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DRY LAWS AND HOMICIDES: EVIDENCE FROM THE SÃO PAULO METROPOLITAN AREA*

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We use a difference-in-differences design to estimate the causal impact of the adoption of dry laws in the São Paulo Metropolitan Area (SPMA) on violent behaviour. Dry laws cause a 10% reduction in homicides. Similar impacts were found on battery and deaths by car accidents.

The empirical literature shows that alcohol consumption causes all sorts of social maladies. In this article, we study the impact of social consumption of alcohol on murder, the utmost form of violence.

Specifically, we estimate the causal effect on homicide of restricting the recreational consumption of alcohol, which is mandatory night closing hours for bars and restaurants (dry laws, hereafter).

We evaluate the impact of dry laws on homicides by taking advantage of a unique empirical opportunity. Between March 2001 and August 2004, 16 out of 39 municipalities in the São Paulo Metropolitan Area (SPMA, hereafter) adopted dry laws. We estimate the reduced form effect of dry laws and find that they cause a 10% drop in homicides. Similar impacts are found on battery and deaths by car accident.

Our article relates to several pieces of literature. First, and rather generally, our results pertain to the literature on alcohol consumption and violence. Experimental studies in psychology suggest that alcohol suppresses inhibition, impairs judgment and induces aggressive behaviour (McClelland *et al.*, 1972). However, the literature with non-experimental data has had difficulty documenting a convincing link. Omission of common determinants such as child abuse and mental problems is one issue; see Currie and Terkin (2006) on child abuse and alcohol consumption. Non-random selection plagues studies that use arrest or victim data because sober offenders or victims are less likely to get caught or be victimised (Martin, 2001). Overall, the epidemiological literature has not settled the issue of causality (Lipsey *et al.*, 1997).

In this context of weak documentation of the causal effects of alcohol consumption, our work relates to a few recent articles that employ sharper identification strategies. Arguably, the most convincing work is Carpenter and Dobkin (2008). They exploit the exogenous variation provided by the 21-year-old legal drinking age in the US to show that alcohol consumption causes car accident deaths and youth suicide. The cost of their high internal validity is losing some external validity: the result concerns only

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youth drinking. In addition, they do not look at violent crime. Somewhat different from our results, Carpenter (2007) finds that youth drinking increases property crime but has no impact on violent crime.

The contrast between results in Carpenter (2007) and ours may be due to the fact that the SPMA dry laws only restrict the *recreational* consumption of alcohol. As expected, such restrictions caused a reduction in bar consumption only partially substituted by consumption at home. At bars, mental impairment and reduction of inhibition combine with altercations that sometimes grow into fights. Settling scores when intoxicated is perhaps the perfect recipe for disaster. Additionally, there is less reason to believe that the impact of *social* consumption of alcohol on property crime is stronger than alcohol consumption in general.

Previous empirical evidence on the link from *social* consumption to violence is unconvincing. Stockwell *et al.* (1993), in a survey of Western Australian adults, found that bars were the preferred venue of alcohol consumption prior to committing violent crimes. But bars could be the preferred venue in general. Roncek and Maier (1991) and Scribner *et al.* (1995) find similar results in other empirical settings; see Martin (2001) for a survey. In contrast, Gorman *et al.* (1998), using data on New Jersey cities, cannot link bar density to crime after controlling for demographics. These articles employ only cross-section variation and thus cannot convincingly control for common determinants of bar presence and violence. Directly related to our article is Duailibi *et al.* (2007), which uses only time-series variation from Diadema, one of the 16 adopting cities in our sample. Their results are in line with ours but they cannot infer causality because of the lack of cross-section variation in adoption. With a difference-in-differences design, we have a sharper identification strategy.

Even if a causal link from alcohol (not necessarily consumed socially) to violence was well established, policy implications are equivocal. The economics of crime literature paints an ambiguous picture of outright prohibition and taxation. On the one hand, Miron and Zwiebel (1991, 1995), for instance, argue that prohibition does not reduce alcohol consumption. Miron (1998) also argues that price-oriented interventions (e.g., taxation) are equally ineffective because the price-elasticity of the demand for alcohol is (presumably) quite inelastic. Perhaps reflecting the relative inefficacy of taxation, Markowitz (2005) finds puzzling results using victimisation data: higher beer taxes reduce assaults but have no impact on rape, a set of results hard to rationalise. However, the literature is not consensual as to the low price elasticity of alcohol demand. Although the Grossman *et al.* (1993) survey shows evidence that the long-run alcohol demand is somewhat elastic, they concede that the demand for alcohol is 'fairly inelastic in the short run' (pp. 220), possibly because alcohol is an addictive good (Becker and Murphy, 1988). In another survey, Chaloupka *et al.* (2002) argue that most of the literature confirms the price responsiveness of alcohol consumption. They do recognise, however, that the literature covered in their survey does not control for endogeneity, making it difficult to infer 'cause-and-effect relationships from the study findings' (pp. 23). Indeed, Chaloupka *et al.* (2002) quote a study from Dee (1999) showing that when state fixed effects are added to the model, 'beer excise tax no longer had a significant effect on consumption'.

In addition to ineffectiveness, making alcohol illegal altogether has perverse effects. One is violence induced by the impossibility of settling contracts through the formal judicial system (Miron and Zweibel, (1991, 1995). Another is a substitution effect: illegality levels alcohol with illicit psychotropic drugs and reduces the relative price of moving to 'stronger' drugs (Thornton, 1998). Colin *et al.* (2005) use county-level variation in alcohol consumption prohibition in Texas to show that access to alcohol reduces crime associated with illicit drugs. Nevertheless, the consequences of this 'substitution effect' for policy are unclear: should we facilitate the access to alcohol in order to fight drug use?

In light of the literature, targeted sales restrictions are interesting from a policy perspective. Because dry laws are less radical than prohibition, they are less likely to trigger substitution effects and contract-enforcement crime. Because they are focused at circumstances in which the effects of alcohol are magnified by social interaction, dry laws are relatively economical from a welfare perspective.

Figure 1 summarises the story of the article. Not surprisingly, adopting cities were more violent than non-adopting before adoption but homicides were dropping at about the same rate before adoption. Around the year 2002, when most cities adopted the dry law (see Table 1), homicides started to drop much faster in adopting cities. While in 2001 homicides in adopting were 15% higher than in non-adopting cities, rates were the same in 2004.

Is Figure 1 indisputable evidence that dry laws caused a reduction on homicides? The answer is no because adoption is a choice of cities. Adopting cities may have implemented other crime-fighting policies, which is all more likely because adoption occurred in violent cities. We control for a long list of 'other suspects', but it is always

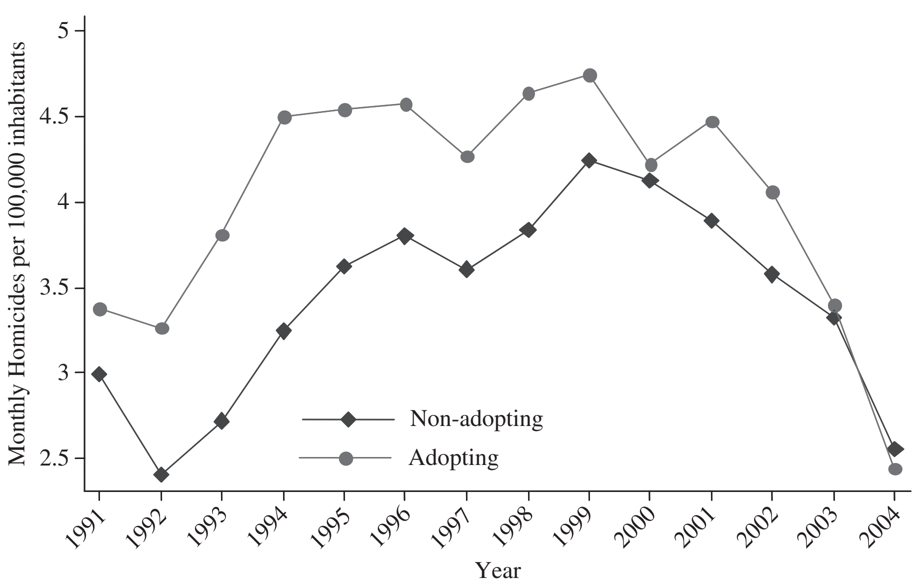

Fig. 1. *Evolution of Homocide Rates* (Adopting and non-adopting Cities over 1991–2004)
Source. Secretaria de Segurança do Estado de São Paulo and Municipal Laws.
Total number of homicide over the year at the city level was aggregated to the group level, adopting and non-adopting cities.

