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Street Prostitution Zones and Crime

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Street Prostitution Zones and Crime

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May 2015

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
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Street Prostitution Zones and Crime

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ABSTRACT

Street Prostitution Zones and Crime*

This paper studies the effects of introducing legal street prostitution zones on both registered and perceived crime. We exploit a unique setting in the Netherlands where legal street prostitution zones were opened in nine cities under different regulation systems. We provide evidence that the opening of these zones was not in response to changes in crime. Our difference-in-difference analysis using data on the largest 25 Dutch cities between 1994 and 2011 shows that opening a legal street prostitution zone decreases registered sexual abuse and rape by about 30% to 40% in the first two years. For cities which opened a legal street prostitution zone with a licensing system we also find significant reductions in drug-related crime and long-term effects on sexual assaults. Perceived drug nuisance increases upon opening but then decreases below pre-opening levels in cities with a licensed prostitution zone. In contrast, we find permanent increases in perceived drug crime in the areas adjacent to the legal prostitution zones.

JEL Classification: J16, J47, K14, K23, K42

Keywords: prostitution, registered crime, perceived crime, regulation, difference-in-difference

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1 Introduction

The Netherlands holds a long tradition of regulated tolerance towards prostitution. Besides the well-known window prostitution in red light districts, the Dutch government also regulates other parts of the sex industry. Prostitution is known to be related to international trafficking organizations and various other forms of crime. For instance, the drug use of prostitutes and clients attracts people in drug trade. The illegal status of sex work also makes prostitutes more vulnerable to sexual violence and abuse (Flight, Van Heerwaarden and Lugtmeijer, 2003; Oostveen, 2008).[1] Despite the ongoing debate about legalizing prostitution, there exists little empirical evidence about the effects of government regulation.[2] An important reason for this is a lack of suitable data.

In this paper, we analyze empirically how the presence of a tippelzone affects aggregate crime in Dutch cities. A tippelzone is a designated legal street prostitution zone where soliciting and purchasing sex is tolerated between strict opening and closing hours at night.[3] The first tippelzone opened in The Hague in 1983 with eight other cities opening zones during the following three decades. The first objective of tippelzones was to deal with complaints of residents in the areas frequented by street prostitutes. A second objective was to improve the health and safety conditions of street prostitutes, who are often heroin and crack addicts and sometimes illegal immigrants. More recently four tippelzones closed again, mainly because of the escalation of conflicts between prostitutes. However, the closings were controversial.

In the empirical analysis we take advantage of the opening and closing of tippelzones to obtain empirical evidence for the relation between regulation of prostitution and crime. Since tippelzones did not come as a response to city specific trends in registered crime, we can exploit the exogenous variation in openings and closings at different time periods in different cities to obtain causal effects of regulation of street prostitution on crime. We estimate a difference-in-difference model using data on registered crime from the Ministry of Justice covering the period between 1994 and 2011. These data contain several measures of sexual, drug-related and violent crime in the 25 largest Dutch municipalities. In a second step we consider the effect of tippelzones on perceived crime obtained from the Population Police Monitor. This is a large-scale survey containing questions about feelings of safety and perceived criminal activity in the respondent's neighborhood covering the period between 1993 and 2006. We perform several empirical tests to assess endogenous crime trends around the moment of opening a tippelzone. Our study is one of the first to provide causal evidence for the connection between the regulation of prostitution and crime. Our study relates to a very recent paper by Cunningham and Shah

[1] Around 4.5 million women, of which one million younger than 18 years, are bought and sold worldwide into forced sexual exploitation in an industry generating profits of about 99 billion annually with women trafficked to Western Europe producing the highest per person revenue (ILO, 2012; 2014).

[2] See the appendix for an overview of prostitution regulation laws in various countries.

[3] Tippelzone is derived from the word *tippelen*, which in Dutch means street walking.

(2014) showing that an unexpected court order in Rhode Island decriminalizing indoor prostitution decreased rape offenses by 31%.

We begin with the premise that the market for sex is connected to criminal activity, such as sexual violence, drug trade, assault and organized crime. A tippelzone may act as a coordination point for these types of crime. Isolating street prostitutes within a delimited area attracts individuals prone to sexual violence and drug dealers which in turn attracts new drug addicts and dealers. However, police monitoring is higher in tippelzones than in other areas of the city so criminals of all types - sex-traffickers, pimps, drug dealers, violent clients - must trade off their willingness to operate in the tippelzone with the higher risk of apprehension. Indeed, upon the closing of tippelzones, supporters of the zones claimed that neither street prostitution nor it's surrounding crime would disappear. Both would simply spread around the city and become less manageable. The intense debate between supporters and opponents of tippelzones emphasizes the need to supplement theoretical models on prostitution and crime spillovers with empirical evidence.

Another argument in favor of tippelzones is that criminalization of prostitution forces the sex industry into the illegal underground market. Lee and Persson (2013) discuss the connection between legislation of the sex market and the involvement of sex-trafficking organizations. Two alternatives to criminalization are to fully legalize prostitution or to legalize it but restrict access to a limited segment of people using licenses. A unique feature of our analysis is that we can study both systems. Some tippelzones allowed free entry while others enforced a licensing system immediately or introduced it after some years.

Our empirical results show that opening a tippelzone reduces sexual abuse and rape. These results are mainly driven by a 30%-40% reduction in the first two years after opening the tippelzone.[4] For tippelzones with a licensing system we additionally find long-term decreases in sexual assaults and a 25% decrease in drug-related crime which persists in the medium to long-run. We do not find evidence for effects on other types of crime such as violent assaults and possession of illegal weapons.

We compare these effects on registered crime with those on perceived crime. Since policy decisions are influenced by public perception it is relevant to know whether these are in line with registered crime. Our results indicate that perceived drug nuisance increases by approximately 6%-points when a tippelzone is opened. These effects however vary depending upon the proximity to the tippelzone and whether the tippelzone enforced licensing from the start. For cities where licensing was introduced immediately, we find some indication that tippelzones achieved their stated goal to reduce the nuisance created by drug-addicted prostitutes and their followers overall in the city, but at the expense of residents living close to tippelzones.

The remainder of the paper proceeds as follows. In the next section, we provide a brief overview of the history of Dutch regulation of prostitution and a description of tippel-

[4] These results are very similar to those of Cunningham and Shah (2014).

zones. Next, we discuss the theoretical literature on the connection between regulation, prostitution and crime, and discuss possible mechanisms through which tippelzones can influence crime. In Section 3 we present the difference-in-difference model. Section 4 describes the data. Section 5 presents the results on registered crime. Section 6 discusses results on perceived crime. Section 7 concludes.

2 Background and Literature

2.1 Dutch Tolerance and Tippelzones

Historically, the Dutch policy towards prostitution has balanced periods of strict abolition with pragmatic regulated tolerance. In 1911, a law passed criminalizing anyone running a brothel or organizing prostitution. Shortly thereafter, a loophole was introduced which gave public prosecutors power to ignore criminal infringements (Outshoorn, 2004). The ban on brothels was therefore only enforced if other laws were broken. During the second half of the 20th century, problems surrounding prostitution became more prominent when a new wave of entrepreneurial criminals became involved in drug trafficking, protection rackets, human trafficking and money laundering (Brants, 1998). In response, the government gave power to local authorities to adapt city by-laws. This allowed for certain areas where sex shops, window prostitution and brothels were tolerated.

The gradual increase in regulated tolerance initiated a law in 2000 stating that prostitutes older than 18 years are allowed to work in legal sex houses or brothels under certain conditions (Daalder, 2007). In particular, they must be registered as workers, pay taxes and maintain regular health checks.[5] The new law affected prostitutes differently across the country since enforcement was again left to local municipalities. According to Brants (1998), the new law only provided a legal stamp to policies which already existed in many cities.

The policy change of interest in this paper is the re-localization of street prostitutes to designated tolerance areas, the so-called tippelzones. Several cities created tippelzones mainly in response to complaints about the noise and squalor created by crack and heroine addicted street prostitutes, their pimps and drug dealers.[6] Tippelzones were also intended to address the health and safety needs of prostitutes. According to Daalder (p.38, 2007) "the changes in policy regarding the streetwalking zones are not connected to the lifting of the brothel ban".[7]

[5]The social position of prostitutes is improved by a labor union and their financial consultancy organization.

[6]See for instance newspaper and municipality reports for Arnhem: *Digibron (1996), Tegenoffensief Arnhem in drugsproblematiek*; for Heerlen: Tops. P and W. Gooren (2009), *Police academy research, Een pact van het hart*; for Groningen: *Digibron (1995), Groningen krijgt na tien jaar tippelzone*; for Utrecht: *Gemeente eindrapport (2009), Evaluatie Utrechts Prostitutiebeleid*; for Amsterdam: *Trouw (1995), Amsterdam richt maandag tippelzone Theemsweg in.*

[7]See Flight, Hulshof, Van Soomeren and Soorsma (2006) for an evaluation of the lifting of the brothel

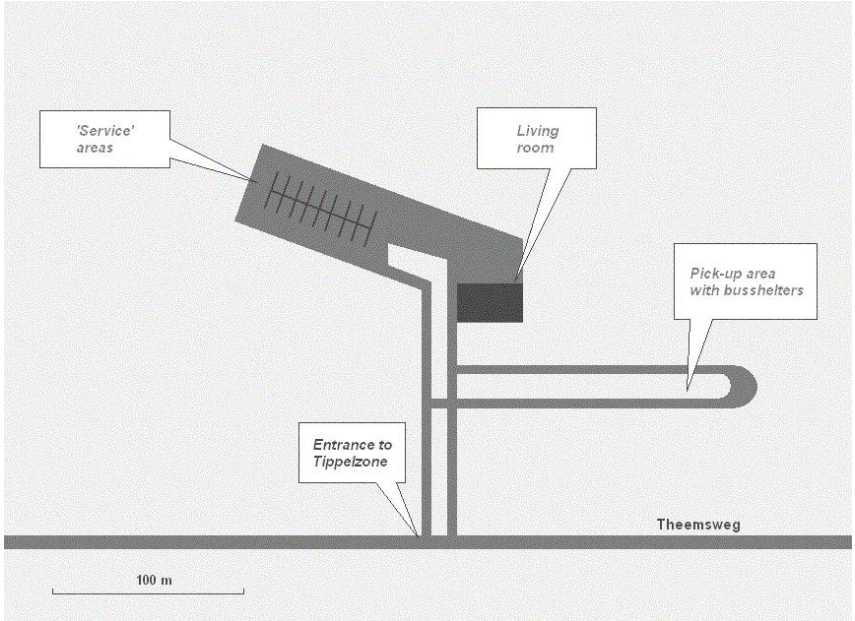


Figure 1: Layout of Amsterdam tippelzone (from Van Soomeren, 2004)

Tippelzones are equipped with a variety of features.[8] Many tippelzones provide resting quarters with washing amenities, clean needles and local medical assistance, and include separate servicing areas where prostitutes remain with clients in a safe environment (see Figure 1 for a map of the tippelzone in Amsterdam). Permanent supervisors or semi-permanent task forces are assigned to monitor the tippelzone and neighboring areas. The task forces are either rotating groups of agents from the local police district or new hirings for cities with larger tippelzones (11 additional officers in Amsterdam). In the early years of tippelzones, an implicit understanding was that the police did not prioritize arresting illegal workers, in particular not when they were in the resting quarters.

