First, there is considerable variation in fines for sex purchase. Second, both variables exhibit an upward trend during the sample period.

Table 1 shows summary statistics for rapes, fines for sex purchase and pimps. Rapes are classified according to whether the sexual intercourse was completed and the place where the crime occurred.[5] This table separates statistics in three time periods. Panels A, B and C respectively display descriptive statistics for the whole sample period, 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. Data show similar patterns across the three panels. The majority of rapes are comprised by completed and indoor rapes. Furthermore, for all variables the mean is greater than the median, as illustrated by the right-skewed distribution of rape displayed in Figure 2.

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

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 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 of Swedish airports are drawn from "The Swedish Transport Agency".[6]

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

[5] Therefore, two mutually exclusive categories: completed vs attempted, and outdoor vs indoor.

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

4 Empirical strategy

4.1 Structural regression model

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

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

where r stands for region, m for month and y for year. The dependent variable is $log(1+rape_{rmy})$ since rape takes value 0 for some months in some regions, $fines_{rmy}$ is the number of fines for sex purchase issued by police officers in region r in month m and year y; α_r, α_m, α_y are respectively fixed effects for region, month and year; $\alpha_r * y$ is a region-year trend and the control variable $officers_{ry}$ is the number of police officers in region r in year y since police officers are hired by regions every year.[7] Variation comes from the different number of issued fines for sex purchase within and between regions across time.

Following the stream of the literature reporting that decriminalizing prostitution decreases rape, it seems reasonable to expect that criminalizing the purchase of prostitution has the opposite effect. However, *ex-ante* the size of the effect is not clear since the two effects might not be *symmetrical*. Even if the penalty associated to each crime considerably differs (being much higher for rape), criminalizing sex purchase increases only the penalty associated to prostitution and therefore this could push some prostitutes' customers to commit rape.[8]

In this setting there might concerns that OLS estimates are not causal due to endogeneity. To address partly this issue, regression model (1) is considerably *demanding*. It includes fixed effects at region, month and year level, plus region-year trends to capture any variation at seasonal or geographical levels.

Given selection into treatment in this setting, reverse causality and omitted variable bias seem the the main concerns connected to endogeneity of the treatment variable. Reverse causality arises from the concern that past values of rape could affect fines for sex purchase. On the one hand rape could affect fines for sex purchase via the supply of

[7] I control for the number of officers hired in each region following a strand of the literature that found that increasing officers decreases crime rate (see, inter alias, Di Tella and Schargrodsky (2004); Draca et al. (2011)). Moreover, it seems reasonable to think that the number of fines might correlate with the number of officers.

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

prostitution, a strand of the literature has found that around 60% to 70% of prostitutes have been victims of rape (Farley and Barkan 1998; Farley et al. 2004) and that rape of prostitutes rarely ends in conviction of aggressors (Anderson 2004; Sullivan 2007). Therefore, prostitutes could prefer to avoid regions which experience large numbers of rape. On the other hand rape could affect fines for sex purchase via the demand of prostitution, given the causal evidence that decriminalizing prostitution reduces rape, periods with larger rapes could be associated with periods with fewer demand of prostitution. More generally, omitted variable bias arises since I cannot control for variables that displace prostitutes and might be correlated with my treatment. Namely, the concern is that such variables would decrease fines for sex purchase and increase rape. Both these issues would cause my OLS estimates to be downward biased.[9]

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

4.2 Instrument

To deal with the issues discussed above I construct two instruments that use variation in flights to proxy access to sex tourism. According to the literature the main destinations of European sex tourists, and in particular Swedish sex tourists, are developing countries. Consequently, intercontinental flights are the main mean of transportation for Swedish sex tourists. Plausibly sex tourists seem more (less) likely to travel in months in which there is a higher (lower) supply of intercontinental (continental) flights. In effect, ideally I would like to exploit monthly boosts in the number of such flights in the main airport of the region.

I solve this issue in two steps. First, in order to locate the main airport of the region, I match each region with the closest airport in a radius of 50 km if any.[10] Second, to measure months in which there is a relatively larger supply of intercontinental flights I use as instruments the number of intercontinental (continental) flights that are one standard deviation above (below) the yearly mean of each airport.[11] This generates two sources of identifying variation. First, within-regions identifying variation arises from months in which there are relatively many (few) intercontinental (continental) flights and the num-

[9]Yet, since my OLS estimates are positive, reverse causality and omitted variable bias imply that the population regression coefficient is larger than OLS estimates. Appendix Section D addresses reverse causality.

[10]In Section 4.3 I show evidence on the robustness of the chosen distance. Furthermore, Appendix Table A.1 shows the closest airport to each county.

[11]Distances from the region to the airport are computed using Google Maps.

ber of such flights. Second, between-regions identifying variation is due to the distance between each region and the closest airport. Formally, fines for sex purchase are instrumented with:

$$z_{1rmy} = IC.\,flights_{rmy} * \mathbb{I}\left(IC.\,flights_{rmy} > \mu_{1ry} + \sigma_{1ry}\right) \tag{2}$$

$$z_{2rmy} = C.\,flights_{rmy} * \mathbb{I}\left(C.\,flights_{rmy} < \mu_{2ry} + \sigma_{2ry}\right) \tag{3}$$

where $IC.\,flights$ and $C.\,flights$ respectively stand for intercontinental flights and continental flights, $\mathbb{I}()$ is the identity function, μ_{1ry} and σ_{1ry} stand for the yearly average and standard deviation of intercontinental flights and μ_{2ry} and σ_{2ry} stand for the yearly average and standard deviation of continental flights. Appendix Section A presents descriptive statistics and figures on the instruments.

4.2.1 Identification assumption

The key identifying assumption is that variation in the offering of (inter)continental flights must be independent of rape and fines for sex purchase patterns. In other words, the choice of flight companies to offer relatively more intercontinental flights does not depend on any reason connected to rape or fines for sex purchase. This seems plausible since there is no evidence of flight companies that choose to offer more flights due to any reason connected to crime patterns.

Indeed, variation in flights seems to be a good instrument in this setting. First (*exogeneity*), as discussed above, flights are plausibly randomly assigned with respect to rape and fines for sex purchase.[12] Second (*exclusion restriction*), it does not seem plausible that they can affect rape in other ways than via prostitution. Sex tourism exploits differences in the regulation of prostitution across countries. So prostitutes' customers travel to other countries where prostitution is more tolerated, or even legal. Yet, to the best of my knowledge, this is not the case for rape since this crime is neither legal nor tolerated in any country. Hence, there is no reason to believe that the offering of flights could directly affect rape.[13] Third (*relevant instrument*), this instrument affects the (potential) endogenous regressor: fines for sex purchase are less likely to occur in months with sex tourism.[14] In other words, increases in the *relative* offering of intercontinental flights in a given region should

[12] Appendix Section B empirically explores the plausibility of this assumption and finds no evidence supporting it.

[13] Section 5.3 offers empirical evidence on this issue.

[14] Section 4.3 addresses this issue.

decrease fines for sex purchase.[15] Furthermore, since the main destination of Swedish sex tourists are developing countries, if these instruments are proxing access to sex tourism the effect of z_{1rmy} should be larger in absolute value than that of z_{2rmy}. An assumption I can easily test in the first stage regression.[16]

4.3 First stage results and robustness

I claim that variation in *relative* offering of intercontinental flights is a good proxy for access to sex tourism. I cannot directly test this hypothesis since, to the best of my knowledge, data on sex tourism in Sweden do not exist. However I can use robustness tests and randomization inference to check how likely is that the instrument is strongly correlated to the endogenous regressor by chance.[17]

Table 2 reports first stage results. Each column reports the results of a different regression. The table is divided in three panels. Panel A reports results using z_{1rmy} and z_{2rmy}, while Panel B and C test the robustness of these results changing the distance of the included airport. All regressions control for the number of police officers and have year, month and region fixed effects, region year trends and clustered standard errors at regional level.

Column (1) of Panel A reports the results for the main specification. As expected, the coefficients associated to z_{1rmy} and z_{2rmy} are negative. Moreover, they are strongly significant: Kleibergen and Papp F-stat (hereafter, KP F-stat) is around 78. The last row of Panel A reports the p-value associated to the null hypothesis that the coefficient associated to z_{1rmy} is larger than that associated to z_{2rmy}. It is encouraging to find that this p-value is lower than 0.01. Since in the sample there are 21 regions and so 21 clusters, column (2) of Panel A reports the results of the same regression model as in column (1) but using wild-cluster bootstrap (Cameron et al. 2008). Results barely change.

There could be the concern that these results depend on the use of the two instruments. To tackle this issue I report results separately for z_{1rmy}, z_{2rmy} and the sum of the two (i.e. $z_{1rmy} + z_{2rmy}$). Columns (3) and (5) respectively use either only z_{1rmy} or z_{2rmy}. Whereas, columns (4) and (6) run the same regressions using wild-cluster bootstrap. Columns (7)

[15] *Relative* offering of intercontinental flights means offering of intercontinental flights with respect to offering of continental flights. Therefore, large *relative* offering of intercontinental flights means large z_{1rmy} and z_{2rmy}. This is a monotonicity assumption. In view of this assumption, it is easy to embed this analysis in a Local Average Treatment Effect (LATE) framework.

[16] There might be the concern that the identifying variation might correlate with seasonality since airlines decide their flight destinations based on months of the year. Appendix Section C tackles this issue.

[17] As for randomization inference, in order to assess the robustness of the first-stage I randomize the instruments across different time periods. Specifically, this exercise is useful as a further robustness check to test whether the instruments are strongly correlated with fines for sex purchase.

and (8) use as an instrument $z_{1rmy} + z_{2rmy}$, the former using standard clustered standard errors at regional level, the latter using wild-cluster bootstrap. Panel B and C repeat the same analysis changing the distance of the airport radius to either 40 km or 60 km. It is reassuring to find that results are stable across each regression of these three panels. In particular, coefficients are statistically negative and KP F-stats support the instruments are relevant.

There could be the concern that the instruments do not proxy access to sex tourism. it might be that they are simply proxing a general effect of flights on fines for sex purchase in the first stage regression and on rape in the reduced form one. To this extent, it is worth mentioning I make use of variation in relative offerings of intercontinental flights since there is a strand of the literature suggesting that inercontinental destinations are the most popular for sex tourisms organized by Swedish.

Hence, to tackle this concern, I use variation of flights that, to the best of my knowledge, are not connected to sex tourism. As a consequence, such flights should not affect either fines for sex purchase or rape. Namely, I use the number of national flights, european flights and total flights.

Table 3 shows the results of the afore-mentioned specifications. z_{3rmy}, z_{4rmy} and z_{5rmy} respectively stand for national flights, european flights and total flights. I find no evidence that these flights affect either fines for sex purchase or rape. Note that since the number of total flights encompasses the number of international flights I might expect this variable to have a negative associated coefficient.

Next, I turn to randomization inference. Figures 4 and 5 respectively present the results of randomizing z_{1rmy} and z_{2rmy} stratified at (larger) regions and time period level with 1,000 permutations.[18] The red vertical line depicts the estimated coefficient in the main specification. The intersection between the red vertical line and the estimated distribution could be interpreted as the probability of finding by chance an estimated coefficient as large as the estimated coefficient of the main regression. Simply put, these p-values measure the probability that, under the null hypothesis of no effect of each instrument, the estimating bias is sufficiently large to explain the size of the estimated coefficient. Figure 4 and 5 show that this probability is extremely low. In the former 1 regression out of 1,000 could replicate such estimate, in the latter only 8 regressions out of 1,000 could replicate such estimate. Furthermore, there could be the concern that variation in my instruments is highly correlated across space and time (i.e. some geographical areas have larger airports and so relatively more intercontinental flights and/or this variation may take place in the same months/season). Randomization inference is useful to

[18]Following the Brå division, Sweden is geographically sub-divided into 6 larger regions.

shed light on this issue as well.

5 Main results

5.1 OLS vs IV results

Table 4 compares OLS and IV results. IV are computed instrumenting $fines_{rmy}$ in equation (1) with z_{1rmy} and z_{2rmy}. Columns (1) and (2) compare OLS and IV results clustering variance at region level and including region FE, region-year trends and controlling for officers. Columns (3) and (4), and columns (5) and (6) respectively add year FE and month FE.

OLS estimates become larger in size and gain significance as controls are added. This pattern supports that there could be confounding factors negatively correlated with the main regressor (in line with 4.1).

Column (6) presents the results of the main specification. In line with the above-mentioned pattern, it is not surprising to find that IV estimates are about an order of magnitude larger than OLS. These results point out that issuing fines for sex purchase boosts rape. Moreover these results are economically meaningful, an increase of one standard deviation of the main regressor increases rape by 15%.[19] With respect to the baseline mean of the dependent variable this coefficient means that an increase of one standard deviation of the main regressor brings about almost one extra rape. In other words, given the baseline mean of the dependent variable (6.16), an increase of one standard deviation of fines for sex purchase increases rape by roughly one unit.

Appendix Section E finds that this effect is temporary and takes place only in the very same month in which fines are issued. Since between 1999 and 2014 there have been on average 23.5 fines for sex purchase per month in each region, this suggests criminalizing the purchase of prostitution raises rape by roughly 47%. These results are in line with those encountered by scholars. Cunningham and Shah (2018) finds that decriminalizing prostitution reduces rape by 30%, whereasBisschop et al. (2017) finds that street prostitution zones decreases rape by 30-40%. Finally, as highlighted in Cameron et al. (2019), these findings also suggest that criminalizing prostitution might bring about larger effects in absolute value than decriminalizing prostitution.

