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# Street Prostitution Zones and Crime[†]

*By* Paul Bisschop, Stephen Kastoryano, and Bas van der Klaauw*

*This paper studies the effects of legal street prostitution zones on registered and perceived crime. We exploit a unique setting in the Netherlands where these tippelzones were opened in nine cities under different regulation systems. Our difference-in-difference analysis of 25 Dutch cities between 1994–2011 shows that opening a tippelzone decreases registered sexual abuse and rape by about 30−40 percent in the first two years. For cities which enforced licensing in tippelzones, we also find reductions in drug-related crime and long-term effects on sexual assaults. Effects on perceived drug nuisance depend on the regulation system and the proximity of respondents to the tippelzone.* (*JEL* J16, J47, K42)

The Netherlands holds a long tradition of regulated tolerance toward 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 the total amount of various types of 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

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[1]Around 4.5 million women, of which 1 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 (SAP-FL 2012, SAP-FL and FPRW 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.

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. The evaluation of tippelzones is relevant for other countries since street prostitution zones have opened in several cities in Germany (e.g., Cologne, Dortmund, Essen, and Hamburg) and more recently in Switzerland (Zurich 2013), Italy (Rome 2015), and England (Leeds 2016).

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. The registered-crime data are not directly linked to prostitutes or tippelzones, but describe all registered cases of a particular crime category in the municipality. 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. It relates to a recent paper by Cunningham and Shah (forthcoming) showing that an unexpected court order in Rhode Island decriminalizing indoor prostitution decreased rape offenses by 31 percent.

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, and 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 its 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. They argue that legalization without licensing can produce unintended increases in trafficked prostitution. 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. As already discussed above, a tippelzone may be a magnet for crime in particular when there is free entry of prostitutes, but in combination with regulation, it may control the market. This motivates our interest in licensing and why we are interested in crime in the entire city rather than only in the surrounding area of the tippelzone.

Our empirical results show that opening a tippelzone reduces sexual abuse and rape. These results are mainly driven by a 30–40 percent 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 percent 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 the effects on registered crime with those on perceived crime. Since policy decisions are influenced by public perception, it is relevant to know whether perceptions on crime are in line with registered crime. Our results indicate that perceived drug nuisance increases by approximately 6 percentage 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 tippelzones. 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 II, we present the difference-in-difference model. Section III describes the data. Section IV presents the results on registered crime. Section V discusses results on perceived crime. Section VI concludes.

## I. Background and Literature

### A. *Dutch Tolerance and Tippelzones*

Historically, the Dutch policy toward 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

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

laws were broken. During the second half of the twentieth 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 bylaws. This allowed for 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. The decision to open a tippelzone is a cooperative agreement between the mayor, the city council, and the local police department. In the 1980s and 1990s, the first tippelzones opened to manage problems created by crack and heroin-addicted street prostitutes, their pimps, and drug dealers. At that time, these problems were prominent in many cities and fines and arrests were proving ineffective to control the problems around street prostitutes. Tippelzones were proposed as an innovative way to reduce nuisance overall in the city by concentrating it in a particular area. Tippelzones were also intended to address the health and safety needs of prostitutes.[6] Mayors and city councils proposing to open a tippelzone in the late 1990s and early 2000s met less resistance given the positive past experience in other cities. According to Daalder (2007, 38), "the changes in policy regarding the streetwalking zones are not connected to the lifting of the brothel ban."[7]

Tippelzones are equipped with a variety of features.[8] They 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 semipermanent 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.

The first tippelzone appeared in The Hague in 1983 followed by a second group in the midnineties including Rotterdam (1994) and Amsterdam (1996). In total, nine Dutch cities introduced tippelzones between 1983 and 2004 (see Table 1). The

[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); for Heerlen, Tops and Gooren (2008); for Groningen, Digibron (1995); for Utrecht, Sector Bestuurs-en Concernzaken (2009); and for Amsterdam, Trouw (1995).

[7] See Flight et al. (2006) for an evaluation of the lifting of the brothel ban.

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

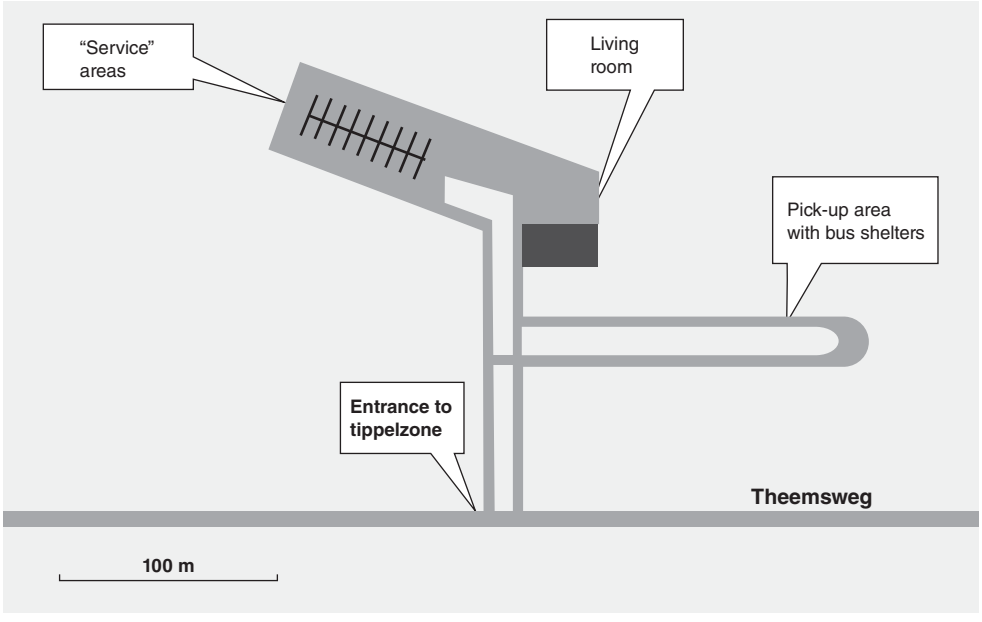


Figure 1. Layout of Amsterdam Tippelzone

*Source:* van Soomeren (2004)

Table 1—Opening and Closing of Tippelzones in the Netherlands

| City | Opening year (month) | Start licensing | Closing year (month) |
|---|---|---|---|
| The Hague | 1983 | never | 2006 (March) |
| Utrecht | 1986 | 2005 (October) | |
| Rotterdam | 1994 (November) | 2003 (April) | 2005 (September) |
| Amsterdam | 1996 (January) | 2002 (June) | 2003 (December) |
| Arnhem | 1996 (June) | 2003 (November) | |
| Groningen | 1998 (January) | never | |
| Heerlen | 2000 (June) | 2000 (June) | 2013 (January) |
| Nijmegen | 2000 (October) | 2007 (September) | |
| Eindhoven | 2004 (December) | 2004 (December) | 2011 (May) |

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

tippelzones were placed in easily accessible nonresidential industrial areas slightly outside city center.[9]

After their introduction, tippelzones remained controversial. In medium-sized cities, they generally functioned well, attracting 20 to 50 prostitutes a night (Oostveen 2008). Tippelzones in larger cities often attracted over 100 prostitutes in a single night (Hulshof and Flight 2008).[10] In the mid-2000s, some tippelzones started

[9] 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 bylaws and ordinances forbidding street prostitution anywhere other than in a tolerated zone.

[10] 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.

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. Flight, Van Heerwaarden, and Lugtmeijer (2003) approximate that just before the licensing in Amsterdam 90 percent of the prostitutes working in the tippelzone were illegal. The licensing in Arnhem reduced the number of drug-addicted prostitutes by about one-third.[11] And also, in Nijmegen and Utrecht, licensing effectively reduced the population of (illegal) prostitutes. In all cities, licensing was maintained adequately.

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, and 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 et al. (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 require 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 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. This change was accompanied by a 30–35 percent 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.

[11] See the newpaper article: "Arnhemse tippelzone blijft open" (*arnhemstad.nl* 2007).