The first tippelzone appeared in The Hague in 1983 followed by a second group in the mid-nineties including Rotterdam (1994) and Amsterdam (1996). In total, nine Dutch cities introduced tippelzones between 1983 and 2004 (see Table 1). The tippelzones were placed in non-residential industrial areas slightly outside the city center. Since full decriminalization of prostitution was not legally enforceable in the 1980s and 1990s, cities which opened a tippelzone simply applied selective decriminalization. They did so by enforcing more strictly the by-laws and ordinances forbidding street prostitution anywhere other than in a tolerated zone.

After their introduction tippelzones remained controversial. In medium-sized cities they generally functioned well attracting 20 to 50 prostitutes a night although some tippelzones faced difficulties in reaching the targeted population of drug-addicted prostitutes (Oostveen, 2008). Tippelzones in larger cities often attracted over 100 prostitutes in a

ban.

[8]See Van Soomeren, (2004) for a detailed discussion of the Amsterdam tippelzone.

Table 1: Opening and closing of tippelzones in the Netherlands

City	Opening year (month)	Start licensing	Closing year (month)
The Hague	1983	never	2006 (Mar.)
Utrecht	1986	2005 (Oct.)	
Rotterdam	1994 (Nov.)	2003 (Apr.)	2005 (Sep.)
Amsterdam	1996 (Jan.)	2002 (Jun.)*	2003 (Dec.)
Arnhem	1996 (Jun.)	2003 (Nov.)	
Groningen	1998 (Jan.)	never	
Heerlen	2000 (Jun.)	2000 (Jun.)	2013 (Jan.)
Nijmegen	2000 (Oct.)	2007 (Sep.)	
Eindhoven	2004 (Dec.)	2004 (Dec.)	2011 (May)

*Amsterdam did not formally have a licensing system but implemented strict policing from June 2002 onwards verifying immigration status.

single night (Hulshof and Flight, 2008).[9] In the mid 2000s, some tippelzones started experiencing difficulties due to an increasing number of prostitutes from Eastern Europe and South America. To limit the number of prostitutes, some tippelzones introduced a licensing system. Table 1 shows the moment licensing was introduced. In some cities like Heerlen and Eindhoven the licensing systems were present from the start while others introduced them later to control the inflow of new workers. Licensing systems favored known local prostitutes and drug-addicted prostitutes.

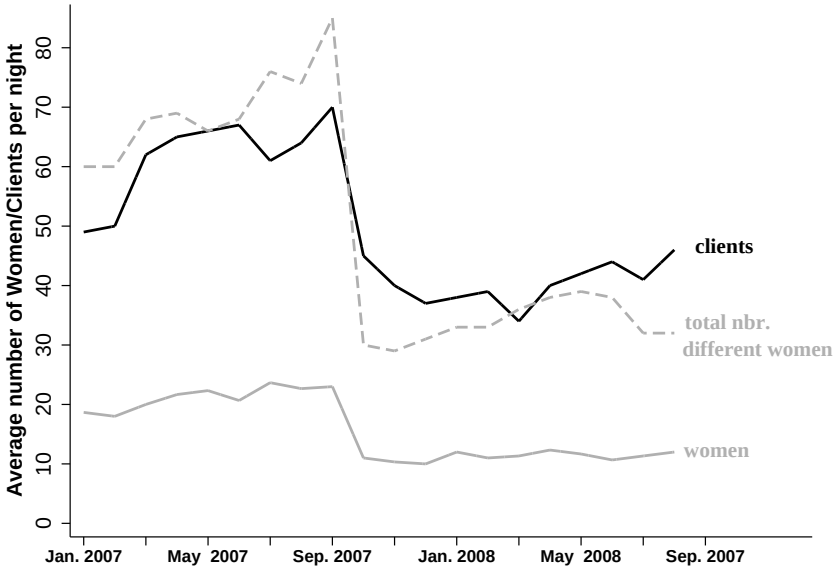
The Rotterdam tippelzone was notoriously turbulent mainly because of conflicts between prostitutes. Similar problems in The Hague and Amsterdam forced the shutdown of the tippelzones in these three cities (Amsterdam in 2003, Rotterdam in 2005, The Hague in 2006). The tippelzone in Eindhoven closed in 2011 despite positive assessments, and in Heerlen in 2013 because too few prostitutes were working there anymore. As of 2014, four tippelzones are still open across the Netherlands. Korf, Van Vliet, Knotter, and Wouters (2005) suggest that a small share of the prostitutes previously working in Amsterdam, The Hague and Rotterdam moved to the tippelzone in Utrecht. However, most were refused due to licensing restrictions from other tippelzones and also barred from brothels, window soliciting and other legal sex establishments which also required licensing.

The closing of tippelzones was controversial. Law enforcement agents were the main supporters for closing tippelzones which they claimed acted as breeding grounds for illegal trafficking of women, blackmail, violence and kidnapping. However, health workers claimed that neither street prostitutes nor the surrounding crime would disappear upon closing tippelzones. The problem would simply spread to other areas in the city and go underground making it more difficult to monitor (Van Soomeren, 2004). Moreover, closing tippelzones would complicate addressing health needs of prostitutes and would make them more vulnerable to sexual abuse and violence.

Oostveen (2008) studies how clients in Nijmegen responded to shifts in the illegal

[9] On average, prostitutes in the Amsterdam charged around €25 for a standard service and earned €80 a night. Prices, however, varied by the number of prostitutes present in the tippelzone.

Figure 2: Supply and demand response to the introduction of licensing for the Nijmegen tippelzone in September 2007 (figure reproduced from Oostveen, 2008)



segment of prostitutes in tippelzones due to the introduction of a licensing system. Confronted with an increasing inflow of Eastern European prostitutes, the tippelzone in Nijmegen introduced strict licensing in September 2007 accompanied by intensive police control during the first two weeks. Figure 2 illustrates the response of prostitutes and clients. Immediately after the introduction of the licensing system the number of prostitutes reduced from over 80 different prostitutes a month to about 35.[10] Similarly, the average number of prostitutes working in the zone decreased from 20 to 10 per night. This change was accompanied by a 30%-35% reduction in the average number of clients. Although it is possible that some clients stopped procuring by prostitutes, these parallel shifts suggest that introducing the licensing system pushed a share of street prostitutes to the underground market.

2.2 Prostitution, Regulation and Crime

Several theories in crime location choice and crime displacement are relevant to understand possible crime spillover effects of tippelzones. From a criminology perspective, tippelzones can be seen as coordination 'hot spots' where the prostitution market attracts criminals who in turn attract other potential criminals. Cohen and Felson (1979) label this convergence the *routine activity approach*. Brantingham and Brantingham (1995) add to this that different urban structures and planning can change the pool of criminals by inducing new people into criminal activity or by inhibiting the actions of existing criminals.

[10] According to case workers this number slightly underestimates the total since it only measures women entering resting quarters.

Assuming the probability of apprehension were to stay constant in all areas of the city, tippelzones could increase crime by accelerating the process of convergence for drugs and human trafficking, and by generating new opportunistic criminals.

However, given the higher probability of apprehension near the tippelzone, a rational criminal (existing or potential) must weigh the expected gains from offending against the probability of apprehension and the size of the punishment (Becker, 1968). Depending on the type of crime, criminals may then be incapacitated or they may be forced to work at the outskirts of the tippelzones with lower expected profit but also lower probability of apprehension (Deutsch and Epstein, 1998). The presence of a tippelzone can therefore prevent some types of crime from occurring by disenabling existing criminals and deterring future ones. For example, a tippelzone should lead to a decrease in sexual violence on street prostitutes who relocate to the tippelzone, which is important considering the vulnerability of street prostitutes. According to Venicz and Vanwesenbeeck (2000) almost half of the women in the sex industry in the Netherlands experience some form of assault, sexual abuse, or other form of violence. In 47% of the cases the perpetrator was a client and in 37% of the cases an (ex-)pimp.[11]

Lee and Persson (2013) provide additional insight in how tippelzone opening and regulation can influence the involvement of sex trafficking organizations. According to their theoretical model, a government which judges full elimination of prostitution to be impractical or unattainable can instead prioritize reducing a certain type of consumption, namely involuntary prostitution. Their model predicts that full criminalization of selling and buying sex, which they label *the traditional model*, will reduce trafficking relative to *full legalization* of the sex market only after eliminating voluntary prostitution. This equilibrium is arguably suboptimal since it forces the entire sex market underground. An alternative model, which they label *the Dutch model*, allows prostitutes to sell sex provided they obtain a license after passing a background check. As long as voluntary prostitution exists, their model predicts that the Dutch model unambiguously decreases trafficking relative to the full criminalization model given a fixed probability of arrest for illegal prostitutes.

According to the model of Lee and Persson (2013), the supply and share of involuntary prostitutes trafficked by organized criminal groups should be larger in cities with tippelzones that do not enforce a licensing system. This involvement may increase other 'transit' crimes since organized criminal groups often also engage in drug trafficking, smuggling illegal immigrants and arms trafficking (Kruisbergen, et al., 2012).[12] The introduction of a licensing system in previously unregulated tippelzones should reduce involuntary prostitution in the tippelzone, but it is unclear what spillovers to expect in other areas of the

[11] Their sample excludes street prostitutes who, Venicz and Vanwesenbeeck (2000) argue, may be even more vulnerable to violence than prostitutes operating in clubs or windows.

[12] Gerben, et al. (1997) study criminals arrested for sex trafficking in the Netherlands and find that the associated criminal organizations range from 2-3 collaborators to very substantial national or international organized crime networks.

city. Ultimately, we need an empirical analysis to evaluate the effect of a tippelzone on aggregate crime in a city.