Taking into account the main specification results (i.e. column (5) vs column(6)), IV estimates are about 15 times larger than OLS. There are four main reasons why this could

[19]Note that issuing a fine increases rape by 2%. Precisely, $\frac{\partial \log(y)}{\partial x} = \frac{\partial \log(1+y)}{\partial x} \frac{\partial \log(y)}{\partial \log(1+y)} = \beta \frac{1+y}{y} \simeq \hat{\beta} \frac{1+\bar{y}}{\bar{y}} = 1.89\% \frac{1+11.38}{11.38} = 2\%$

happen. First, it might be that the instruments are weak. Second, it might be that the exclusion restriction is violated. Third, it might be due to endogeneity: reverse causality/confounding factors correlated with the endogenous regressor. Fourth, since IV is local, it might be that compliers are more sensitive to changes in fines for sex purchase to commit rape.

Except for the first reason: instrument relevance, the other three reasons are untestable. However, this paper deals with each one of them. Section 4.3 shows that instruments are strongly correlated with the endogenous regressor. Section 5.3 handles the exclusion restriction and finds no evidence supporting any violation of such an hypothesis. Appendix Section D finds evidence in favor of reverse causality. Lastly, Section 5.4 compares OLS and IV estimates in two ways. First, it makes use of the methodology developed in Oster (2017), and finds evidence supporting the plausibility of IV estimates. Second, this section also explores whether there is evidence that the IV estimates differ from OLS since their averaging the causal effect only for the compliers.

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

This section shows that results are robust to changes to: instruments, regression model and functional form of the dependent variable. This section addresses this issue separately. First, it provides evidence that such results are robust across changes to the instruments and regression model. Second, it explores whether such results are robust to changes in the functional forms of the dependent variable.

Table 5 reports IV results using different instruments. Panel A displays regression outcomes for the instruments used in the main specification. As a matter of fact, column (1) reports results of the main specification (i.e. instrumenting $fines_{rmy}$ in equation (1) with z_{1rmy} and z_{2rmy}) for ease of comparison. While, columns (2) and (3) respectively show results of using only z_{1rmy} or z_{2rmy} as instruments. Lastly, column (3) displays results of using $z_{1rmy} + z_{2rmy}$ as instrument. This analysis is in line with table 2.

Results are stable across columns. In particular, it is easy to see that estimated coefficients are always closer than one-standard-error distance to the estimated coefficient of my main specification (i.e. Panel A, column (1)). Moreover, as expected the point estimate of the estimated coefficient using z_{1rmy} is larger than the one using z_{2rmy}.

Panel B and C repeat the same analysis changing the distance of the airport radius to either 40 km or 60 km. Also in this case, estimated coefficients are statistically equal to the main specification one. These findings suggest that results are robust across regression models and changes in instruments.

Finally, Table 6 presents regression results using, as functional form of the dependent variable, the inverse hyperbolic sine (hereafter, IHS) transformation.[20] Columns (1),(2), and (3) respectively use these two instruments with a distance of 50 km, 40 km and 60 km. While, column (4) uses the sum of the two instruments as one instrument.[21] Results are stable across regressions.

5.3 Exclusion restriction

There might be the concern that the exclusion restriction is not valid. In other words, there might be concerns that variation in offering of flights directly affects rape. This hypothesis looks unlikely since, as I explained previously, sex tourism is a direct alternative to prostitution and not to rape.

This identification strategy rests on the assumption that variation in offering of flights affects rape only through fines for sex purchase (i.e. demand of prostitution). This is tantamount to stating that sex tourism is an alternative to sex purchase and affects rape only via its effect on sex purchase.

Testing the credibility of the exclusion restriction requires deep knowledge of the subject matter. In this setting it might be argued that it is possible to test the plausibility of such assumption using data prior and posterior to the introduction of the ban. In effect, if the instruments only affect rape via fines for sex purchase, the effect of the instruments on rape should be weaker when there were no fines for sex purchase since the ban was not effective. On the other hand, if the instruments directly affect rape there is no reason to believe that their effect on rape should be less strong before the introduction of the ban.

Table 7 tests this assumption comparing reduced form estimated coefficients prior and posterior to the introduction of the ban. Columns (1) to (3) present the results before the introduction of the ban. As in table 4, each column respectively adds year fixed effects and region-year trends. Columns (4) to (6) present results in the same fashion but for the period after the introduction of the ban.

It is heartening to find that prior to the introduction of the ban no estimated coefficient is statistically negative in any regression. As a matter of fact, the estimated coefficient associated with z_{2rmy} is even positive. After the introduction of the ban both estimated coefficients are statistically negative in the three regressions. This evidence suggests that

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

[21]A table with the same format as Table 5 but IHS is available upon request. Also in this case in each regression the estimated coefficients are not statistically different from the main regression one.

the effect of the instruments on rape changed with the introduction of the ban, and as a consequence, the introduction of fines for sex purchase.

Since the coefficient associated to z_{1rmy} of Table 7 is negative but statistically equal to zero, there might be the concern that this lack of significance is merely due to lack of precision since the number of observations available before the ban are fewer than those available after the ban. To this end, I focus on the period after the ban and define a cumulative variable for fines by region. This variable cumulates the number of fines for each region as time elapses. Next, I separate the sample in data above and below the median of the cumulative variable to have roughly the same amount of observations.

It seems plausible to believe that cumulative fines proxy both prostitution and how strict the police enforces the ban. According to the exclusion restriction, the effect on rape should be larger in absolute value whenever the law is enforced stricter and prostitution is more difficult to purchase. Note that this analysis leverages variation during the ban while the previous one was using variation before vs after the ban.

Table 8 shows the results of these reduced form regressions. Columns (1) to (3) show results below the median, whereas columns (4) to (6) display results above the median. It is encouraging to find that coefficients below the median are positive, while coefficients above the median are negative as the exclusion restriction would suggest.

As a whole, this evidence supports the exclusion restriction: it seems the instruments affect rape differently depending on the introduction of the ban and, after its introduction, depending on how difficult buying prostitution is. This evidence supports that the instruments affect rape only through fines for sex purchase.

5.4 Size IV estimates

A simple comparison of the ratio between OLS and IV estimates could raise concerns that IV estimates are far too large than the OLS. To tackle this issue, I use a new methodology suggested by Oster (2017) and adapted to an IV framework in Ciacci et al. (2019). Oster (2017) develops a methodology that makes use of changes in the estimated coefficient and R^2, as controls are included, to test for omitted variable bias. The afore-mentioned paper shows that, if selection on observables is proportional to selection on unobservables, then one can compute an estimated coefficient taking into account omitted variable bias. The set bounded between such estimated coefficient and the OLS estimates is the set of values of the coefficient that could be explained given omitted variable bias. Intuitively this set spans all the "true" values of the treatment effect given omitted variable bias.

It is a common praxis to comment the size of IV estimates compared to OLS. To the best of my knowledge, usually this comparison is carried out using "subjective" rules of thumb comparing the relative size of both coefficients. In effect such rules of thumb do not take into account information on coefficient movements as controls are included nor movements in R^2. Yet there is no paper suggesting a method to quantify such size.

When endogeneity boils down to omitted variable bias, Oster (2017)'s identified set is a valid benchmark for OLS vs IV estimates comparison since it includes all the values of the estimated coefficient that could be driven by this sort of bias. In other words, if the IV estimates are in this set they are not too far from the OLS estimates. In addition, since Oster (2017) establishes that for each estimated coefficient there is a single coefficient of proportionality between selection on observables and selection on unobservables, computing the coefficient of proportionality corresponding to the IV estimates yields an objective benchmark about the size of omitted variable bias needed to produce such result using OLS.

In order to compute this set one needs to have a prior belief about the sign and size of proportionality between selection on observables and unobservables, in the setting studied in this paper the main concern is that $Cov(fines_{rmy}, \varepsilon_{rmy}) < 0$, or following Oster (2017)'s notation, denoting the set of controls with W_1 and the omitted variable causing the endogeneity with W_2:

$$\delta \frac{Cov(fines_{rmy}, W_1)}{Var(W_1)} = \frac{Cov(fines_{rmy}, W_2)}{Var(W_2)}$$

for some $\delta \leq -1$. Hence, I evaluate negative coefficients of proportionality.[22]

I compute Oster (2017)'s estimated coefficient taking into account omitted variable bias for different negative values of the coefficient of proportionality (denoted by δ). In each case I get values larger than the OLS estimates, hence upper-bounds of Oster (2017)'s identified set.

Figure 6 shows the estimated upper-bounds of the identified set as a function of the coefficient of proportionality δ. Note the lower-bound of the identified set is the OLS estimate of structural specification (1) (i.e. Column (5) of Table 4). On the vertical axis of Figure 6 there is δ, on the horizontal axis there is the estimated coefficient, upper-bound of the identified set, associated to each delta. The vertical red line is the IV estimated coefficient.

This figure shows that a low δ, such as -1.17, is associated with an identified set including the IV estimated coefficient. Therefore, the IV estimates fall into any identified set associated with $\delta \leq -1.17$. To put it simply, it suffices that selection on unobservables is

[22]Note in the data set $Cov(fines_{rmy}, police_{ry}) > 0$.

20% larger than selection on observables for the IV estimate of the main specification to lie in the identified set. This evidence supports that IV estimates are not too large compared to OLS estimates.

There might be the further concern that OLS and IV estimates differ from each other because of effect heterogeneity. If this is the case, insofar as the effect is greater in magnitude for the subgroup of the compliers, IV estimates would be different from OLS simply because the former are averaging the causal effect for the compliers and not for the entire population as the latter. To shed light on this issue I carry out an analysis similar to Bhuller et al. (2020).

First, I use data in 1997 and 1998 to cumulate the number of pimps arrested in each region. The aim of this variable is to proxy prostitution prior to the introduction of the ban. I then create an indicator variable taking value one if pimps were arrested in those years and zero otherwise. Second, I cumulate the number of fines during the ban in each region. Hence, by splitting the sample in below and above the median of this variable, I can divide the total observations in two subsamples of similar size.

If the IV estimates differ from OLS ones since effects are heterogeneous across the population and the are former averaging only the effect for the share of population with larger effects, it would make sense to think that such effects would be larger for regions with higher prostitution known by the police where the risk to get a fine is higher. Similarly, such effects should be higher in regions where the police issues many fines for prostitution, either because there is higher demand or because the police is more "capable" to catch customers of prostitutes. Hence, I would expect to find that the first stage results are statistically negative and larger, in absolute value, in regions that are either above the median of cumulated fines or that exhibited higher levels of prostitution prior to the ban. By exploring the first-stage results by subgroups I can check whether there is evidence supporting that the subgroups were defined correctly.

The last column of Table 9 shows the results of the first stage regression separated for regions either below or above the median. As expected results for regions above the median are statistically negative and larger, in absolute value, than those for regions above the median. This result suggests that this division of the sample makes sense and there are heterogeneous effects across regions.

Likewise, the last row of Table 9 shows the results of the first stage regression separated for regions with lower and higher prostitution. Also in this case the instruments have stastically negative effects on fines for sex purchase in regions with more prostitution, while the point estimates are positive and statistically zero for regions with low prostitution.

The inner part of Table 9 shows results of the first stage regression for each sub-sample combining the two categories above discussed. For the sample below the median we find that the the effect of the second instrument is statistically negative in the sub-sample where prostitution is high. While for the sample above the median the first instrument is statistically negative in the sub-sample where prostitution is high and the second instrument has a negative point estimate.

As a whole, these results suggest that there might be effect heterogeneity and that these divisions splitted the sample in four subcategories where the effect might differ. Thereby, based on the first-stage analysis, Table 10 presents results for the structural and reduced form equation re-weighted such that the proportion of compliers in each subgroup matches the share of estimation sample for that subgroup. Results for both structural and reduced form equation are similar in sign and size to those for the unweighted sample. This finding suggests that the difference between IV and OLS estimates cannot be explained by heterogeneous effects due to observables.

6 Underlying mechanisms

This section uses secondary data to explore the underlying mechanisms that could drive the findings of the paper. Namely, there are two mechanisms that could lead to the found results: fines for the purchasing of sexual services might affect rape either via demand of prostitution or via supply of prostitution.[23]

6.1 Supply of prostitution

Shifts of the supply of prostitution could affect both fines for the purchasing of sexual services and rape. As a matter of fact, given the causal evidence that decriminalizing prostitution reduces rape (Bisschop et al. 2017; Cunningham and Shah 2018), a downward shift of the supply of prostitution could affect fines for the purchasing of sexual services and, as a consequence, could boost rape.

However, a priori the effect of fines for the purchasing of sexual services on the supply of prostitution is unclear. 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 since this law makes clear that prostitutes are not going to be prosecuted.[24] Given the negative causal relationship between rape and prostitution men-

[23] Appendix Section F considers additional specifications that might be useful to shed further light on the mechanisms here analyzed.

[24] Especially with respect to before 1999, at that time prostitution was not regulated in Sweden.

tioned above and the findings of this paper, it seems reasonable to expect that banning the purchase of prostitution might have shifted downward the supply of prostitution .

To shed light on this issue, I gather data about the number of *pimps* to proxy the supply of prostitution.[25] There are two issues worth mentioning. First, since each pimp controls many prostitutes, the number of pimps might be seen as a lower bound of the supply of prostitution. Second, I make use of the number of arrested pimps. This variable is the outcome of an equilibrium between arrests and the prostitution market. Compositional changes in such two variables (e.g. changes in the number of prostitutes that work without a pimp or in the behavior of officers choosing whether to search for pimps) might affect the results using this proxy.