Table 1
Month of Dry Law Adoption

City	Date of Adoption	Closing Hours	Population in year 2004
Barueri	Mar-01	11pm–6am all week	250,385
Jandira	Aug-01	11pm–6am all week	105,024
Itapevi	Jan-02	11pm–6am all week	193,475
Diadema	Mar-02	11pm–6am all week	389,354
Juquitiba	May-02	11pm–6am weekdays, 2am–6am Fridays, Saturdays, Sundays and Holidays	28,353
São Lourenço da Serra	Jun-02	11pm–6am all week	14,915
Suzano	Jun-02	11pm–5am all week	267,769
Itapecerica	July 02	11pm–6am all week	149,977
Mauá	July 02	11pm–6am all week	396,717
Ferraz de Vasconcelos	Sep-02	11pm–6am all week	167,583
Embu	Dec-02	11pm–5am all week	239,144
Osasco	Dec-02	0am-5am all week	684,079
Embu – Guaçu	Apr-03	11pm–6am weekdays, 1am–6am Fridays, Saturdays, 0am-6am Sundays and Holidays	60,696
Vargem Grande Paulista	Dec-03	11pm–5am all week	40,083
São Caetano	July 04	11pm–6am weekdays, 0am–6am Fridays, Saturdays, Sundays and Holidays	142,692
Poá	Aug-04	11pm–4am all week	104,328

Sources. Municipal Laws and IBGE.

possible that the dry law is confounded with other *unobserved* policies. Furthermore, adopting and non-adopting cities may differ in time-varying dimensions. For example, homicides could be following different secular trends prior to adoption, although Figure 1 suggests otherwise. Finally, mean reversion could produce the results mechanically.

The article is organised as follows. Information on data sources is in Section 1. Section 2 describes the empirical setting and narrates the chronology of the events. Section 3 contains an extensive description of the empirical strategy designed to address the difficulties raised by the non-random adoption of dry laws. Results are presented in Section 4, which also contains an extensive robustness analysis, as well as validation and falsification tests. Section 5 concludes.

1. Data

Data come from several sources. Crime and enforcement data are from the Secretaria Estadual de Segurança Pública de São Paulo (*Secretaria* hereafter), the state-level enforcement authority. Crime data are at monthly frequency. Homicides and vehicle robbery data run from April 1999 to December 2004. Other crime categories are available from January 2001. Police, incarceration and arms apprehension data are only available with annual frequency and starting in 2001. Deaths by car accidents are from DATASUS, a hospital database from the Ministry of Health.

Also from *Secretaria*, we have report-level data from INFOCRIM, a compustat crime-tracking system. INFOCRIM started in 1999 in the São Paulo City. Implementation in other cities in the SPMA was gradual, as precincts were slowly incorporated in the system. Cities enter the sample as INFOCRIM was implemented at its precincts but not all precincts within a city enter at the same time. Thus levels are not comparable over time. Still, with INFOCRIM we can compute the distribution of crime during the day, which is useful for corroboration purposes.

Although crime data usually suffer from under-reporting, our two main dependent variables – homicide and vehicle robbery – are well measured. Under-reporting is negligible for homicides because an investigation is mandatory as long as a body is produced.[1] Vehicle robbery is well measured for three reasons: avoiding receiving traffic tickets; avoiding having one's name involved in criminal activities related to the subsequent use of a stolen car; and for insurance purposes.

A few remarks on under-reporting are needed because we use other crime categories such as battery as corroborative evidence. Most crime statistics suffer from serious under-reporting in Brazil, stemming from historical lack of confidence in authorities. Under-reporting *per se* does not invalidate the use of other categories, but extra caution must be exercised because reporting improved over the sample period. Institutional innovations in the state-level bureaucracy reduced the costs of reporting. Among them are:

(*i*) the creation of *Poupa-Tempo*, whose claque is 'time-saver', which are offices where all bureaucratic errands, including reporting crimes, may be done;
(*ii*) *Delegacia Eletrônica* (electronic police station) for on-line reporting; and
(*iii*) *Delegacias da Mulher*, police stations specialised in domestic violence.

Recorded crime rates hint that reporting improved over time. Figure 2 shows three categories: homicides, vehicle theft/robbery and common theft/robbery (all except vehicle). In 1999 vehicle and common theft/robbery rates were similar, an evidence of under-reporting. In the US, recorded common theft/robbery is three times higher than vehicle theft/robbery (Mueller, 2006). Overtime, homicides and vehicle theft/robbery follow a similar pattern of reduction, reflecting the general drop in crime in the SPMA (see Section 2). In contrast, common theft/robbery increased during the period, which is hard to rationalise except for improvements in reporting.

An additional problem is that reporting did not improve simultaneously across cities. *Poupa-Tempo* started in São Paulo City. *Delegacia Eletrônica* was available across the state, but internet penetration varied wildly both across cities and over time. For all these reasons, under-reported categories are used only as additional evidence and with caution.

Demographic data are from Instituto Brasileiro de Geografia e Estatística (IBGE), the Brazilian Bureau of Statistics. We have annual city-level income per capita, population and male population in ages 15 to 30 years, which are interpolated to obtain monthly frequencies. From Fundação SEADE, a state government think-tank, comes information on municipal-level policies such as the date of establishment of a municipal police force (if any), its size, spending on education and welfare, and the creation date of a municipal secretary of justice (if any). Information on the dry laws

[1] Homicides are attributed to a city if the crime was committed in that city (or if the dead body was found within the city limits and the investigation cannot determine where the crime was committed). Some 'miscoding' happens because the dead body could be moved. Except for very elaborate stories, this only introduces noise in the homicide data. Incidentally, reporting is mandatory in the case of deaths by car accident.

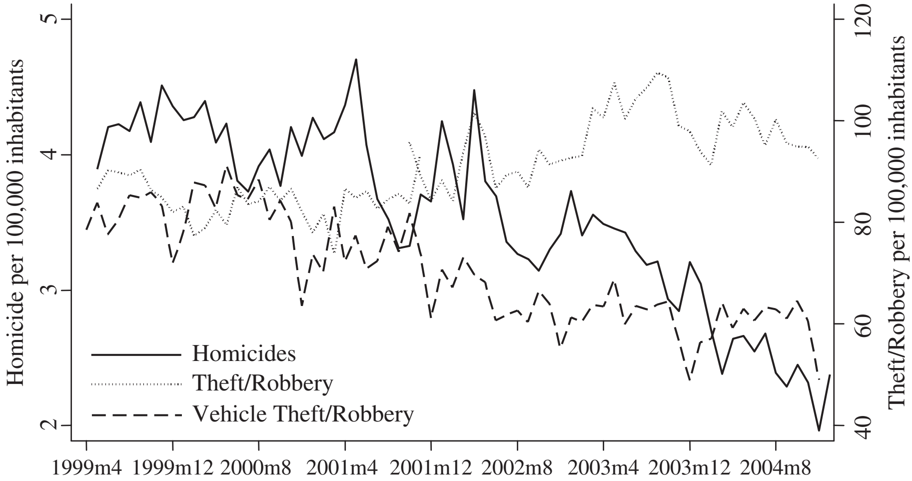

Fig. 2. *Evolution of Crime by Categories*

Source. Secretaria Estadual de Segurança do Estado de São Paulo.
Common theft/robbery includes all categories except vehicle. Both theft/robbery categories are plot on the right axis. Homicides are plotted on the left axis.

comes from the text of the law, which we collected on-line or requested from the city council by telephone.