To the best of our knowledge, there exists no empirical study evaluating the effects of regulating street prostitutes on crime.[13] The closest study to ours is a paper by Cunningham and Shah (2014) which exploits an unexpected legal change in the state of Rhode Island that temporarily decriminalized indoor prostitution. They find that decriminalization leads to a 31% decrease in reported rape offenses. In addition, there are some empirical studies discussing spatial spillovers of crime control (see Hesseling, 1994, and Guerette and Bowers, 2009, for overviews in criminology). Weisburd et al. (2006) find that an increase in police surveillance in two high-crime neighborhoods in Jersey City reduces drug-related crime both within and around the targeted area. They attribute this to gang affiliation which avoids that drug-related crime transfers easily to new locations. Di Tella and Schargrodsky (2004) and Draca, Machin, Witt (2011) find no sign of displacement effects when focusing on exogenous increases in the supply of police in specific areas in the wake of terrorist attacks. Although both studies find a decrease in crime in the areas with additional police, neither study find any reduction in crime in adjacent locations. Machin and Marie (2011) reach the same conclusion when looking at a street crime initiative allocating extra resources to certain police force areas in England and Wales.

While spatial relocation receives most attention, displacement of crime can also be intertemporal or by changing target, offense, tactic, or offender. The evidence for such crime spillovers is mixed. Adda, McConnell and Rasul (2014) consider an experiment where cannabis possession in small quantities is depenalized in one London borough. They find that the depenalization leads to an increase in offences for large quantity cannabis possession in this area. Half of the increase is attributable to drug tourism from neighboring boroughs. Jacob, Lefgren and Moretti (2007) focus on weather shocks and show that criminals who are prevented from committing property offenses in a given week try to compensate for lost income by engaging in higher levels of criminal activity in subsequent weeks.

3 Empirical Model

We use a difference-in-difference specification to study the effects of tippelzones and licensing on various types of crime. Let Y_{it} denote the observed crime of city i in year t

[13] Akee, Bedi, Basu and Chau, (2010) and Cho, Dreher and Neumayer, (2013) use national level data to investigate correlations between prostitution legislation in different countries and constructed variables for sex-trafficking. Their findings remain inconclusive since the constructed measures for trafficking are likely endogenous to country legislation and institutions. The Dutch publication *Trafficking in Human Beings: Visible and Invisible. A quantitative report (2007-2011)* provides more detailed arguments on the problems of measuring human trafficking at a national level.

which is modeled in our baseline model as

$$\ln(\mathrm{Y}_{it}) = \alpha_i + \delta_0\,\mathrm{D}^-_{it} + \delta_1\,\mathrm{L}_{it} + \delta_2\,\mathrm{D}^+_{it} + \beta\,\mathrm{X}_{it} + \mu_t + u_{it} \tag{1}$$

The city fixed effects are captured by α_i and the time trend μ_t is modeled using year fixed effects. The variables D^-_{it} and D^+_{it} take value one if in city i in year t there was a tippelzone which opened without a licensing system (-) or with a licensing system (+), respectively. The variable L_{it} denotes the presence of a licensing system which was introduced some time period after the opening. So, L_{it} can only take value one if initially there was a tippelzone without licensing, so if D^-_{it} equals one. Finally, X_{it} describes other time-varying regressors. In the estimation we use a logarithmic specification for our outcome variable to deal with the larger variation in crime in larger cities.

The parameters of interest are δ_0, δ_1 and δ_2. Given our log-linear specification, these parameters represent the proportional effect of a tippelzone and licensing on local crime. If $\delta_0 = \delta_2$, then enforcing a licensing system upon opening the tippelzone has no additional effect on crime. And if $\delta_1 = \delta_2 - \delta_0$, then implementing a licensing system immediately or after some time has the same effect on crime. In order to give a causal interpretation to δ_0, δ_1 and δ_2 we assume that cities follow a common time trend in crime. We justify this assumption in subsection 4.1.

It is not unlikely that crime rates within cities are serially correlated. Given that our data only contain 25 cities, we produce statistical inference based on the Cameron, Gelbach and Miller (2008) wild bootstrap approach. The associated wild bootstrap standard errors turn out only slightly larger than the usual Huber-White cluster robust standard errors. As additional robustness checks on the parameters and standard errors we also estimated the model using polynomial time trends and specifying an AR(1) process for the error terms. Our results are robust to these alternatives so we present only the results from our main specification which imposes less structure on the model.[14]

4 Registered Crime Data

Our data are made available by Statistics Netherlands and contain administrative records of crime reports collected by the Dutch Prosecutor General (PG). We observe the total annual number of reports for different crime categories rounded to the nearest fifth integer for the 25 largest municipalities. The balanced panel data cover the period 1994-2011. Our crime outcome variables are sexual abuse and rape, drug crime (excessive drug possession, processing or trafficking), assaults and illegal weapon possession.[15] The latter two give an indication about the presence of criminal networks. Our motivation for focusing on these

[14]The data and STATA programming code for all results are available at skastoryano.com.

[15]Since the data are based on records from the police administration, there is likely underreporting for certain types of crime. This problem may be particularly relevant for sexual crime and some types of violent crime committed on people fearing extradition, incarceration or social stigma from reporting.

Table 2: Crime rates for cities with and without tippelzones (standard deviations in brackets)

	Tippelzone				No Tippelzone	
	big cities		medium cities			
Annual crime reports per 1000 inhabitants						
Sexual Abuse & Rape	0.18	[0.05]	0.15	[0.06]	0.14	[0.07]
Sexual Abuse	0.08	[0.02]	0.06	[0.03]	0.07	[0.05]
Rape	0.10	[0.03]	0.09	[0.04]	0.07	[0.04]
Drugs	1.49	[0.53]	1.56	[1.00]	1.28	[1.42]
Assault	2.13	[0.86]	1.87	[0.59]	1.79	[0.58]
Weapons	0.56	[0.14]	0.44	[0.25]	0.42	[0.51]
City characteristics						
Population	597,489	[115,163]	172,419	[54,891]	113,114	[35,475]
Density (Population per km^2)	4326	[1135]	2298	[505]	1956	[1456]
Males 15-65	210,145	[45,886]	61,702	[20,054]	39,226	[12,739]
Household Income (1000 €)	29.05	[1.34]	28.99	[2.00]	30.50	[1.85]
Higher education (%)	0.30	[0.08]	0.32	[0.09]	0.25	[0.07]
Immigrants (%)	0.11	[0.02]	0.06	[0.02]	0.05	[0.02]
Benefits recipients (%)	0.07	[0.01]	0.08	[0.02]	0.08	[0.01]
Political party of mayor						
Socialist (*PVDA*)	0.48	[0.50]	0.56	[0.50]	0.41	[0.49]
Christian (*CDA* or *CU*)	0.26	[0.44]	0.12	[0.33]	0.31	[0.46]
Liberal (*VVD* or *D66*)	0.26	[0.44]	0.32	[0.47]	0.28	[0.45]

3 big cities with a tippelzone include Amsterdam, Rotterdam and The Hague. *6 medium cities* with a tippelzone include Utrecht, Eindhoven, Groningen, Nijmegen, Heerlen and Arnhem. *16 cities* without tippelzone include Almelo, Breda, Deventer, Dordrecht, Enschede, Haarlem, Helmond, Hengelo, Leeuwarden, Leiden, Maastricht, Schiedam, Tilburg, Venlo, Zwolle and 's-Hertogenbosch.

crime categories is their frequent association with prostitutes and trafficking organizations as described in subsection 2.2.

The first panel of Table 2 presents the average yearly crime rates during our observation period. We distinguish between the three largest cities which all had a tippelzone (Amsterdam, Rotterdam and The Hague), the six medium-sized cities which opened a tippelzone (Utrecht, Eindhoven, Groningen, Arnhem, Nijmegen and Heerlen), and the sixteen medium-sized cities which never opened a tippelzone. Larger cities have, on average, higher crime rates, with the exception of drug-related crime. Average crime rates in medium-sized cities with and without tippelzones are very similar, but again the exception is drug-related crime. In general, drug-related crime rates are slightly higher in cities with tippelzones.

The second panel describes characteristics of the cities. Tippelzones cities have, on average, more inhabitants and are more densely populated. Other characteristics do not differ substantially. On average, about 35% of the total population are men between 15 and 65 years old. Individuals in cities with tippelzones are a bit more educated but have a slightly lower average household income. Medium-size cities and cities without a tippelzone have a similar amount of immigrants and both have lower amounts than large cities. There are also no differences in the share of social security benefits recipients

between cities. Finally, there is no clear relation between the political party of the mayor and whether the city has a tippelzone.[16] This is not surprising since tippelzones were not a partisan policy.

4.1 Assessing Crime Trends

As mentioned in section 3, our key identifying assumption is that cities follow a common trend in crime. This common trend assumption imposes that, withholding any effect of opening a tippelzone or enforcing a licensing system, tippelzone cities and non-tippelzone cities would have followed the same trend in aggregate crime. It excludes, for instance, the possibility that tippelzones are responses to city-specific increases in aggregate crime, that other crime-targeting policies were introduced at the same time as tippelzones, or that the introduction of tippelzones produced spillovers in crime to other cities.

To justify the common trend assumption, we exploit the variation between cities in the moment of opening tippelzones. For each city which opened a tippelzone, we compare the trend in crime rates prior to the opening with the trend in average crime rate in cities which never opened a tippelzone for the same time period. Figure 6 presents this difference in sexual abuse and rape in years leading up to opening a tippelzone for each opening city separately. The solid line is the average for the five medium-sized cities which opened a tippelzone during our observation period.

The trends in crime show no systematic increases or decreases in sexual abuse and rape in the years prior to opening a zone.[17] However, all medium-sized cities show a sharp decrease in sexual abuse and rape immediately following the opening of a tippelzone both for cities which enforced a licensing system (Eindhoven, Heerlen) and those which did not (Nijmegen, Arnhem, Groningen). The decrease in the first two years is in the order of $(\exp(-0.4) - 1) \times 100\% \approx -33\%$.

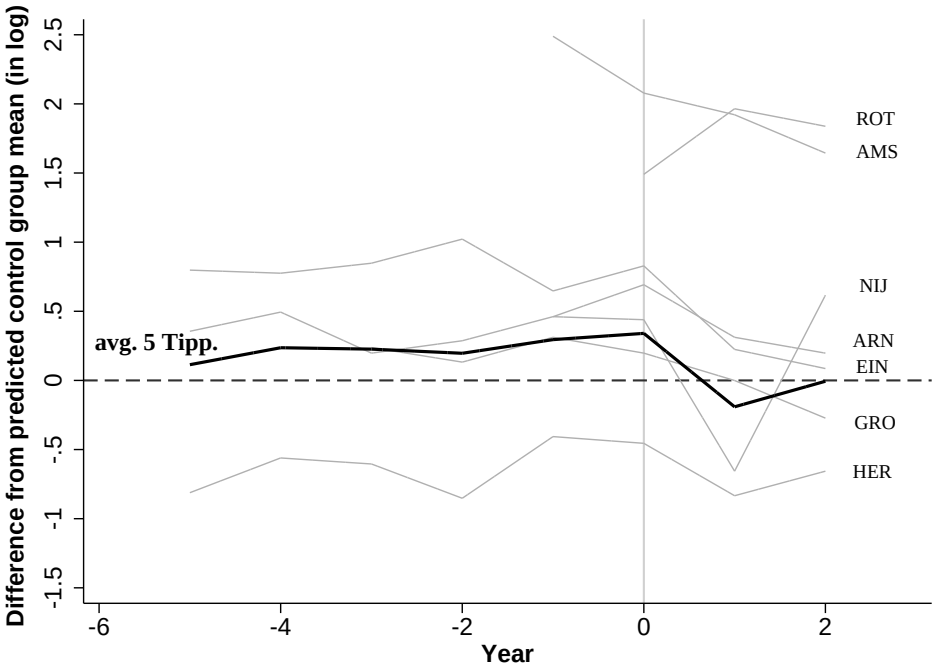
We only observe one pre-opening period for Rotterdam, two pre-opening periods for Amsterdam, and none for The Hague. This means we can not use long-term trends in both cities to justify a common trend for these cities. Also, the three large cities closed tippelzones in a span of three years which implies that most of the identifying power for these large cities comes from comparison with medium-sized cities. For this reason we limit most of the subsequent analysis and discussion to medium-sized cities.