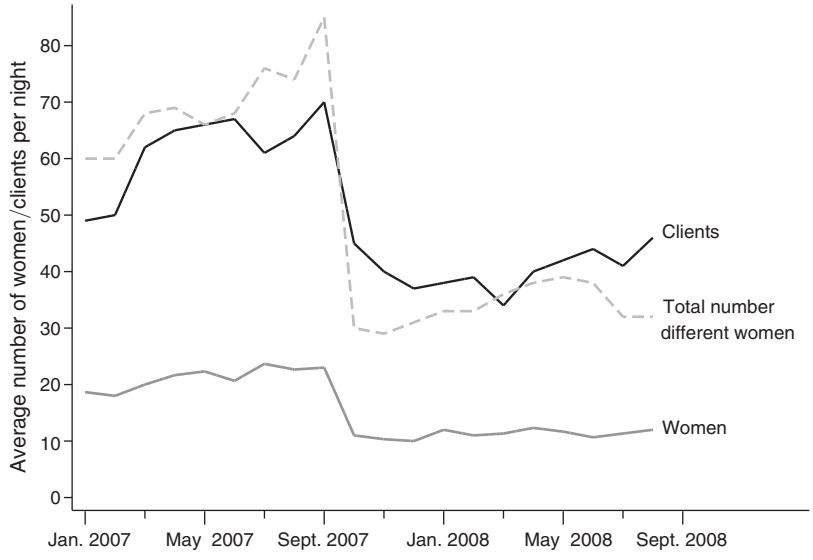


Figure 2. Supply and Demand Response to the Introduction of Licensing for the Nijmegen Tippelzone in September 2007

*Source:* Figure reproduced from Oostveen (2008)

## B. *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. Assuming the probability of apprehension stays constant in all areas of the city, tippelzones can 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 decrease 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 percent of the cases, the perpetrator was a client and in 37 percent of the cases an (ex-)pimp.

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, Van de Bunt, and Kleemans 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 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 Cunningham and Shah (forthcoming), 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 percent 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).[14] Weisburd et al. (2006) find that an increase in police surveillance in two

[12] Bruinsma and Meershoek (1999) study criminals arrested for sex trafficking in the Netherlands and find that the associated criminal organizations range from two to three collaborators to very substantial national or international organized crime networks.

[13] Akee et al. (2014) 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* (National Rapporteur on Trafficking in Human Beings and Sexual Violence against Children 2012) provides more detailed arguments on the problems of measuring human trafficking at a national level.

[14] Displacement of crime can also be intertemporal by changing target, offense, tactic, or offender. Adda, McConnell, and Rasul (2014) find that depenalization of cannabis possession in small quantities leads to an increase in offences for large-quantity cannabis possession. 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.

high-crime neighborhoods in Jersey City reduces drug-related crime both within and around the targeted area. Di Tella and Schargrodsky (2004) and Draca, Machin, and 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. 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.

## II. Empirical Model

We use a difference-in-difference specification to study the effect of the presence of a tippelzone on various types of crime. Let $y_{it}$ denote the observed crime in city $i$ in year $t$, which in our simple baseline model is given by

$$\ln(y_{it}) = \alpha_i + \delta d_{it} + \beta \mathbf{x}_{it} + \mu_t + u_{it}. \tag{1}$$

The city fixed effects are captured by $\alpha_i$, and the time trend $\mu_t$ is modeled using year fixed effects. The variable $d_{it}$ denotes if city $i$ had an open tippelzone in year $t$, and $\mathbf{x}_{it}$ 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. Therefore, our parameter of interest $\delta$ represents the proportional effect of a tippelzone on local crime.

However, we are not only interested in the overall effect of a tippelzone, but also in different regulation systems in tippelzones. For this reason, in most of our empirical analyses, we consider the extended baseline model

$$\ln(y_{it}) = \alpha_i + \delta_0 d_{it}^{-} + \delta_1 l_{it} + \delta_2 d_{it}^{+} + \beta \mathbf{x}_{it} + \mu_t + u_{it}. \tag{2}$$

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.

The parameters of interest are $\delta_0$, $\delta_1$, and $\delta_2$. 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 $\delta_0$, $\delta_1$, and $\delta_2$, we assume that cities would have followed a common time trend in crime were it not for the changes in tippelzone policy. We give evidence in support of this assumption in Subsection IIIA.

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 estimate 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.[15] In Subsection IVE, we also present a placebo analysis following Bertrand, Duflo, and Mullainathan (2004) to show the robustness of the wild-bootstrap standard errors.

## III. 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.[16] The latter two give an indication about the presence of criminal networks. Our motivation for focusing on these crime categories is their frequent association with prostitutes and trafficking organizations as described in Subsection IB. We provide a more thorough description of all variables in Table A3 in the Appendix.

The first panel of Table 2 presents the average yearly crime rates during our observation period. We distinguish between the 3 largest cities which all had a tippelzone (Amsterdam, Rotterdam, and The Hague), the 6 medium-sized cities which opened a tippelzone (Utrecht, Eindhoven, Groningen, Arnhem, Nijmegen, and Heerlen), and the 16 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 percent of the total population are men between 15 and 65 years old. Individuals in cities with tippelzones are somewhat higher 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 insurance benefits recipients between cities. Finally, although tippelzones are not a partisan policy, they were still opposed on moral grounds by the Christian Union (CU), which is evident in the table. Among the other parties, there is no clear relation between the political party of the mayor and whether the city has a tippelzone.[17]

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

[16] 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.

[17] 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.

Table 2—Crime Rates for Cities with and without Tippelzones

| | Tippelzone | | No tippelzone |
|---|---|---|---|
| | Big cities | Medium cities | |
| *Panel A. Annual crime reports per 1,000 inhabitants* | | | |
| Sexual abuse and rape | 0.18 | 0.15 | 0.14 |
| | (0.05) | (0.06) | (0.07) |
| Sexual abuse | 0.08 | 0.06 | 0.07 |
| | (0.02) | (0.03) | (0.05) |
| Rape | 0.10 | 0.09 | 0.07 |
| | (0.03) | (0.04) | (0.04) |
| Drugs | 1.49 | 1.56 | 1.28 |
| | (0.53) | (1.00) | (1.42) |
| Assault | 2.13 | 1.87 | 1.79 |
| | (0.86) | (0.59) | (0.58) |
| Weapons | 0.56 | 0.44 | 0.42 |
| | (0.14) | (0.25) | (0.51) |
| *Panel B. City characteristics* | | | |
| Population | 597,489 | 172,419 | 113,114 |
| | (115,163) | (54,891) | (35,475) |
| Density (population per $km^2$) | 4,326 | 2,298 | 1,956 |
| | (1,135) | (505) | (1,456) |
| Males 15–65 | 210,145 | 61,702 | 39,226 |
| | (45,886) | (20,054) | (12,739) |
| Household income (1,000 €) | 29.05 | 28.99 | 30.50 |
| | (1.34) | (2.00) | (1.85) |
| Higher education (percent) | 0.30 | 0.32 | 0.25 |
| | (0.08) | (0.09) | (0.07) |
| Immigrants (percent) | 0.11 | 0.06 | 0.05 |
| | (0.02) | (0.02) | (0.02) |
| Benefits recipients (percent) | 0.07 | 0.08 | 0.08 |
| | (0.01) | (0.02) | (0.01) |
| *Panel C. Political party of mayor* | | | |
| Socialist (*PVDA*) | 0.48 | 0.56 | 0.41 |
| | (0.50) | (0.50) | (0.49) |
| Christian (*CDA* or *CU*) | 0.26 | 0.12 | 0.31 |
| | (0.44) | (0.33) | (0.46) |
| Liberal (*VVD* or *D66*) | 0.26 | 0.32 | 0.28 |
| | (0.44) | (0.47) | (0.45) |

*Notes: Three big cities* with a tippelzone include Amsterdam, Rotterdam, and The Hague. *Six medium cities* with a tippelzone include Utrecht, Eindhoven, Groningen, Nijmegen, Heerlen, and Arnhem. *Sixteen cities* without tippelzone include Almelo, Breda, Deventer, Dordrecht, Enschede, Haarlem, Helmond, Hengelo, Leeuwarden, Leiden, Maastricht, Schiedam, Tilburg, Venlo, Zwolle, and 's-Hertogenbosch. Standard deviations are in parentheses.