Alcohol consumption data are from *Pesquisa de Orçamento Familiar* (POF, hereafter), a household income and consumption survey conducted by IBGE. POF was conducted twice: 1995/6 and 2002/3. Thirteen municipalities enacted the law between March 2001 and July 2003, and 89% of the adopting cities' population was in cities that adopted before January 2003 (see Table 1). Thus, most dry laws were effective when interviews were conducted. POF has consumption by type of outlet, i.e., bars and restaurants versus supermarkets and grocery stores, allowing us to measure not only the impact of the dry laws on *bar* consumption but also substitution effects from bar to supermarket purchases. One caveat is that the public file does not identify the municipality where the household is located, only whether the household is located at the São Paulo City or at any other municipality in the SPMA. Still, we can compare a group of cities that contains adopting cities to a group without adopting cities.

2. The Empirical Setting and the Chronology of Events

With roughly 19 million inhabitants in 2005, the SPMA is the largest contiguous urban area in South America and the third largest worldwide. Politically, it is defined as an administrative region in the state of São Paulo. It is composed of 39 independent municipalities, each with its own mayor and city council. City sizes vary widely, from Santa Isabel with a population of 11,000 to São Paulo City with its 11 million inhabitants in 2005.

Despite a recent reduction in crime, the SPMA is a violent place. In our 69-month sample, more than 45,000 people were murdered, which gives a monthly rate of 3.65 homicides per 100,000 inhabitants. For comparison, in New York City at its 1990 peak the rate was 3.56. Figure 1 shows homicides increasing steadily through the 1990s and reaching a peak in 1999. Since then they fell sharply, a reversion comparable to that of New York in the 1990s. Several factors contributed to this reversion. For example, De Mello and Schneider (2007) show the role demography: the proportion of youngsters rose in the 1990s and fell in the 2000s.

In reaction to the sharp increase in crime during the 1990s policy interventions took place at every level of government. The most famous are:

(*i*) the Lei do Desarmamento (LD) (December 2003), a strict federal legislation on firearms' possession; and
(*ii*) INFOCRIM, a compustat-like system that improved police intelligence at the state level.

It is likely that both contributed to the decline in homicide depicted in Figures 1 and 2; see Marinho de Sousa *et al.* (2007) on the impact of the LD. For our purposes, however, the relevant fact is that these policy interventions cannot be confounded with the dry laws because they were either too broad (LD) or too restricted (INFOCRIM).

Municipalities have jurisdiction over the regulation of local commerce. This allowed Barueri to pass in March 2001 legislation imposing mandatory closing hours for bars and restaurants, from 11pm to 6am all week long. The law allowed for exceptions under certain circumstances. In Barueri, less than 60 bars and restaurants out of roughly 4,000 were exempt.[2] Several cities followed suit and, as of December 2004, 16 out of 39 cities in the SPMA had adopted similar legislation. Table 1 has the adoption dates, the closing hours and the population in 2004 for all adopting cities. Figure 3 depicts the geographical distribution of adoption. Laws varied somewhat in strictness, with a few adopting cities having laxer rules during weekends. Still, 71.68% of the population in adopting cities were in municipalities where the curfew at 11pm was in place all week; only Osasco has a midnight curfew during weekdays. Adopting cities' population was 3.2 million in 2004, representing 17% of the SPMA (40% excluding São Paulo City). Prior to dry laws, no restrictions in opening hours were in place. Bars typically worked on 'last client served' basis and opened between 6am and 7am.

Anecdotal evidence suggests that the laws worked. One newspaper story is illustrative. The owner of a bar in Diadema, a particularly violent adopting city, reports that '...before [adoption] it was a little messy here. The law is good because it avoids fights.'[3]

In weak institutional settings such as Brazil, it is not obvious that dry laws were actually enforced, i.e., whether bar consumption of alcohol dropped following of adoption. For example, Romano *et al.* (2007) find that despite the minimum drinking (18 years old) adolescents find it easy to purchase alcohol. Anecdotal evidence again

[2] Conditions for exemption included not being located near schools, being outside 'crime zones' or zones without nuisance complaints. The presence of acoustic isolation and of private security in front of bar was also a necessary condition. See http://www.propagandasembebida.org.br, in Portuguese.

[3] This story is at Globo Online, the electronic version of O Globo, the second largest circulation newspaper in Brazil. In Portuguese at http://g1.globo.com/Noticias/SaoPaulo/0,,AA1359613-5605,00.html. *The Economist*, 20/10/2005, reporting a story on Diadema, lists dry laws as an important factor contributing for the decline in murder rates starting in 2001. In an interview to O Globo, Barueri's Municipal Secretary of Communication claims that homicides 'fell up to 70%' after the city implemented the dry law.

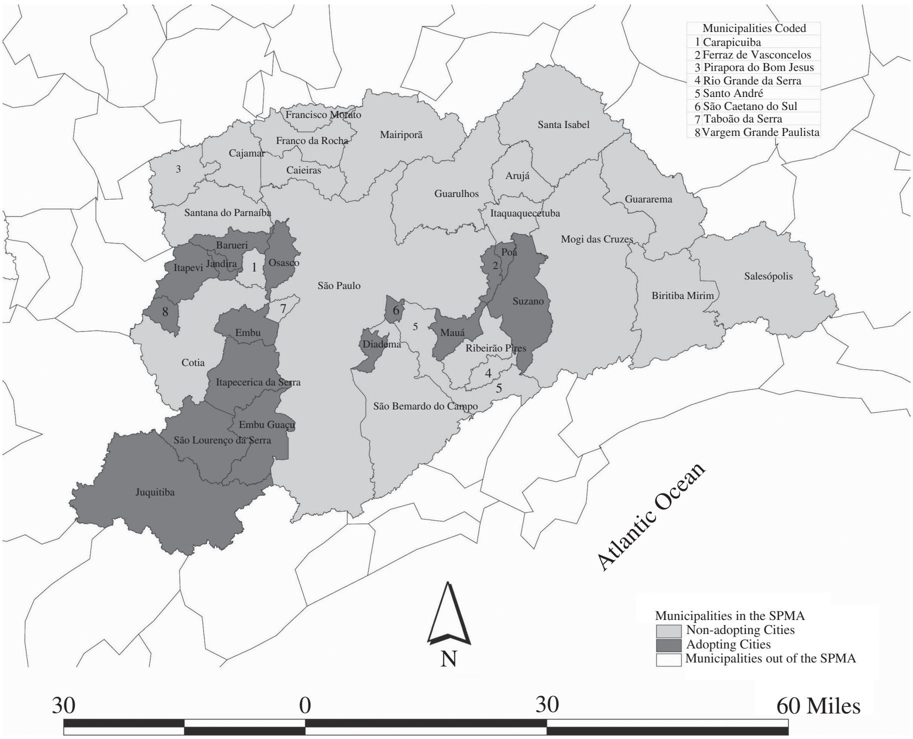

Fig. 3. *Geographical Distribution of Adoption*

suggests that the laws were effective. In the same newspaper story, the husband of the bar owner reports that '…sales have fallen after the law was passed'. We confirm this anecdotal evidence using household consumption data. We measure the impact of dry law on the consumption of two alcoholic beverages: beer and cachaça, which represent roughly 82% of total alcohol consumption in value (figures are from POF).[4] The model is:

$$Alcohol_{it} = \gamma_0 + \gamma_1 SPMA_i + \gamma_2 2003_t + \gamma_3 SPMA_i \times 2003_t + \Sigma CONTROLS_{it} + \varepsilon_{it} \qquad (1)$$

where $SPMA_i$ is 1 if household i lived in the SPMA excluding the city of São Paulo, 0 otherwise. 2003_t is 1 if the interview was done in the 2002–3 POF, 0 otherwise. Controls include gender, age, years of schooling and household income of the respondent. $Alcohol_{it}$ is the total household consumption in bars or in grocery stores, in reais (R$). We use sampling weights to make observations representative of the population. Estimated standard errors should be viewed with caution because we only have three cross-section units and thus we lack degrees of freedom to estimate the standard errors properly (Donald and Lang, 2007). With this caveat in mind, we proceed to interpret results in Table 2.

[4] Cachaça is the national liquor, distilled from fermented sugar cane. Its alcohol strength ranges from 38%/Vol. to 48%/Vol.