Figure 7 presents the estimated coefficients, and respective 90% confidence intervals, of running the main IV specification, with pimps as dependent variable, using either both instruments (main first stage regression), only z_{1rmy}, only z_{2rmy} or their sum (i.e. $z_{1rmy} + z_{2rmy}$).[26] Estimates are statistically positive and range between 0.05 and 0.06. This evidence suggests that the introduction of fines for sex purchase raises convicted pimps. Hence, these results do not support a decline in the supply of prostitution. It is worth noting that pimps are a better proxy of coercive prostitution than of non-coercive prostitution. Given that non-coercive prostitution is legal in Sweden, there is no reason to believe that the introduction of the ban discourages such activity. While, since the introduction of ban raises awareness on prostitution, it could discourage procuring, and as a consequence, coercive prostitution. Hence, coercive prostitution seems to be a lower bound to non-coercive prostitution.

6.2 Demand of prostitution

Using as dependent variable a proxy of the demand of prostitution would violate the exclusion restriction since the instruments affect the behavior of prostitutes' customers. Hence in this section, in order to assess whether fines for sex purchase might shift the demand of prostitution, I explore changes in the composition of rapes.

Given the causal evidence that decriminalizing prostitution reduces rape found in the afore-mentioned literature, economic theory would predict that changes in the demand of prostitution could affect the types of rape committed. In this section I explore the effect

[25] Pimp (or procurer) means a person, especially a man, who controls prostitutes and arrange customers for them, usually in return for a share of the earnings. In Sweden even if selling sex is not penalized, making money out of prostitutes, such as *pimping* is a crime. For this reason, Brå also collects data on the number of convicted pimps.

[26] Exclusion restriction and exogeneity of the instruments seem plausible also for this dependent variable. Recall the instruments *work* through customers and so the demand of prostitution.

of fines for the purchasing of sexual services on attempted vs completed rapes and indoor vs outdoor rapes.

Figure 8 shows the estimated coefficients, and respective 90% confidence intervals, of running the main IV specification for attempted and completed rape using either both instruments (main first stage regression), only z_{1rmy}, only z_{2rmy} or their sum (i.e. $z_{1rmy} + z_{2rmy}$).

Findings are stable across regression models. Fines for sex purchase reduce attempted rapes but increase completed rapes. This evidence supports that fines for sex purchase affect the demand of prostitution.

Two reasonings could justify the increase in completed rape with respect to attempted rape. First, given the causal evidence that prostitution reduces rape, but the higher penalty for the latter than for the former, economic theory would predict that increasing the relative price of prostitution would raise the *consumption* of rape. This would explain why completed rapes went up at the expense of a decline in attempted rapes.

Second, a branch of the literature of evolutionary biology and evolutionary psychology predicts that when consensual sex becomes more difficult (i.e. competition for females is most intense) completed rape increases since it is an adaptive strategy in past human environments (Thornhill and Thornhill 1983; Thornhill and Palmer 2000a,b).

Likewise, Figure 9 shows the estimated coefficients, and respective 90% confidence intervals, of running the main IV specification for indoor and outdoor rape using either both instruments (main first stage regression), only z_{1rmy}, only z_{2rmy} or their sum (i.e. $z_{1rmy} + z_{2rmy}$). This figure shows that fines for sex purchase led to an increase in outdoor rape, while they did not affect indoor rape.[27]

All in all, from both figures it is clear that the composition of rapes changed. This evidence is in line with the hypothesis that banning the purchase of prostitution affects the demand of prostitution.

7 Conclusion

This article leverages variation in access to sex tourism to estimate the causal effect of criminalizing the purchase of prostitution on rape in Sweden. It finds that criminalizing

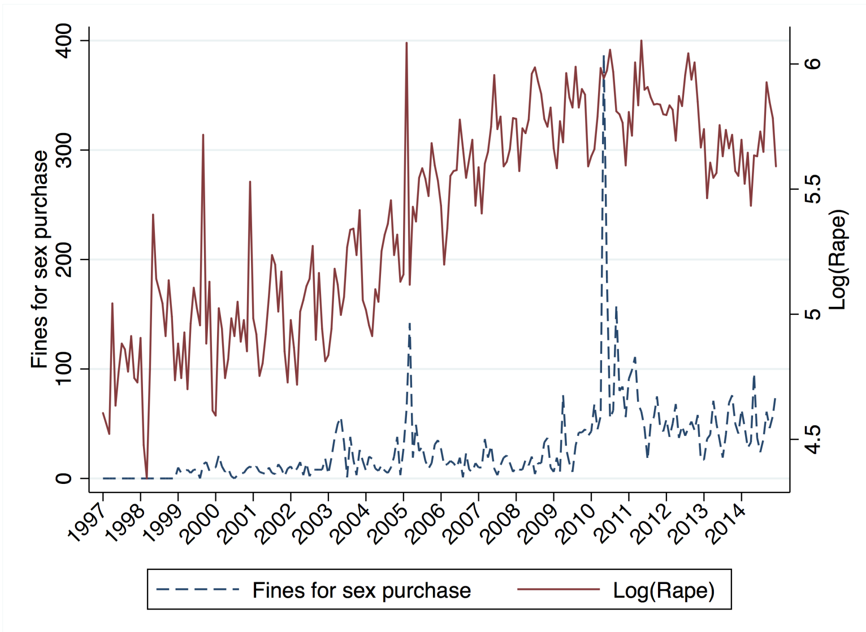
[27] This result differs from (Berlin et al. 2019). It might be that this discrepancy is due to the distinct source of identifying variation used. Their paper leverages variation in the share of female politicians and police officers to proxy stricter enforcement of the ban against sex purchase. It might be that these main regressors also correlate with stricter enforcement of the Kvinnofrid law (which encompasses rape as well). Yet, as a whole, their results are coherent with the findings of this paper. They find that banning the purchase of prostitution might have reduced the size of the prostitution market but had important spillover effects on crimes outside the prostitution market, namely on domestic violence.

the purchase of prostitution increases rape temporarily.

Specifically, the findings of this paper suggest this regulation rose rape by 47 % from 1999 to 2014. Results also indicates that this regulation changes the composition of rapes committed: increasing completed and outdoor rapes. This evidence supports that the found effect is demand-driven. Lastly, to the best of my knowledge, this paper is one of the first to suggest usage of a comprehensive methodology such as the one developed in (Oster (2017)) to compare sizes of OLS and IV estimates.

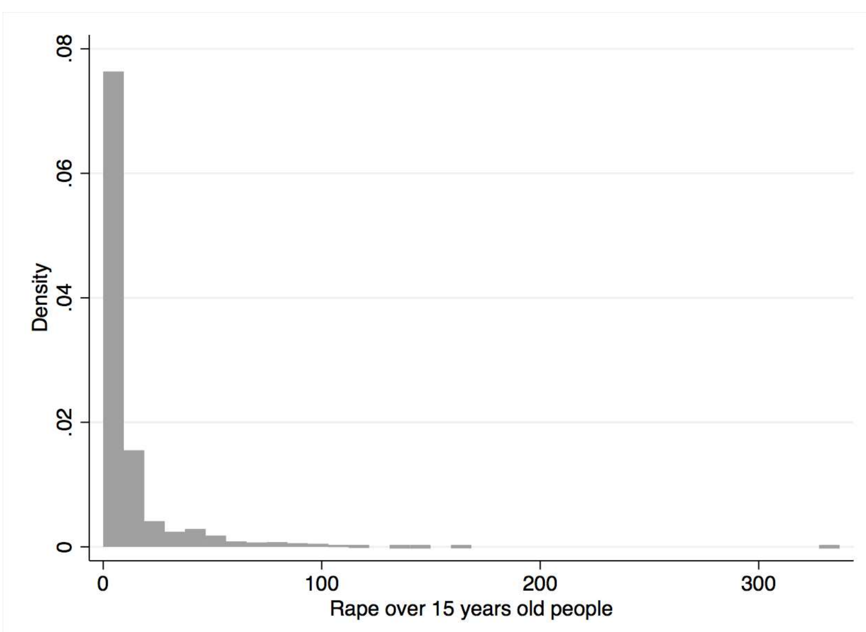
Figures & Tables

Figure 1: Evolution of fines for sex purchase and rape in Sweden



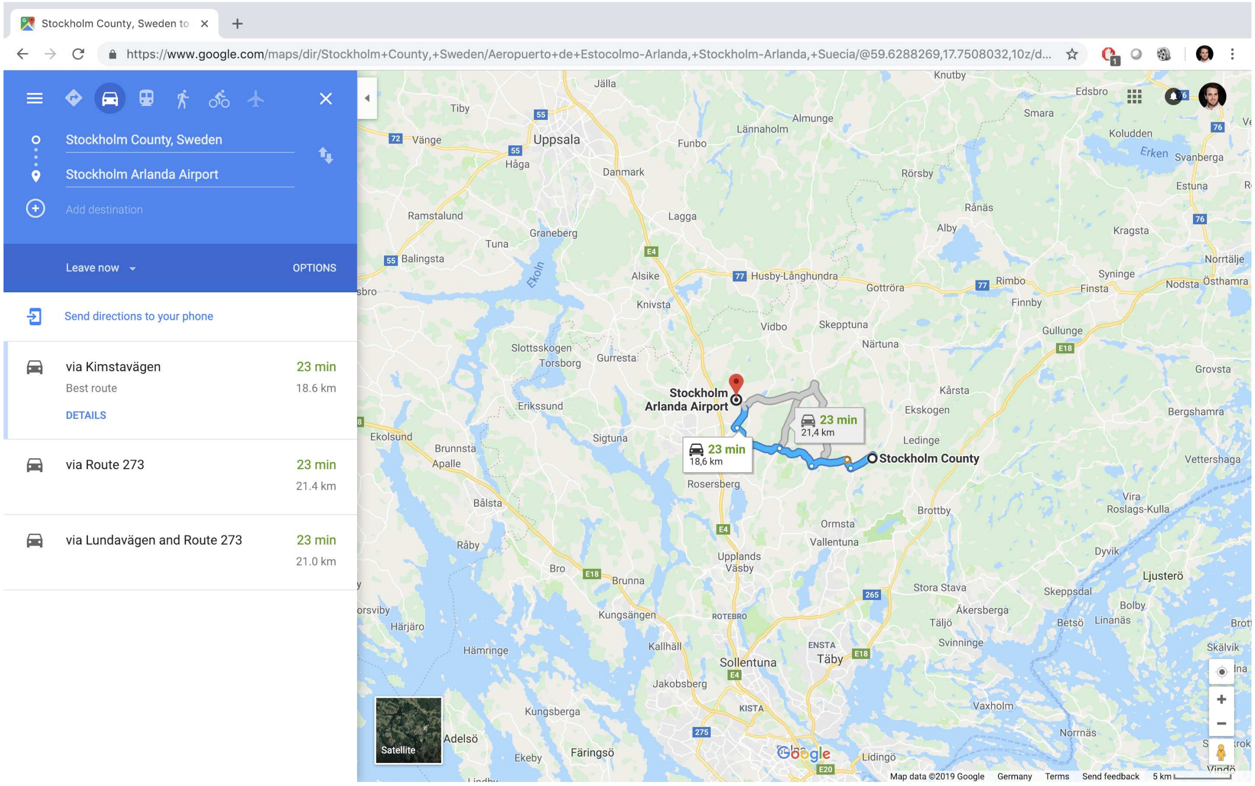
Notes: This figure shows the number of rapes (in logs) and fines for sex purchase in Sweden according to Brå during the period 1997-2014.

Figure 2: Distribution of rape



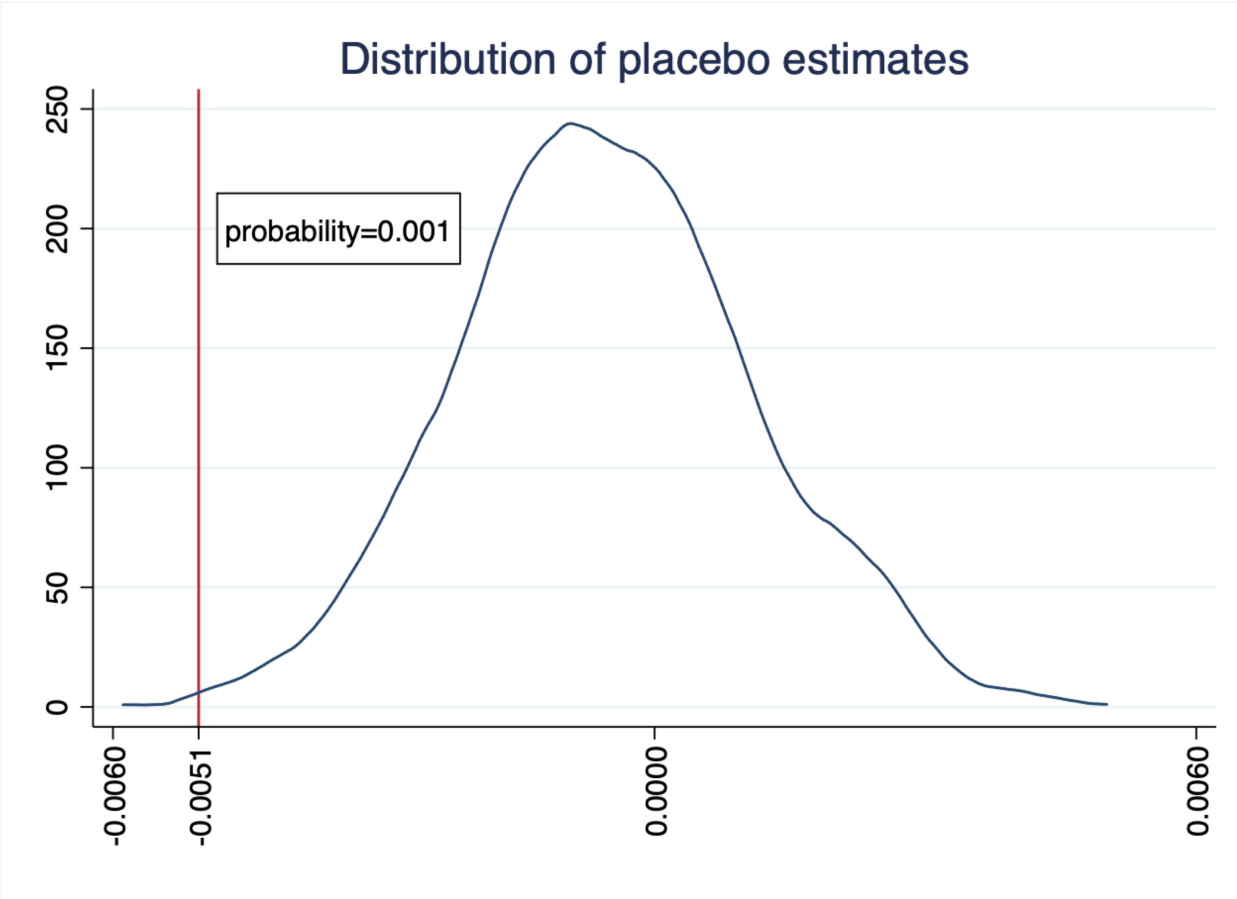
Notes: Histogram of rapes in Sweden according to Brå during the period 1997-2014.