Figures 5 and 6 in the appendix provide more evidence in favor of the common trend assumption. In particular, these figures show that prior to opening the tippelzones trends between cities for the different types of crime are very similar and the same holds for the crime trend prior to introducing a licensing system.

[16] In the Netherlands mayors are not elected, but appointed. The political power of mayors is therefore limited. Cities often have mayors from the same political party for a long period.

[17] In our empirical analysis the level differences in crime will be captured by the city fixed effects α_i.

Figure 3: Crime rates in cities with a tippelzone compared to cities without a tippelzone.



The no-tippelzone counterfactual for a city i is generated by estimating model (1) on twenty-one cities leaving out city i and the three largest cities and then averaging the fitted values fixing $\mathrm{D}_{it}^{-} = 0$, $\mathrm{L}_{it} = 0$ and $\mathrm{D}_{it}^{+} = 0$.

5 Estimation results: registered crime

In this section we first discuss the results from our baseline model on registered crime data. Next, we look at time varying effects and provide robustness checks accounting for possible spillover effects between cities.

5.1 Baseline results

Table 3 shows estimation results for our baseline difference-in-difference model. The left frame presents results using data on the 22 medium-sized cities, the right frame also takes the three large cities into account.[18] The estimation results show that an open tippelzone significantly reduces citywide sexual abuse and rape by about $(\exp(-0.19) - 1) \times 100\% \approx -17\%$. This effect is mainly driven by a $37\% - 40\%$ reduction in sexual abuse.

The effects of opening a tippelzone are very similar regardless of whether licensing was imposed from the start. However, we find that introducing licensing later substantially increases sexual abuse and rape, and the effect is significant for sexual abuse. These opposite effects of licensing are not contradictory. Cities which immediately imposed licensing initially distributed licenses to all known street prostitutes but barred future entrants. Non-licensed tippelzones allowed free entry and therefore attracted a large share of foreign prostitutes, in particular Eastern European prostitutes after the opening of EU

[18] In Tables 11 and 12 in the appendix we present additional results without covariates and specifications which include a two year lead dummy to check for pre-opening shifts in crime.

Table 3: Effect of opening a tippelzone and licensing on citywide registered crime

	Sex Ab. & Rape	Sex Ab.	Rape	Sex Ab. & Rape	Sex Ab.	Rape
	22 cities with covariates			*25 cities with covariates*		
Open noLic. (δ_0)	-0.198**	-0.358	-0.090	-0.205*	-0.304	-0.133
	(0.098)	(0.217)	(0.072)	(0.114)	(0.201)	(0.101)
Intro. Lic. (δ_1)	0.286	0.413**	0.220	0.168	0.250**	0.115
	(0.258)	(0.208)	(0.277)	(0.134)	(0.124)	(0.179)
Open Lic. (δ_2)	-0.184*	-0.447***	0.012	-0.169*	-0.411***	0.004
	(0.094)	(0.154)	(0.139)	(0.094)	(0.154)	(1.456)
Closing				0.023	0.160	-0.045
				(0.144)	(0.208)	(0.398)
N (city x year)	395	395	395	450	450	450
R^2	0.63	0.44	0.55	0.82	0.69	0.77
City fixed effects	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes
Covariates	yes	yes	yes	yes	yes	yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Significance (and standard errors in parenthesis) based on Cameron, Gelbach and Miller (2008) wild bootstrap approach with 499 replications; Based on yearly data over the period 1994-2011. The 22 cities exclude the large cities, Amsterdam, Rotterdam, The Hague (and Eindhoven in 2011). Covariates are indicators for political party of mayor, *log(population male 15-65)*, *log(pop. density)*, *income (%)*, *immigrants (%)*, *unemployment insurance recipients (%)*, *higher educated (%)*.

borders (Flight et. al., 2003). When licensing was later enforced in these non-licensed zones, a large fraction of prostitutes as well as clients were sent away from tippelzones to less controlled environments (see again Figure 2). If tippelzones attract criminals then the criminal environment in a city prior to opening a tippelzone may be different than the environment in a tippelzone which has been open for several years. We postpone further discussion of the results to subsection 5.3.

The right panel of Table 3 shows estimation results for the sample including the three large cities. Since these three cities closed their tippelzone during our observation period we can also allow for an effect of closing the tippelzone. The effects of closing are never significant. Furthermore, the other estimated effects are quite robust against including the three large cities.

5.2 Time-Varying Effects Sexual Abuse and Rape

A tippelzone is introduced at a specific moment in time but the composition of prostitutes changes continuously in a city. Delayed market responses from sex-trafficking organizations or capacity restrictions inside the zones may produce time-varying effects of tippelzones. We explore this in Table 4 where we split the opening effects of tippelzones into short-run and medium to long-run effects. In particular, we allow for different effects in the first two years after opening and afterwards. All effects (except the ex-post licensing) describe proportional shifts from the pre-opening crime levels. As in the baseline specification, the effect of ex-post licensing represents a proportional shift compared to

Table 4: Time-varying effects and robustness checks on citywide sexual abuse and rape (22 cities).

Dependent variable:	*Sexual Abuse & Rape*					Sex Ab.	Rape
pre-opening		0.062					
		(0.069)					
1st-2nd year Open noLic.	-0.385***	-0.350***	-0.377***	-0.350***	-0.369***	-0.438**	-0.325**
	(0.133)	(0.121)	(0.130)	(0.121)	(0.127)	(0.208)	(0.164)
3rd+ year Open noLic.	-0.131	-0.054	-0.061	-0.024	-0.064	-0.269	0.029
	(0.097)	(0.106)	(0.083)	(0.121)	(0.109)	(0.243)	(0.131)
pre-Intro Lic.		-0.202					
		(0.184)					
Intro. Lic.	0.252	0.180	0.196	0.163	0.186	0.306	0.144
	(0.252)	(0.181)	(0.231)	(0.189)	(0.215)	(0.223)	(0.275)
1st-2nd year Open Lic.	-0.518***	-0.499**	-0.525***	-0.525***	-0.543***	-0.944***	-0.238
	(0.179)	(0.206)	(0.197)	(0.181)	(0.188)	(0.326)	(0.290)
3rd+ year Open Lic.	-0.062	-0.046	-0.065	-0.065	-0.051	-0.295**	0.123
	(0.056)	(0.058)	(0.058)	(0.056)	(0.063)	(0.143)	(0.141)
Spillover Lic.			-0.003	0.030	0.038	0.104	0.029
			(0.057)	(0.087)	(0.089)	(0.121)	(0.114)
Spillover Closing			-0.057	-0.096	-0.109	-0.294	-0.039
			(0.155)	(0.236)	(0.242)	(0.281)	(0.270)
Brothel ban lift				-0.094	-0.072	-0.050	-0.082
				(0.169)	(0.152)	(0.136)	(0.251)
Spillover Tipp.					yes	yes	yes
N (city x year)	395	395	395	395	395	395	395
R^2	0.64	0.64	0.64	0.63	0.63	0.44	0.55
City fixed effects	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes
Covariates	yes	yes	yes	yes	yes	yes	yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Significance (and standard errors in parenthesis) based on Cameron, Gelbach and Miller (2008) wild bootstrap approach with 499 replications; Based on yearly data over the period 1994-2011. The 22 cities exclude the large cities Amsterdam, Rotterdam, The Hague (and Eindhoven in 2011). Covariates are indicators for political party of mayor, *log(population male 15-65)*, *log(pop. density)*, *income (%)*, *immigrants (%)*, *unemployment insurance recipients (%)*, *higher educated (%)*.

the presence of a tippelzone without licensing.

The results in the first column show clear differences between short and long-run effects. The negative effect on sexual abuse and rape observed in the baseline specification is driven by sharp decreases in crime in the first two years after opening. The average decrease in sexual abuse in the first two years of opening is 40% for cities with a licensed tippelzone and 32% for cities with free entry zones, but the difference between the two is insignificant. Beyond the first two years there is no difference in crime relative to the pre-opening period. The ex-post introduction of licensing again increases sex-related crime. The effect is substantial, but lacks sufficient power to be significant.

The second column of the table provides evidence for the common trend assumption by including indicators for pre-opening and pre-licensing periods. These variables take value one in the two years before opening a tippelzone or introducing licensing. As expected from Figure 6 we do not find any sign of pre-opening shifts in crime. Introducing these

lead dummies also does not influence our parameter estimates of interest. This supports the assumption of a common trend in sex-related crime between cities with and without a tippelzone, and with and without licensing.

To interpret our findings as causal effects we must exclude that crime shifts non-randomly between cities following the opening of a tippelzone or changes in licensing policy. As discussed in subsection 2.1, case workers reported some displacement of prostitutes from closed tippelzones to zones in other cities. We therefore include in the third column of Table 4 variables for spillover effects from closed tippelzones (Spillover Closing) or from zones which introduced a licensing system (Spillover Tippelzone) dispelling illegal workers. These variables are non-zero only for cities which had an open tippelzone without a licensing system. The spillover variable for these cities increases in increments of one for every newly closed or newly licensed tippelzone.[19] Our results do not indicate any shifts in sexual abuse or rape due to spillovers. This is not surprising. The movements of prostitutes were limited since the closing of tippelzones in Rotterdam, Amsterdam and The Hague occurred simultaneously with the introduction of licensing systems in other cities which refused new entrants into tippelzones. We also do not find in the fourth column any effect of the end of the brothel ban on crime in the cities with free entry zones.[20]

In the last three columns we account for possible crime spillovers to neighbouring cities. In this model, we include a set of dummy variables taking value one for cities within 50km of a city which opened a tippelzone or introduced a licensing system. The parameters of interest for sexual violence remain unaffected and the spillover effects[21] show no reversed changes in crime in neighbouring cities.[22] The final two columns reproduce these same results for sexual abuse and rape separately. The decreases in sexual abuse are stronger in cities with licensed tippelzones and reductions in rape are larger in cities with free entry tippelzones. Furthermore, we find that the reductions in sexual abuse persist beyond the first year in cities which implemented licensing from the start.

