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. There is a recent economic literature reporting evidence on how rape and prostitution behave as substitutable crime activities. This relationship suggests that prostitution regulation could affect rape. In addition, sex tourism is a growing phenomenon: prostitutes' customers travel to countries where prostitution is tolerated to buy sex there. This paper exploits 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 increases rape on impact. In particular, this regulation gave rise to 712 rapes from 1999 to 2014. Moreover, my findings show that this regulation also changes the composition of rapes committed: increasing completed and outdoor 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]

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

In this respect, recent economic literature (Cunningham and Shah 2014; Bisschop et al. 2017; Ciacci and Sviatschi 2016) has found evidence that prostitution and rape tend to behave as substitutes: higher prostitution rates are associated with lower rape crimes. In light of this evidence, a relevant question is whether criminalizing the purchase of prostitution affects rape. 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 13%. Next, I explore whether supply

[1] European Union Agency for Fundamental Rights (2014)

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

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

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

or demand are driving this result. I find evidence supporting that the effect is demand driven.

I do not find any evidence in favour 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 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 law on crime outcomes (see, inter alias,Lee and Persson (2013); Cho et al. (2013); Jakobsson and Kotsadam (2013)). 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

In Sweden traditionally rape was defined as forced sexual intercourse. In 1962 the Swedish Penal Code included a legal definition of rape. 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 offence counts and crime data is collected when the offence in question is first reported, even if later investigations indicate that the offence must be given an alternative classification. Expansive offence 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 offence 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 offences.

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.[5]

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;

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

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 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.[6]

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.[7] 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

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

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

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".[8]

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.[9] Variation comes from the different number of issued fines for sex purchase within and between regions across time.

Following the stream of the literature reporting some degree of substitutability between prostitution and rape, I expect to find that criminalizing the purchase of prostitution would boost rape. In effect, as fines for sex purchase make the purchase of prostitution more expensive, rape offences increase.[10] 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.

The key threat that prevent OLS estimates from being causal is endogeneity. To address partly this issue, regression model (1) is highly demanding. It includes fixed effects at region, month and year level, plus region-year trends to capture any variation at seasonal or geographical levels. Yet, endogeneity could still bias OLS estimates.

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

[9]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)).

[10]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.

Given selection into treatment in this setting, reverse causality and omitted variable bias are the main concern 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 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 substitutable relation between rape and prostitution, 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. Such variables would decrease fines and increase rape. Both these issues would cause my OLS estimates to be downward biased.[11]

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 50km if any.[12] 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

[11] Yet, since my OLS estimates are positive, reverse causality and omitted variable bias imply that the population regression coefficient is larger than OLS estimates. Section C in the Appendix finds evidence in favour of reverse causality.

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

deviation above (below) the yearly mean of each airport.[13] 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 number 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. Section A in the Appendix presents descriptive statistics and figures on the instruments.

4.2.1 Identification assumption

The key identification assumption is that variation in the offering of intercontinental 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 is 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. 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 number of passengers could directly affect rape.[14] Third (*relevant instrument*), this instrument affects the (potential) endogenous regressor: fines

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

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

for sex purchase are less likely to occur in months with sex tourism.[15] In other words, increases in the *relative* offering of intercontinental flights in a given region should decrease fines for sex purchase.[16] 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.

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. Yet 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 my 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 such p-value is lower than 0.01. Column (2) of Panel A reports the results of the same regression model as in column (1) but using Wild-Cluster Bootstrap. Results barely change.

There could be the concern that my 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,

[15] Section 4.3 addresses this issue.

[16] *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 my analysis in a Local Average Treatment Effect (LATE) framework.

[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.

columns (4) and (6) run the same regressions using Wild-Cluster Bootstrap. Columns (7) 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.

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 my main specification. The intersection between the red vertical line and the estimated distribution could be interpreted as the probability of finding by chance an estimated coefficient as large as the estimated coefficient of the main regression. To put it differently, 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.[19]

5 Main results

5.1 OLS vs 2SLS results

Table 3 compares OLS and 2SLS results. 2SLS are computed instrumenting $fines_{rmy}$ in equation 1 with z_{1rmy} and z_{2rmy}. Columns (1) and (2) compare OLS and 2SLS 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 IV estimates are about an order of mag-

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

[19]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 shed light on this issue as well.

nitude 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 my main regressor increases rape by 13%. 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 one unit. Since between 1999 and 2014 there have been 5933 fines for sex purchase, this implies that in this period rape rose by roughly 712 units. In addition, Section D in the Appendix finds that this effect is temporary and takes place in the very same month.

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 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. This paper deals with each one of them but the fourth one. Section 4.3 shows that instruments are strongly correlated with the endogenous regressor. Section 5.3 handles the exclusion restriction and find no evidence supporting violation of such hypothesis. Section C in the Appendix finds evidence in favour of reverse causality. Lastly, Section 5.4 addresses this issue comparing OLS and IV estimates using the methodology developed in Oster (2017), and finds evidence supporting the size of the IV estimates.

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

This section shows that my 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 my results are robust across changes to the instruments and regression model. Second, it explores whether my results are robust to changes in the functional forms of the dependent variable.

Table 4 reports 2SLS results using different instruments. Panel A displays regression outcomes for the instruments used in my main specification. As a matter of fact, column (1) reports results of my main specification 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 my results are robust across regression models and changes in instruments.

Finally, Table 5 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 the 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.

My 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. Note this does not imply that sex tourism is not an indirect alternative to rape. To this extent it is important to distinguish between direct and indirect alternatives. Sex tourism is an alternative to sex purchase, and, as a consequence of the substitutive relation between sex purchase and rape, it is an alternative to rape.

Testing the credibility of the exclusion restriction requires deep knowledge of the subject matter. In my setting it is possible to test such assumption using data pre and post introduction of the ban. In effect, if the instruments only affect rape via fines for sex pur-

[20]The IHS transformation is defined as $log\left(y + \left(y^2+1\right)^{1/2}\right)$. It is a popular 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 4 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.

chase, 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 6 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. Likewise Table 3, 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 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. This evidence supports the exclusion restriction: it seems the instruments only affect rape via fines for sex purchase (i.e. after the introduction of the ban).

5.4 Size IV estimates

There could be the concern that IV estimates are far too large than the OLS. To tackle this issue, I use a new methodology suggested by Oster (2017). Oster (2017) develops a methodology that makes use of changes in the coefficient as controls are included and R^2 to test for omitted variable bias. The aforementioned 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 common knowledge that the IV estimates should not be much larger (in absolute value) than its OLS counterparts. 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 my set of controls with W_1 and the omitted variable causing the endogeneity with W_2:

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

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 my 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 the structural main specification (i.e. Column (5) of Table 3). 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 -1.2 is associated with an identified set including the IV estimated coefficient. Therefore, the IV estimates fall into any identified set associated with $\delta \leq -1.2$. Put it differently, my OLS estimates are statistically equivalent to my IV estimates even with a low coefficient of proportionality such as $\delta \leq -1.2$. This evidence supports that IV estimates are not too large compared to OLS estimates.

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

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 my results: fines of sex purchase might affect rape either via demand of prostitution or via supply of prostitution.

6.1 Supply of prostitution

Shifts of the supply of prostitution could affect both fines for sex purchase and rape. For instance, given the substitutable relation between rape and prostitution, a general decrease in the supply of prostitution could affect fines for sex purchase and boost rape.[23]

A priori the effect of fines of sex purchase 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 fines for sex purchase incentivize the sale of sex since this law makes clear that prostitutes are not going to be prosecuted. Given the substitutable relation between rape and prostitution, since this paper finds an increase in rape as a result of fines for sex purchase it seems reasonable to expect that supply of prostitution decreases.