Figure 3: Airport-region distance by car using Google maps, example



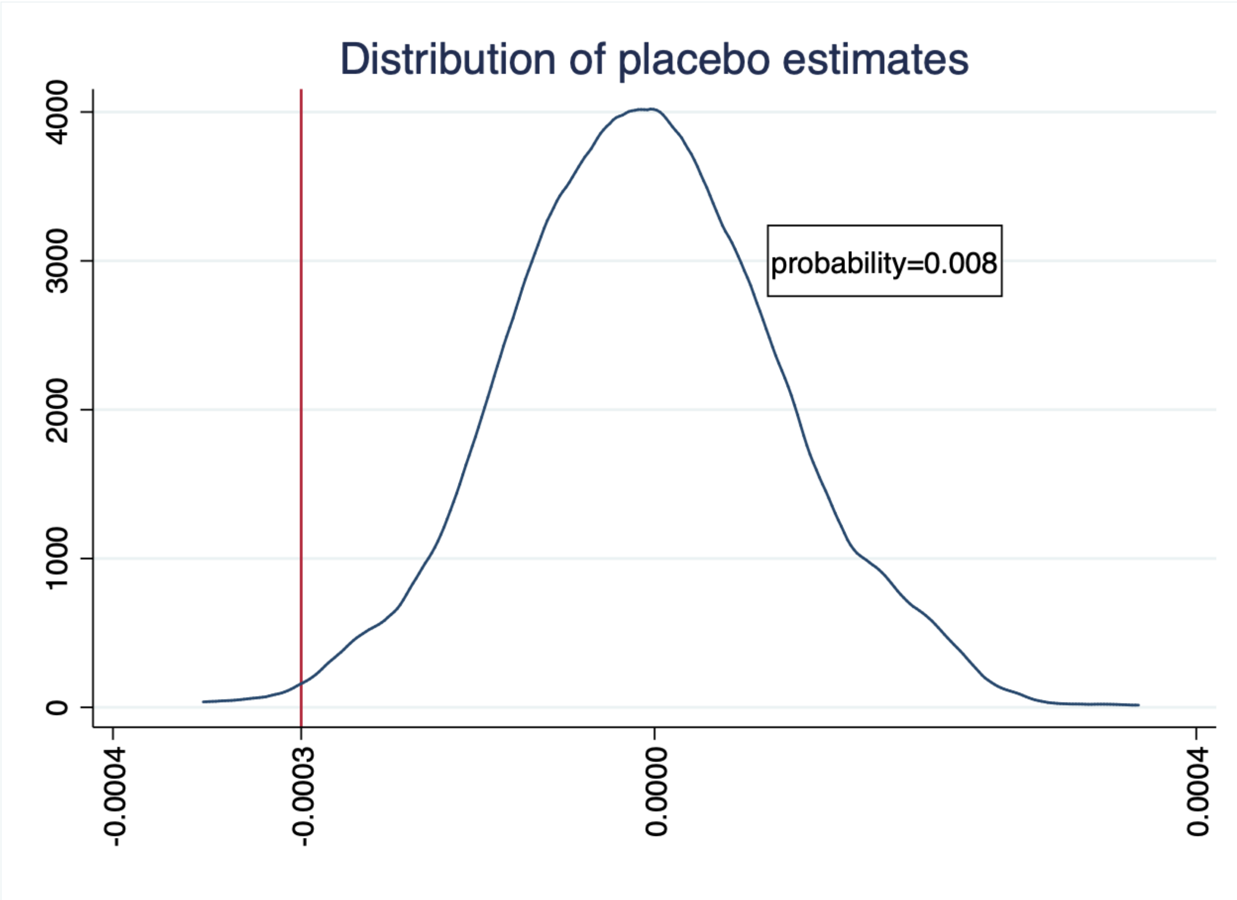
Notes: Distance from the closest airport to the region computed via Google maps and using car vehicle option. Example: Stockholm county.

Figure 4: First-stage placebo test: randomization inference z_{1rmy}



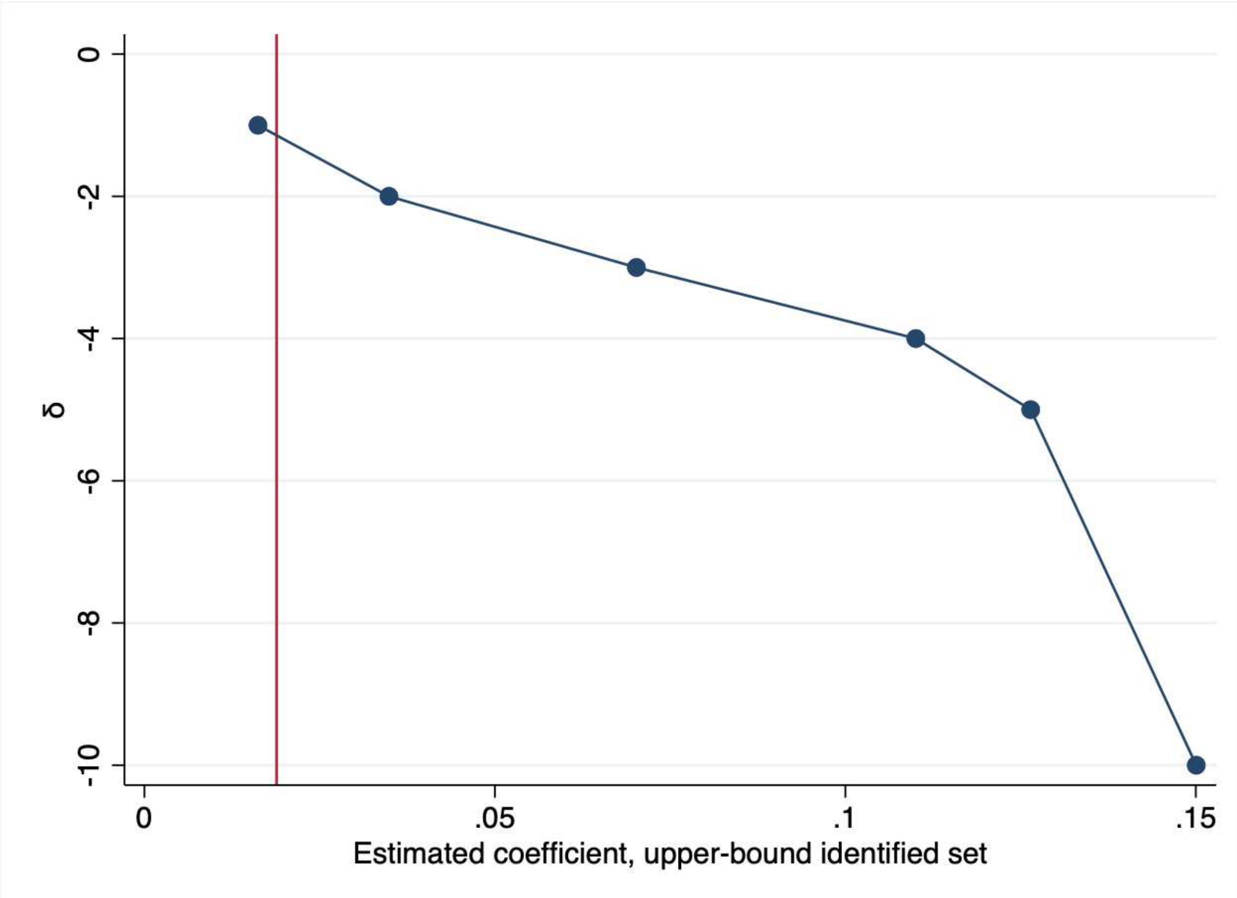
Notes: results of randomizing z_{1rmy} stratified at time period level with 1,000 permutations. The red vertical line represents the estimated coefficient of the main specification. The intersection between the red vertical line and the estimated distribution could be interpreted as the probability of finding an estimated coefficient as large as my estimates by chance. Only 1 regression, out of 1,000, could replicate such estimate.

Figure 5: First-stage placebo test: randomization inference z_{2rmy}



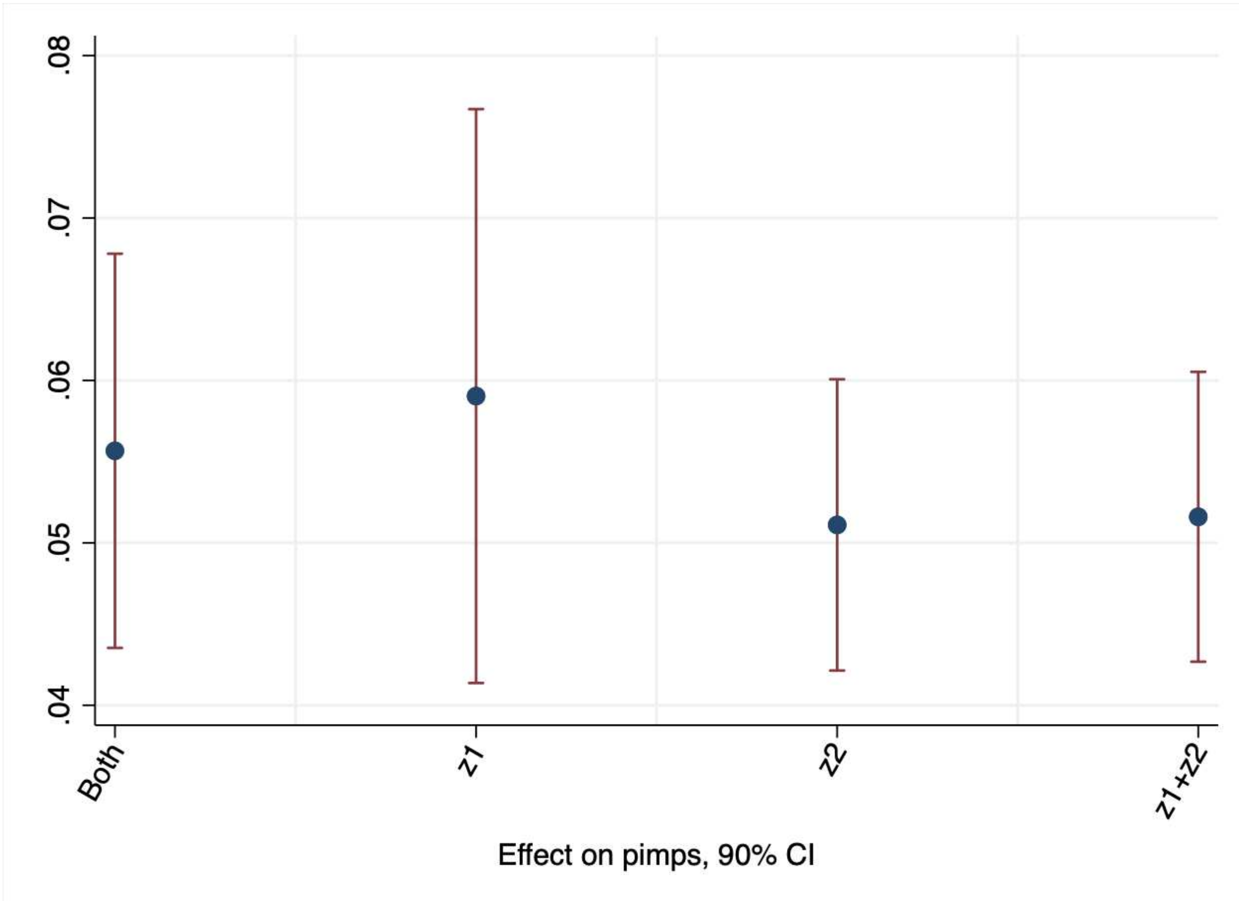
Notes: results of randomizing z_{2rmy} stratified at time period level with 1,000 permutations. The red vertical line represents the estimated coefficient of the main specification. The intersection between the red vertical line and the estimated distribution could be interpreted as the probability of finding an estimated coefficient as large as my estimates by chance. Only 8 regressions, out of 1,000, could replicate such estimate.