Table 2

The Mechanism and Substitution Effects

Dependent variable: total monthly consumption of alcohol by type (in R$)						
	All sample				Only 15–30 year-old males	
	Beer		Cachaça		Beer	Cachaça
	(1) In bars	(2) In stores	(3) In bars	(4) In stores	(5) In bars	(6) In bars
SPMA × 2003	−28.554	11.572	−2.176	0.238	−66.210	−2.324
	(6.382)***	(4.601)***	(1.073)**	(0.367)	(41.675)	(1.370)*
No. of Observations	5,294	5,294	4,810	4,810	721	638

Source. Pesquisa de Orçamento Familiar (POF). Robust standard errors in parentheses. Omitted regressors are: dummy for SPMA excluding São Paulo City, dummy for 2003, age in years, log of income, years of schooling and dummy for gender.
*** = significant at the 1% level.
** = significant at the 5%.
* = significant at the 10%.

Columns (1) and (3) show the impact of dry law adoption on alcohol consumption. Monthly household consumption of beer drops by R$28, which represents 70% of the average bar consumption. For cachaça, the drop is R$2.2, which represents 58% of the mean household bar consumption. Since male youngsters are the main perpetrators of homicides, we restrict the sample to households headed by males age 15–30. Results are similar (columns (5) and (6)). In columns (2) and (4) we measure possible substitution effects. For beer there is a substitution effect smaller than the direct effect: consumption in stores increases by R$11. For cachaça, no substitution effect arises, confirming the perception that cachaça is a bar drink. In summary, household data show that dry laws reduced bar consumption, with a small substitution for grocery purchases in the case of beer.

3. The Empirical Strategy

Our identification strategy is based on six pillars. First, with a difference-in-differences strategy we control for all time-invariant heterogeneity across cities, a necessary condition for causal inference. Several common determinants of crime and alcohol (ab)use – such as child abuse, poverty and psychological disturbances – are not observable and remain fairly constant over short periods of time. Second, the staggered nature of adoption provides additional identifying variation. Different adoption periods allow us to compare early adopting cities with late adopting cities, mitigating the problems posed by endogenous adoption. Third, dry laws should have different impacts on different types of crimes. Thus, other crime categories provide the basis for validation and falsification tests. Fourth, if dry laws have an impact on homicides, then the distribution of homicides during the day must have changed in response to the restriction in bar opening hours. Fifth, we evaluate the empirical determinants of the adoption of dry laws and show that adoption of dry laws is not explained by the adoption of other observable municipal and state level policies.

Finally, we conduct an extensive sensitivity analysis to probe the robustness of our results.

3.1. *Summary Statistics: Adopting and Non-adopting Cities*

Summary statistics on adopting and non-adopting cities are in Table 3. Observations are weighted by city population. Non-adopting cities resemble adopting in demographics, a desirable feature of a control group. They have similar percentages of male population between 15 and 30 years old, income per capita and school attainment measured both by the number of years of schooling and by the high-school drop-out rate. Non-adopting cities seem larger than adopting ones but the difference is due to São Paulo City, which represents roughly 60% of the

Table 3

Summary Statistics, Adopting and Non-adopting Cities

	Adopting (16 cities)		non-Adopting (23 cities)		non-Adopting excl. São Paulo	
	6-month period pre-adoption	6-month period post-adoption	6-month period pre-adoption	6-month period post-adoption	6-month period pre-adoption	6-month period post-adoption
Monthly Crime Rate per 100,000 inhabitants						
Homicide	4.83	2.24	4.29	2.40	3.89	2.23
	(3.00)	(1.11)	(0.94)	(0.56)	(1.66)	(0.97)
Vehicle Robbery	31.78	18.85	44.51	30.80	42.00	25.43
	(16.96)	(12.11)	(17.13)	(12.95)	(31.43)	(22.81)
Battery	26.82	26.69	24.07	30.19	28.43	32.29
	(7.03)	(11.14)	(6.40)	(7.22)	(10.42)	(12.61)
Deaths by Car Accident	0.72	0.48	0.56	0.60	0.42	0.41
	(0.79)	(0.52)	(0.33)	(0.35)	(0.58)	(0.59)
Cargo Robbery	1.00	1.40	1.37	2.49	1.38	1.61
	(0.94)	(1.32)	(0.74)	(0.98)	(1.31)	(1.31)
Bank Robbery	0.01	0.05	0.07	0.14	0.03	0.05
	(0.04)	(0.23)	(0.15)	(0.12)	(0.26)	(0.16)
Demographics						
Population	176	201	639	683	199	227
(in thousands)	(156)	(167)	(208)	(216)	(260)	(292)
%Male Population,	14.63	14.15	13.99	13.14	14.35	14.05
age 15–30	(0.67)	(0.92)	(0.41)	(0.72)	(0.62)	(0.76)
Educational Attainment (in year 2000)						
High-school drop-out	11.01		10.08		9.89	
rate (in %)	(2.87)		(1.23)		(2.23)	
Average number of	7.19		8.10		7.47	
years of schooling	(0.75)		(0.60)		(0.77)	
(age 15–64)						
Income in 2004 reais						
Income per capita	10,045	13,165	10,233	13,023	8,811	11,484
	(6,425)	(6,990)	(2,242)	(9,317)	(3,778)	(5,523)

Source. Secretaria de Segurança do Estado de São Paulo, Fundação SEADE and Municipal Laws. Except for population, all means are computed using population as a weight. Standard deviations in parentheses. Pre-adoption period is July 1999/December 1999; post-adoption period is July 2004/December 2004. The observation from Poá in July 2004 was excluded from the post-adoption in adopting cities.

population of the SPMA. Excluding São Paulo, average population is similar across groups.

Average characteristics may disguise time-series heterogeneity. For a clean, seasonality-free pre- and post-treatment periods comparison, we use the six-month periods July 1999/December 1999 and July-2004/December 2004 for homicides, vehicle robbery, deaths by car accidents and the demographics.[5] For the other crime categories we compare six-month periods July 2001/December 2001 and July 2004/December 2004, and drop Barueri and Jandira, who adopted in 2001.

Start with the demographics. Nominal per capita income rose by 31% and 27% in adopting and non-adopting cities, respectively. The proportion of population in the crime-prime age (male in the 15–30 age bracket) dropped by the same magnitude in both groups. Population growth is also similar. Excluding São Paulo City from the non-adopting group does not change any conclusion.

Homicides evolved differently in the adopting and non-adopting cities. In the post-adoption period, the average six-month rate was 2.24 in adopting cities. This is 54% lower than the 4.83 rate in July 1999 to December 1999. In non-adopting cities the reduction was less pronounced: 44%. Reported battery rates increased in the SPMA area as a whole. In adopting cities, however, they fell slightly, suggesting that dry laws also had an impact on assault. Finally, while deaths by car accident dropped markedly in adopting cities, they stayed flat in non-adopting ones. Results are not sensitive to the presence of the São Paulo City in the non-adopting group. In line with Figure 1, pre- and post-treatment average comparison suggest that dry laws reduced the violent crime and deaths by car accident.

In contrast, no marked pre-post difference arises for the bank, cargo and vehicle robbery. We argue below that one should not expect these categories to be affected by the dry law. In fact we will use them as falsification tests.

Before proceeding to confirm the suggestion of the difference in means, we do an in depth investigation of the determinants of the decision to adopt the dry law.

3.2. *Investigating the Decision to Adopt the Law*

Endogenous adoption of dry laws poses two threats to causal inference. First, if adoption occurred in reaction to surges in homicides, then it is likely that other unobserved policies were adopted concurrently. Second, if observed policies explain dry law adoption, then it is likely that all policies – observed and unobserved – were adopted in bundle. We estimate a duration model for the probability of transiting from non-adoption to adoption and evaluate the empirical relevance of the two threats (Jenkins, 1995). The following factors are included in the duration analysis:

- *Municipal and state-level policy variables.* Policies are divided into two sets:
  (*a*) municipal enforcement policies, such as the presence of a municipal secretary of justice, of a municipal police force, their adoption time if they were established during the sample period, and the size (in personnel) of the

[5] We drop the observation from Poá in July 2004 when computing the post-adoption means for adopting cities because Poá adopted in August 2004.

municipal police force and policy choices that are arguably related to crime prevention, such as the municipal expenditures on welfare (social assistance), education and cultural activities;

(*b*) state-level enforcement variables (at the city level): number of police officers per capita, arrests per capita and firearms apprehended per capita. By constitutional mandate, enforcement is mostly done at the state-level in Brazil.