### A. *Assessing Crime Trends*

As mentioned in Section II, 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

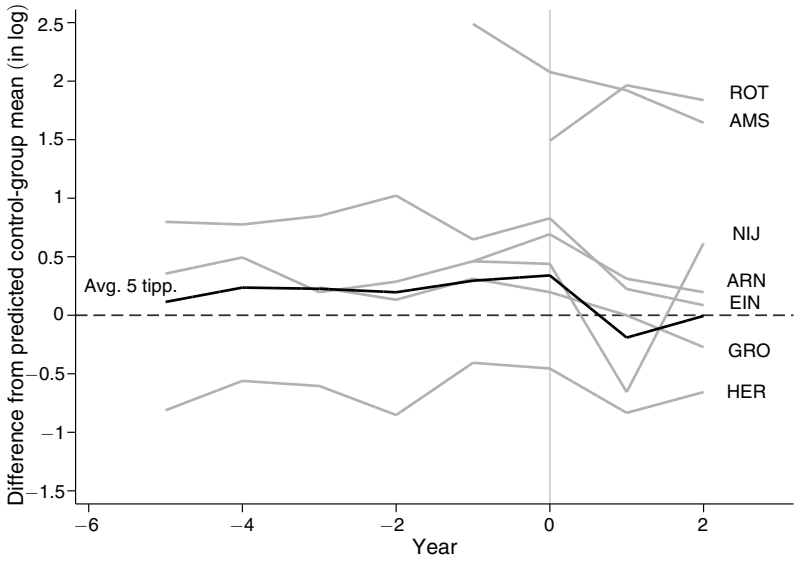


FIGURE 3. CRIME RATES IN CITIES WITH A TIPPELZONE COMPARED TO CITIES WITHOUT A TIPPELZONE

*Note:* The no-tippelzone counterfactual for a city $i$ is generated by estimating model (2) on 21 cities leaving out city $i$ and the 3 largest cities, and then averaging the fitted values fixing $d_{it}^{-} = 0$, $l_{it} = 0$, and $d_{it}^{+} = 0$.

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. In Figure 3, we present the city crime rates to check for diverging trends prior to the opening of a tippelzone. Each grey line in the figure represents the difference between the logarithm of the crime outcome in a city which opened a tippelzone, and its predicted control crime trend. The black line is the average for the medium-size cities. To calculate the predicted control crime trend for a city $i$, we first estimate model (2) on all cities. We then set $\delta_0 = \delta_1 = \delta_2 = 0$ and average the fitted values, leaving out city $i$ and the three largest cities. This procedure allows us to standardize the opening time to the year before the tippelzone was first introduced (marked at zero).

Besides a level difference,[18] the trends in crime show no systematic increases or decreases in sexual abuse and rape relative to their predicted control group in the years prior to opening a zone. 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 $(\exp(-0.4) - 1) \times 100\% \approx -33\%$.

[18] In our empirical analysis, the level differences in crime will be captured by the city fixed effects $\alpha_i$.

We only observe one preopening period for Rotterdam, two preopening periods for Amsterdam, and none for The Hague. This also means we cannot use past crime rates to inspect preopening trends 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 A1 and A2 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.

## IV. 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 study possible spillover effects between cities. Finally, we provide some robustness analyses and discuss our findings.

### A. *Baseline Results*

The upper panel of Table 3 shows the estimation results on the simple baseline model only considering the effect of an open tippelzone. The left frame presents results using data on the 22 medium-sized cities; the right frame also takes the 3 large cities into account.[19] The results show that an open tippelzone significantly reduces citywide sexual abuse and rape by about $(\exp(-0.175) - 1) \times 100\% \approx -16\%$. This effect is mainly driven by about a 32 percent reduction in sexual abuse. The results are not very sensitive to including the three largest cities in our sample.

The bottom panel of Table 3 shows estimation results for the extended baseline model, which also takes regulation of tippelzone into account. 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 borders (Flight, Van Heerwaarden, and Lugtmeijer 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 IVC.

[19] In Tables A4 and A5 in the Appendix, we present additional results without covariates and specifications, which include a two-year lead dummy to check for preopening shifts in crime.

Table 3—Effect of Opening a Tippelzone and Licensing on Citywide Registered Crime

| | 22 cities with covariates | | | 25 cities with covariates | | |
|---|---|---|---|---|---|---|
| | Sexual abuse and rape | Sex abuse | Rape | Sexual abuse and rape | Sex abuse | Rape |
| *Simple baseline model:* | | | | | | |
| Tippelzone | −0.175 | −0.379 | −0.027 | −0.144 | −0.302 | −0.042 |
| | (0.085) | (0.150) | (0.136) | (0.060) | (0.113) | (0.052) |
| $N$ (city × year) | 395 | 395 | 395 | 450 | 450 | 450 |
| $R^2$ | 0.63 | 0.43 | 0.55 | 0.82 | 0.69 | 0.77 |
| *Extended baseline model:* | | | | | | |
| Open noLic.($\delta_0$) | −0.198 | −0.358 | −0.090 | −0.205 | −0.304 | −0.133 |
| | (0.105) | (0.262) | (0.077) | (0.109) | (0.195) | (0.097) |
| Intro. Lic.($\delta_1$) | 0.286 | 0.413 | 0.220 | 0.168 | 0.250 | 0.115 |
| | (0.254) | (0.187) | (0.290) | (0.145) | (0.146) | (0.200) |
| Open Lic.($\delta_2$) | −0.184 | −0.447 | 0.012 | −0.169 | −0.411 | 0.004 |
| | (0.073) | (0.154) | (0.092) | (0.097) | (0.142) | (0.034) |
| Closing | | | | 0.023 | 0.160 | −0.045 |
| | | | | (0.222) | (0.191) | (0.187) |
| $N$ (city × 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 |

*Notes:* Standard errors are in parentheses based on Cameron, Gelbach, and Miller's (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* (percent), *immigrants* (percent), *unemployment insurance recipients* (percent), and *higher educated* (percent).

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.

### B. *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 afterward. All effects (except the ex post licensing) describe proportional shifts from the preopening crime levels. As in the baseline specification, the effect of ex post licensing represents a proportional shift compared to the presence of a tippelzone without licensing.

Table 4—Time-Varying Effects and Robustness Checks on Citywide Sexual Abuse and Rape (22 *cities*)

| Dependent variable: | Sexual abuse and rape | | | | | Sex abuse | Rape |
|---|---|---|---|---|---|---|---|
| preopening | | 0.062 | | | | | |
| | | (0.063) | | | | | |
| 1st–2nd year | −0.385 | −0.350 | −0.377 | −0.350 | −0.369 | −0.438 | −0.325 |
| Open noLic. | (0.133) | (0.121) | (0.130) | (0.121) | (0.127) | (0.199) | (0.164) |
| 3rd+ year | −0.131 | −0.054 | −0.061 | −0.024 | −0.064 | −0.269 | 0.029 |
| Open noLic. | (0.112) | (0.127) | (0.108) | (0.143) | (0.115) | (0.225) | (0.115) |
| pre-Intro Lic. | | −0.202 | | | | | |
| | | (0.201) | | | | | |
| Intro. Lic. | 0.252 | 0.180 | 0.196 | 0.163 | 0.186 | 0.306 | 0.144 |
| | (0.247) | (0.174) | (0.210) | (0.171) | (0.184) | (0.198) | (0.312) |
| 1st–2nd year | −0.518 | −0.499 | −0.525 | −0.525 | −0.543 | −0.944 | −0.238 |
| Open Lic. | (0.179) | (0.172) | (0.181) | (0.181) | (0.188) | (0.326) | (0.245) |
| 3rd+ year | −0.062 | −0.046 | −0.065 | −0.065 | −0.051 | −0.295 | 0.123 |
| Open Lic. | (0.055) | (0.057) | (0.053) | (0.053) | (0.060) | (0.159) | (0.132) |
| Spill. Lic. | | | −0.003 | 0.030 | 0.038 | 0.104 | 0.029 |
| | | | (0.048) | (0.102) | (0.093) | (0.152) | (0.186) |
| Spill. Closing | | | −0.057 | −0.096 | −0.109 | −0.294 | −0.039 |
| | | | (0.153) | (0.253) | (0.258) | (0.362) | (0.205) |
| Ban lift | | | | −0.094 | −0.072 | −0.050 | −0.082 |
| | | | | (0.169) | (0.201) | (0.191) | (0.299) |
| Spill. Tipp. | | | | | Yes | Yes | Yes |
| $N$ (city × year) | 395 | 395 | 395 | 395 | 395 | 395 | 395 |
| $R^2$ | 0.64 | 0.64 | 0.64 | 0.63 | 0.63 | 0.44 | 0.55 |
| $p$-val($F_{1,2}$) | 0.55 | 0.48 | 0.51 | 0.43 | 0.44 | 0.19 | 0.77 |
| $p$-val($F_{3,+}$) | 0.58 | 0.96 | 0.98 | 0.79 | 0.92 | 0.93 | 0.59 |
| City fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year D. | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Standard errors are in parentheses based on Cameron, Gelbach, and Miller's (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* (percent), *immigrants* (percent), *unemployment insurance recipients* (percent), and *higher educated* (percent). $p$-val($F_{1,2}$) is the $p$-value for the test $H_0$ : *1st–2nd year Open noLic.* = *1st–2nd year Open Lic.*, and $p$-val($F_{3,+}$) is the $p$-value for the test $H_0$ : *3rd+ year Open noLic.* = *3rd+ year Open Lic.*

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 percent for cities with a licensed tippelzone and 32 percent for cities with free entry zones, and the difference between the two is insignificant.[20] Beyond the first two years, there is no difference in crime relative to the preopening period. The ex post introduction of licensing again increases sex-related crime. The effect is substantial, but lacks power to be significant.