5.3 Discussion of effects on sexual violence

The opening of a tippelzone with or without a licensing system is correlated with a short-run decrease of $30\% - 40\%$ in sexual abuse and rape, and the results are robust to different specifications. A first possibility is that the reductions follow directly from a decrease in sexual violence on prostitutes. A survey of street prostitutes in the Nijmegen tippelzone reports that 27% were victim of abuse and 16% were raped in the previous

[19] We also estimated the models with dummy variables for spillovers. None of these specifications shows significant spillover effects or relevant changes in our parameters of interest.

[20] The variable *brothel ban lift* takes value one in cities with an open tippelzone and no licensing system, and zero otherwise.

[21] The results are presented fully in Table 13 of the appendix.

[22] Spillovers on *1st-2nd year open Lic.* and *1st-2nd year open noLic.* are close to zero in magnitude and insignificant.

year alone (Oostveen 2008). Despite this high sexual violence, 95% of the interviewed prostitutes report feeling safer within the tippelzone. The study explains these seemingly contradictory findings by the fact that most prostitutes also work during the day for private clients in more insecure settings.[23] Tippelzones may therefore directly reduce crime on street prostitutes by providing a relatively safe and controlled working environment.

However, limiting the explanation to street prostitutes may obscure effects on a wider group of victimized women in the population. Indeed, street prostitutes are a prominent example of a group which shies away from reporting crime due to their illegal status and drug addiction.[24] According to Van Soomeren (2004), tippelzones actually provide support to immigrants whose illegal status and lack of knowledge of their rights prevent them from seeking help from officials. If this support increases reporting of sexual abuse and rape, then our estimated effect is an underestimate of the true effect.

A second possibility is that opening a tippelzone leads to a decrease in sexual violence on women more generally by providing an anonymous, appealing and easily accessible outlet for sex to otherwise violent individuals.[25] Under the theoretical predictions of subsection 2.2, a tippelzone may attract potential instigators of sexual abuse and rape but have the effect of diffusing sexual violence elsewhere in the city. If this type of substitution behavior occurs then the opening effect of tippelzones may reflect reductions in sexual abuse and rape on all women, not only on prostitutes. Without more precise data on the victims of sexual violence we can not separate these two hypotheses.

A last possibility is that the decreases in sexual violence are driven by changes in crime reporting behaviour. Regarding policing behavior, we did not find any strong arguments or articles describing citywide policing efforts to have changed concurrently and systematically with the opening of a tippelzone. Another possible concern would be if potential criminals shift their crime to victims with a different propensity to report crime than their previous victims. For instance, in the case of sexual crime, we must exclude that upon the opening of a tippelzone potential criminals of sexual violence switch to new victims who are less willing to report sexual assaults and rapes than those women (prostitutes or others) who were previously victimized. We can not test this type of substitution empirically. However, it is unclear why the change in victimized individuals would operate in such a way, in particular given the low propensity of street prostitutes to report crime.

Overall we find only weak evidence of long-run effects for cities with and without licensing. It is not clear why the initial reductions in crime fade away over time. One

[23] A larger survey of prostitutes in nine countries reports that 71% had been physically assaulted and 63% had been raped while working as prostitutes (Farley et al., 2003).

[24] Among the window, escort and club prostitutes interviewed in Venicz and Vanwesenbeeck (2000) 71% respond to have withheld reporting one or several personal incidents to the police in the previous year. The main reasons are fear for reprisals by their procurers, and lack of hope and trust in the police.

[25] Farley et al. (2011) find that 15% of sex buyers revealed that "they would rape a woman if they could get away with it and if no one knew about it" in comparison with 2% for non-sex buyers.

Table 5: Effect of tippelzone on citywide registered crime

	Drugs	Weapons	Assault	Drugs	Weapons	Assault
	22 cities with covariates			*25 cities with covariates*		
Open noLic. (δ_0)	-0.034	0.052	0.139	-0.067	0.002	0.113
	(0.364)	(0.184)	(0.126)	(0.155)	(0.034)	(0.096)
Intro. Lic. (δ_1)	-0.244	-0.095	-0.053	-0.155	-0.070	0.030
	(0.206)	(0.200)	(0.154)	(0.104)	(0.129)	(0.123)
Open Lic. (δ_2)	-0.324**	-0.210	0.052	-0.284***	-0.168	0.034
	(0.128)	(0.324)	(0.118)	(0.107)	(0.310)	(0.127)
Closing				-0.124	0.095	0.207
				(0.134)	(0.098)	(–)
N (city x year)	395	395	395	450	450	450
R^2	0.81	0.76	0.89	0.89	0.89	0.95
City fixed effects	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes
Covariates	yes	yes	yes	yes	yes	yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Significance (and standard errors in parenthesis) based on Cameron, Gelbach and Miller (2008) wild bootstrap approach with 499 replications; Based on yearly data over the period 1994-2011. Wild bootstrap procedure fails on closing parameter for *Assaults*. The 22 cities exclude the large cities Amsterdam, Rotterdam, The Hague (and Eindhoven in 2011). Covariates are indicators for political party of mayor, *log(population male 15-65)*, *log(pop. density)*, *income (%)*, *immigrants (%)*, *unemployment insurance recipients (%)*, *higher educated (%)*.

possibility, for tippelzones without a licensing system, is that increased competition over time in the tippelzones forced some prostitutes to seek opportunities in other less safe areas thereby falling victim to more sexual abuse and rape.[26] This interpretation can also explain the increases in sexual violence following the ex-post introduction of licensing systems. For tippelzones with licensing, there were capacity limitations set on the number of workers within these zones. The constant inflow and turnover of prostitutes means the later entrants were forced to work in less safe areas outside of the zones. If we consider the reductions in sexual violence to affect women more generally, then the fading out of effects in the medium- to long-run may reflect a short-lived thrill effect of tippelzones for potential offenders.

5.4 Drugs, Illegal Weapons and Assaults

Recall that one of the initial goals of tippelzones was to remove the nuisance created by drug-addicted prostitutes, their pimps and their clients. Furthermore, when licensing systems were enforced, they favored drug-addicted prostitutes. For these reasons, we next focus on crimes related to drugs and violence. We again consider our baseline model.

The first column of Table 5 explores possible effects of tippelzones and their regulation on drug crime. Our estimation results follow the theoretical predictions, but only when

[26] It is difficult to verify this re-sprawl in the data by, for example, looking at the number of arrests for street prostitution. This is because soliciting by prostitutes comes under the crime category of 'Public order offenses' which includes a wide variety of other public disturbances.

tippelzones enforce licensing. In those cities, we find a significant decrease of approximately 25% in drug crime. The results are robust to different specifications and persist beyond the first two years (presented in Tables 12 and 13 in the appendix). This suggests that in cities which enforced licensed tippelzones from the start local governments achieved one of their stated goals to address drug problems related to prostitutes.

The theoretical models also predict that cities with non-licensed tippelzones are more likely to attract prostitutes trafficked from the underground sex industry since they provide fewer barriers to entry. If trafficking organizations are associated to trafficking of drugs, weapons or more violent crimes, then opening a free entry tippelzone may produce unforeseen spillovers on aggregate city crime. Our panel estimation results in the second and third columns of Table 5 do not give evidence for spillovers on illegal weapons or violent assaults.

6 Perceived Crime

The second data source is the Population Police Monitor (PPM) which examines perceived crime and safety.[27] This nationwide survey was conducted every other year from 1993 to 2001 and annually from 2001 to 2006. Respondents are contacted by telephone and are asked questions about victimization, feelings of safety, contact with police, and crime in their neighborhood. The participation rate in the survey ranges from 46% to 72% with higher participation in later years.

We focus on two questions concerning the perception of drug crime and violent crime: "Is drug nuisance common in your neighborhood?" and "Is violent crime common in your neighborhood?". The question on drug crime only entered the survey in 1997. Answers can take four alternatives: (1) Happens regularly, (2) Happens sometimes, (3) Never happens/Hardly ever happens, and (4) Don't know/No opinion. This dataset also includes the four digit postal code of each respondent which allows us to define their proximity to the tippelzone. In the analysis, we take all postcodes for the 25 largest Dutch municipalities based on the geographic delimitations defined by Statistics Netherlands.

Table 6 presents the fraction of answers within each perceived crime category. For medium-sized cities, about 70% of respondents indicate that violent crime and drug crime are never or hardly ever observed. Approximately 10% respond that drug nuisance occurs regularly and about 5% claim that violent crime happens regularly. Again there are differences between the three largest cities and the rest of the sample. A graphical analysis (not presented) shows that the trends in these larger cities differ from the rest. For these reasons, we focus the empirical analysis of perceived crime on the 22 medium-sized cities.

Figure 4a shows how perceived crimes change over time. The trends for all response

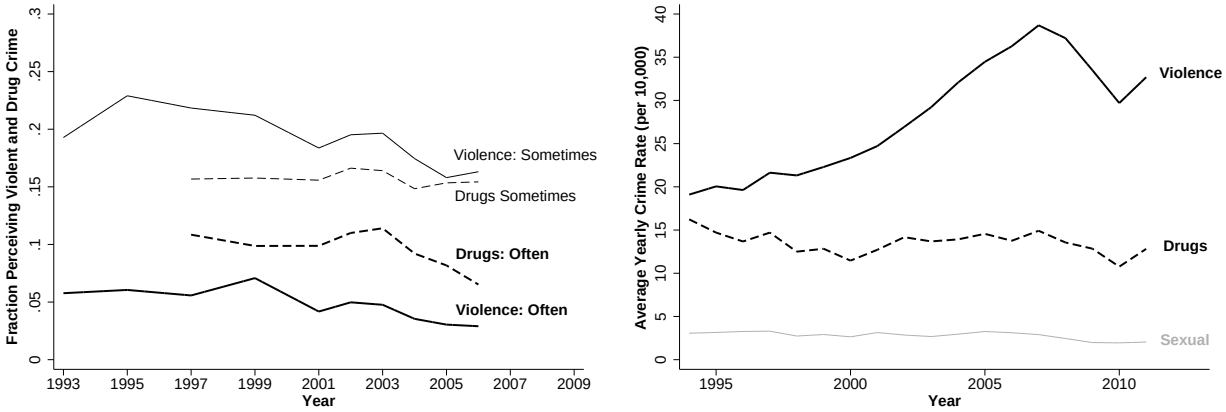
[27] This survey (in Dutch: *Politie Monitor Bevolking*) is conducted by two research bureaus commissioned by the Dutch Ministry of Security and Justice: B&A Groep Beleidsonderzoek & - Advies BV, and Intomart BV.