To address this concern, I gather data about the number of *pimps* to proxy the supply of prostitution.[24] 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 prostitution market. Compositional changes in such two variables (e.g. changes in the number of prostitutes that work without a pimp or in the behaviour of officers towards pimps) might affect the results using this proxy.

Figure 7 presents the estimated coefficients, and respective 90% confidence intervals, of running my main 2SLS 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. $z_{1rmy} + z_{2rmy}$). Estimates are statistically positive and range between 0.05 and 0.06. This evidence indicates that introducing fines for sex purchase raises convicted pimps. Hence, these results do not support a decline in the supply of prostitution.[25]

[23]Given the substitutive relationship between rape and prostitution.

[24] 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.

[25]It is worth noting that pimps are a proxy for coercive prostitution. Given that non-coercive prostitution

6.2 Demand of prostitution

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

Given the substitutable relation between rape and prostitution, 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 of fines of sex purchase on attempted vs completed rapes and indoor vs outdoor rapes.

Figure 8 shows the estimated coefficients, and respective 90% confidence intervals, of running my main 2SLS 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 substitutive relation between rape and prostitution, but the higher penalty for the former than for the latter, 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) rape increases and, in particular, completed rape 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 my main 2SLS 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.

As a whole, from both figures it is clear that the composition of rapes changed. This

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.

evidence is in line with the hypothesis that banning the purchase of prostitution reduces the demand of prostitution.

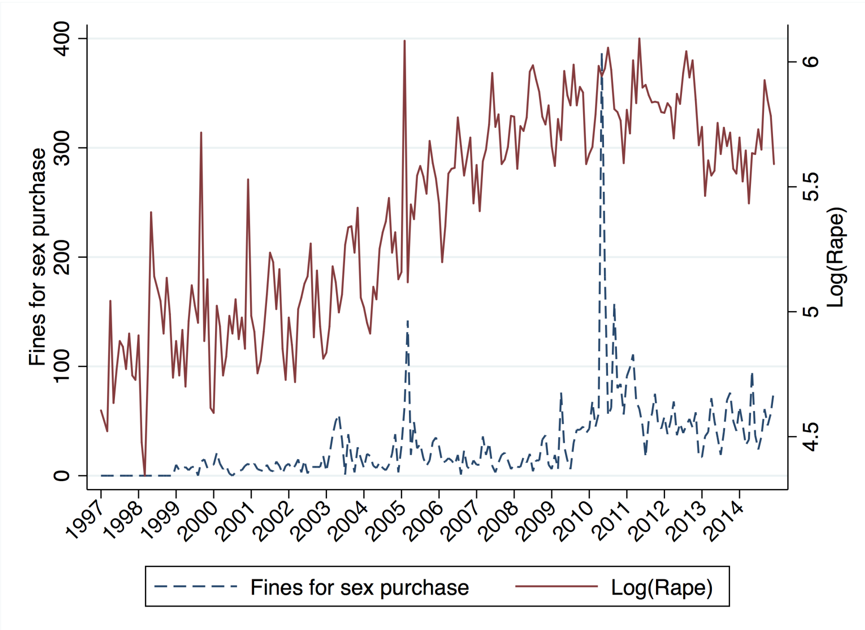
7 Conclusion

This paper analyzes the connection between the market of rape and prostitution law taking into account the substitutable relation between the two that emerged in the recent literature (Cunningham and Shah 2014; Bisschop et al. 2017; Ciacci and Sviatschi 2016). Specifically, this paper studies the effect of criminalizing the purchase of prostitution on rape.

This paper instruments fines for sex purchase with a proxy of access to sex tourism and finds that criminalizing the purchase of prostitution increases rape. In particular, this regulation gave rise to 712 rapes from 1999 to 2014. Moreover, my findings show that this regulation also changes the composition of rapes committed: increasing completed and outdoor rapes. Lastly, to the best of my knowledge, this paper is one of the first to suggest usage of a comprehensive methodology (Oster 2017) to compare sizes of OLS and IV estimates.