- *Recent dynamics of homicide.* This allows us to test the hypothesis that dry law adoption was related to recent shocks to homicides. We also include the average homicides in 2000 as a baseline measure of homicides to evaluate if overall violence affects the decision to adopt.
- *Demographic controls.* Income, population and male population between 15 and 30 are included because they may affect homicides and the decision to adopt dry laws (a younger constituency may oppose the adoption). In some specifications a polynomial of time is included to account for time varying hazard rates. Adoption occurs over time and homicides are declining in the sample period.
- *Number of adopting neighbours.* Figure 3 shows that adoption is clustered geographically, suggesting that emulation or fear of spillover effects may be important drivers of adoption.

Table 4 has the results. The first column has the results of a stripped-down model. Neither the dynamics of homicide nor competing municipal or state-level policies are included.[6] In line with descriptive statistics, demographics are unrelated to the adoption of dry laws. Time explains adoption, but only weakly (the p-value on log(*time*) is 22.2%). Base line homicides in 2000 increase the hazard rate of adoption, i.e., more violent cities were more prone to adopt earlier. Finally, the number of adopting neighbours explains adoption. Taken together, these variables explain less than 9% of *variation* in the timing of adoption. In column (2) we include the municipal and state-level policies, the competing explanations. Only the size of the state police force has an impact on adoption. However, it has the wrong sign: an increase in the number of state police officers in the city *retards* adoption. In column (3) the lags of homicide are included. They are neither individually nor jointly significant. Relative to column (2), the dynamics of homicides explain only one additional percentage point of the variation in adoption. Thus, dry law adoption did not occur as a reaction to a recent increase in homicides. In column (4) we exclude all policy variables. They explain no more than 6% of the variation in adoption above and beyond the variables included in column (3). Lastly, the model in column (5) excludes the base line homicides. The dynamics of homicide are still unrelated to adoption.

We interpret these results as follows. Violent cities adopted dry laws as a measure to fight crime and neighbours followed suit, perhaps because of anecdotal evidence that dry laws worked or for fear of spillovers. Thus, the two threats posed by endogenous adoption are not relevant empirically.

[6] The sample is restricted to the period January 2001 to December 2004 to include the state-level enforcement variables. Although we lose observations between January 1999 and December 2000, no adoption occurred during this period. Thus, for the duration model it does not make much difference if we include 1999 and 2000 since adoption occurred in this period.

Table 4

Log Normal Duration Regression of Adoption of Dry Law

	(1)	(2)	(3) Marginal Effects	(4)	(5)
Dynamics of Homicides					
Homicides t − 1			0.035	0.045	0.086
			(0.061)	(0.083)	(0.079)
Homicides t − 2			−0.083	−0.090	−0.067
			(0.079)	(0.103)	(0.099)
Homicides t − 3			−0.040	−0.036	−0.005
			(0.069)	(0.092)	(0.088)
Homicides t − 4			0.002	0.008	0.051
			(0.063)	(0.084)	(0.080)
Competing Municipal Policies					
Dummy for the Presence of a Municipal Police Force		0.517	0.512		0.829
		(0.401)	(0.384)		(0.488)*
Dummy for the Presence of a Municipal Secretary of Justice		−0.107	−0.068		−0.205
		(0.325)	(0.322)		(0.480)
Log(*Size of Police Force per capita*)		0.041	0.031		0.062
		(0.075)	(0.069)		(0.091)
Log(*Education Spending per capita*)		−0.215	−0.200		−0.283
		(0.303)	(0.283)		(0.036)
Log(*Welfare Spending per capita*)		0.342	0.335		0.411
		(0.002)	(0.221)		(0.259)*
Competing State Policies					
Log(*Prison per capita*)		0.284	0.286		−0.003
		(0.403)	(0.376)		(0.488)
Log(*Number of Policemen per capita*)		−0.448	−0.440		−0.515
		(0.206)**	(0.199)***		(0.235)**
Log(*Guns Aprehended per capita*)		−0.147	−0.109		0.128
		(0.337)	(0.316)		(0.462)
Demographic controls					
Log(*City Level GDP per capita*)	0.510	0.324	0.257	0.478	0.162
	(0.346)	(0.336)	(0.320)	(0.339)	(0.401)
Log(*Population*)	1.772	2.612	2.124	1.311	0.330
	(3.964)	(3.196)	(2.997)	(3.954)	(3.779)
Log(*Male Population, 15 and 30 years*)	−2.145	−2.972	−2.416	−1.629	−0.545
	(4.040)	(3.266)	(3.068)	(4.041)	(3.843)
Time Trends					
Time	−15.958	−10.265	−8.832	−15.271	−14.959
	(12.808)	(9.933)	(9.300)	(12.589)	(0.118)
$(Time)^2$	0.027	0.017	0.015	0.026	0.025
	(0.022)	(0.017)	(0.016)	(0.022)	(0.021)
Log(*Time*)	1185.235	766.387	661.159	1134.459	1110.041
	(919.776)	(714.433)	(669.410)	(904.265)	(852.040)
Number of Adopting Neighbours	0.196	0.173	0.161	0.184	0.128
	(0.117)*	(0.126)	(0.971)*	(0.113)*	(0.150)
Time Invariant Controls					
Base Line Homicides	0.389	0.300	0.311	0.406	
	(0.140)***	(0.124)***	(0.135)***	(0.158)***	
Pseudo-R^2	0.088	0.148	0.159	0.095	0.112

Source. Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE and Municipal Laws. Sample period is January 2001 to December 2004; all five specifications have 1,469 observations. Standard errors in parentheses. ***significant at the 1% level, **significant at the 5% level, *significant at the 10% level. All variables divided by 100.

3.3. *The Empirical Model*

We estimate several versions of the following model:

$$\begin{aligned} Homicide_{it} = \beta_0 + \beta_1 AdoptLaw_{it} + \sum_{t=1}^{T} \omega_t Month_t \\ + \sum_{i=1}^{I} \eta_i City_i + \Phi CONTROLS_{it} + \varepsilon_{it} \end{aligned} \quad (2)$$

where i is a city in the SPMA, and t is a month. $AdoptLaw_{it}$ is a dummy variable that assumes the value 1 if the dry law was in place in city i at period t, and 0 otherwise. Hence, for non-adopting cities, it assumes only the value 0. We test whether the parameter β_1 is negative, i.e., whether dry laws reduced homicides. $Month_t$ is a full set of period dummies. Their inclusion is important because homicides were falling in the SPMA as a whole. If period specific effects are not accounted for, $AdoptLaw_{it}$ will capture aggregate shocks because it assumes more values of 1 at the end of the sample period. $City_i$ is a full set of city dummies to control for city fixed-effects.

Although model (2) discards all pure cross-sectional and time-series variation, objections to causal interpretation still arise. First, the procedure does not account for all time-*varying* heterogeneity, which is true in any policy evaluation but poses a more serious threat when policy adoption is a choice.

$Controls_{it}$ are the most direct way to account for time-varying heterogeneity. They include income, population and the percentage of population between 15 and 30 years, a problematic age bracket. These demographic variables affect homicide and are observed at the annual frequency.

Figure 1 suggests that results are not driven by different secular trends in homicides. Nevertheless, we play it safe and we implement two procedures to account for this possibility. In most specifications $Controls_{it}$ includes several lags of the homicide as explanatory variables. We have no specific theoretical reason to believe that past homicides cause present homicides, after time and city dummies are included. However, a rich dynamic model serves the dual purpose of controlling for different secular trends and proxying for possible unobserved policy reactions. Alternatively, we estimate a 'city-specific trends' model in which each city has its own linear trend $\theta_i t$.