Table 6: Crime rates for tippelzone and non-tippelzone cities

	Tippelzone		No Tippelzone
	big cities	medium cities	
Fraction perceiving Drug Crime			
Often	0.15	0.11	0.10
Sometimes	0.20	0.17	0.15
Never	0.62	0.70	0.73
Fraction perceiving Violent Crime			
Often	0.09	0.05	0.05
Sometimes	0.27	0.21	0.19
Never	0.59	0.70	0.73

3 big cities with a tippelzone include Amsterdam, Rotterdam and The Hague. *6 medium cities* with a tippelzone include Utrecht, Eindhoven, Groningen, Nijmegen, Heerlen and Arnhem. *16 cities* without tippelzone include Almelo, Breda, Deventer, Dordrecht, Enschede, Haarlem, Helmond, Hengelo, Leeuwarden, Leiden, Maastricht, Schiedam, Tilburg, Venlo, Zwolle and 's-Hertogenbosch.

Figure 4: Trends in registered and perceived crime



(a) Perceived Crime, 22 cities.

(b) Registered Crime, 22 cities.

categories remain relatively constant during the observation period. For comparison we show in Figure 4b the trends in registered crime. The public perception of violent crime does not in general follow the trend in reported crime. As for perceived drug crime, drug-related registered crime shows no trend but the difference in trends for violence is clear. Perceived violent crime is slightly decreasing over time, whereas registered violent crime shows a strong increase until 2007 and then a drop until 2010.

6.1 Estimation Results for Perceived Crime

We repeat the difference-in-difference analysis with the perceived crime data. The outcome variable is an indicator which takes value one if a person responds that she is experiencing nuisance from drugs or violence often (or sometimes) in her surrounding. Given the binary outcome, we are estimating a linear probability model. We control for individual characteristics (gender, age, education, nationality) and the same city characteristics as in the registered crime analysis. We cluster standard errors at the postcode

Table 7: Effect of tippelzone on citywide perceived crime in medium-sized cities

	Drugs				Violence			
	Often		Often/Some.		Often		Often/Some.	
	Non-Tipp. area	*Tipp. area*	*Non-Tipp. area*	*Tipp. area*	*Non-Tipp. area*	*Tipp. area*	*Non-Tipp. area*	*Tipp. area*
Open noLic. (δ_0)	0.020*	-0.025	0.028**	0.010	-0.009	-0.015	-0.002	-0.029
	(0.012)	(0.034)	(0.013)	(0.029)	(0.005)	(0.013)	(0.012)	(0.022)
Intro. Lic. (δ_1)	-0.001	-0.003	-0.000	-0.047	-0.001	-0.018	0.008	-0.023
	(0.014)	(0.018)	(0.021)	(0.037)	(0.009)	(0.020)	(0.021)	(0.035)
Open Lic. (δ_2)	-0.016*	0.013	0.007	0.025	-0.003	-0.001	0.001	-0.001
	(0.010)	(0.013)	(0.013)	(0.025)	(0.005)	(0.004)	(0.008)	(0.023)
N (city x year)	176		176		220		220	
N (individuals)	83,494		83,494		107,811		107,811	
R^2	0.094		0.13		0.026		0.077	
Postcode fixed effects	yes		yes		yes		yes	
Year dummies	yes		yes		yes		yes	
Covariates	yes		yes		yes		yes	

* $p < 0.10$, ** $p < 0.05$. Clustered standard errors in parentheses; Based on data over the period 1993-2006 for violent crime and over 1997-2006 for drug crime. Fixed effects at postcode level. 22 cities excludes Amsterdam, Rotterdam, The Hague. Covariates are indicators for political party of mayor, *log(population male 15-65)*, *log(pop. density)*, *income (%)*, *immigrants (%)*, *unemployment insurance recipients (%)*, *higher educated (%)*, *gender*, *age*, *education*, *Dutch nationality*.

level since they are more conservative than those when clustering at the city level.

Table 7 presents results from our baseline model for medium-sized cities. Since residents may react differently depending upon their proximity to the tippelzone, we stratify the regression by within city locality. We define the *tippelzone area* as the postcode in which the tippelzone is located as well as any adjacent postcode. Perceived drug nuisance due to the presence of a tippelzone is significantly higher in neighborhoods not located near the tippelzone, but this only holds for non-licensed tippelzones. There are no significant effects in the tippelzone area. Furthermore, our baseline specification does not show significant effects on violence.

Our baseline specification does not account for changes in public perception over time. Changes in perception may, for example, arise because media attention on tippelzones was stronger around opening periods and periods of introducing licensing. Table 8 considers the extended model with time-varying effects in perceived drug nuisance which we specified earlier for registered crime. The first two columns focus on often perceived drug nuisance and show opposing effects depending on the licensing system and the proximity to the tippelzone. In the first two years after opening, perceived drug nuisance in non-tippelzone areas increases by 5.8%-points in cities which did not open a tippelzone with a licensing system from the start. After the first two years, perceived drug crime in those areas still remains higher than in the pre-opening period. These are large impacts since on average only 10% of residents respond that drug nuisance occurs often.

In contrast, residents in non-tippelzone areas of cities which introduced licensing from

Table 8: Within city effects of tippelzone on perceived crime (22 cities with covariates)

	Drugs: Often		Drugs: Often/Some.	
	Non-Tipp. area	*Tipp. area*	*Non-Tipp. area*	*Tipp. area*
pre-opening	0.008	0.012	0.032*	0.057**
	(0.013)	(0.027)	(0.017)	(0.022)
1st-2nd year Open noLic.	0.058***	0.000	0.064***	0.051
	(0.022)	(0.034)	(0.023)	(0.032)
3rd+ year Open noLic.	0.047*	0.032	0.079**	0.082*
	(0.027)	(0.040)	(0.032)	(0.049)
Introduce Lic.	0.015	0.011	0.025	-0.022
	(0.015)	(0.021)	(0.027)	(0.041)
1st-2nd year Open Lic.	0.001	-0.002	0.029**	0.012
	(0.010)	(0.015)	(0.014)	(0.028)
3rd+ year Open Lic.	-0.053***	0.042***	-0.016	0.082***
	(0.015)	(0.009)	(0.019)	(0.013)
Spillover Lic.	✓		✓	
Spillover Closing.	✓		✓	
Brothel ban lift	✓		✓	
N (city x year)	176		176	
N (individuals)	83,494		83,494	
R^2	0.095		0.13	
Postcode fixed effects	yes		yes	
Year dummies	yes		yes	
Covariates	yes		yes	

* $p < 0.10$, ** $p < 0.05$. Clustered standard errors in parentheses; Based on data over the period 1993-2006 for violent crime and over 1997-2006 for drug crime. Fixed effects at postcode level. 22 cities excludes Amsterdam, Rotterdam, The Hague. Covariates are indicators for political party of mayor, *log(population male 15-65)*, *log(pop. density)*, *income (%)*, *immigrants (%)*, *unemployment insurance recipients (%)*, *higher educated (%)*, *gender*, *age*, *education*, *Dutch nationality*.

the start do not see an initial increase in crime but perceive a 5.3%-point reduction in drug nuisance in the medium to long-run. In addition, residents in areas adjacent to the tippelzone perceive an 4.2%-point increase in drug nuisance in the medium to long-run. Although these results are identified only on two cities which immediately enforced a licensing system, they suggest that drug nuisance was successfully re-located to the tippelzone areas.

The third and fourth column of Table 8 for "often or sometimes" perceived drug crime mostly follow the patterns observed in the first two columns. However, perceived drug nuisance also increases for the first two years in non-tippelzone areas of cities which enforced licensing immediately upon opening.[28] We also notice that perceived drug nuisance increased already prior to opening a tippelzone. These shifts may be due to media coverage of the announcement that a tippelzone would be opened. Again, we do not observe any change in perceived drug nuisance in response to the introduction of ex-post licensing.

The results for non-tippelzone areas are consistent with reports for tippelzones which imposed licensing immediately.[29] Initially, the tippelzones were met with opposition.[30] Later, as city residents became more informed about the purpose of a tippelzone they also became more accepting of it's presence. Furthermore, our empirical results indicate that the tippelzones achieved one of their stated goals which was to reduce the nuisance created by drug-addicted prostitutes overall in the city. However, the results also indicate that this relocation of street prostitutes and their following came at the expense of those living near the tippelzones who became increasingly exposed to the drug dealers and drug-addicted clients.

Results for time-varying effects on perceived violence do not show any effect of opening a tippelzone or licensing (presented in Table 14 in the appendix). As such, they are in line with the results on registered crime in Table 5 for aggregate illegal weapons and assaults.

7 Conclusion

In this paper we study the effects of opening a legal street prostitution zone on citywide crime. Theories of crime predict that the effect of opening such a tippelzone depends on the imposed regulation. Becker's rational choice theory suggests that opening a tippelzone with higher police monitoring reduces sexual violence against prostitutes. Theories

[28]Note that the response categories are exclusive. For this reason, if fewer residents responding "often" then the share of "sometimes" responses is likely to increase. This can explain some of the changes in parameter - e.g. -0.053 in the "often" response regression and -0.016 in the "often or sometimes" regression for *3rd+ year Open Lic.* in non-tippelzone areas.

[29]Oostveen (2008) reports for Nijmegen that residents in the adjacent area to the Tippelzone mention drugs, junkies and dealers as the main reason for feeling unsafe. About 35% of these residents report feeling unsafe, compared to 22% of the individuals living in the rest of the city.

[30]Van Soomeren (2004) states that in Amsterdam "the day after the opening, more than a hundred residents from neighborhoods south of the zone took to the streets in protest, but the tippelzone remained open." (p. 6).

from criminology add that opening a tippelzone can reduce sexual violence on a wider population by attracting sexually violent potential criminals. Theoretical models also predict that regulation through licensing should reduce involuntary prostitution, but the predicted effect on total crime is ambiguous.

Our empirical results for aggregate registered sexual abuse and rape are in line with theoretical predictions. We find that the opening of a tippelzone in a city is associated with a $30\% - 40\%$ decrease in sexual abuse and rape in the first two years of opening. These effects do not depend on whether the tippelzone immediately enforced a licensing system or not. Our data do not allow us to distinguish between victims. In terms of policy it is highly relevant to know whether the reduction in sexual violence is attributable to the relocated street prostitutes or to a wider set of victims. We also find some evidence that introducing a licensing system some years after opening, effectively forcing a large share of illegal prostitutes to work outside the tippelzone, leads to a positive increase in citywide sexual abuse.