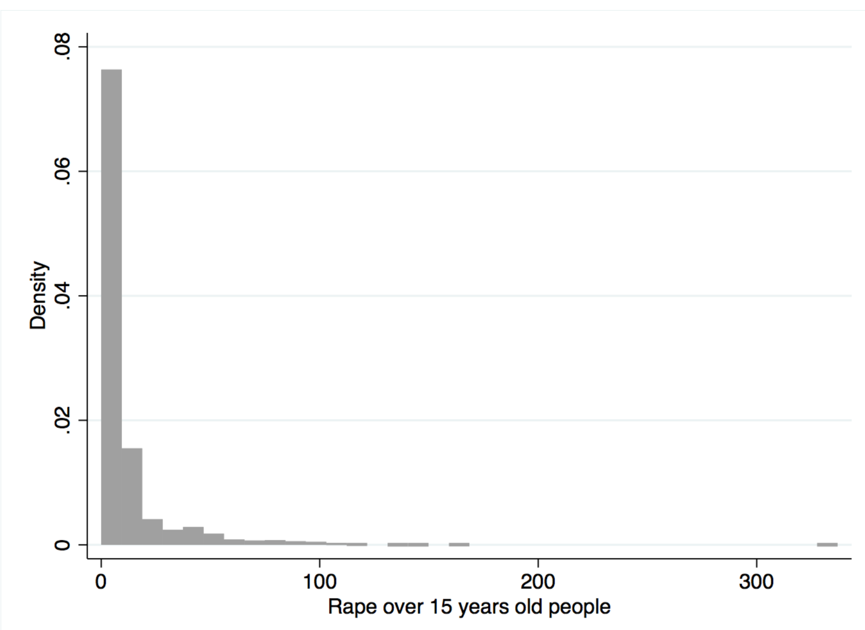
Figures & Tables

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



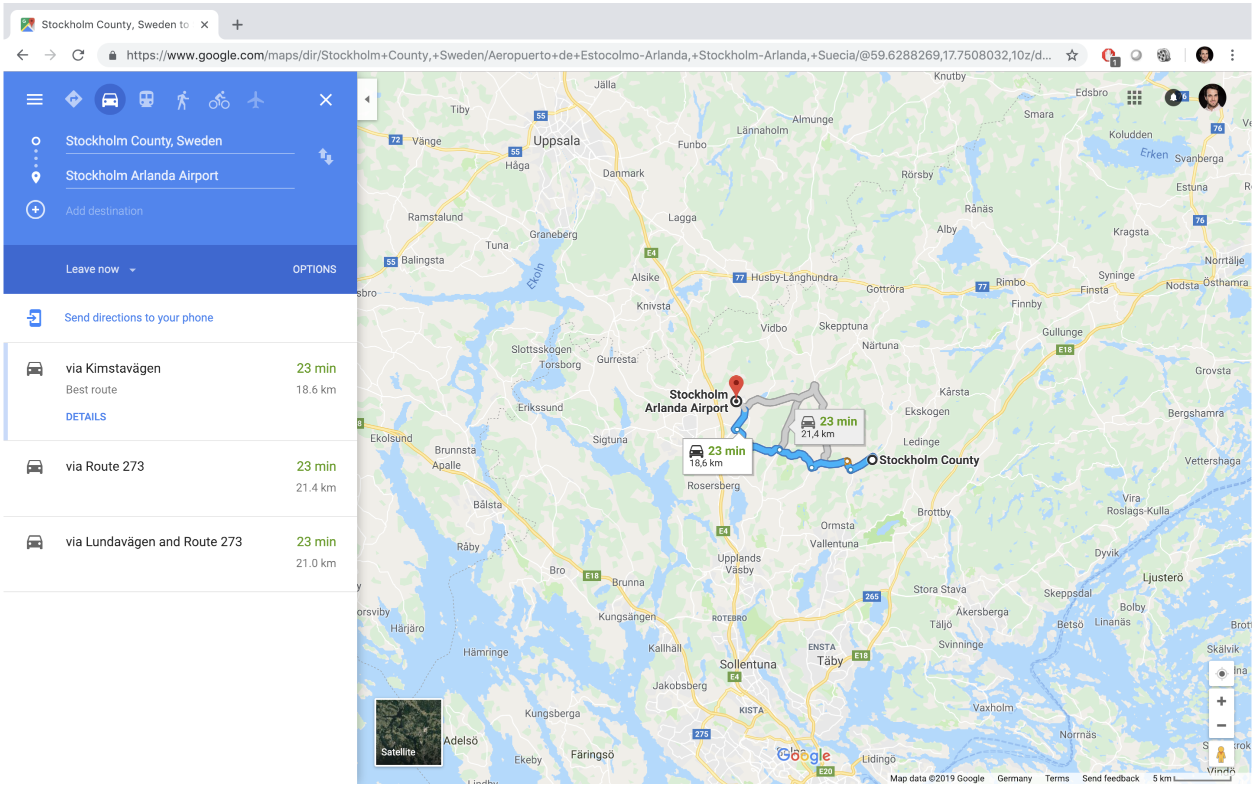
Notes: This figure shows the number of rapes (in logs) and fines for sex purchase in Sweden according to Brå during the period 1997-2014.

Figure 2: Distribution of rape



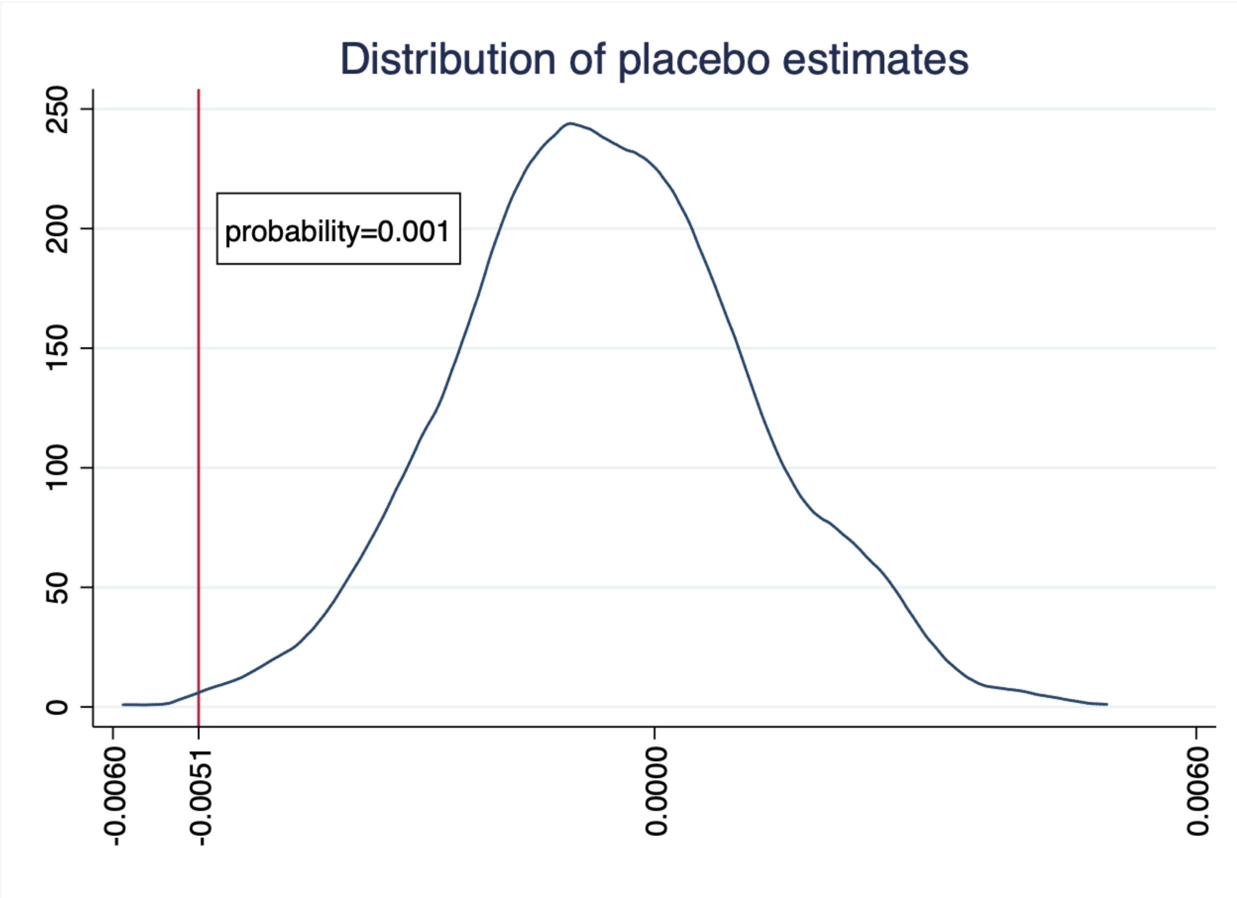
Notes: Histogram of rapes in Sweden according to Brå during the period 1997-2014.

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



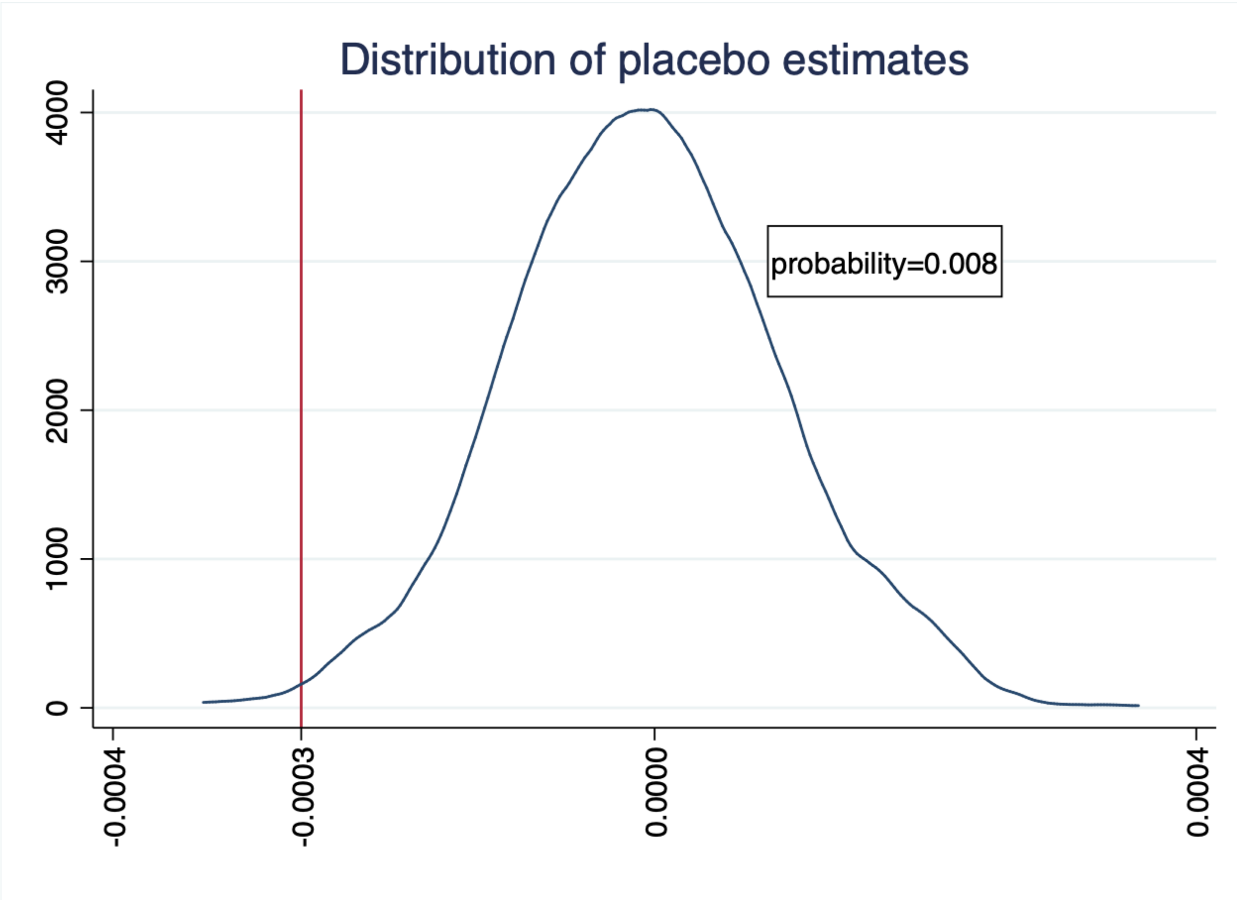
Notes: Distance from the closest airport to the region computed via Google maps and using car vehicle option. Example: Stockholm county.