Figure 6: Estimated coefficients, upper-bound of the identified set depending on δ



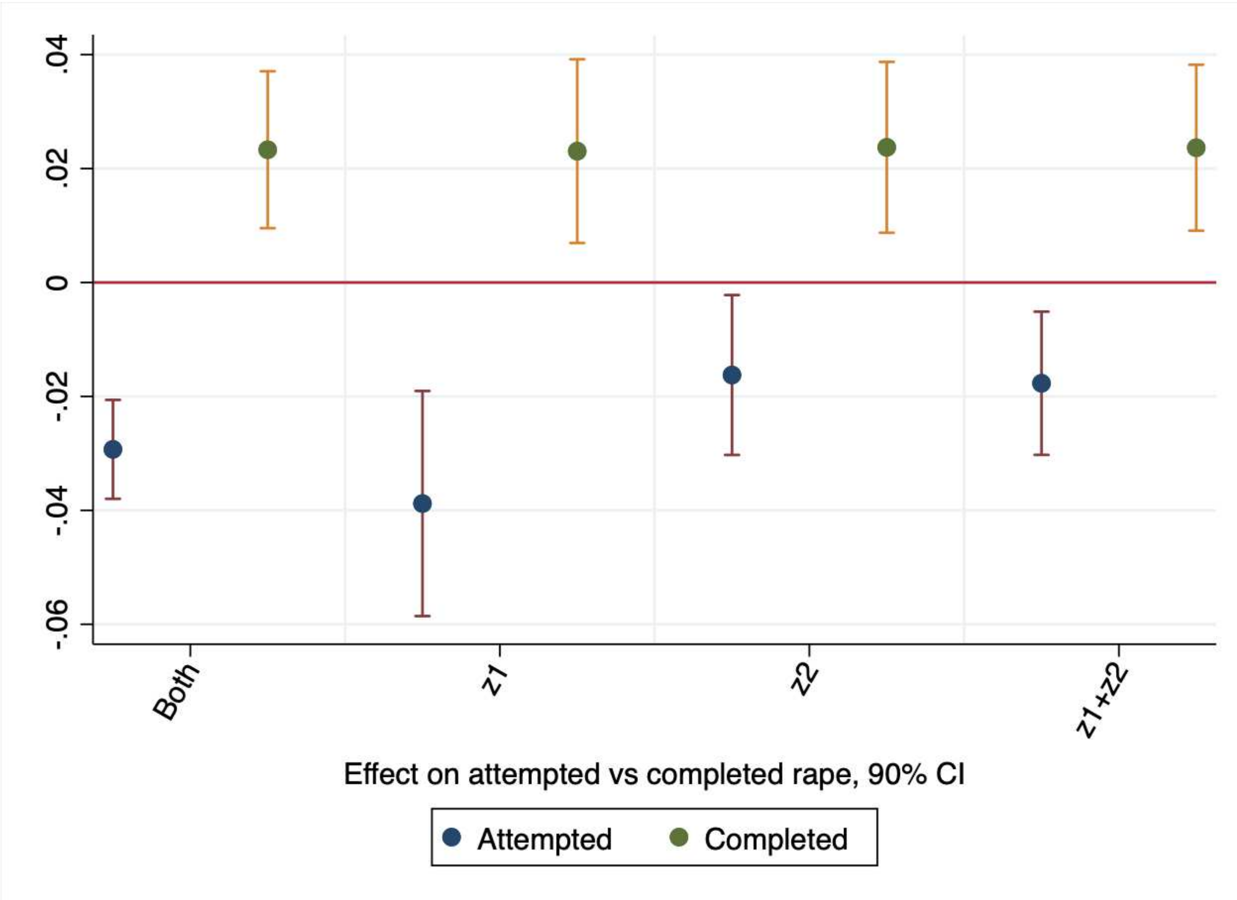
Notes: results of using Oster (2017) methodology to estimate identified sets of the estimated coefficient assuming selection on observables is proportional to selection on unobservables. The red vertical line represents the IV estimate of the main specification. The figure shows any δ lower than -1.2 is associated to an identified set containing such IV estimates.

Figure 7: Effect on pimps



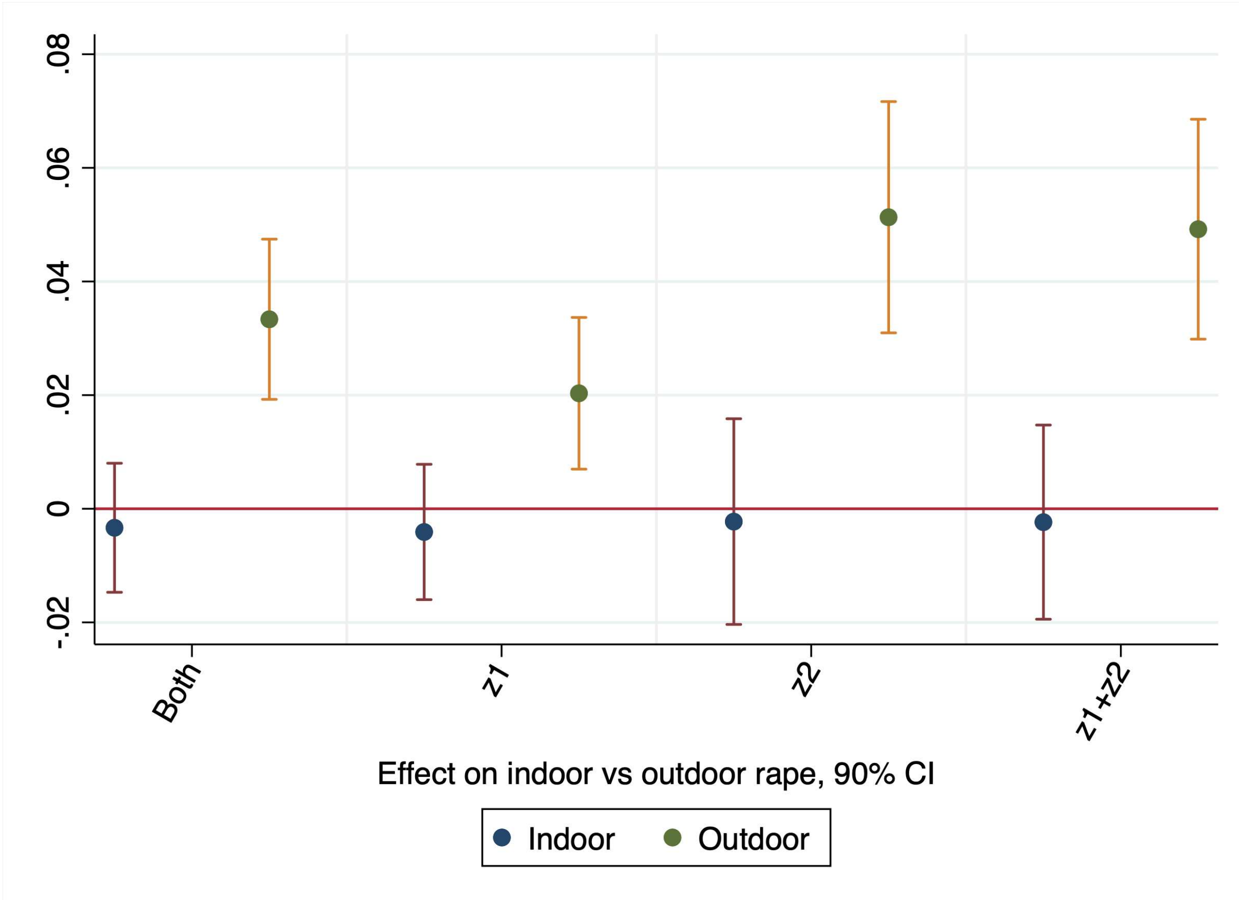
Notes: This figure shows the estimated coefficients, and respective 90 % confidence intervals, of running the main IV specification for pimps using either both instruments (main first stage regression), only z_{1rmy}, only z_{2rmy} or their sum. These findings suggest fines for sex purchase increase pimps. Results are robust across specifications.

Figure 8: Effect on attempted vs completed rape



Notes: This figure shows the estimated coefficients, and respective 90 % confidence intervals, of running the main IV specification for attempted and completed rape using either both instruments (main first stage regression), only z_{1rmy}, only z_{2rmy} or their sum. Completed rapes increase, while attempted rapes reduce. Results are robust across specifications.

Figure 9: Effect on indoor vs outdoor rape



Notes: This figure shows the estimated coefficients, and respective 90 % confidence intervals, of running the main IV specification for attempted and completed rape using either both instruments (main first stage regression), only z_{1rmy}, only z_{2rmy} or their sum. Outdoor rapes increase, while indoor rapes stay unchanged. Results are robust across specifications.

Table 1: Summary statistics

Panel A: Whole period			
Rape	mean	median	s.d.
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
Pimps	.28	0	.93
Observations 4,536			
Panel B: Before the introduction of the ban			
Rape	mean	median	s.d.
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
Pimps	.09	0	.37
Observations 504			
Panel C: After the introduction of the ban			
Rape	mean	median	s.d.
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
Pimps	.3	0	.97
Observations 4,032			

Table 2: IV: First stage

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
z_{1rmy}	-0.00505***	-0.00505***	-0.00568***	-0.00568***				
	(0.000889)	(0.00163)	(0.000895)	(0.00184)				
z_{2rmy}	-0.000261***	-0.000261***			-0.000307***	-0.000307***		
	(2.10e-05)	(8.44e-05)			(2.58e-05)	(9.94e-05)		
$z_{1rmy} + z_{2rmy}$							-0.000322***	-0.000322***
							(2.74e-05)	(0.000104)
KP F-stat	77.54		40.20		141.98		137.87	
p value coeff	0.00							
Panel B								
z_{1rmy} 40km	-0.00503***	-0.00503***	-0.00567***	-0.00567***				
	(0.000901)	(0.00163)	(0.000901)	(0.00183)				
z_{2rmy} 40km	-0.000265***	-0.000265***			-0.000312***	-0.000312***		
	(2.42e-05)	(8.57e-05)			(2.82e-05)	(0.000101)		
$z_{1rmy} + z_{2rmy}$ 40km							-0.000326***	-0.000326***
							(2.94e-05)	(0.000105)
KP F-stat	64.71		39.61		121.79		123.26	
p value coeff	0.00							
Panel C								
z_{1rmy} 60km	-0.00505***	-0.00505***	-0.00568***	-0.00568***				
	(0.000889)	(0.00163)	(0.000895)	(0.00184)				
z_{2rmy} 60km	-0.000261***	-0.000261***			-0.000308***	-0.000308***		
	(2.12e-05)	(8.46e-05)			(2.59e-05)	(9.96e-05)		
$z_{1rmy} + z_{2rmy}$ 60km							-0.000323***	-0.000323***
							(2.75e-05)	(0.000104)
KP F-stat	77.07		40.27		141.64		138.01	
p value coeff	0.00							
Observations	4,416	4,416	4,416	4,416	4,416	4,416	4,416	4,416
Clustered variance at Regional level	Y	Wild	Y	Wild	Y	Wild	Y	Wild
Region FE	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Regional Year Trends	Y	Y	Y	Y	Y	Y	Y	Y
IV	z_{1rmy} and z_{2rmy}	z_{1rmy} and z_{2rmy}	Only z_{1rmy}	Only z_{1rmy}	Only z_{2rmy}	Only z_{2rmy}	z_{1rmy}+z_{2rmy}	$z_{1rmy} + z_{2rmy}$

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

Table 3: IV: Placebo

	(1)	(2)	(3)	(4)	(5)	(6)
	First stage			Reduced form		
z_{3rmy}	0.00173	0.00112	0.00895	0.00135	-0.000230	-5.31e-06
	(0.00781)	(0.00766)	(0.0127)	(0.00159)	(0.000191)	(0.000199)
z_{4rmy}	0.00280	0.00218	0.00832	0.00180	-0.000340	-8.84e-05
	(0.00858)	(0.00838)	(0.0131)	(0.00185)	(0.000267)	(0.000246)
z_{5rmy}	-0.00275	-0.00211	-0.00882	-0.00161	0.000309	7.36e-05
	(0.00814)	(0.00799)	(0.0129)	(0.00169)	(0.000235)	(0.000219)
Observations	4,416	4,416	4,416	4,416	4,416	4,416
Clustered variance at Regional level	Y	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y	Y	Y
Year FE	N	Y	Y	N	Y	Y
Regional Year Trends	N	N	Y	N	N	Y

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

Table 4: Regression results for Sweden

VARIABLES	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
Fines for sex purchase	0.00104	0.0859	0.00118**	0.0291	0.00131**	0.0189**
	(0.00124)	(0.0680)	(0.000432)	(0.0205)	(0.000470)	(0.00745)
Observations	4,536	4,416	4,536	4,416	4,536	4,416
Clustered variance at Regional level	Y	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y	Y	Y
Year FE	N	N	Y	Y	Y	Y
Regional Year Trends	N	N	N	N	Y	Y
Baseline mean	6.16	6.16	6.16	6.16	6.16	6.16
Baseline std. dev.	9.92	9.92	9.92	9.92	9.92	9.92

Clustered standard errors at region level in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Robustness: specification

	(1)	(2)	(3)	(4)
Panel A				
Fines for sex purchase	0.0189**	0.0219**	0.0147*	0.0152*
	(0.00745)	(0.00933)	(0.00852)	(0.00821)
Panel B	40 km			
Fines for sex purchase	0.0200***	0.0264***	0.0115	0.0126*
	(0.00718)	(0.00985)	(0.00763)	(0.00738)
Panel C	60 km			
Fines for sex purchase	0.0190**	0.0219**	0.0150*	0.0154*
	(0.00750)	(0.00935)	(0.00862)	(0.00829)
Observations	4,416	4,416	4,416	4,416
Clustered variance at Regional level	Y	Y	Y	Y
Region FE	Y	Y	Y	Y
Regional Year Trends	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
IV	z_{1rmy} and z_{2rmy}	Only z_{1rmy}	Only z_{2rmy}	z_{1rmy}+z_{2rmy}
Baseline mean	6.16	6.16	6.16	6.16
Baseline std. dev.	9.92	9.92	9.92	9.92