In addition to effects on sexual violence, theoretical models predict that licensing can produce different spillover effects of tippelzones on other crimes linked to the prostitution market. Our results on registered drug crime show that opening a tippelzone is associated with a 25% decrease in average citywide drug crime and this result persists over time but only in cities which enforced a licensed tippelzone from the start. We do not however find any evidence for effects on other crimes linked to trafficking organizations such as illegal weapons or violent assaults.

Next, we consider the effect of tippelzones on perceived crime. Our results indicate that residents in a city which opened a tippelzone without a licensing system perceive a significant increase of $5 - 6\%$-points in drug nuisance in the first two years after opening. The results in the medium- to long-run are less precise and differ depending on the residents' proximity to the tippelzone and whether or not a licensing system was enforced from the start. In general, the results on registered and perceived drug crime in cities which enforced licensed tippelzones suggest that local governments successfully achieved their goal of reducing drug crime overall in the city.

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Appendix

Table 9: Overview of prostitution laws in selected countries

<table>
<tr><th rowspan="2">Country</th><th colspan="3">Legal Status</th></tr>
<tr><th>Prostitution</th><th>Brothel Ownership</th><th>Pimping</th></tr>
<tr><td>Netherlands</td><td rowspan="3">legal</td><td rowspan="3">legal</td><td rowspan="2">legal</td></tr>
<tr><td>Germany</td></tr>
<tr><td>Belgium</td><td rowspan="8">illegal</td></tr>
<tr><td>Canada</td><td rowspan="2">buying sex illegal</td><td rowspan="2">legal</td></tr>
<tr><td>Sweden</td><td rowspan="6">illegal</td></tr>
<tr><td>USA</td><td>illegal (except Nevada)</td></tr>
<tr><td>Japan</td><td rowspan="2">limited legality</td></tr>
<tr><td>Spain</td></tr>
<tr><td>UK</td><td rowspan="3">legal</td></tr>
<tr><td>France</td></tr>
<tr><td>Italy</td></tr>
</table>

Most countries enforce additional limitations and requirements for soliciting, procuring and sex establishments. Source: http://prostitution.procon.org/view.resource.php?resourceID=000772

Table 10: Type of licensed sex establishments in the Netherlands.

Type of establishment	Approximate total in the Netherlands
Window prostitution	580
Sex club	260
Private home	130
Escort service	90
Erotic massage salon	60
Sex cinema	60
Swingers club	20
Other	70

Approximations from Flight, et al. (2006) based on survey responses from medium and large sized municipalities across the Netherlands.

Table 11: Effect of a tippelzone and licensing on citywide registered crime.

	Sex.A. & Rape			Sex.A.			Rape		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					22 cities				
pre-opening			0.046			-0.066			0.133
			(0.068)			(0.136)			(0.179)
Open noLic.	-0.136	-0.198**	-0.170	-0.296	-0.358	-0.397	-0.022	-0.090	-0.011
	(0.130)	(0.098)	(0.125)	(0.298)	(0.217)	(0.249)	(0.073)	(0.072)	(1.419)
Intro. Lic.	0.214	0.286	0.287	0.360**	0.413**	0.413**	0.136	0.220	0.221
	(0.195)	(0.258)	(0.259)	(0.171)	(0.208)	(0.208)	(0.191)	(0.277)	(0.276)
Open Lic.	-0.120	-0.184*	-0.170	-0.417***	-0.447***	-0.467**	0.110	0.012	0.052
	(0.155)	(0.094)	(0.111)	(0.144)	(0.154)	(0.185)	(0.178)	(0.139)	(0.158)
N (city x year)	395	395	395	395	395	395	395	395	395
R^2	0.62	0.63	0.63	0.43	0.44	0.44	0.54	0.55	0.55
					25 cities				
pre-opening			0.057			-0.103			0.169
			(0.093)			(0.175)			(0.188)
Open noLic.	-0.131	-0.205*	-0.167	-0.229	-0.304	-0.373	-0.055	-0.133	-0.019
	(0.102)	(0.114)	(0.124)	(0.200)	(0.201)	(0.260)	(0.077)	(0.101)	(0.146)
Intro. Lic.	0.167	0.168	0.169	0.258**	0.250**	0.250**	0.125	0.115	0.115
	(0.123)	(0.134)	(0.134)	(0.111)	(0.124)	(0.124)	(0.153)	(0.179)	(0.183)
Open Lic.	-0.108	-0.169*	-0.152	-0.385***	-0.411***	-0.442**	0.099	0.004	0.055
	(0.112)	(0.094)	(0.112)	(0.133)	(0.154)	(0.201)	(0.173)	(1.456)	(0.218)
Closing	0.098	0.023	0.021	0.319**	0.160	0.164	-0.044	-0.045	-0.051
	(0.122)	(0.144)	(0.142)	(0.155)	(0.208)	(0.208)	(0.156)	(0.398)	(0.318)
N (city x year)	450	450	450	450	450	450	450	450	450
R^2	0.82	0.82	0.82	0.69	0.69	0.69	0.76	0.77	0.77
City fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes
Covariates	no	yes	yes	no	yes	yes	no	yes	yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Significance (and standard errors in parenthesis) based on Cameron, Gelbach and Miller (2008) wild bootstrap approach with 499 replications; Based on yearly data over the period 1994-2011. The 22 cities exclude the large cities Amsterdam, Rotterdam, The Hague (and Eindhoven in 2011). Covariates are indicators for political party of mayor, *log(population male 15-65)*, *log(pop. density)*, *income (%)*, *immigrants (%)*, *unemployment insurance recipients (%)*, *higher educated (%)*.

Table 12: Effect of a tippelzone and licensing on citywide registered crime.

	Drugs			Weapons			Assault		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	22 cities								
pre-opening			0.080			0.065			-0.052
			(0.332)			(0.153)			(0.095)
Open noLic.	-0.024	-0.034	0.013	-0.047	0.052	0.090	0.108	0.139	0.108
	(0.499)	(0.364)	(0.214)	(0.135)	(0.184)	(0.184)	(0.106)	(0.126)	(0.117)
Intro. Lic.	-0.283	-0.244	-0.244	-0.166	-0.095	-0.095	-0.121	-0.053	-0.053
	(0.205)	(0.206)	(0.205)	(0.170)	(0.200)	(0.206)	(0.181)	(0.154)	(0.158)
Open Lic.	-0.306**	-0.324**	-0.300**	-0.163	-0.210	-0.191	0.055	0.052	0.037
	(0.127)	(0.128)	(0.149)	(0.222)	(0.324)	(0.297)	(0.088)	(0.118)	(0.104)
N (city x year)	395	395	395	395	395	395	395	395	395
R^2	0.80	0.81	0.81	0.74	0.76	0.76	0.88	0.89	0.89
	25 cities								
pre-opening			0.099			0.105			-0.056
			(0.285)			(0.139)			(0.082)
Open noLic.	-0.080	-0.067	-0.000	-0.090	0.002	0.073	0.074	0.113	0.075
	(0.159)	(0.155)	(0.021)	(0.133)	(0.034)	(0.171)	(0.093)	(0.096)	(0.111)
Intro. Lic.	-0.283**	-0.155	-0.155	-0.189	-0.070	-0.070	-0.047	0.030	0.029
	(0.134)	(0.104)	(0.105)	(0.118)	(0.129)	(0.130)	(0.081)	(0.123)	(0.122)
Open Lic.	-0.281**	-0.284***	-0.254*	-0.142	-0.168	-0.136	0.043	0.034	0.017
	(0.134)	(0.107)	(0.151)	(0.215)	(0.310)	(0.252)	(0.087)	(0.127)	(0.231)
Closing	0.000	-0.124	-0.127	0.105	0.095	0.091	0.234	0.207	0.209
	(0.004)	(0.134)	(0.136)	(0.085)	(0.098)	(0.098)	(–)	(–)	(–)
N (city x year)	450	450	450	450	450	450	450	450	450
R^2	0.88	0.89	0.89	0.89	0.89	0.89	0.95	0.95	0.95
City fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes
Covariates	no	yes	yes	no	yes	yes	no	yes	yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Significance (and standard errors in parenthesis) based on Cameron, Gelbach and Miller (2008) wild bootstrap approach with 499 replications; Based on yearly data over the period 1994-2011. Wild bootstrap procedure fails on closing parameter for *Assaults*. The 22 cities exclude the large cities Amsterdam, Rotterdam, The Hague (and Eindhoven in 2011). Covariates are indicators for political party of mayor, *log(population male 15-65)*, *log(pop. density)*, *income (%)*, *immigrants (%)*, *unemployment insurance recipients (%)*, *higher educated (%)*.

Table 13: Time varying effects and robustness checks on citywide crime categories (22 cities).

Dependent variable:	SexA. & Rape	SexA.	Rape	Drugs	Weapons	Assault
1st-2nd year Open noLic.	-0.369***	-0.438**	-0.325**	0.107	0.230	0.029
	(0.127)	(0.208)	(0.164)	(0.363)	(0.167)	(0.097)
3rd+ year Open noLic.	-0.064	-0.269	0.029	0.078	0.036	-0.031
	(0.109)	(0.243)	(0.131)	(0.382)	(0.283)	(0.181)
Intro. Lic.	0.186	0.306	0.144	-0.462	-0.066	0.129
	(0.215)	(0.223)	(0.275)	(0.354)	(0.158)	(0.165)
1st-2nd year Open Lic.	-0.543***	-0.944***	-0.238	-0.359**	-0.355*	-0.062
	(0.188)	(0.326)	(0.290)	(0.163)	(0.185)	(0.091)
3rd+ year Open Lic.	-0.051	-0.295**	0.123	-0.246***	-0.239	0.114
	(0.063)	(0.143)	(0.141)	(0.085)	(0.352)	(0.132)
Spillover Lic.	0.038	0.104	0.029	-0.007	-0.057	0.016
	(0.089)	(0.121)	(0.114)	(0.202)	(0.127)	(0.024)
Spillover Closing	-0.109	-0.294	-0.039	-0.018	0.009	0.015
	(0.242)	(0.281)	(0.270)	(0.077)	(0.246)	(0.039)
Brothel ban lift	-0.072	-0.050	-0.082	-0.175	0.104	0.153
	(0.152)	(0.136)	(0.251)	(0.164)	(0.189)	(0.121)
Spill. Opening noLic.	-0.016	-0.065	-0.007	0.043	-0.077	0.074
	(0.095)	(0.163)	(0.068)	(0.272)	(0.162)	(0.111)
Spill. Opening Lic.	-0.057	-0.043	-0.060	0.163	-0.083	-0.097
	(0.126)	(0.153)	(0.113)	(0.168)	(0.133)	(0.101)
Spill. Intro. Lic.	0.106	0.145	0.109	0.220**	-0.094	0.068
	(0.068)	(0.144)	(0.090)	(0.097)	(0.101)	(0.059)
N (city x year)	395	395	395	395	395	395
R^2	0.63	0.44	0.55	0.82	0.76	0.89
City fixed effects	yes	yes	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes	yes	yes
Covariates	yes	yes	yes	yes	yes	yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Significance (and standard errors in parenthesis) based on Cameron, Gelbach and Miller (2008) wild bootstrap approach with 499 replications; Based on yearly data over the period 1994-2011. The 22 cities exclude the large cities Amsterdam, Rotterdam, The Hague (and Eindhoven in 2011). Covariates are indicators for political party of mayor, *log(population male 15-65)*, *log(pop. density)*, *income (%)*, *immigrants (%)*, *unemployment insurance recipients (%)*, *higher educated (%)*.