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



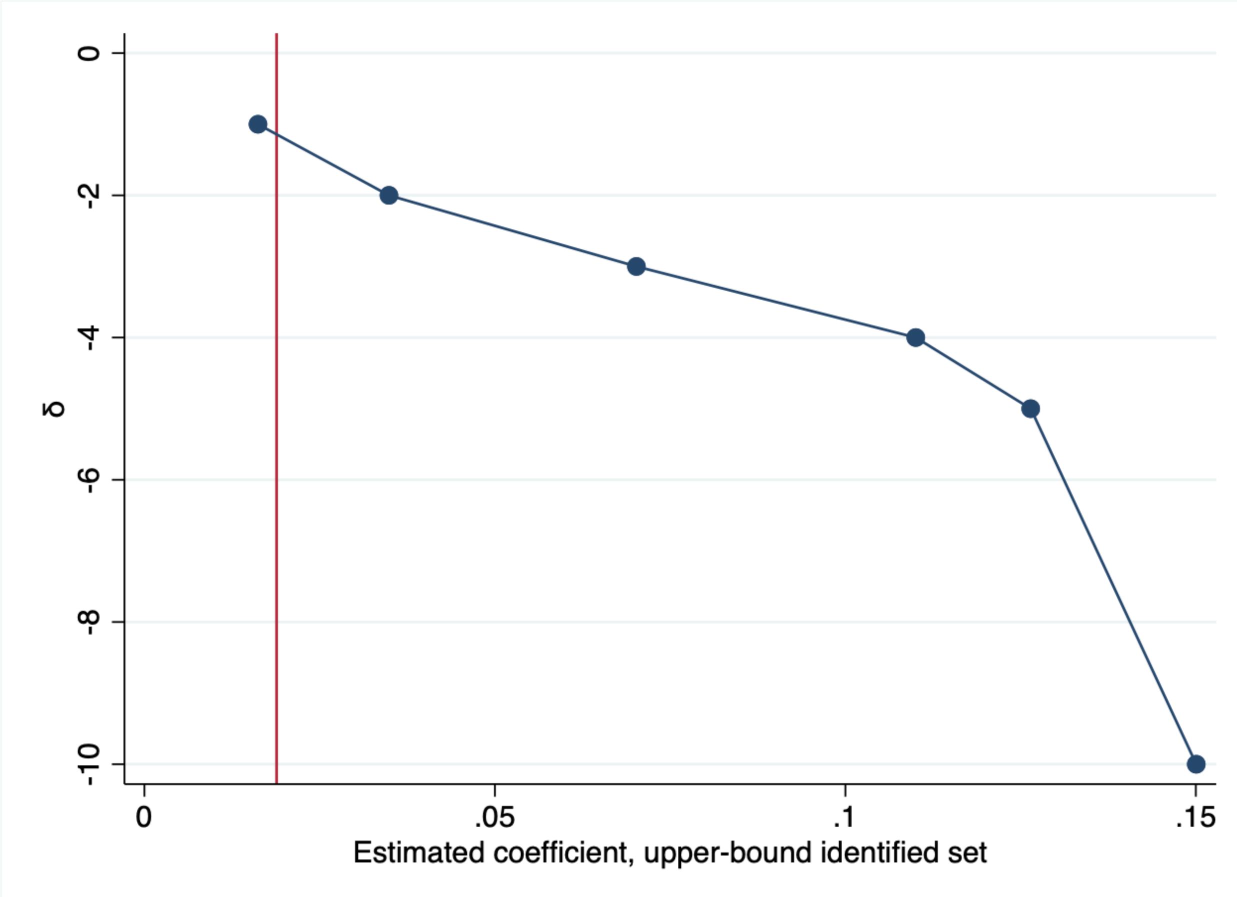
Notes: results of randomizing z_{1rmy} stratified at time period level with 1,000 permutations. The red vertical line represents the estimated coefficient in my main specification. The intersection between the red vertical line and the estimated distribution could be interpreted as the probability of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