[20]The $p$-values in Table 4 show that the difference in the time-varying effect between tippelzones with and without licensing is never significant.

The second column of Table 4 provides evidence for the common-trend assumption by including indicators for preopening and prelicensing periods. These variables take value one in the two years before opening a tippelzone or introducing licensing. As expected from Figure 3, we do not find any sign of preopening 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 nonrandomly between cities following the opening of a tippelzone or changes in licensing policy. As discussed in Subsection IA, caseworkers 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 Lic.) dispelling illegal workers. These variables are nonzero 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. For example, Utrecht *Spillover Lic.* takes value zero up until 2001 and after 2005 but takes value one in 2002, two in 2003, and three in 2004–2005 as licensing was introduced in Amsterdam, Rotterdam, and Arnhem. Similarly, the variable *Spillover Closing* for Utrecht takes value zero up until 2003 and after 2005 but takes value one in 2004–2005 due to the closing of the Amsterdam tippelzone.[21]

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.[22]

In the last three columns, we account for possible crime spillovers to neighboring cities. In this model, we include three additional spillover indicator variables, one for each of $d_{it}^{-}$, $l_{it}$, and $d_{it}^{+}$. These variables turn on and off for any control city within 50 kilometers (km) of a city which opened a tippelzone or introduced a licensing system. These spillover effects will capture any displacement effects of crime to cities within a 50km radius of a tippelzone city at the moment the latter opens a tippelzone or changes its licensing regulation. The parameters of interest for sexual violence remain unaffected, and the spillover effects[23] show no reversed changes in crime in neighboring cities.[24] 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. Furthermore, we find that the reductions in sexual abuse persist beyond the first year in cities which implemented licensing from the start.

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

[22] The variable *brothel ban lift* takes value one in cities with an open tippelzone and no licensing system in 2001 or later, and zero otherwise.

[23] The results are presented fully in Table A6 of the Appendix.

[24] When estimating a model with saturated spillover effects (not presented), the parameters on *1st–2nd year open Lic.* and *1st–2nd year open noLic.* are close to zero in magnitude and insignificant.

### C. *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 percent 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 percent were victims of abuse and 16 percent were raped in the previous year alone (Oostveen 2008). Despite this high sexual violence, 95 percent 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.[25] 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. In particular, among the window, escort, and club prostitutes interviewed in Venicz and Vanwesenbeeck (2000), 71 percent 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. 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 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.[26] Under the theoretical predictions of Subsection IB, 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 cannot separate these two hypotheses.

A last possibility is that the decreases in sexual violence are driven by changes in crime-reporting behavior. Inside the tippelzones, prostitutes may be encouraged to report certain types of crime such as sexual violence more often or the police may register crime themselves more frequently. This would imply that our estimates provide a lower bound to the estimated effects. However, potential criminals may have shifted 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

[25] A larger survey of prostitutes in 9 countries reports that 71 percent have been physically assaulted and 63 percent have been raped while working as prostitutes (Farley et al. 2004).

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

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 cannot 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. Furthermore, regarding policing behavior, we did not find strong arguments or articles describing citywide policing efforts to have changed concurrently and systematically with the opening of a tippelzone.[27]

Overall, we find only weak evidence of long-run effects for cities with and without licensing. For tippelzones without a licensing system, one explanation for why the initial reductions in crime fade away over time is that increased competition over time in the tippelzones forced some prostitutes to seek opportunities in other less safe areas thereby becoming victim of more sexual abuse and rape.[28] This interpretation can also explain the increases in sexual violence following the ex post introduction of licensing systems. A last possibility is that time-varying effects are due to changes in monitoring by the police force. While there exists no longitudinal data on police monitoring, reports on tippelzones do not mention structural changes or substantial reductions in monitoring (e.g., Flight, Van Heerwaarden, and Lugtmeijer 2003; Hulshof and Flight 2008; Oostveen 2008; and van Soomeren 2004). The only significant policy changes are regulation by introducing a licensing system. 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.

### D. *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 tippelzones enforce licensing. In those cities, we find a significant decrease of approximately 25 percent in drug crime. The results are robust to different specifications and persist beyond the first two years (presented in Tables A5 and A6 in the Appendix). This suggests that in cities which enforced licensed tippelzones

[27] Information on the size of the police force is only available from 2005 onwards.

[28] 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.

Table 5—Effect of Tippelzone on Citywide Registered Crime

| | 22 cities with covariates | | | 25 cities with covariates | | |
|---|---|---|---|---|---|---|
| | Drugs | Weapons | Assault | Drugs | Weapons | Assault |
| *Simple baseline model:* | | | | | | |
| Tippelzone | −0.191 | −0.083 | 0.093 | −0.057 | −0.074 | −0.017 |
| | (0.150) | (0.201) | (0.075) | (0.084) | (0.101) | (0.048) |
| $N$ (city × year) | 395 | 395 | 395 | 450 | 450 | 450 |
| $R^2$ | 0.81 | 0.76 | 0.89 | 0.89 | 0.90 | 0.95 |
| *Extended baseline model:* | | | | | | |
| Open noLic. ($\delta_0$) | −0.034 | 0.052 | 0.139 | −0.067 | 0.002 | 0.113 |
| | (0.286) | (0.113) | (0.115) | (0.153) | (0.083) | (0.102) |
| Intro. Lic. ($\delta_1$) | −0.244 | −0.095 | −0.053 | −0.155 | −0.070 | 0.030 |
| | (0.260) | (0.156) | (0.154) | (0.113) | (0.136) | (0.120) |
| Open Lic. ($\delta_2$) | −0.324 | −0.210 | 0.052 | −0.284 | −0.168 | 0.034 |
| | (0.128) | (0.351) | (0.105) | (0.125) | (0.335) | (0.141) |
| Closing | | | | −0.124 | 0.095 | 0.207 |
| | | | | (0.145) | (0.110) | (—) |
| $N$ (city × 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 |

*Notes:* Standard errors are in parentheses based on Cameron, Gelbach, and Miller's (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* (percent), *immigrants* (percent), *unemployment insurance recipients* (percent), and *higher educated* (percent).

from the start, local governments achieved one of their stated goals to address drug problems related to prostitutes.

It is likely that the effects on drug crime are driven by changes in policing behavior due to the policy goals of tippelzones. Before tippelzones opened, arrest for drug crimes was the main response to complaints by local residents about drug dealing to addicted prostitutes. One of the main objectives of tippelzones was to reduce the nuisance rather than the drug dealing itself. Therefore, the police did not prioritize arresting street prostitutes and pimps for drug dealing in and around the tippelzone.[29] The combination of these factors could explain the decrease in drug crime. Also, health-care providers present in tippelzones may have had some small influence on the drug use of 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 the trafficking of drugs, weapons, or more violent crimes, then opening a free entry

[29] This guideline of turning a blind eye to drug dealing around tippelzones became a key point of contention when discussing the closing of tippelzones (van Soomeren 2004).

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.

### E. *Falsification Analyses*

In this subsection, we provide some additional analysis to obtain insight in the validity of our identification. We first focus on types of crime that are not likely associated to tippelzones, but are considered to be strongly related to citywide monitoring by the police force. Next, we discuss a placebo analysis.