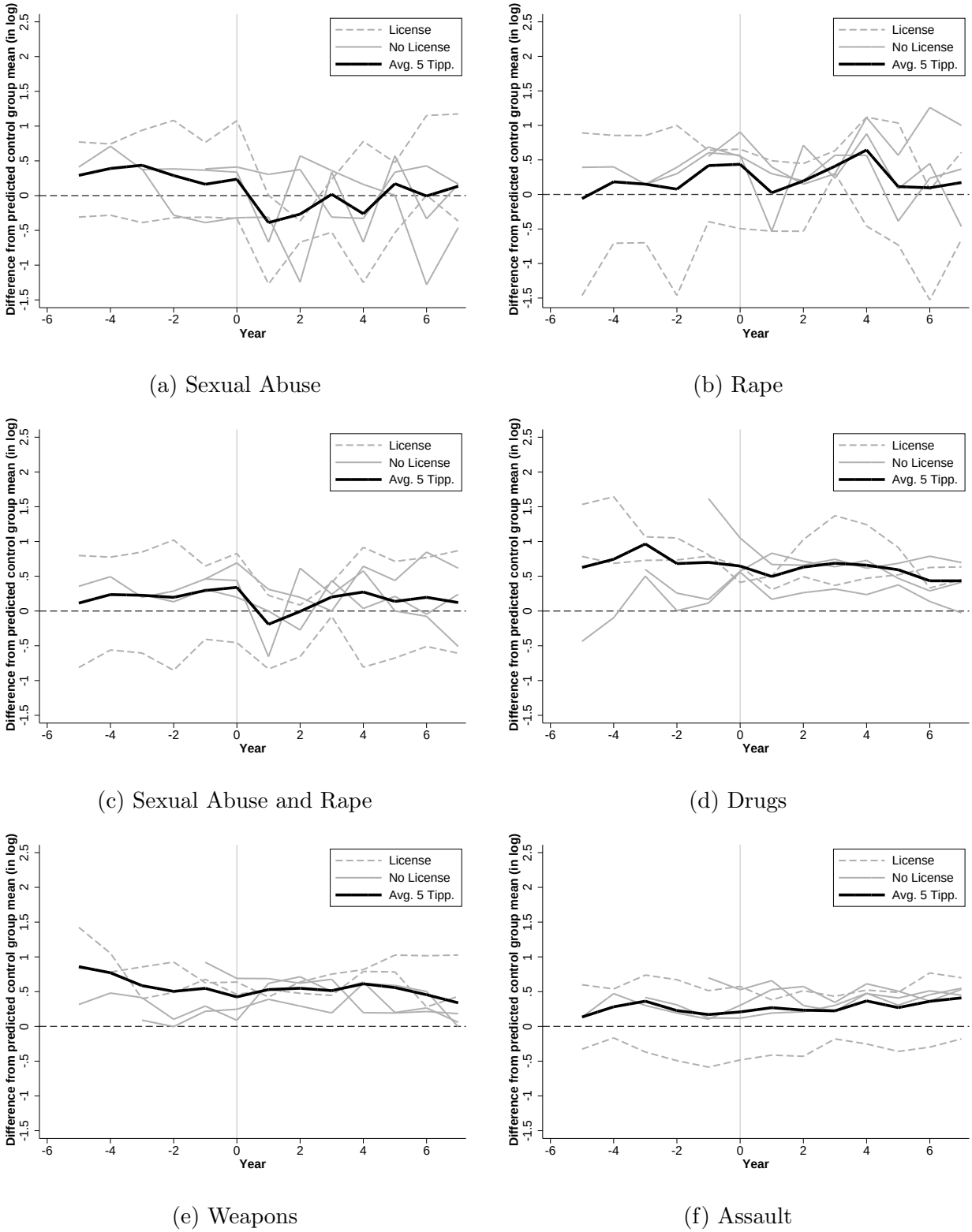
Table 14: Within city effects of tippelzone on perceived crime (22 cities with covariates)

	Drugs: Often		Drugs: Often/Some.	
	Non-Tipp. area	*Tipp. area*	*Non-Tipp. area*	*Tipp. area*
pre-opening	0.008	0.012	0.032*	0.057**
	(0.013)	(0.027)	(0.017)	(0.022)
1st-2nd year Open noLic.	0.058***	0.000	0.064***	0.051
	(0.022)	(0.034)	(0.023)	(0.032)
3rd+ year Open noLic.	0.047*	0.032	0.079**	0.082*
	(0.027)	(0.040)	(0.032)	(0.049)
Introduce Lic.	0.015	0.011	0.025	-0.022
	(0.015)	(0.021)	(0.027)	(0.041)
1st-2nd year Open Lic.	0.001	-0.002	0.029**	0.012
	(0.010)	(0.015)	(0.014)	(0.028)
3rd+ year Open Lic.	-0.053***	0.042***	-0.016	0.082***
	(0.015)	(0.009)	(0.019)	(0.013)
Spillover Lic.	-0.006		-0.015	
	(0.007)		(0.010)	
Spillover Closing.	0.003		0.007	
	(0.011)		(0.014)	
Brothel ban lift	-0.023		-0.007	
	(0.017)		(0.018)	
N (city x year)	176		176	
N (individuals)	83,494		83,494	
R^2	0.095		0.13	

	Violence: Often		Violence: Often/Some.	
	Non-Tipp. area	*Tipp. area*	*Non-Tipp. area*	*Tipp. area*
pre-opening	-0.009	0.000	-0.012	0.009
	(0.007)	(0.017)	(0.012)	(0.032)
1st-2nd year Open noLic.	-0.001	-0.011	0.016	-0.004
	(0.012)	(0.019)	(0.023)	(0.031)
3rd+ year Open noLic.	-0.017	-0.017	0.020	-0.001
	(0.011)	(0.019)	(0.024)	(0.037)
Introduce Lic.	0.002	0.016	0.044*	0.014
	(0.010)	(0.021)	(0.025)	(0.037)
1st-2nd year Open Lic.	-0.006	0.002	0.001	0.022
	(0.006)	(0.009)	(0.011)	(0.024)
3rd+ year Open Lic.	-0.004	-0.005	-0.005	-0.023
	(0.009)	(0.007)	(0.013)	(0.024)
Spillover Lic.	0.002		-0.007	
	(0.004)		(0.010)	
Spillover Closing.	-0.004		-0.004	
	(0.005)		(0.014)	
Brothel ban lift	-0.005		-0.016	
	(0.010)		(0.020)	
N (city x year)	220		220	
N (individuals)	107,811		107,811	
R^2	0.026		0.078	
Postcode fixed effects	yes		yes	
Year dummies	yes		yes	
Covariates	yes		yes	

* $p < 0.10$, ** $p < 0.05$. Clustered standard errors in parentheses; Based on data over the period 1993-2006 for violent crime and over 1997-2006 for drug crime. Fixed effects at postcode level. 22 cities excludes Amsterdam, Rotterdam, The Hague. Covariates are indicators for political party of mayor, *log(population male 15-65)*, *log(pop. density)*, *income (%)*, *immigrants (%)*, *unemployment insurance recipients (%)*, *higher educated (%)*, *gender*, *age*, *education*, *Dutch nationality*.

Figure 5: Trends in Registered Crime Categories



(a) Sexual Abuse

(b) Rape

(c) Sexual Abuse and Rape

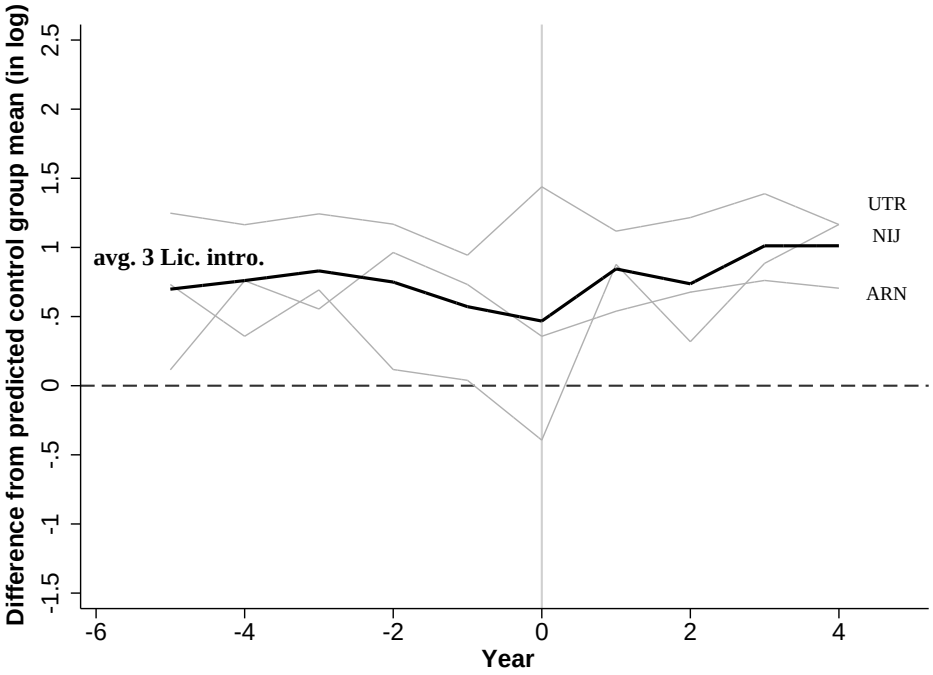
(d) Drugs

(e) Weapons

(f) Assault

The no-tippelzone counterfactual for a city i is generated by estimating model (1) on twenty-one cities leaving out city i and the three largest cities and then averaging the fitted values fixing $\mathrm{D}_{it}^{-} = 0$, $\mathrm{L}_{it} = 0$ and $\mathrm{D}_{it}^{+} = 0$. Also note that in contrast to the panel estimations, the plotted trends of tippelzone cities do not control for time varying covariates.

Figure 6: Licensing introduction



The no-tippelzone or licensing counterfactual for a city i is generated by estimating model (1) on twenty-one cities leaving out city i and the three largest cities and then averaging the fitted values fixing $\mathrm{D}_{it}^{-} = 1$, $\mathrm{L}_{it} = 0$ and $\mathrm{D}_{it}^{+} = 0$.