Finally, $Controls_{it}$ also includes a long list of policies that may compete with dry laws. They are the same in the duration model:

(*i*) municipal spending in education and welfare, the presence of a municipal secretary of justice, the presence of a municipal police force and its size (if any);
(*ii*) state-level enforcement variables, which are the size of the police force in the city, the number of arrests and the number of guns apprehended.

The state-level enforcement variables are particularly important because the state is the main law enforcer by constitutional mandate and the empirical literature has established the link from enforcement to crime (Marvell and Moody, 1996; Corman and Mocan, 2000; Di Tella and Schargrodsky, 2004; Levitt, 2002).

We weight observations by population, which serves two purposes. First, it emulates a regression at the individual level, i.e., weighting observations provides estimates closer to a random sample in the SPMA. Second, homicides are not a common occurrence and observations from small cities are much noisier than those from larger cities (the variance of ε_{it} decreases with population). Thus variation from smaller cities should be discounted. In order to avoid giving more weight to observations in the later part of the sample, the weight is the city population in 2000. Finally, observations are clustered at the city level. Thus, all estimated standard errors are robust to within city correlation, an important feature in light of results in Bertrand *et al.* (2004).

4. Results

4.1. *Main Estimates*

Table 5 shows estimates of several versions of model (2). For conciseness, only $\hat{\beta}_1$ is reported. All models include a full set of city and period dummies. Start in panel (*a*). Column (1) shows the estimates of a stripped-down model, with no controls besides period and city dummies. The estimated coefficient on the variable *AdoptLaw* ($\hat{\beta}_1$) is −0.616, and it is reasonably well estimated (p-value = 5.73%). Considering the

Table 5

Main Estimates

Dependent Variable: Homicides per 100,000 inhabitants				
	Full Sample		January 01 to December 04	
	(*a*) adopting and non-adopting cities			
	(1)	(2)	(3)	(4)
AdoptLaw	−0.616	−0.490	−0.605	−0.613
	(0.342)*	(0.210)**	(0.252)**	(0.245)**
Covariates?†	No	Yes	Yes	Yes
4 Lags of Homicide?	No	Yes	Yes	Yes
Enforcement Variables?‡	No	No	No	Yes
no of Observations	2,535	2,535	1,872	1,872
	(*b*) Only adopting cities			
AdoptLaw	−0.877	−0.668	−0.649	−0.654
	(0.309)***	(0.291)**	(0.362)*	(0.381)*
Covariates?†	No	Yes	Yes	Yes
4 Lags of Homicide?	No	Yes	Yes	Yes
Enforcement Variables?‡	No	No	No	Yes
no of Observations	1,040	1,040	768	768

Source. Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE and Municipal Laws.
*** = significant at the 1% level, ** = significant at the 5%, * = significant at the 10%. In all specifications, observations are weighted according to population. Standard errors in parentheses are clustered at the city level. Period of Analysis is May 1999 to December 2004, unless otherwise noted. All specifications contain a full set of period (month) and city dummies.

†Covariates include: logs of population, of income per capita, of the number of 15–30 year-old males, the number of neighbouring cities that adopted the law, a dummy for the presence of a municipal secretary of justice, a dummy for the presence of a municipal police force and log of its size, the log of the municipal per capita spending on education, and the log of the municipal per capita spending on welfare programmes.

‡Yearly data on the number of guns apprehended per capita, the number of prisons per capita and the number of police officers per capita.

homicide rate in adopting cities in the period July 1999 to December 1999 (4.83 in Table 3),$\hat{\beta}_1 = -0.616$ means a 13% drop in homicides per 100,000 inhabitants, a significant reduction. In terms of lives, had the law been adopted in the city of São Paulo (10 million inhabitants), 740 lives would have been saved annually ($0.616 \times 100 \times 12$).

Results in column (2) show that the estimated impact of dry law adoption is robust to the inclusion of controls. Although the estimated coefficient is a little smaller in magnitude (−0.490), it is still quite significant practically, and more precisely estimated (p – value = 2.6%).

In column (3) we restrict the sample to January 2001 to December 2004, the period for which we have data on the enforcement variables. Results are stronger than in column (2). In column (4) the enforcement variables are included. Results are, if anything, slightly stronger. Since including enforcement variables restrict the sample but does not change results significantly, our benchmark estimate is −0.490 (column (4)), the point estimate from the most complete model whose sample is full (May 1999 to December 2004).

In panel (*b*) we restrict the sample to adopting cities. Since adoption did not occur simultaneously across cities, we may use the staggered nature of adoption as the source of identifying variation. The control group is now adopting cities before adoption. Restricting the attention to adopting cities involves a variance-bias trade-off. On the one hand, excluding non-adopting cities discards relevant variation and increases variance. On the other hand, restricting the sample to adopters reduces potential bias for two reasons. First, late adopters have a very high 'propensity' to adopt, given that they eventually adopted. Thus, concentrating on them helps to 'homogenise' the control and treatment groups. Second, it reduces the risk of capturing potential unobserved policies. It may be that late adopters adopted unobserved policies later and the effects would still be confounded. However, the 'unobserved policies bias' story now needs a very fine tuning of timing to work. Incidentally, when attention is restricted to adopting cities, São Paulo City is excluded. This is important for robustness purposes because observations are weighted by population and 60% of the population of the SPMA live in the São Paulo City.

Within a column, estimates should be compared across panels. Comparing the stripped-down models results are, if anything, stronger (−0.877 versus −0.616 in column (1)). In terms of the benchmark model results are again stronger (−0.668 versus −0.490). In column (3) we include the state-level enforcement variables. Results are again unchanged.

Table 6 has a long list of robustness checks. Column (1) has the benchmark estimate for comparison (Table 5, Panel (*a*), column (2)). In column (2) we estimate the model by OLS without weights to check whether the weighting procedure is driving results. The point estimate is similar but the estimated standard errors are larger under OLS, confirming the efficiency of the weighting scheme.

Column (3) deals with the econometric challenges posed by including the lags of the dependent variable as regressors. The fixed-effect transformation does not work if N is large and T small, unless the error term is strictly exogenous, which rules out unobserved serial correlation. Since in our case N is small and T is large, OLS has small bias but Monte Carlo experiments suggest that both large N and very large T are necessary.

Table 6

Robustness Checks

Dependent Variable:	Homicides per 100,000 inhabitants					Log of Homicides
	WLS (1)	OLS (2)	Arellano-Bond (3)	WLS (4)	WLS (5)	WLS (6)
AdoptLaw	−0.490 (0.210)**	−0.406 (0.245)*	−0.536 (0.206)***	−0.583 (0.291)**	−0.433 (0.244)*	−0.152 (0.059)***
4 Lags of homicide?	Yes	Yes	Yes	No	Yes	Yes
City-specific Trends?§	No	No	No	Yes	Yes	No
no of Observations	2,535	2,535	2,496	2,535	2,535	1,573

Source. Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE and Municipal Laws.
*** = significant at the 1% level, ** = significant at the 5%, * = significant at the 10%.
WLS = Observations weighted by population as in Table 5. OLS = Observations un-weighted. Standard Errors in parentheses are clustered at the city level Period of Analysis is May 1999 to December 2004. Arellano-Bond GMM procedure, four lags included (p), dependent variable and regressors are first-differences, one-stage standard deviations, $Ti - p - 2$ lags of the dependent variable used as instruments. No weights included. All specifications include the set of covariates as in Table 5. All specifications contain a full set of period (month) and city dummies.
§: One linear trend ($\theta_i t$) for each city i in the sample (city dummies interacted with time).

Despite complications in identifying models with fixed-effects and lagged dependent variables, we implement a GMM procedure that instruments for the lags of homicide with further lags of homicide (Arellano and Bond, 1991).[7] Results are stronger than the benchmark. Thus, any bias caused by inclusion of lags of the dependent variable is towards zero, if anything.

Adopting cities were more violent than in non-adopting cities around the period of adoption. Thus mean reversion may be driving results. In columns (4) and (5) we allow each city to have its own linear trend θ_{it}. Results are again similar, both with and without dynamics. Finally, results are similar when the model is estimated in logs (column (6)): dry laws cause a 15% reduction in homicides.