We start by considering the effects of tippelzones on theft (without assault), public-order offense, and public violence. These are types of crime which are typically not directly associated to prostitution, drug use, or organized crime but can be affected by police monitoring. Estimating the effects of opening a tippelzone and introducing licensing on these additional crime categories has two purposes. First, to establish that the policy surrounding a tippelzone is not part of a more extensive policy to reduce crime. Were this the case, then we would expect the effects on theft, public-order offenses, and public violence to parallel those on sexual violence. And second, there may be substitution if the tippelzone diverts police attention away from other tasks. The left part of Table 6 does not show any significant or substantial effects of opening a tippelzone and introducing licensing on theft, public-order offenses, and public violence. These results therefore do not indicate any signs that the effects on sexual violence and drug crime are due to coinciding changes in citywide-crime control policies.

Next, we provide a placebo analysis for the estimated effects of our baseline models. More specifically, we follow Bertrand, Duflo, and Mullainathan (2004) by randomly generating placebo moments of opening a tippelzone for cities that never actually had a tippelzone. For each draw of placebo interventions, we estimate the effect of the tippelzone, which provides us with the distribution of estimated treatment effects under the null hypothesis that there is no effect of tippelzones.[30] We draw placebo interventions 999 times and calculate the *p*-values as the number of times the placebo estimate is larger than the estimate using the real tippelzone cities. These one-sided *p*-values for significance are shown in brackets in Table 6 and show that our placebo inference follows closely our results in Table 3 and Table 5.

## V. Perceived Crime

The second data source is the Population Police Monitor (PPM), which examines perceived crime and safety.[31] 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

[30] For the extended baseline model, we follow the same procedure but randomly draw combinations of opening with or without licensing, or imposing licensing ex post.

[31] 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—Falsification Analysis for Registered-Crime Results

| | | | | Placebo inference ($p$-values for one-sided test in brackets) | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Theft | PO offense | Public violence | Sex abuse and rape | Sex abuse | Rape | Drugs | Weapons | Assault |
| *Simple baseline model:* | | | | | | | | | |
| Tippelzone | 0.030 | 0.120 | 0.109 | −0.175 | −0.379 | −0.027 | −0.191 | −0.083 | 0.093 |
| | (0.082) | (0.125) | (0.129) | [0.089] | [0.007] | [0.510] | [0.060] | [0.152] | [0.123] |
| $N$ (city × year) | 395 | 395 | 395 | 395 | 395 | 395 | 395 | 395 | 395 |
| $R^2$ | 0.94 | 0.80 | 0.76 | 0.63 | 0.43 | 0.55 | 0.82 | 0.69 | 0.77 |
| *Extended baseline model:* | | | | | | | | | |
| Open noLic. ($\delta_0$) | 0.102 | 0.196 | 0.153 | −0.198 | −0.358 | −0.090 | −0.034 | 0.052 | 0.139 |
| | (0.071) | (0.202) | (0.214) | [0.123] | [0.038] | [0.381] | [0.330] | [0.508] | [0.118] |
| Intro. Lic. ($\delta_1$) | −0.212 | −0.121 | −0.147 | 0.286 | 0.413 | 0.220 | −0.244 | −0.095 | −0.053 |
| | (0.201) | (0.097) | (0.089) | [0.173] | [0.019] | [0.451] | [0.094] | [0.120] | [0.326] |
| Open Lic. ($\delta_2$) | −0.021 | 0.054 | 0.080 | −0.184 | −0.447 | 0.012 | −0.324 | −0.210 | 0.052 |
| | (0.087) | (0.141) | (0.172) | [0.063] | [0.050] | [0.184] | [0.098] | [0.232] | [0.394] |
| $N$ (city × year) | 395 | 395 | 395 | 395 | 395 | 395 | 395 | 395 | 395 |
| $R^2$ | 0.94 | 0.80 | 0.76 | 0.63 | 0.44 | 0.55 | 0.82 | 0.69 | 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 | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* Standard errors are in parentheses based on Cameron, Gelbach, and Miller's (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* (percent), *immigrants* (percent), *unemployment insurance recipients* (percent), and *higher educated* (percent). Placebo inference $p$-values are based on a one-sided test of 999 placebo treatment sequence replications on the cities in which a tippelzone never opened.

with police, and crime in their neighborhood. The participation rate in the survey ranges from 46 percent to 72 percent 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: Happens regularly, Happens sometimes, Never happens/Hardly ever happens, and 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 7 presents the fraction of answers within each perceived crime category. For medium-sized cities, about 70 percent of respondents indicate that violent crime and drug crime are never or hardly ever observed. Approximately 10 percent respond that drug nuisance occurs regularly and about 5 percent 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 4, panel A shows how perceived crimes change over time. The trends for all response categories remain relatively constant during the observation period. For comparison, we show in Figure 4, panel B the trends in registered crime. The public

Table 7—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 |

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

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.

### A. *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, and nationality) and the same city characteristics as in the registered-crime analysis. We cluster standard errors at the postcode level since they are more conservative than those when clustering at the city level.

Table 8 presents results from our baseline models 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. The upper panel shows that perceived drug nuisance due to the presence of a tippelzone is significantly higher in neighborhoods not located near the tippelzone. However, the bottom panel shows that this only holds for non-licensed tippelzones. There are no significant effects in the tippelzone area. Furthermore, our baseline specifications do not show significant effects on violence.

Our extended 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 9 considers the further extended model with time-varying effects in perceived drug nuisance, which we specified earlier for registered crime. The first

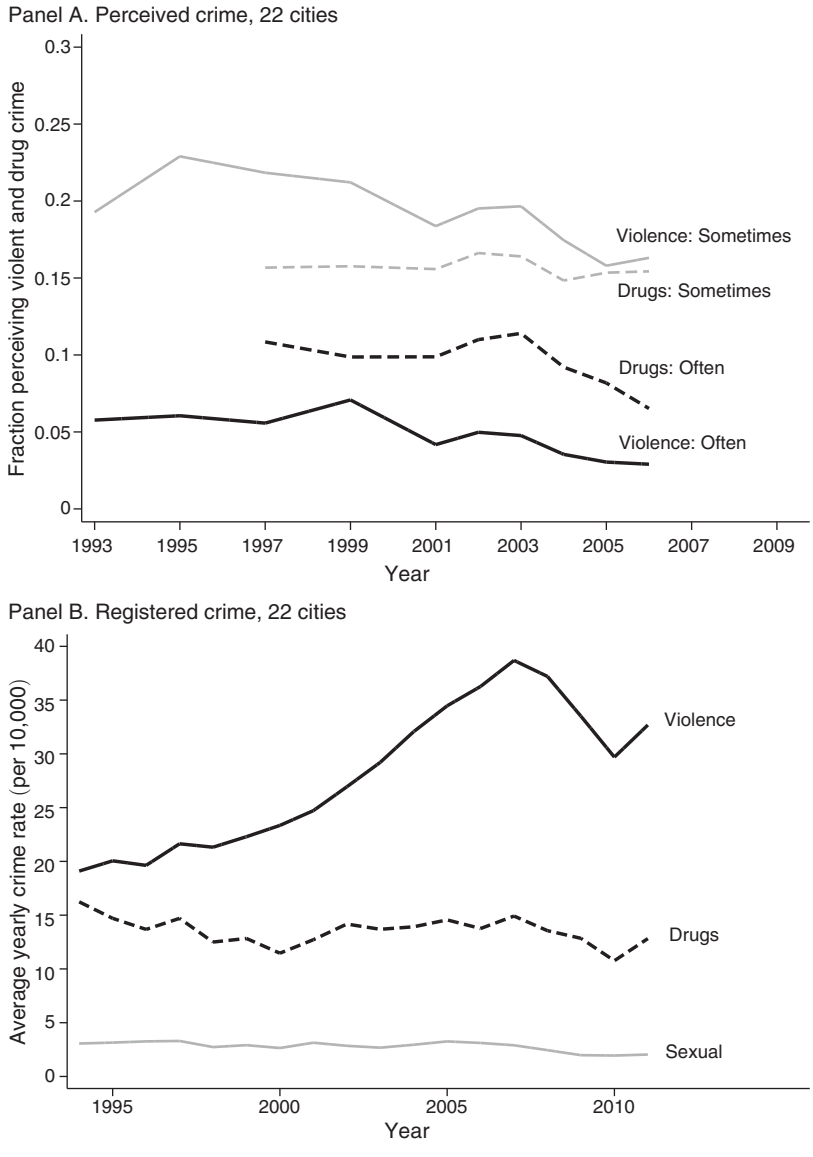


Figure 4. Trends in Registered and Perceived Crime

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 percentage 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 preopening period. These are large impacts since on average only 10 percent of the residents respond that drug nuisance occurs often.