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



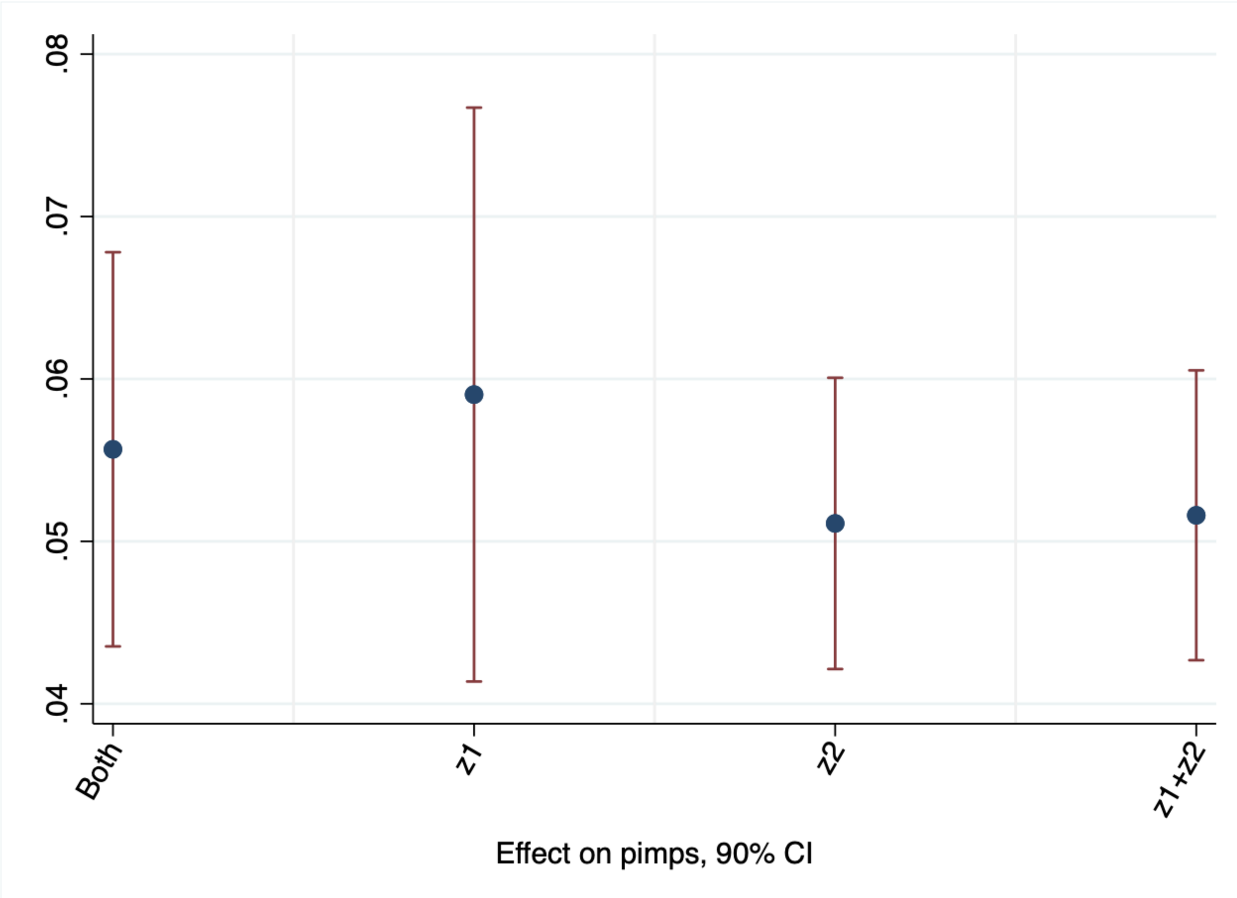
Notes: results of randomizing z_{2rmy} stratified at time period level with 1,000 permutations. The red vertical line represents the estimated coefficient in my main specification. The intersection between the red vertical line and the estimated distribution could be interpreted as the probability of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

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



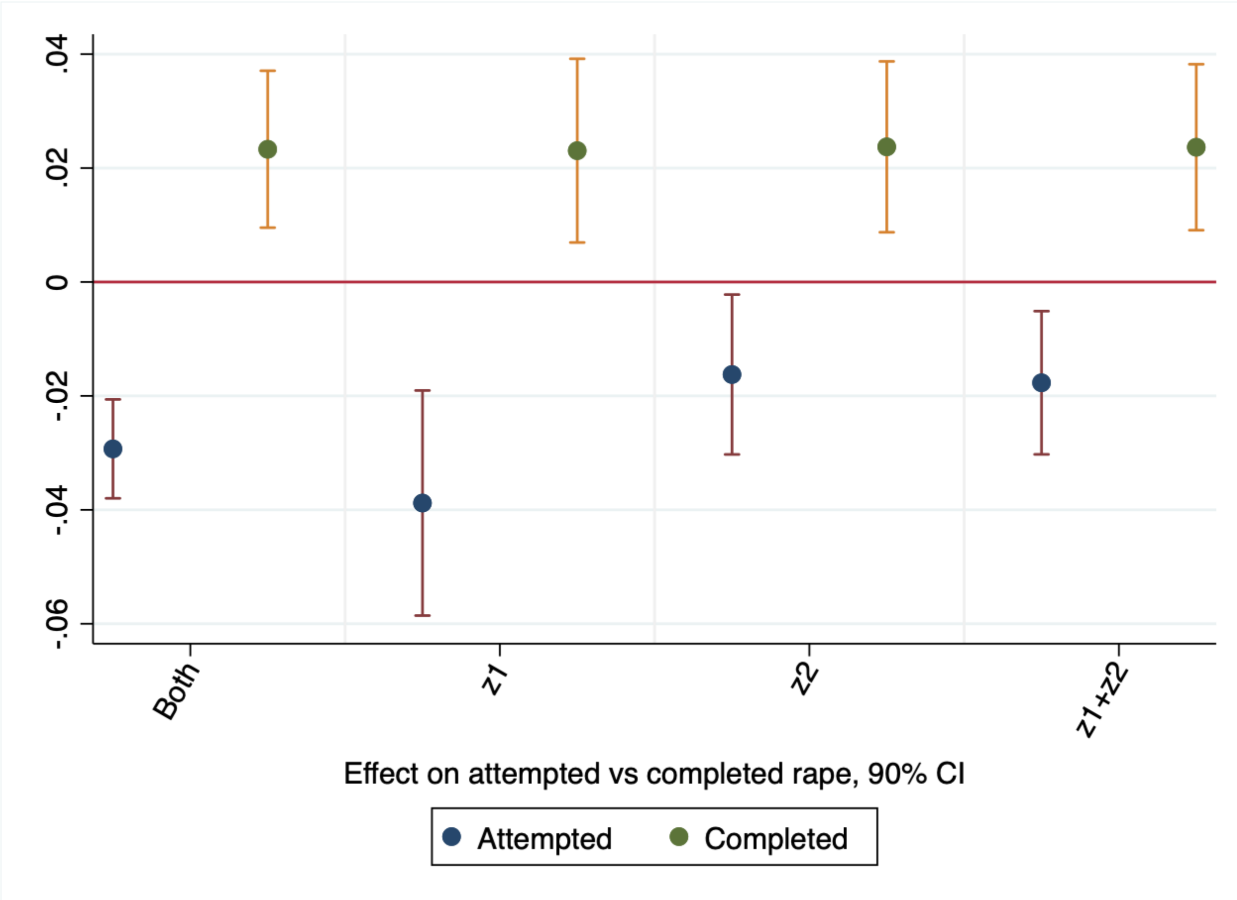
Notes: results of using Oster (2017) methodology to estimate identified sets of the estimated coefficient assuming selection on observables is proportional to selection on unobservables. The red vertical line represents the IV estimates in my main specification. The figure shows any δ lower than -1.2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