Figure 4 presents the coefficients of a different specification. Treatment is coded as a set of dummies for the number of months to the introduction of the law. A total of 36 dummy coefficients are estimated, 18 for the months before and 18 for after the law. The sample is restricted to 18 months before and after adoption. Two patterns arise. Before adoption, the estimated dummies are all zero, except for the 12th month before adoption, a *positive* outlier. At the month of adoption, we estimate a big negative coefficient on the dummy. For subsequent months, estimated dummy coefficients fluctuate around −1, in line with the hypothesis that dry law had a causal impact on homicide.

[7] A wide range of possible specifications for the Arellano-Bond estimator is available. For conciseness and because this is only one of the many robustness checks, we do not dwell into the several implications of different estimation methods. We implement the standard version on the STATA package. All variables are first-differenced, the one-step estimator for the standard deviation is used and $T_i - p - 2$ lags are used as instruments for the p included lagged dependent variable. Only one slight modification: four lags (the p) of the dependent variable are included (instead of two).

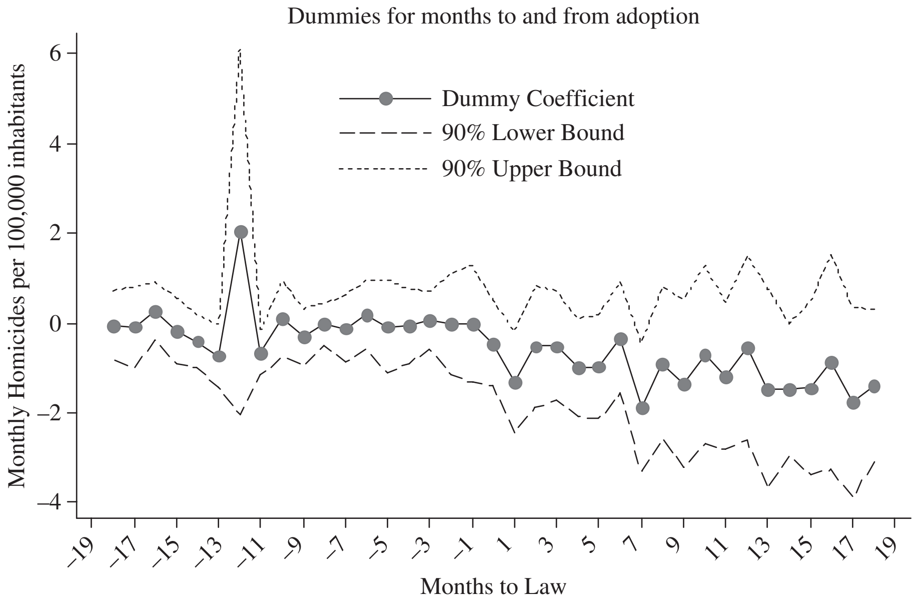

Fig. 4. *Impact of Dry Laws: Dummies for Months To and From Adoption*
Source. Secretaria de Segurança do Estado de São Paulo, Fundação SEADE and Municipal Laws. Homicides are regressed on covariates (listed in Table 5), four lags of homicides, city-specific trends and a treatment variable. Treatment is coded as a set of 37 dummies for 18 months before the law, the month of adoption and 18 months subsequent to the adoption of the law. The figure shows the dummy coefficient estimates. Only 18 months before and after adoption included in sample for this regression. Only adopting cities included in this regression.

4.2. *Distribution of Crime over the Day*

Report-level data from INFOCRIM provide additional evidence that dry laws worked. Cities enter the sample as INFOCRIM was implemented at its precincts but not all precincts within a city enter at the same time. Thus levels are not comparable over time. For this reason, we use report-level data to compare the distribution of crime throughout the day in adopting and non-adopting cities before and after adoption. We have INFOCRIM data for 10 adopting cities (Barueri, Diadema, Embu, Embu-Guaçu, Ferraz de Vasconcelos, Itapecerica, Jandira, Mauá, Osasco e Suzano). São Paulo City is the control group.

The estimation strategy is as follows. An observation is a homicide (indexed by j). Let i be a city, and d be a day. The dependent variable is multinomial:

$$H_{jid} = \begin{cases} 0, & \text{if the homicide } j \text{ was commited between 11:00pm and 6:59am} \\ 1, & \text{if the homicide } j \text{ was commited between 7:00am and 12:59pm} \\ 2, & \text{if the homicide } j \text{ was commited between 1:00pm and 6:59pm} \\ 3, & \text{if the homicide } j \text{ was commited between 7:00pm and 10:59pm} \end{cases}$$

AL_{jid} is 1 if city i had a dry law in effect in day d. We run a multinomial logit regression of H_{jid} on AL_{jid} with baseline category being 3. We then compute the predicted probabilities for $AL = 1$ and 0. Typically, curfews are from 11:00pm to 6:00am. Thus,

Table 7

Impact of Dry Law Adoption on the Distribution of Crime over the Day

Dependent Variable: Hour of the Day = 0, 1, 2 and 3. Baseline category = 3	
	Multinomial Logit Effect on the Predicted Probabilities
	(*a*) São Paulo included
0 (between 11:00pm and 6:59am)	−0.075
	(0.045)*
1 (between 7:00am and12:59pm)	0.016
	(0.036)
2 (between 13:00pm and 6:59pm)	0.007
	(0.039)
3 (between 7:00pm and 10:59pm)	0.052
	(0.048)
Observations	23,885
	(*b*) São Paulo excluded
0 (between 11:00pm and 6:59am)	−0.118
	(0.073)*
1 (between 7:00am and12:59pm)	−0.043
	(0.066)
2 (between 13:00pm and 6:59pm)	0.019
	(0.067)
3 (between 7:00pm and 10:59pm)	0.142
	(0.761)*
Observations	145

Source. INFOCRIM and Municipal Laws.
Coefficients represent the difference in predicted probabilities with and without the presence of the dry law that a homicide occurred in a given hour of the day.
*** = significant at the 1% level, ** = significant at the 5%, * = significant at the 10%. Standard errors in parentheses. An observation is a homicide. Sample is composed of observations from Barueri, Diadema, Embu, Embu-Guaçu, Ferraz de Vasconcelos, Itapecerica, Jandira, Mauá, Osasco e Suzano and the city of São Paulo. $AL = AdoptLaw$. Baseline category is $H = 3$ (hours between 7pm and 10:59pm)

we expect that the proportion of homicides committed in the late night-early morning period to fall following adoption. We also expect the proportion of homicides in the evening (7pm to 22:59pm) to increase because these are now the busiest bar hours.

Results are in Table 7. Panel (*a*) shows that the presence of the dry law reduces by 7.5% the probability that the homicide was committed between 11pm and 6:59am. This impact is significant at the 10% level. The proportion of homicides in the evening increases (5.2%) but the impact is not precisely estimated. In panel (*b*) São Paulo City is excluded. It is not surprising that the number of observations is dramatically reduced. Nonetheless, results are stronger, if anything. Now both expected effects arise: the share of late night-early morning homicides drops *and* evening share increases.

4.3. *Spillover Effects*

Adoption in a city may shift bar drinking to its non-adopting neighbours. Thus, the control group could be affected by the treatment, introducing additional challenges for causal inference. Table 8 shows several specifications that measure the spillover effect and assess its consequences. Columns (1) to (3) present direct evidence on spillovers. The sample is restricted to non-adopting cities and adopting cities before

Table 8
Spillover Effects

Dependent Variable: Homicides per 100,000 inhabitants

	non-adopting and adopting before			Whole Sample	Population >100,000	Population >200,000	Largest Areas
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
AdoptLaw				−0.735 (0.258)***	−0.573 (0.231)**	−0.812 (0.298)**	−0.432 (0.189)**
Interaction				0.238 (0.204)			
Number of Adopting Neighbours	−0.028 (0.022)			−0.028 (0.048)			
% Adopting Neighbours		0.004 (0.003)					
% Adopting Neighbouring Population			0.001 (0.002)				
no of Observations	1,495	1,495	1,495	2,535	1,008	528	1,536

Source. Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE and Municipal Laws.
*** = significant at the 1% level, ** = significant at the 5%, * = significant at the 10%. Weighted Least Squares procedure with population as weights. In columns (1) through (3) the period of Analysis is May 1999 to December 2004. In columns (4) to (7) they it is May 1999 to December 2004 Observations are clustered at the city level. City and period (month) dummies, four lags of homicides and covariates as defined in Table 5 included in all specifications.

adoption, the 'control group'. The main variable of interest is the intensity of neighbour adoption, which is measured as:

(*i*) number of adopting neighbours,
(*ii*) % of adopting neighbours and
(*iii*) % of adopting neighbour population.