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

Table 6: Robustness: functional form

VARIABLES	(1) IHS Rape	(2) IHS Rape	(3) IHS Rape	(4) IHS Rape
Fines for sex purchase	0.0247***	0.0260***	0.0248***	0.0166*
	(0.00945)	(0.00914)	(0.00951)	(0.00994)
Observations	4,416	4,416	4,416	4,416
Clustered variance at Regional level	Y	Y	Y	Y
Region FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Regional Year Trends	Y	Y	Y	Y
IV	z_{1rmy} and z_{2rmy}	z_{1rmy} and z_{2rmy} 40 km	z_{1rmy} and z_{2rmy} 60 km	$z_{1rmy} + z_{2rmy}$
Baseline mean	6.16	6.16	6.16	6.16
Baseline std. dev.	9.92	9.92	9.92	9.92

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

Table 7: Exclusion restriction: Before vs After the ban

	(1)	(2)	(3)	(4)	(5)	(6)
z_{1rmy}	-0.000220	-0.000194	-0.000268	-0.000245*	-0.000122**	-0.000108**
	(0.000175)	(0.000179)	(0.000167)	(0.000136)	(5.54e-05)	(4.63e-05)
z_{2rmy}	1.15e-05	1.14e-05	1.11e-05	-2.04e-05***	-5.16e-06*	-5.27e-06*
	(1.56e-05)	(1.56e-05)	(1.56e-05)	(6.52e-06)	(2.81e-06)	(2.75e-06)
Observations	504	504	504	3,912	3,912	3,912
Clustered variance at Regional level	Y	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y	Y	Y
Year FE	N	Y	Y	N	Y	Y
Regional Year Trends	N	N	Y	N	N	Y
Period	Before the ban	Before the ban	Before the ban	After the ban	After the ban	After the ban

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

Table 8: Exclusion restriction: During the ban

	(1)	(2)	(3)	(4)	(5)	(6)
z_{1rmy}	0.00251	0.00322	0.00358*	-8.17e-05	-6.50e-05	-5.95e-05*
	(0.00165)	(0.00219)	(0.00207)	(8.95e-05)	(3.95e-05)	(3.27e-05)
z_{2rmy}	-5.75e-07	1.43e-05	9.36e-06	-1.10e-05**	-2.37e-06	-2.43e-06
	(2.26e-05)	(2.16e-05)	(1.93e-05)	(4.21e-06)	(3.04e-06)	(2.81e-06)
Observations	1,850	1,850	1,850	2,062	2,062	2,062
Clustered variance at Regional level	Y	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y	Y	Y
Year FE	N	Y	Y	N	Y	Y
Regional Year Trends	N	N	Y	N	N	Y
Sample	Below median	Below median	Below median	Above median	Above median	Above median

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

Table 9: First stage

			Proxy prostitution		
			Low (i.e. 0)	High (i.e. > 0)	
Cumulated fines	Below median	z_{1rmy}	0.00137	0.000378	-0.000305
			(0.254)	(0.000232)	(0.000403)
		z_{2rmy}	-6.02e-05	-9.63e-05***	-9.67e-05***
			(4.23e-05)	(9.71e-06)	(4.89e-06)
		Observations	1,373	562	1,935
	Above median	z_{1rmy}	0.259	-0.00297***	-0.00377***
			(0.254)	(0.000663)	(0.00111)
		z_{2rmy}	0.00138	-0.000112	-0.000209***
			(0.00120)	(0.000106)	(5.53e-05)
		Observations	1,243	734	1,977
		z_{1rmy}	0.224	-0.00467***	
			(0.210)	(0.000859)	
		z_{2rmy}	0.000604	-0.000245***	
			(0.000418)	(6.45e-05)	
		Observations	2,616	1,296	

Notes: First stage results of dividing the sample in 4 mutually-exclusive subcategories according to prostitution and fines for sex purchase. All regressions include region FE, month FE, year FE, police control and regional year trends. Clustered variance at region level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Reweighted OLS & RF

	(1)	(2)	(3)
Panel A		OLS: Structural equation	
Fines for sex purchase	0.000956	0.00125***	0.00141***
	(0.00141)	(0.000419)	(0.000410)
Panel B		Reduced Form	
z_{1rmy}	-0.000329*	-0.000162**	-0.000144**
	(0.000185)	(7.71e-05)	(5.76e-05)
z_{2rmy}	-2.38e-05**	-4.95e-06*	-5.62e-06**
	(8.44e-06)	(2.62e-06)	(2.67e-06)
Observations	4,032	4,032	4,032
Clustered variance at Regional level	Y	Y	Y
Region FE	Y	Y	Y
Month FE	Y	Y	Y
# of Policemen	Y	Y	Y
Year FE	N	Y	Y
Regional Year Trends	N	N	Y

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

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Appendix

A Descriptive statistics and figures of the instruments

Table A.1 shows the airport used for each county. Table A.2 shows descriptive statistics of the instruments. As expected, variation in offering of flights increased over years. The number of observations in the whole period is smaller than in Table 1 since data on flights presented missing values in 2005.

Figures A.1 and A.2 respectively plot the distribution over months of z_{1rmy} and z_{2rmy} with respect to fines for sex purchase. Two features are clear from these figures. First, as sex tourism patterns would predict, there appears to be a negative correlation between each instrument and the endogenous variable: i.e. when the former increases the latter decreases. Second, the bulk of the variation in the instruments takes place in summer and winter months; this motivates the inclusion of month fixed effects.

Likewise, Figures A.3 and A.4 respectively plot the evolution over years of z_{1rmy} and z_{2rmy} compared to fines for sex purchase. Both figures show that there appears to be a negative correlation between the instruments and the endogenous variable also at year level. Moreover, the three variables show an upward trend. This last feature motivates the inclusion of year fixed effects and year trends.[28]

[28] Note that these figures are graphical depictions of the first stage.

Table A.1: Airport used for each county

County	Closest main airport
Blekinge	RNB Ronneby Airport
Dalarna	MXX Mora Siljan Airport
Gotland	VBY Visby Airport
Gävleborg län	MXX Mora Siljan Airport
Halland	HAD Halmstad City Airport
Jämtland län	OSD Åre Östersund Airport
Jönköping	JKG Jönköping Airport
Kalmar	KLR Kalmar Airport
Kronoberg	VXO Växjö Airport
Norrbotten	GEV Gällivare Airport
Skäne län	KID Kristianstad Airport
Stockholm	ARN Stockholm Arlanda Airport
Södermanland	NYO Stockholm Skavsta Airport
Uppsala	ARN Stockholm Arlanda Airport
Värmland	KSD Karlstad Airport
Västerbotten	SQO Storuman Airport closed in June 2010 then HMV Hemavan Airport
Västernorrland län	KRF Höga Kusten Airport
Västmanland	VST Stockholm Västerås Airport
Västra Götaland	THN Trollhättan Vänersborg Airport
Örebro län	ORB Örebro Airport
Östergötland	LPI Linköping City Airport

Table A.2: Summary statistics: instruments

	mean	median	s.d.
Panel A: Whole period			
z_{1rmy}	4.02	0	41.15
z_{2rmy}	73.02	0	670.80
Observations 4,416			
Panel B: Before the introduction of the ban			
z_{1rmy}	2.3	0	23.26
z_{2rmy}	49.12	0	520.16
Observations 504			
Panel C: After the introduction of the ban			
z_{1rmy}	4.24	0	42.91
z_{2rmy}	76.1	0	687.81
Observations 3,912			