Figure 7: Effect on pimps



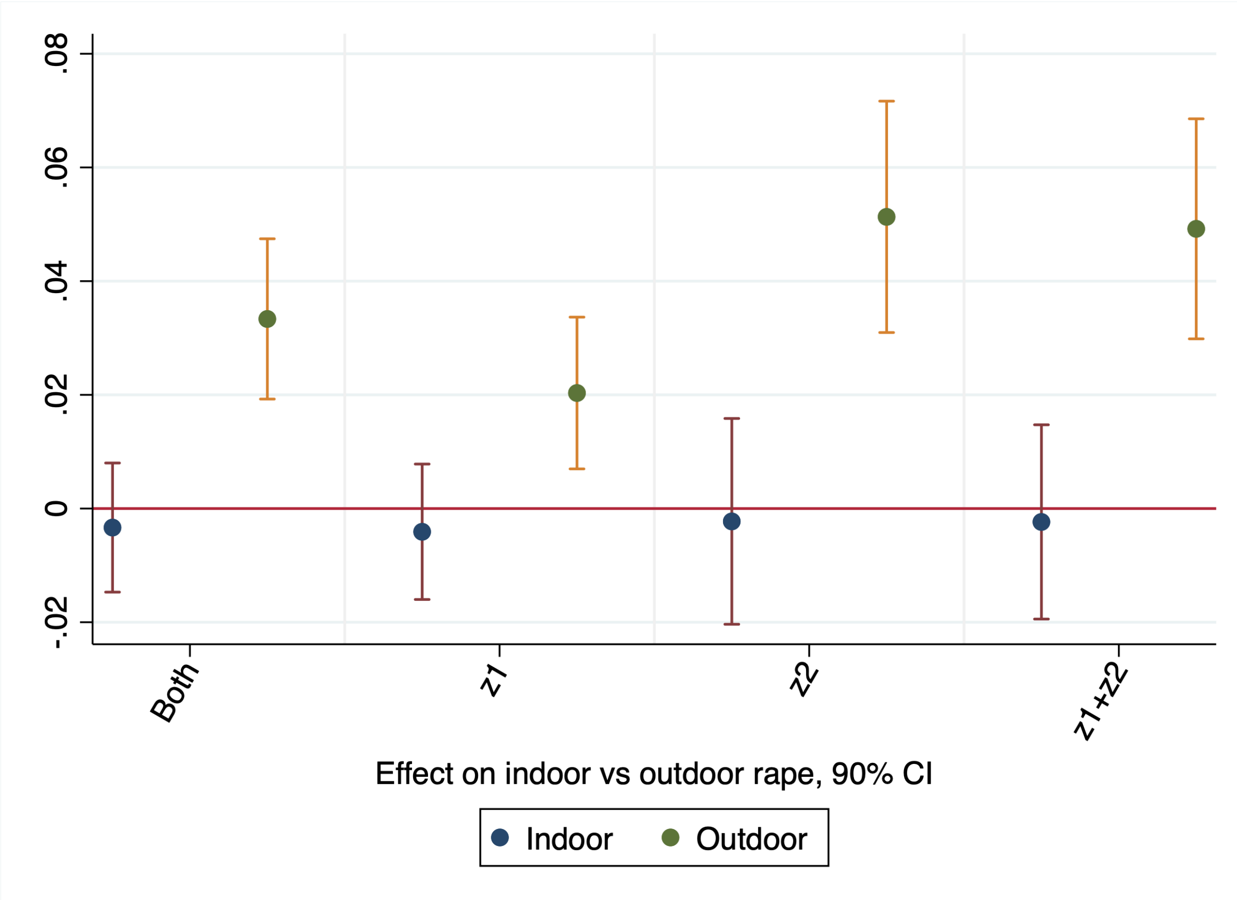
Notes: This figure shows the estimated coefficients, and respective 90 % confidence intervals, of running my main 2SLS specification for pimps using either both instruments (main first stage regression), only z_{1rmy}, only z_{2rmy} or their sum. These findings suggest fines for sex purchase increase 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 my main 2SLS 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 my main 2SLS 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: 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 regional level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: 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 5: 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 6: Exclusion restriction

VARIABLES	(1) Log(1+Rape)	(2) Log(1+Rape)	(3) Log(1+Rape)	(4) Log(1+Rape)	(5) Log(1+Rape)	(6) Log(1+Rape)
High Intercontinental flights	-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)
Low Continental flights	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

Robust standard errors 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 andA.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 andA.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.[26]

[26]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

Panel A: Whole period			
	mean	median	s.d.
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			
	mean	median	s.d.
z_{1rmy}	2.3	0	23.26
z_{2rmy}	49.12	0	520.16
Observations 504			
Panel C: After the introduction of the ban			
	mean	median	s.d.
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