In contrast, residents in non-tippelzone areas of cities which introduced licensing from the start do not see an initial increase in crime but perceive a 5.3-percentage-point reduction in drug nuisance in the medium to long run. In addition, residents in

TABLE 8—EFFECT OF TIPPELZONE ON CITYWIDE PERCEIVED CRIME IN MEDIUM-SIZED CITIES

| | Drugs | | | | Violence | | | |
|---|---|---|---|---|---|---|---|---|
| | Often | | Often/sometimes | | Often | | Often/sometimes | |
| | Non-tipp. area | Tipp. area | Non-tipp. area | Tipp. area | Non-tipp. area | Tipp. area | Non-tipp. area | Tipp. area |
| *Simple baseline model:* | | | | | | | | |
| Tippelzone | 0.021 | −0.024 | 0.027 | 0.009 | −0.009 | −0.016 | −0.002 | −0.030 |
| | (0.012) | (0.034) | (0.013) | (0.029) | (0.005) | (0.013) | (0.012) | (0.022) |
| $N$ (city × 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 | |
| *Extended baseline model:* | | | | | | | | |
| Open noLic. ($\delta_0$) | 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. ($\delta_1$) | −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. ($\delta_2$) | −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 × 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 | |

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

areas adjacent to the tippelzone perceive an 4.2-percentage-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 relocated to the tippelzone areas.

The third and fourth columns of Table 9 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.[32] 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.

[32] Note that the response categories are exclusive. For this reason, if fewer residents respond "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.

Table 9—Within-City Effects of Tippelzone on Perceived Crime (22 *cities with covariates*)

| | Drugs: Often | | Drugs: Often/ sometimes | |
|---|---|---|---|---|
| | Non-tipp. area | Tipp. area | Non-tipp. area | Tipp. area |
| preopening | 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. | Yes | | Yes | |
| Spillover Closing. | Yes | | Yes | |
| Brothel ban lift | Yes | | Yes | |
| *N* (city × 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 | |

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

The results for non-tippelzone areas are consistent with reports for tippelzones which imposed licensing immediately.[33] Initially, the tippelzones were met with opposition.[34] Later, as city residents became more informed about the purpose of a tippelzone they also became more accepting of its 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 A7 in the Appendix). As such,

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

[34] van Soomeren (2004, 6) 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."

they are in line with the results on registered crime in Table 5 for aggregate illegal weapons and assaults.

## VI. 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 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 percent 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 the 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 percent decrease in average citywide drug crime, and this result persists over time but only in cities which enforced a licensed tippelzone from the start. However, we do not find any evidence for the effects on other crimes linked to trafficking organizations such as illegal weapons or violent assaults.

The tippelzone experiment for cities like Amsterdam clearly indicates that the zoning and legalizing of prostitutes when left unchecked can attract a set of existing criminals and also create a new market for illegally trafficked prostitutes. However, Oostveen (2008) indicates that the combination of licensing and rotating agents was sufficient to maintain order in and around the tippelzones. Assuming that the costs of the additional routine police surveillance are sufficiently low, tippelzones can be a cost effective policy measure to reduce crime surrounding prostitutes. In addition, tippelzones reduce search costs of police officers for certain noise complaints and crimes, which can also free up time for police to patrol other areas of the city.

The public debate about tippelzones also focused on health concerns. Given the proclivity of prostitutes for risky activity, policymakers may also be interested in observing whether a tippelzone influences STDs, hospital admissions, or deaths due

to overdose. Despite our efforts we were unable to find detailed enough data on health questions.

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 percentage 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.

It is regularly observed that registered and perceived crime do not match. For example, Warr (2000) suggests that the public is likely to exaggerate the frequency of rare, serious crimes and underestimate the frequency of more common, less serious ones. This can be explained by people fixating on available heuristics presented in the media since the media aims for sensationalism rather than producing an unbiased picture of actual crime (Lowry, Nio, and Leitner 2003; Weitzer and Kubrin 2004). In the Netherlands, crime is a relatively rare event and the opening of tippelzones drew a lot of media attention affecting the public opinion. Our preferred interpretation of the results is that the media coverage influenced the perception of drug crime in the entire city. However, over time this increased perception faded away except in the area surrounding the tippelzone. For the people in these areas, drug crime was simply more visible than before the opening of the tippelzone.

## Appendix

Table A1—Overview of Prostitution Laws in Selected Countries

| | Legal status | | |
|---|---|---|---|
| Country | Prostitution | Brothel ownership | Pimping |
| Netherlands | legal | legal | legal |
| Germany | legal | legal | legal |
| Belgium | legal | legal | illegal |
| Canada | buying sex illegal | legal | illegal |
| Sweden | buying sex illegal | illegal | illegal |
| United States | illegal (except Nevada) | illegal | illegal |
| Japan | limited legality | illegal | illegal |
| Spain | limited legality | illegal | illegal |
| United Kingdom | legal | illegal | illegal |
| France | legal | illegal | illegal |
| Italy | legal | illegal | illegal |

*Note:* Most countries enforce additional limitations and requirements for soliciting, procuring, and sex establishments.

*Source:* http://prostitution.procon.org/view.resource.php?resourceID=000772

TABLE A2—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 |

*Note:* Approximations are from Flight et al. (2006) based on survey responses from medium and large-sized municipalities across the Netherlands.

TABLE A3—DESCRIPTION OF VARIABLES

| | |
|---|---|
| *Outcome variables* | |
| Sexual Abuse | Forced to commit or tolerate obscene sexual acts through violence or threat (excluding penetration) |
| Rape | Forced to commit or tolerate obscene sexual acts including penetration through violence or threat |
| Drugs | International trafficking, manufacturing, processing, selling, transporting, or possession of illegal drugs |
| Assault | Inflicted grievous bodily harm, possibly resulting in death of victim (includes small up to aggravated assault) |
| Weapons | Trafficking, manufacturing, processing, selling, or possession of (illegal) weapons or ammunition |
| Theft | Theft of property without assault on a person |
| Public order offense | Includes public violence, trespassing, and discrimination |
| Public violence | Act of violence against persons or property in open society |
| *Policy Variables* | |
| Table 3: *Simple baseline model* | |
| Tippelzone | 1 if city $i$ has open tippelzone in year $t$, 0 otherwise |
| Table 3: *Extended baseline model* | |
| Open noLic. | 1 if city $i$ ever has open tippelzone in or before year $t$ which opened without licensing, 0 otherwise |
| Intro. Lic. | 1 if city $i$ has open tippelzone and introduced licensing ex post in or before year $t$, 0 otherwise (additive effect) |
| Open Lic. | 1 if city $i$ ever has open tippelzone in or before year $t$ which opened with licensing, 0 otherwise |
| Closing | 1 if city $i$ closes tippelzone in or before year $t$, 0 otherwise (additive effect) |
| Table 4: | |
| preopening | 1 if city $i$ opens tippelzone in year $t + 1$ or $t + 2$, 0 otherwise |
| 1st–2nd year Open noLic. | (Open noLic.) × (1st–2nd year after opening) |
| 3rd+ year Open noLic. | (Open noLic.) × (3rd year and later after opening) |
| pre-Intro Lic. | 1 if city $i$ introduces licensing in year $t + 1$ or $t + 2$, 0 otherwise |
| Intro. Lic. | Same as in Table 3 |
| 1st–2nd year Open Lic. | (Open Lic.) × (1st–2nd year after opening) |
| 3rd+ year Open Lic. | (Open Lic.) × (3rd year and later after opening) |
| Spillover Lic. | Count variable: increases by increments of 1 in city $i$ for each city $j$ which introduces licensing ex post but only if city $i$ has open tippelzone without licensing in year $t$, 0 otherwise |
| Spillover Closing | Count variable: increases by increments of 1 in city $i$ for each city $j$ which closes tippelzone but only if city $i$ has open tippelzone without licensing in year $t$, 0 otherwise |
| Brothel ban lift | 1 if city $i$ has an open tippelzone and no licensing system in 2001 or later, 0 otherwise |
| Spillover Tipp. | |
| Spill. Opening noLic. | 1 if city $j$ is within 50 km from city $i$, and city $i$ ever has open tippelzone in or before year $t$ which opened without licensing, 0 otherwise |
| Spill. Opening Lic. | 1 if city $j$ is within 50 km from city $i$, and city $i$ has open tippelzone and introduced licensing ex post in or before year $t$, 0 otherwise (additive effect) |
| Spill. Intro. Lic. | 1 if city $j$ is within 50 km from city $i$, and city $i$ ever has open tippelzone in or before year $t$ which opened with licensing, 0 otherwise |

*Notes:* Dutch translations: Sexual Abuse = Seksueel Misdrijf:Aanranding; Rape = Seksueel Misdrijf:Verkrachting; Drugs = Drugsmisdrijven; Assault = Mishandeling; Weapons = (Vuur)wapenmisdrijven; Theft = Gekwalificeerde diefstal+Eenvoudige diefstal; Public order offense = Openbare orde misdrijf; Public Violence = Openlijke geweldpleging. Full descriptions of each category can be found at http://www.wetboek-onlinenl/wet/Wetboek%20van%20Strafrecht.html.