Figure A.1: z_{1rmy} distribution over months

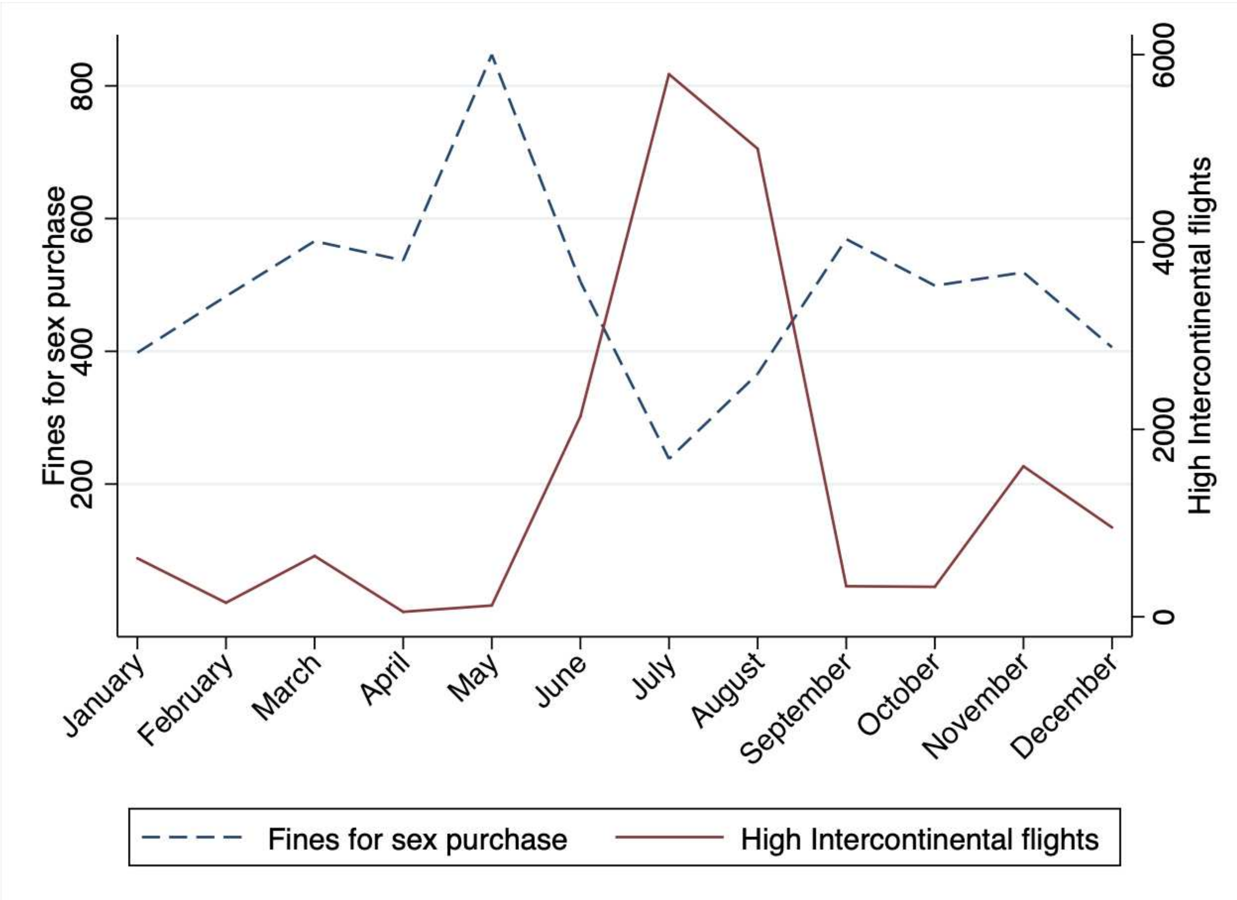


Figure A.2: z_{2rmy} distribution over months

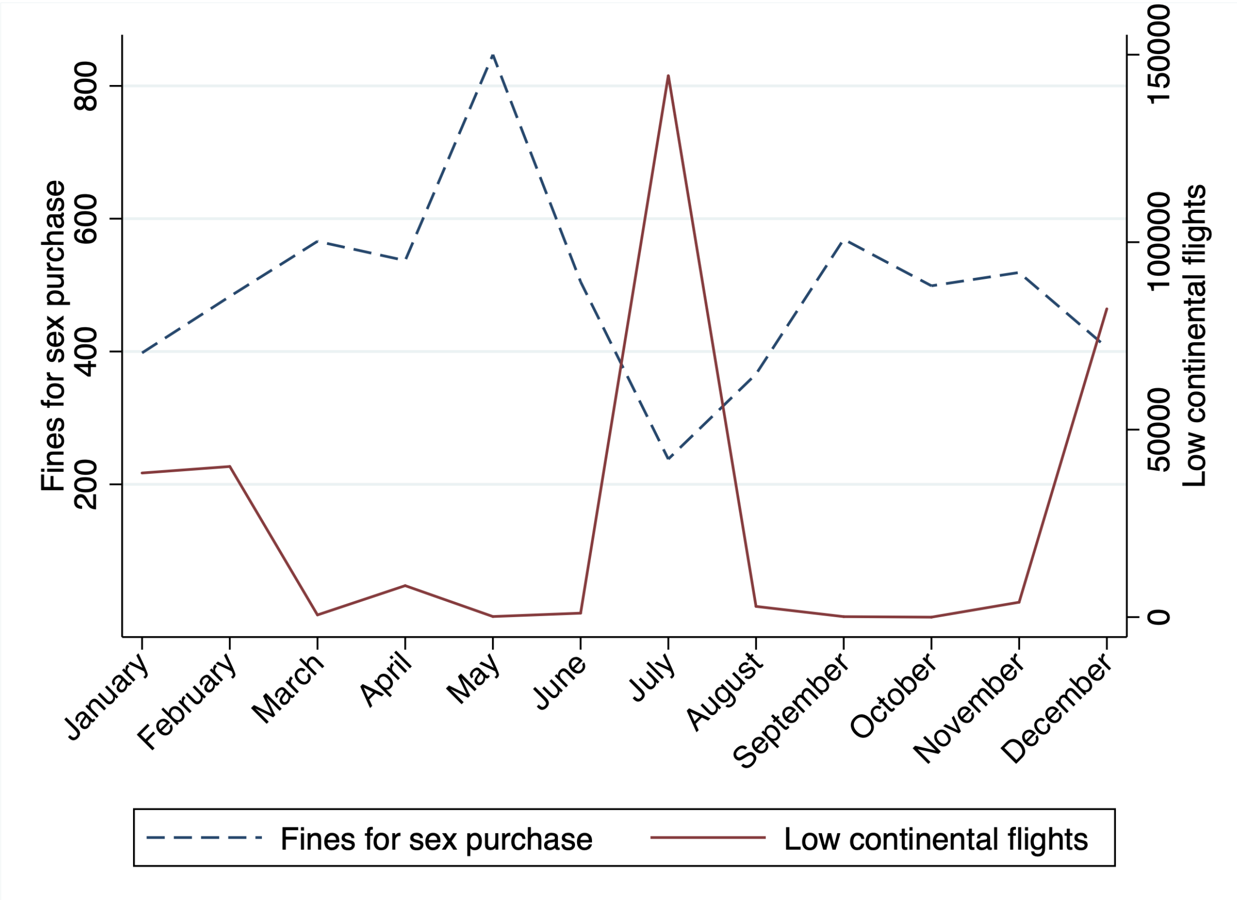


Figure A.3: z_{1rmy} evolution over years

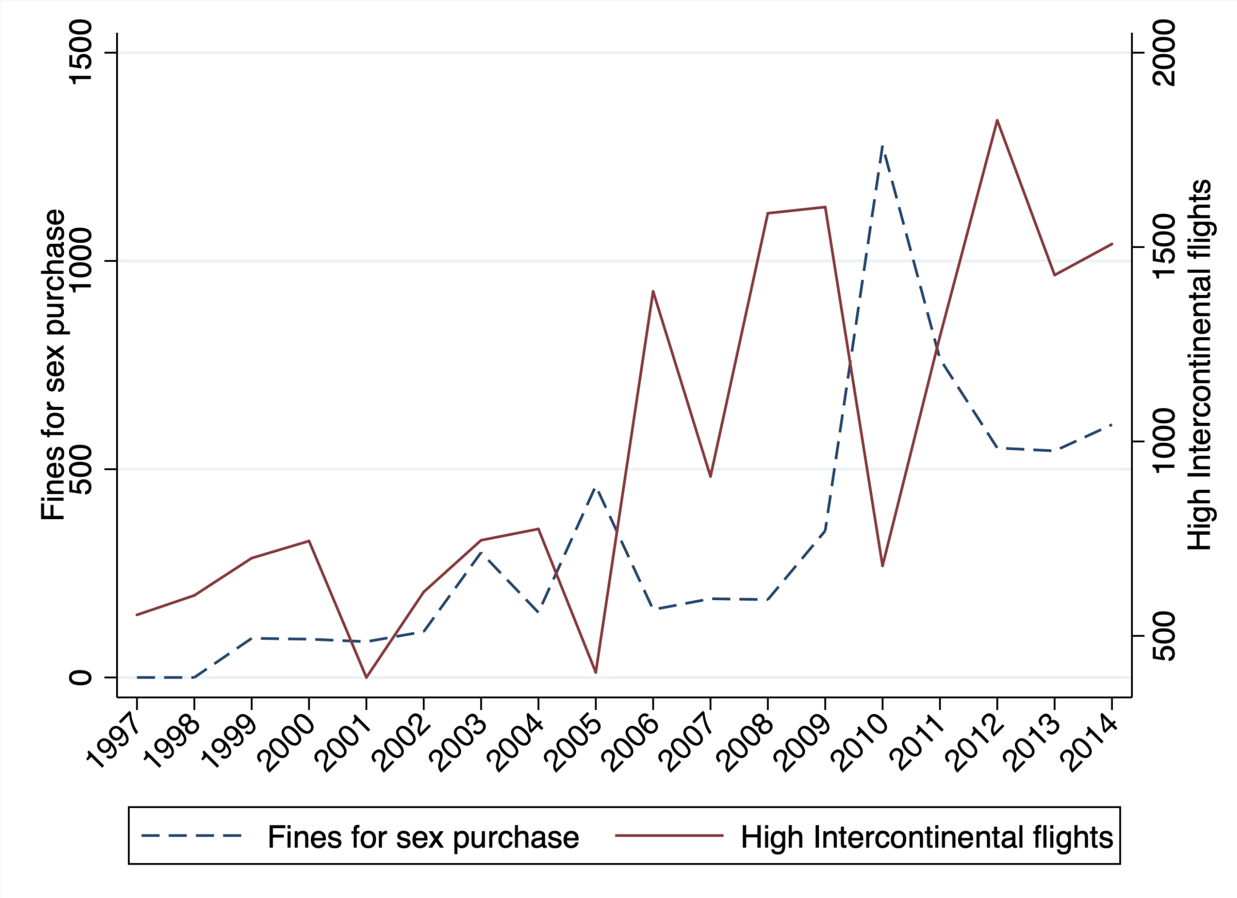
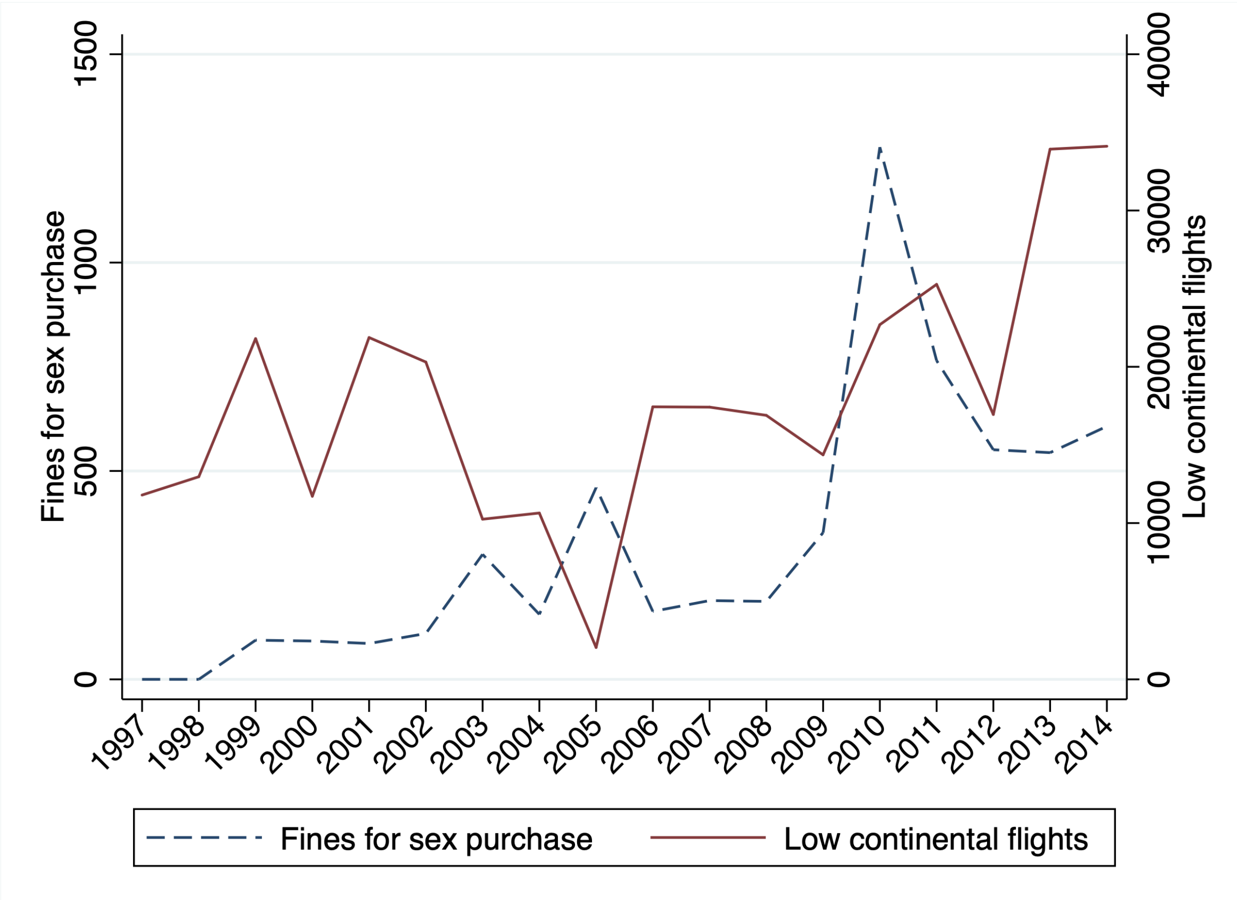


Figure A.4: z_{2rmy} evolution over years



B Balancing tests: Instruments

A key assumption of the identification strategy is that variation in offering of flights is not affected by the number of fines for sex purchase. This assumption seems plausible since sex tourism comprises only a small fraction of demand for flights. Yet, this section exploits the high frequency of the data set to shed light on this issue.[29]

There could be concerns that seasonal changes in fines for sex purchase influence flight company decisions. In fact, since both offering of flights and sex tourism are seasonal, and offering new flights go through a long approval process, it seems plausible to think airlines could base their decision using fines for sex purchase in the same month of the previous year. If this were the case I would expect airlines to offer more flights when they think the demand is higher, this could happen in two different ways. On the one hand, it could be that airlines base their decision on the amount of fines. On the other

[29]A similar analysis is available in Dustmann et al. (2016).

hand, it could be they use the change in such fines. Thereby, I test whether either fines for sex purchase in the year before, or their change, affects variation in offering of flights by estimating the following regression models for both instruments $i = 1, 2$:

$$\Delta z_{irmy} = \theta \Delta fines_{rmy-1} + \alpha_r + \alpha_m + \alpha_y + \alpha_r * y + \gamma officers_{ry} + \varepsilon_{rmy} \quad \text{(A.1)}$$

$$\Delta z_{irmy} = \theta fines_{rmy-1} + \alpha_r + \alpha_m + \alpha_y + \alpha_r * y + \gamma officers_{ry} + \varepsilon_{rmy} \quad \text{(A.2)}$$

where Δ is the first difference operator (at month level). Table A.3 present the results of running regression models A.1 and A.2 for both z_{1rmy} and z_{2rmy}. In particular, columns (1)-(3) and (4)-(6) present results for regression models A.1 and A.2 for instrument z_{1rmy}, while columns (7)-(9) and (10)-(12) respectively do the same for instrument z_{2rmy}.

Estimated coefficients are negative and not statistically significant in any regression. This evidence supports that fines for sex purchase do not affect variation of offering of flights, in line, with the identification strategy.

Table A.3: Balancing test: Instruments

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	First difference											
	z_{1rmy}						z_{2rmy}					
First difference 12 months before	-0.186	-0.186	-0.186				-2.716	-2.713	-2.708			
	(0.214)	(0.215)	(0.215)				(3.150)	(3.151)	(3.154)			
Lag 12 months before				-0.205	-0.208	-0.210				-1.019	-1.145	-1.278
				(0.232)	(0.237)	(0.244)				(1.152)	(1.309)	(1.472)
Observations	4,133	4,133	4,133	4,154	4,154	4,154	4,133	4,133	4,133	4,154	4,154	4,154
Clustered variance at Regional level	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Regional Year Trends	N	N	Y	N	N	Y	N	N	Y	N	N	Y

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

C Seasonality

This section explores the concern that results are driven by seasonality. Specifically, there might be the concern that since flights change according to season, and the specification is at month level, results might be driven by flights concentrating in certain months of the year. To this extent, it is worth noting that each regression model previously considered includes month FE to capture such seasonal changes.

However, this section further addresses this issue by averaging the instruments across months for each year such that variation only comes by each region and year (i.e. it is constant for a certain year and region). Column (1) of Table A.4 shows the results of the first stage specification using such two instruments. Similarly to the main first stage specification, results suggest the two instruments reduce fines for sex purchase. Moreover, the associated F-statistic is above 10 and the two instruments are statistically different from each other as in the previous first stage regressions.

Column (2) of Table A.4 shows the IV estimates of this specification. Results are similar in sign and size with the main IV estimates suggesting seasonality is not driving the results.

Table A.4: Seasonality check

VARIABLES	(1) Fines for sex purchase	(2) Log(1+Rape)
$\bar{z}_{1ry}$	-0.115***	
	(0.00883)	
$\bar{z}_{2ry}$	-0.00593***	
	(0.000267)	
Fines for sex purchase		0.0152***
		(0.00567)
Observations	4,416	4,416
Clustered variance at Regional level	Y	Y
Region FE	Y	Y
Month FE	Y	Y
# of Policemen	Y	Y
Regional Year Trends	Y	Y
Regression	First stage	Second stage
KP F-stat	506.35	
p value coeff	0.00	

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

D Balancing test: Reverse causality

Likewise, I can use the same analysis of Appendix Section B to shed light on the potential reverse causality affecting OLS estimates. Reverse causality arises from the concern that rape could affect fines for sex purchase. I run the same regression model as in Appendix Section B but replacing the dependent variable with fines for sex purchase and the main regressor with rape:

$$\Delta fines_{rmy} = \theta \Delta rape_{rmy-1} + \alpha_r + \alpha_m + \alpha_y + \alpha_r * y + \gamma officers_{ry} + \varepsilon_{rmy} \tag{A.3}$$

$$\Delta fines_{rmy} = \theta rape_{rmy-1} + \alpha_r + \alpha_m + \alpha_y + \alpha_r * y + \gamma officers_{ry} + \varepsilon_{rmy} \tag{A.4}$$

Results of running regressions A.3 and A.4 are respectively shown from columns (1) to (3) and (4) to (6) of Panel A of Table A.5. The coefficient associated with the main regressor of equation A.3 is negative but statistically insignificant in all the three regressions (columns (1) to (3)), while the coefficient associated with the main regressor of equation A.4 is statistically negative in all three regressions. Since this evidence could seem inconclusive due to the different findings, Panel B and Panel C repeat the same analysis but using respective the logarithmic and the IHS transformation of the dependent variable. In both cases, both regressions produce statistically negative coefficients.