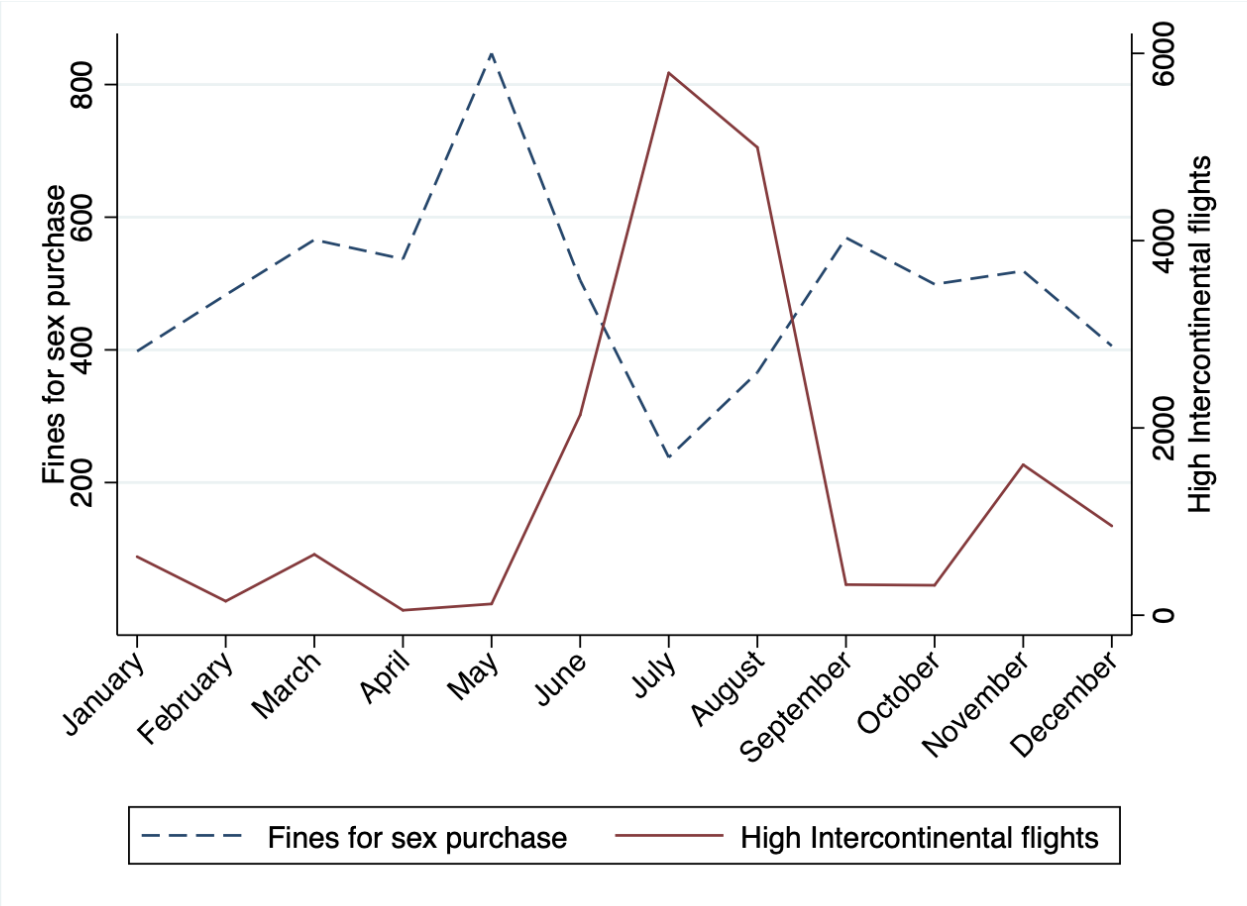


Figure A.2: z_{2rmy} distribution over months

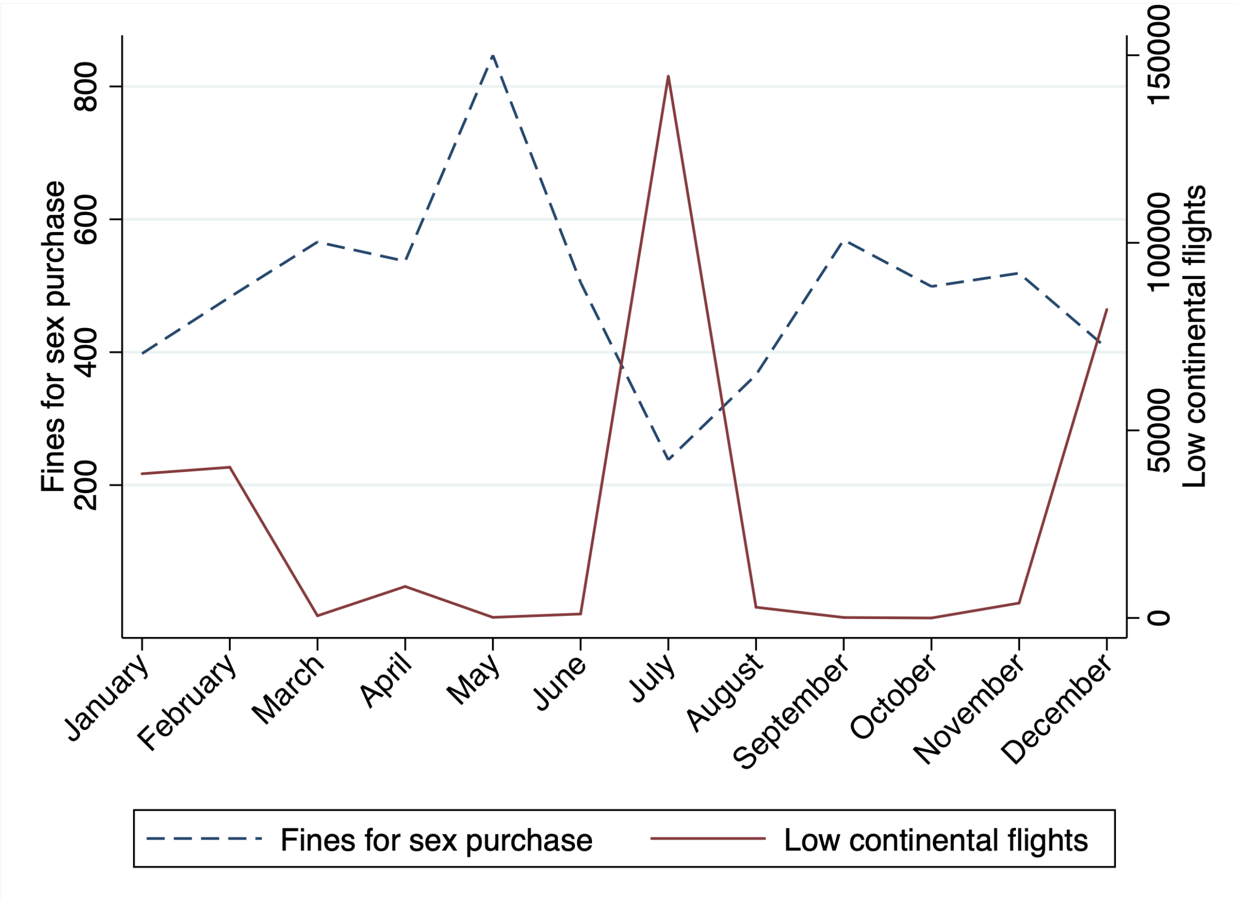


Figure A.3: z_{1rmy} evolution over years

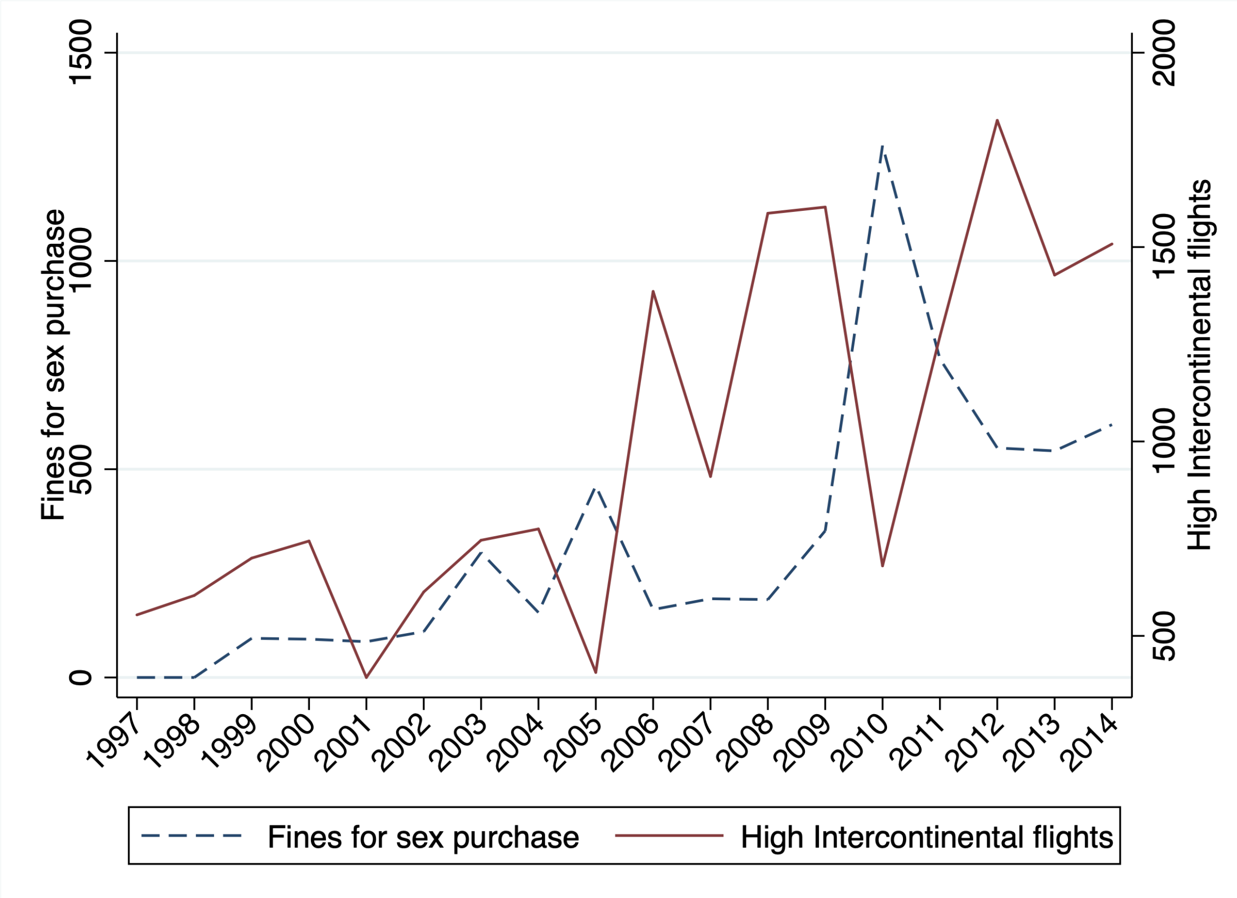
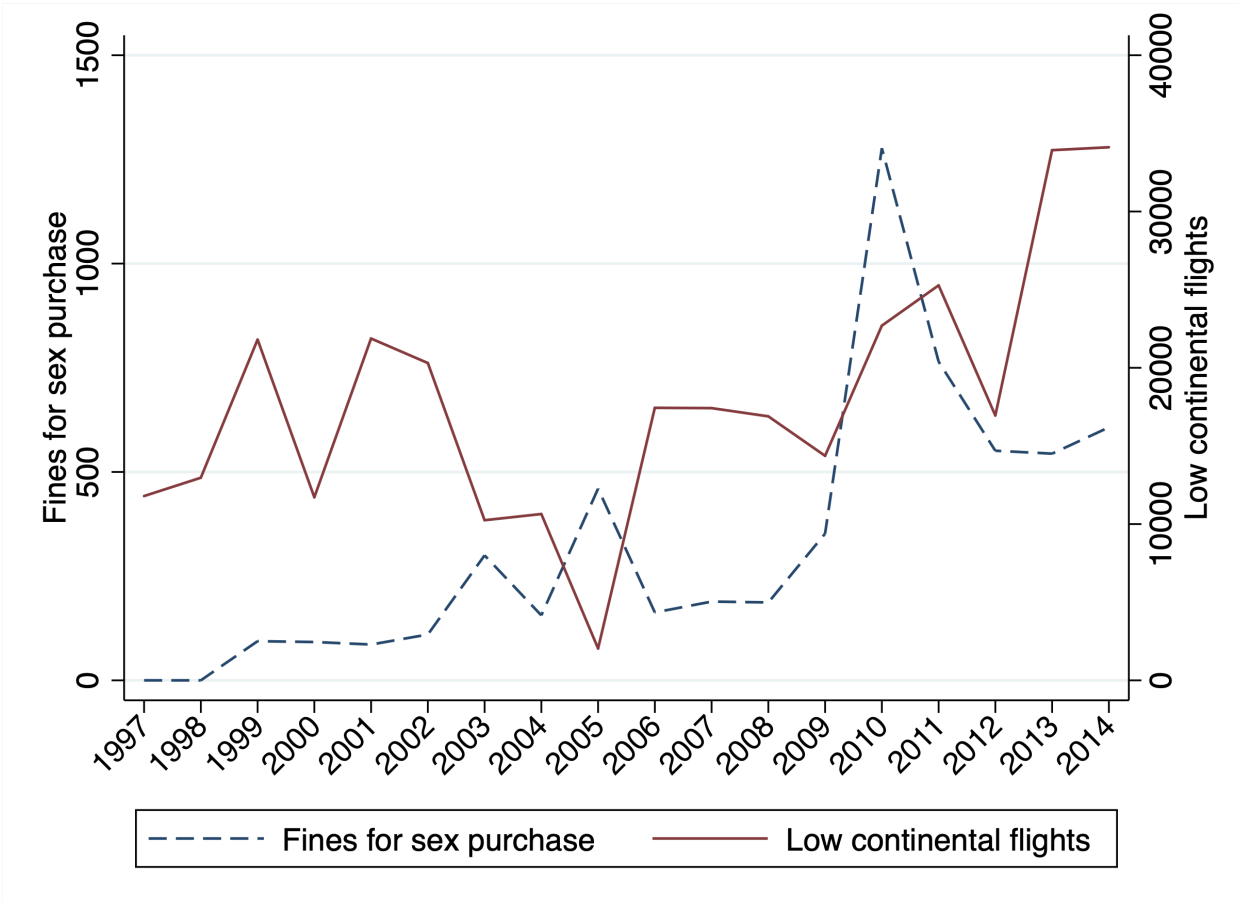