Table A4—Effect of a Tippelzone and Licensing on Citywide Registered Crime

| | Sexual abuse and rape | | | Sexual abuse | | | Rape | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| 22 *cities* | | | | | | | | | |
| preopening | | | 0.046 | | | −0.066 | | | 0.133 |
| | | | (0.059) | | | (0.211) | | | (0.203) |
| Open noLic. | −0.136 | −0.198 | −0.170 | −0.296 | −0.358 | −0.397 | −0.022 | −0.090 | −0.011 |
| | (0.134) | (0.105) | (0.147) | (0.329) | (0.262) | (0.315) | (0.072) | (0.077) | (0.109) |
| Intro. Lic. | 0.214 | 0.286 | 0.287 | 0.360 | 0.413 | 0.413 | 0.136 | 0.220 | 0.221 |
| | (0.230) | (0.254) | (0.252) | (0.143) | (0.187) | (0.187) | (0.212) | (0.290) | (0.297) |
| Open Lic. | −0.120 | −0.184 | −0.170 | −0.417 | −0.447 | −0.467 | 0.110 | 0.012 | 0.052 |
| | (0.137) | (0.073) | (0.098) | (0.144) | (0.154) | (0.175) | (0.197) | (0.092) | (0.146) |
| $N$ (city × 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* | | | | | | | | | |
| preopening | | | 0.057 | | | −0.103 | | | 0.169 |
| | | | (0.088) | | | (0.190) | | | (0.203) |
| Open noLic. | −0.131 | −0.205 | −0.167 | −0.229 | −0.304 | −0.373 | −0.055 | −0.133 | −0.019 |
| | (0.107) | (0.109) | (0.123) | (0.213) | (0.195) | (0.250) | (0.069) | (0.097) | (0.069) |
| Intro. Lic. | 0.167 | 0.168 | 0.169 | 0.258 | 0.250 | 0.250 | 0.125 | 0.115 | 0.115 |
| | (0.129) | (0.145) | (0.143) | (0.133) | (0.146) | (0.142) | (0.167) | (0.200) | (0.200) |
| Open Lic. | −0.108 | −0.169 | −0.152 | −0.385 | −0.411 | −0.442 | 0.099 | 0.004 | 0.055 |
| | (0.109) | (0.097) | (0.120) | (0.133) | (0.142) | (0.189) | (0.211) | (0.034) | (0.248) |
| Closing | 0.098 | 0.023 | 0.021 | 0.319 | 0.160 | 0.164 | −0.044 | −0.045 | −0.051 |
| | (0.128) | (0.222) | (0.215) | (0.184) | (0.191) | (0.188) | (0.120) | (0.187) | (0.182) |
| $N$ (city × 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 |

*Notes:* Standard errors are in parentheses based on Cameron, Gelbach, and Miller's (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* (percent), *immigrants* (percent), *unemployment insurance recipients* (percent), and *higher educated* (percent).

Table A5—Effect of a Tippelzone and Licensing on Citywide Registered Crime

| | Drugs | | | Weapons | | | Assault | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| 22 *cities* | | | | | | | | | |
| preopening | | | 0.080 | | | 0.065 | | | −0.052 |
| | | | (0.294) | | | (0.125) | | | (0.107) |
| Open noLic. | −0.024 | −0.034 | 0.013 | −0.047 | 0.052 | 0.090 | 0.108 | 0.139 | 0.108 |
| | (0.193) | (0.286) | (1.073) | (0.129) | (0.113) | (0.162) | (0.104) | (0.115) | (0.095) |
| Intro. Lic. | −0.283 | −0.244 | −0.244 | −0.166 | −0.095 | −0.095 | −0.121 | −0.053 | −0.053 |
| | (0.209) | (0.260) | (0.262) | (0.187) | (0.156) | (0.158) | (0.164) | (0.154) | (0.163) |
| Open Lic. | −0.306 | −0.324 | −0.300 | −0.163 | −0.210 | −0.191 | 0.055 | 0.052 | 0.037 |
| | (0.160) | (0.128) | (0.162) | (0.246) | (0.351) | (0.315) | (0.105) | (0.105) | (0.088) |
| $N$ (city × 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 |

(*continued*)

TABLE A5—EFFECT OF A TIPPELZONE AND LICENSING ON CITYWIDE REGISTERED CRIME (*continued*)

| | Drugs | | | Weapons | | | Assault | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| 25 *cities* | | | | | | | | | |
| preopening | | | 0.099 | | | 0.105 | | | −0.056 |
| | | | (0.182) | | | (0.136) | | | (0.089) |
| Open noLic. | −0.080 | −0.067 | −0.000 | −0.090 | 0.002 | 0.073 | 0.074 | 0.113 | 0.075 |
| | (0.165) | (0.153) | (0.010) | (0.127) | (0.083) | (0.136) | (0.080) | (0.102) | (0.097) |
| Intro. Lic. | −0.283 | −0.155 | −0.155 | −0.189 | −0.070 | −0.070 | −0.047 | 0.030 | 0.029 |
| | (0.125) | (0.113) | (0.114) | (0.122) | (0.136) | (0.135) | (0.089) | (0.120) | (0.125) |
| Open Lic. | −0.281 | −0.284 | −0.254 | −0.142 | −0.168 | −0.136 | 0.043 | 0.034 | 0.017 |
| | (0.139) | (0.125) | (0.162) | (0.244) | (0.335) | (0.269) | (0.109) | (0.141) | (0.143) |
| Closing | 0.000 | −0.124 | −0.127 | 0.105 | 0.095 | 0.091 | 0.234 | 0.207 | 0.209 |
| | (0.035) | (0.145) | (0.143) | (0.089) | (0.110) | (0.110) | (—) | (—) | (—) |
| $N$ (city × 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 |

*Notes:* Standard errors are in parentheses based on Cameron, Gelbach, and Miller's (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* (percent), *immigrants* (percent), *unemployment insurance recipients* (percent), and *higher educated* (percent).

TABLE A6—TIME-VARYING EFFECTS AND ROBUSTNESS CHECKS ON CITYWIDE-CRIME CATEGORIES (22 *cities*)

| *Dependent variable:* | Sexual abuse and rape | Sexual abuse | Rape | Drugs | Weapons | Assault |
|---|---|---|---|---|---|---|
| 1st–2nd year Open noLic. | −0.369 | −0.438 | −0.325 | 0.107 | 0.230 | 0.029 |
| | (0.127) | (0.199) | (0.164) | (0.345) | (0.180) | (0.129) |
| 3rd+ year Open noLic. | −0.064 | −0.269 | 0.029 | 0.078 | 0.036 | −0.031 |
| | (0.115) | (0.225) | (0.115) | (0.364) | (0.262) | (0.146) |
| Intro. Lic. | 0.186 | 0.306 | 0.144 | −0.462 | −0.066 | 0.129 |
| | (0.184) | (0.198) | (0.312) | (0.403) | (0.152) | (0.148) |
| 1st–2nd year Open Lic. | −0.543 | −0.944 | −0.238 | −0.359 | −0.355 | −0.062 |
| | (0.188) | (0.326) | (0.245) | (0.199) | (0.205) | (0.113) |
| 3rd+ year Open Lic. | −0.051 | −0.295 | 0.123 | −0.246 | −0.239 | 0.114 |
| | (0.060) | (0.159) | (0.132) | (0.112) | (0.376) | (0.132) |
| Spillover Lic. | 0.038 | 0.104 | 0.029 | −0.007 | −0.057 | 0.016 |
| | (0.093) | (0.152) | (0.186) | (0.058) | (0.095) | (0.023) |
| Spillover closing | −0.109 | −0.294 | −0.039 | −0.018 | 0.009 | 0.015 |
| | (0.258) | (0.362) | (0.205) | (0.109) | (0.057) | (0.032) |

(*continued*)

Table A6—Time-Varying Effects and Robustness Checks on Citywide-Crime Categories (22 *cities*) (*continued*)

| *Dependent variable:* | Sexual abuse and rape | Sexual abuse | Rape | Drugs | Weapons | Assault |
|---|---|---|---|---|---|---|
| Brothel ban lift | −0.072 | −0.050 | −0.082 | −0.175 | 0.104 | 0.153 |
| | (0.201) | (0.191) | (0.299) | (0.179) | (0.152) | (0.122) |
| 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 |
| | (0.106) | (0.120) | (0.093) | (0.168) | (0.108) | (0.093) |
| Spill. Intro. Lic. | 0.106 | 0.145 | 0.109 | 0.220 | −0.094 | 0.068 |
| | (0.064) | (0.142) | (0.086) | (0.083) | (0.115) | (0.061) |
| $N$ (city × 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 |

*Notes:* Standard errors are in parentheses based on Cameron, Gelbach, and Miller's (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* (percent), *immigrants* (percent), *unemployment insurance recipients* (percent), and *higher educated* (percent).