These coefficients are economic meaningful. In effect, column (6) of Panel A indicates that an increase in rape of one standard deviation is associated to a decrease of about 0.3 fines for sex purchase. Given the average of fines for sex purchase this result stands for 20% decrease in fines for sex purchase.

If it is true that rape is negatively associated with prostitution, I should observe a similar pattern to the one just described also using pimps as dependent variable. As explained in Section 6 this variable proxies supply of prostitution. Columns (7) to (12) of Table A.5 repeat the same analysis carried out above but using pimps as dependent variable, in particular, these tables report results of the two following regressions:

$$\Delta pimps_{rmy} = \theta \Delta rape_{rmy-1} + \alpha_r + \alpha_m + \alpha_y + \alpha_r * y + \gamma officers_{ry} + \varepsilon_{rmy} \tag{A.5}$$

$$\Delta pimps_{rmy} = \theta rape_{rmy-1} + \alpha_r + \alpha_m + \alpha_y + \alpha_r * y + \gamma officers_{ry} + \varepsilon_{rmy} \tag{A.6}$$

Results show that coefficients associated to the main regressor are negative across all regression models and statistically significant in four out of six cases. All in all, this evidence suggests that reverse causality affects OLS estimates.

Table A.5: Balancing test: Reverse causality

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	First difference											
	Fines for sex purchase						Pimps					
Panel A: Levels												
First difference 12 months before	-0.0134	-0.0134	-0.0134				-0.00238*	-0.00237*	-0.00236*			
	(0.00930)	(0.00931)	(0.00932)				(0.00115)	(0.00114)	(0.00114)			
Lag 12 months before				-0.0150*	-0.0154*	-0.0164*				-0.000982	-0.00106	-0.00122
				(0.00802)	(0.00838)	(0.00904)				(0.00105)	(0.00109)	(0.00114)
Panel B: Log(1+y)												
First difference 12 months before	-0.00232**	-0.00231**	-0.00232**				-0.00155	-0.00154	-0.00154			
	(0.000821)	(0.000818)	(0.000820)				(0.000902)	(0.000898)	(0.000901)			
Lag 12 months before				-0.00204***	-0.00207***	-0.00212***				-0.00113*	-0.00119*	-0.00124*
				(0.000676)	(0.000695)	(0.000737)				(0.000627)	(0.000647)	(0.000718)
Panel C: IHS												
First difference 12 months before	-0.00277**	-0.00276**	-0.00276**				-0.00205*	-0.00205*	-0.00205*			
	(0.00101)	(0.00101)	(0.00101)				(0.00117)	(0.00116)	(0.00117)			
Lag 12 months before				-0.00235***	-0.00238**	-0.00243**				-0.00148*	-0.00156*	-0.00163*
				(0.000818)	(0.000839)	(0.000889)				(0.000807)	(0.000833)	(0.000922)
Observations	4,263	4,263	4,263	4,284	4,284	4,284	4,263	4,263	4,263	4,284	4,284	4,284
Clustered variance at Regional level	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
Regional Year Trends	N	N	Y	N	N	Y	N	N	Y	N	N	Y

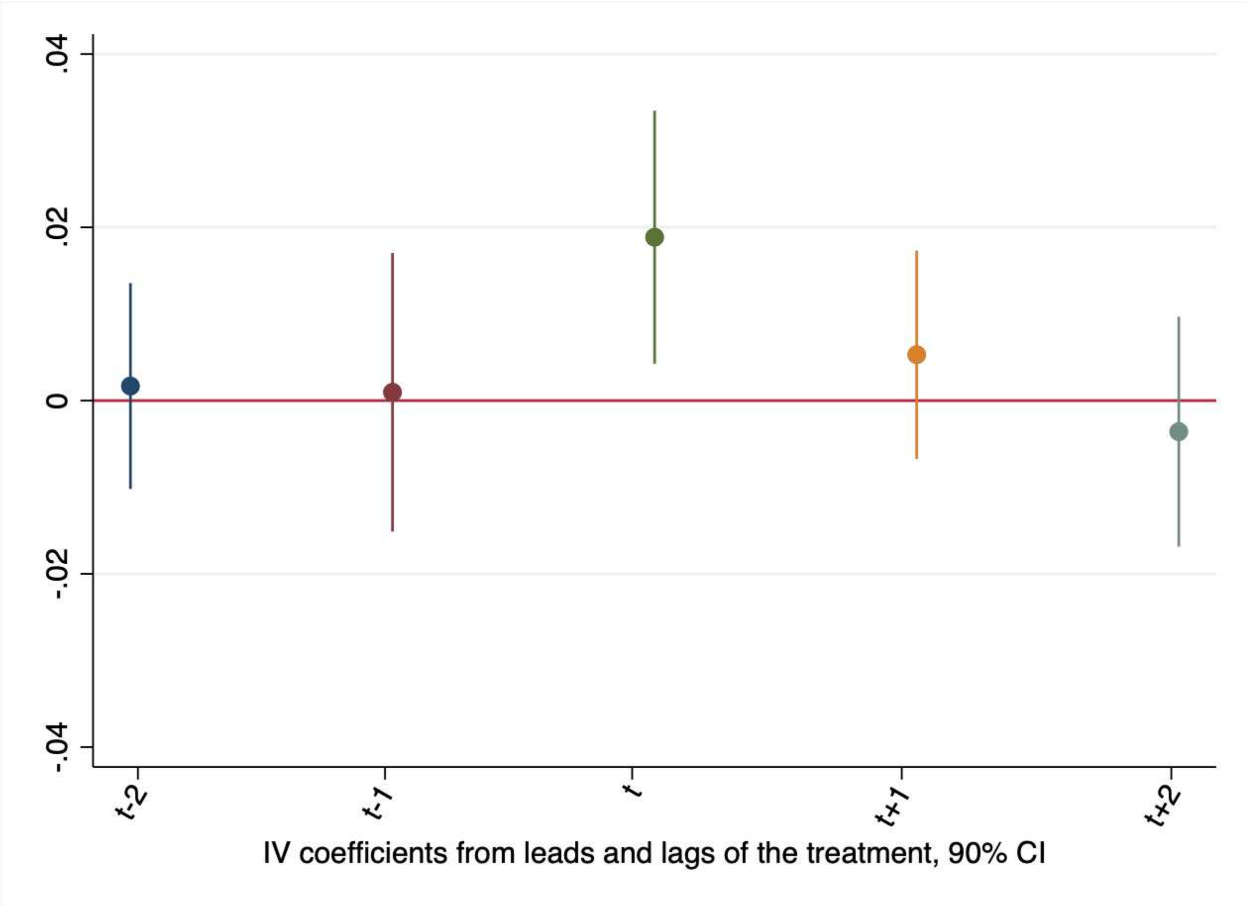
Clustered standard errors at region level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

E Temporary vs permanent effect

This section explores whether the found effect is permanent or temporary. To do so it runs five regressions (2 months prior and after the treatment takes place plus the main specification). In each regression fines for sex purchase is instrumented with the corresponding contemporaneous values of z_{1rmy} and z_{2rmy}. For example, for the $t-2$ regression the endogenous variable is $fines_{rm-2y}$ and the instruments are z_{1rm-2y} and z_{2rm-2y}, for the $t-1$ regression the endogenous variable is $fines_{rm-1y}$ and the instruments are z_{1rm-1y} and z_{2rm-1y}.

Figure A.5 plots the results of these five regressions. Results show that fines increase rape occurring in the same month. This evidence suggest that the effect of fines for sex purchase on rape is temporary (i.e. on impact) rather than permanent.

Figure A.5: Temporary vs permanent effect



Notes: This figure shows the estimated coefficients, and respective 90 % confidence intervals, of running the main IV specification for leads and lags of the treatment variable instrumented with the corresponding contemporaneous values of z_{1rmy} and z_{2rmy}. Fines for sex purchase have an effect only on rapes happening in the same month.

F Additional Specifications

This section explores additional specifications to address two concerns. First, there might be the concern that the identifying variation correlates with periods where there is a lower amount of officers. Put it differently, since the variation in the instruments come from periods with relative larger offerings of intercontinental flights, there might be the concern that these periods are also when fewer officers work. If this is the case the decrease in fines for sex purchase might be explained by this reduction in the number of officers at work. To this end, columns (1) and (2) of Table A.6 respectively present results, for both the reduced form and the second stage specifications, using shoplifting and bicycle thefts as dependent variable.

These two crimes are much more likely to occurr than purchases of prostitution and might be considered a proxy for the number of officers patrolling the streets. Furthermore, given the first-stage result that the instruments affect fines for sex purchase, these two crimes might also be seen as placebo outcomes since, to the best of my knowledge, there is no evidence that any of the two is connected with prostitution. Therefore, we expect to find statistically insignificant estimates of the effect of fines for sex purchase on these two outcomes.

Columns (1) and (2) of Panel A of Table A.6 presents the results for the reduced form specification. These results show that the instruments have no clear connection with any of the crimes. Regarding shoplifting the two instruments are not statistically associated with such a crime. As for bicycle thefts, the instruments show statistically significant estimated coefficients but of opposite signs which makes no sense either with the concern that both instruments correlates with a decrease in the number of officers working. In addition, the point estimates associated to each instrument for both crimes change sign across regressions (i.e. the first instrument has a negative point estimate with shoplifting and positive with bike thefts, the opposite is true for the second instrument) suggesting that there is no robust effect of such instruments on any of the two crimes. Panel B for these regressions shows that, as expected, results suggest that fines for sex purchase do not affect either of the two crimes.

Next, I explore crimes connected to either rape or prostitution. First, I consider sexual coercion (column (3)), which differs from rape in the fact that in this case there might be no intercourse, as long as sexual relations are asked ”quid pro quo” (i.e. in return for something). If the instruments are proxing acces to sex tourism it might seem reasonable to expect to find similar results to the main ones for this crime as well. Results suggest this is exactly the case: both the reduced form and second stage regressions present similar results to my main specification. Larger access to sex tourism reduces sexual coercion, while the effect of fines for sex purchase on this crime is positive and of similar size (one s.e. difference) to the main results. These findings suggest that a further unintended effect of the ban has been to boost sexual coercion.

Second, I consider three crimes connected to prostitution but for which I do not have data spanning the whole sample period, specifically, these crimes are non-sexual human trafficking, sexual human trafficking and domestic violence. Given the restricted sample period, their close connection to prostitution and the fact that the identification assumption was not thought for this analysis; these results should be interepreted carefully. Non-sexual and sexual human trafficking might be viewed as proxies of the supply of prostitution. Columns (4) and (5) respectively show the results for these two crimes. The effect of the instruments on these two crimes is unclear: the first instrument seems to be negatively associated with human trafficking while the second seem to positively associated. At least, it is reassuring to find that across the two regressions the two estimates do not flip sign. In regards of the effect of fines for sex purchase on these two crimes results differ. There seems to be a surge in non-sexual trafficking and a decay in sexual trafficking, but the latter is statistically insignificant. This might make sense since the ban was introduced in an effort to reduce human trafficking in general and sexual trafficking in particular, raising the charges for the latter. Hence, human traffickers might substitute sexual trafficking with non-sexual one due to the increment in its *relative price*.

As for domestic violence (column (6)), the first instrument seem to be insignificant while the second is positively associated to this crime. The second stage results fines for sex purchase are associated to an ebb in domestic violence. These findings differ from Berlin et al. (2019). To this extent it is worth mentioning that their analysis is focused on domestic violence while mine is not and that the identifying variation and sample period differ as well.[30]

[30] There might be concerns about the main results in this restricted sample (i.e. 2008 to 2014). Results of both the first and second stage are stable along this sample period. While, the second stage estimate is larger, 0.037, and statistically significant.

Table A.6: Additional specifications

VARIABLES	(1) Shoplifting	(2) Bike theft	(3) Sexual coercion	(4) Non-sex trafficking	(5) Sex trafficking	(6) Domestic violence
Panel A			Reduced Form			
z_{1rmy}	-9.80e-06	0.000299***	-0.000137***	-0.000275***	-5.56e-05***	2.01e-05
	(1.38e-05)	(7.33e-05)	(2.79e-05)	(3.78e-05)	(1.87e-05)	(4.86e-05)
z_{2rmy}	2.30e-07	-1.48e-05***	-8.24e-06**	1.07e-05***	1.13e-05***	1.59e-05***
	(1.99e-06)	(2.91e-06)	(3.28e-06)	(1.75e-06)	(1.98e-06)	(2.82e-06)
Panel B			Second Stage			
Fines for sex purchase	0.000738	-0.0103	0.0290***	0.0164***	-0.00777	-0.0314**
	(0.00347)	(0.00653)	(0.00903)	(0.00461)	(0.00934)	(0.0140)
Observations	4,416	4,416	4,416	1,029	1,533	1,580
Clustered variance at Regional level	Y	Y	Y	Y	Y	Y
Region FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
# of Policemen	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Regional Year Trends	Y	Y	Y	Y	Y	Y
Sample Period	All	All	All	2008-2012	2008-2014	2008-2014

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