Figure A.4: z_{2rmy} evolution over years



B Balancing tests: Instruments

A key assumption of my 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.[27]

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

[27] 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 $i = 1, 2$:

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

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

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 my 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 Balancing test: Reverse causality

Likewise, I can use the same analysis of Section B to shed light on the potential reverse causality affecting my OLS estimates. Reverse causality arises from the concern that rape could affect fines for sex purchase. I run the same regression model as in 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.4. 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. This evidence could seem inconclusive even if it supports reverse causality. To this effect, 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.4 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 my OLS estimates.

Table A.4: 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

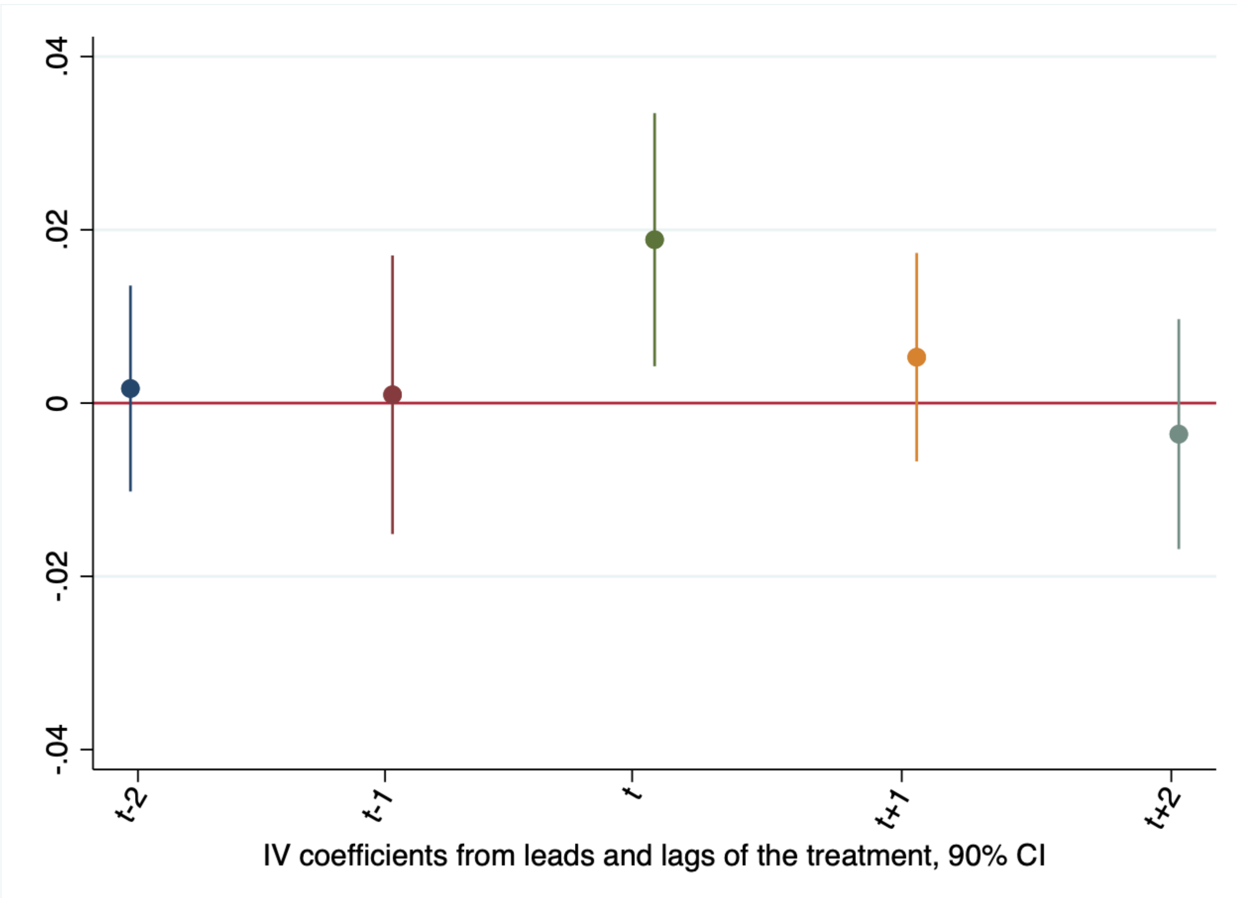
D Temporary vs permanent effect

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 my main 2SLS 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.