Table A7—Within-City Effects of Tippelzone on Perceived Crime (22 *cities with covariates*)

| | Drugs: Often | | Drugs: Often/sometimes | |
|---|---|---|---|---|
| | Non-tipp. area | Tipp. area | Non-tipp. area | Tipp. area |
| preopening | 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 × year) | 176 | | 176 | |
| $N$ (individuals) | 83,494 | | 83,494 | |
| $R^2$ | 0.095 | | 0.13 | |

(*continued*)

TABLE A7—WITHIN-CITY EFFECTS OF TIPPELZONE ON PERCEIVED CRIME (22 *cities with covariates*) (*continued*)

| | Violence: Often | | Violence: Often/sometimes | |
|---|---|---|---|---|
| | Non-tipp. area | Tipp. area | Non-tipp. area | Tipp. area |
| preopening | −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 × 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 | |

*Notes:* Clustered standard errors are in parentheses; based on data over the period 1993–2006 for violent crime and over 1997–2006 for drug crime. Fixed effects are at postcode level. The 22 cities exclude the large cities: Amsterdam, Rotterdam, and The Hague. Covariates are indicators for political party of mayor, log(*population male* 15–65), log(*pop. density*), *income* (percent), *immigrants* (percent), *unemployment insurance recipients* (percent), *higher educated* (percent), *gender*, *age*, *education*, and *Dutch nationality.*

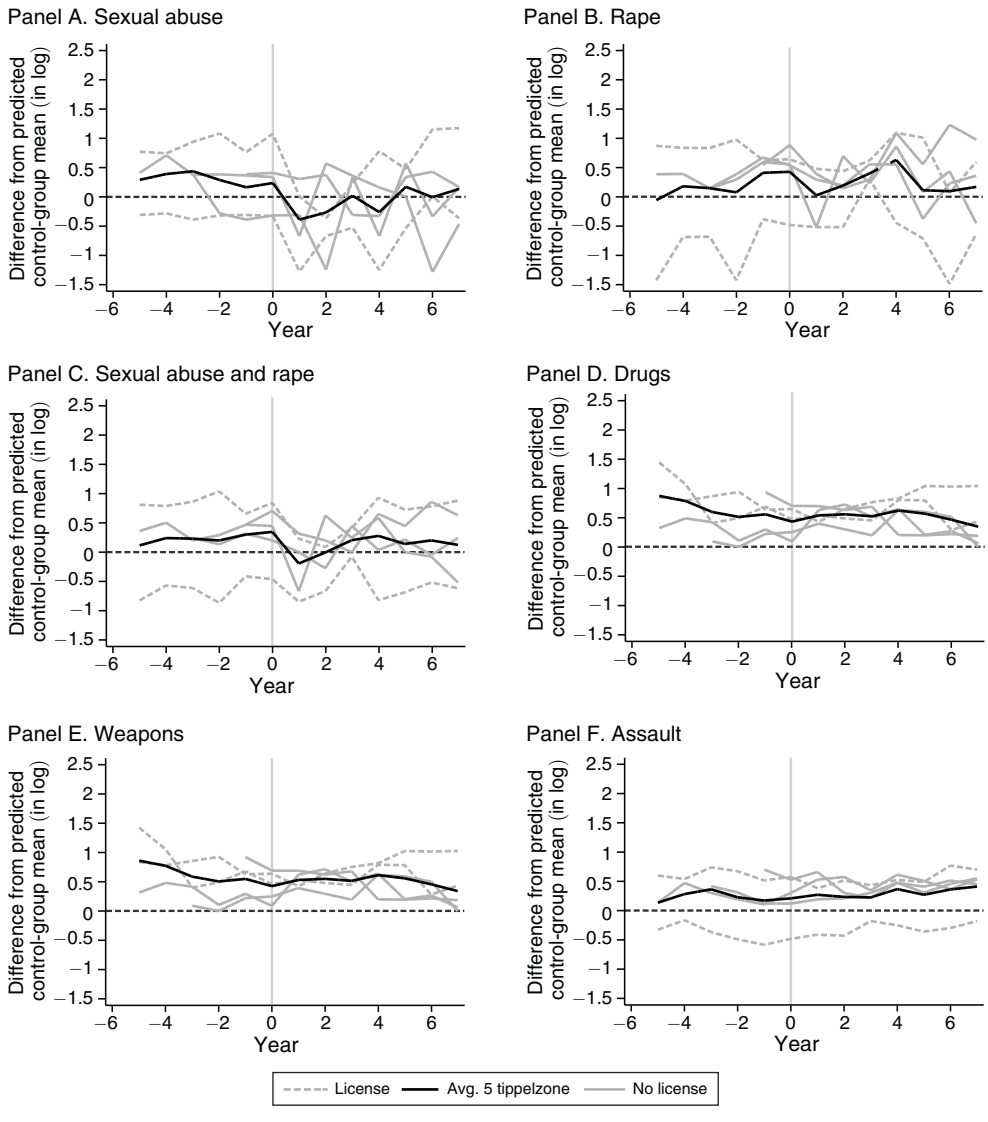


FIGURE A1. TRENDS IN REGISTERED-CRIME CATEGORIES

*Notes:* The no-tippelzone counterfactual for a city $i$ is generated by estimating model (2) on 21 cities leaving out city $i$ and the 3 largest cities and then averaging the fitted values fixing $d_{it}^{-} = 0$, $l_{it} = 0$, and $d_{it}^{+} = 0$. Also note that in contrast to the panel estimations, the plotted trends of tippelzone cities do not control for time-varying covariates.

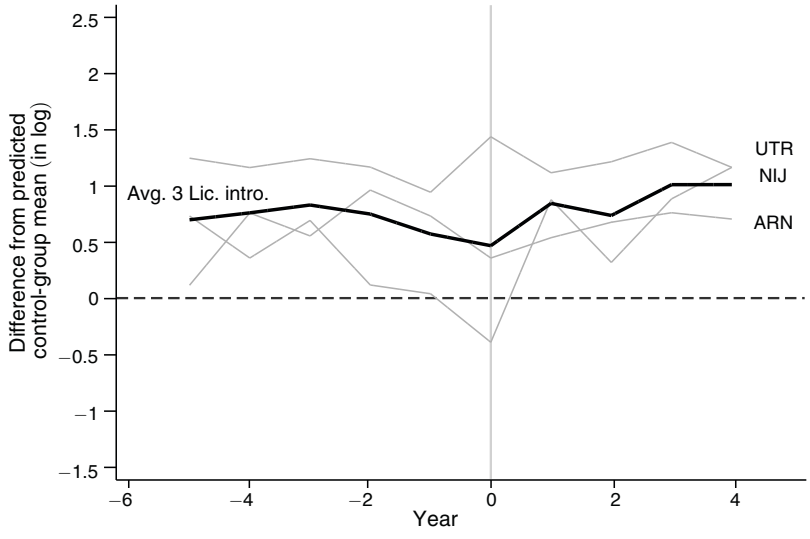


FIGURE A2. LICENSING INTRODUCTION

*Notes:* The no-tippelzone or licensing counterfactual for a city $i$ is generated by estimating model (2) on 21 cities leaving out city $i$ and the 3 largest cities and then averaging the fitted values fixing $d_{it}^{-} = 1, l_{it} = 0$, and $d_{it}^{+} = 0$.

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