In all three cases, spillover effects are small and statistically insignificant. In column (4) the sample is full again. We interact the number of adopting neighbours with the presence of the dry law in the city. If spillovers are relevant, then whether a dry law neighbour comes across the boundary to drink will depend on whether the receiving city adopted the dry law. We expect the own law effect to be negative, the neighbours' law positive (since it captures spillovers from neighbours if one does not have a law) and the interaction negative (undoing the positive neighbour effect). Only the own effect has the expected negative sign. The coefficient on the interaction is positive but insignificant. Moreover, the number of adopting neighbours seems to *reduce* homicides, although the coefficient is small in magnitude and statistically insignificant. Again, results suggest that spillover effects are not relevant.

Despite their absence, we assess whether results are affected by spillovers. In columns (5) to (7) the sample is restricted to large cities, where it is more costly for drinkers go to bars in non-adopting neighbouring cities. In columns (5) and (6) the criteria for staying in the sample is population. Results are, if anything, stronger. However, physical size may be a better measure of the cost of moving around. In column (7) the estimated coefficient is slightly a smaller (−0.432) but still statistically and practically significant. In summary, spillovers do not affect our estimates.

4.4. *Validation Tests*

Arguably, dry laws should have an impact on other outcome variables. As a validation exercise we measure the impact of dry law adoption on battery and deaths by car accidents.

4.4.1. *Impact of dry laws on battery*

The newspaper story suggests that dry laws reduced fights. Thus, we expect them to reduce violent crimes other than murder. We test this conjecture by estimating the impact of dry laws on battery.[8] Table 9 presents some of the models in Table 5. Columns (1) through (3) show that dry laws reduced battery, regardless of the inclusion of controls. Consider the full model estimate −2.175 in column (3). The coefficient means an 8% reduction in batteries due to adoption (see Table 3), which resembles the impact on homicides. Results are robust to including state-level enforcement variables and to using only the staggered nature of adoption (columns (4) and (5), respectively).

4.4.2. *Impact of dry laws on deaths by car accident*

Table 10 shows results for deaths by car accidents. The estimated coefficient in column (1) (−0.055) represents a 7% reduction in car accident deaths, an impact comparable to the one on homicides. However, the effect is not precisely estimated, which is not surprising for several reasons.

Bar drinking relates to traffic fatalities more tenuously than it relates to homicides. Most bars are in the periphery, whose dwellers are poor and use the public

Table 9

Battery

Dependent Variable: Battery per 100,000 inhabitants

	All Sample (1)	All Sample (2)	All Sample (3)	All Sample (4)	Only Adopters (5)
AdoptLaw	−4.419	−3.601	−2.175	−2.301	−2.159
	(2.292)*	(1.359)***	(0.664)***	(0.642)***	(0.708)***
4 Lags of Battery?	No	No	Yes	Yes	Yes
Covariates?	No	Yes	Yes	Yes	Yes
Enforcement Variables?	No	No	No	Yes	Yes
no of Observations	1,716	1,716	1,716	704	704

Source. Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE and Municipal Laws
*** = significant at the 1% level, ** = significant at the 5%, * = significant at the 10%. Weighted Least Squares with population as weights. Observations are clustered at the city level. Covariates are as defined in table 5. All specifications include city and period dummies. Sample is May-2001/December–2004 unless otherwise noted.

[8] Battery is actual physical violence. Assault is defined as the threat of violence. The Brazilian Penal Code does not have the assault category, only *Lesão Corporal Dolosa* ('Bodily Injury with Intent'), which in Common Law is battery.

Table 10

Deaths in Car Accidents

Dependent Variable: Deaths by Car Accidents per 100,000 inhabitants	Whole Sample	Only largest Adopters and all non-adopters	Only largest adopters and non-adopters	Only largest adopters and all non-adopters	Only largest adopters and non-adopters
	(1)	(2)	(3)	(4)	(5)
AdoptLaw	−0.055	−0.116	−0.108	−0.119	−0.110
	(0.048)	(0.053)**	(0.071)	(0.082)	(0.086)
4 Lags of Deaths by Car Accident?	Yes	Yes	Yes	Yes	Yes
Covariates?	Yes	Yes	Yes	Yes	Yes
Enforcement Variables?	No	No	No	Yes	Yes
no of Observations	2,535	2,080	1,430	1,536[†]	1,056[†]

Source. DATASUS, Fundação SEADE and Municipal Laws. All specifications include a full set of city and period dummies. Sample period runs from January 1999 to December 2004.
[†]Sample runs from January 2001 to December 2004
*** = significant at the 1% level, ** = significant 5%, * = significant 10%. Weighted Least Squares with population as weights. Observations are clustered at the city level. Covariates are as defined in Table 5.

transportation system. Thus, for the majority of bar drinkers car accidents are irrelevant simply because they do not own a car. The geography of the relationship between bar drinking and deaths by car accident is also unfavourable. It is unclear whether an accident will happen at the city where the bar is located or somewhere else. The odds that the homicide will be committed nearby are higher because committing homicides do not imply driving. Hospital data is also problematic. The victim may end up in hospital in a city other than where the bar is located or the accident took place. Finally, if a victim is declared dead at the scene, she goes directly to the morgue and does not show up in the hospital data.[9]

To mitigate the fact that accidents may happen outside the adopting cities limits, we discard the half of adopting cities that are smallest in terms of area. Results are now stronger and precisely estimated. In column (3), we also discard the same group of non-adopting cities. Results are similar but precision is lost due to the small number of observations. Including state-level enforcement variables does not change any conclusion.

4.5. *Falsification Tests*

Some crimes should not be affected by the dry law. If they are, we would suspect that the estimated impact of the dry law is spurious and may be attributed to other unobserved policies. Thus they serve as falsification tests. We use three categories: vehicle, bank and cargo robbery.

[9] Adams and Cotti (2008) show that smoking restrictions in the US caused an increase in deaths by car accidents because people drove longer distances to go to bars in counties without smoking restrictions. The same could apply here, although this effect is second order because most bar drinkers do not drive in the SPMA.

4.5.1. *Impact of dry laws on vehicle robbery*

Vehicle robbery is our preferred falsification category for several reasons. First, it does not suffer from under-reporting. Accuracy, however, does not imply that it is a good falsification category. If it was an impulsive crime it would be affected by dry laws. It is hard to argue that the dampening inhibition effect of alcohol does not induce all sorts of bad behaviours. Differently from homicides, however, alcohol consumed *socially* should not have a pronouncedly larger impact on vehicle robbery.

It is well known (but hard to quantify) that in the SPMA vehicle robbery is a professional crime, driven by the secondary market for parts and, to a less extent, by smuggling to neighbouring states and countries, which is hardly an impulsive type of crime. The same argument applies for vehicle *theft* but robbery is a better falsification category because, by definition, it involves an imminent threat to life, normally with the presence of weapon. Thus, the victim must be present and the crime occurs mainly during hours when people are circulating in the streets. Panel (*a*) of Figure 5 shows that only 20% of robberies occur during the hours in which the dry laws are 'binding'. Most vehicle robberies occur in the evening rush hour when dry laws are not binding. In contrast, panel (*b*) shows that 36% vehicle *theft* occur during the dry law hours (11pm–6am), which is also the mode of the distribution. This is unsurprising because theft does not require threat, and the typical target is a vehicle parked in a dark empty street, i.e., late night and early morning, when dry laws are binding.

Panel (*a*) of Table 11 shows some of the models we estimated for homicides. In column (1) we report the stripped down model. The impact of dry law is *negative* but insignificant statistically and practically (compare the point estimate with the means in Table 3). In column (2) we add covariates and state-level enforcement variables. The impact is now *positive* but again insignificant statistically and practically. Column (3)

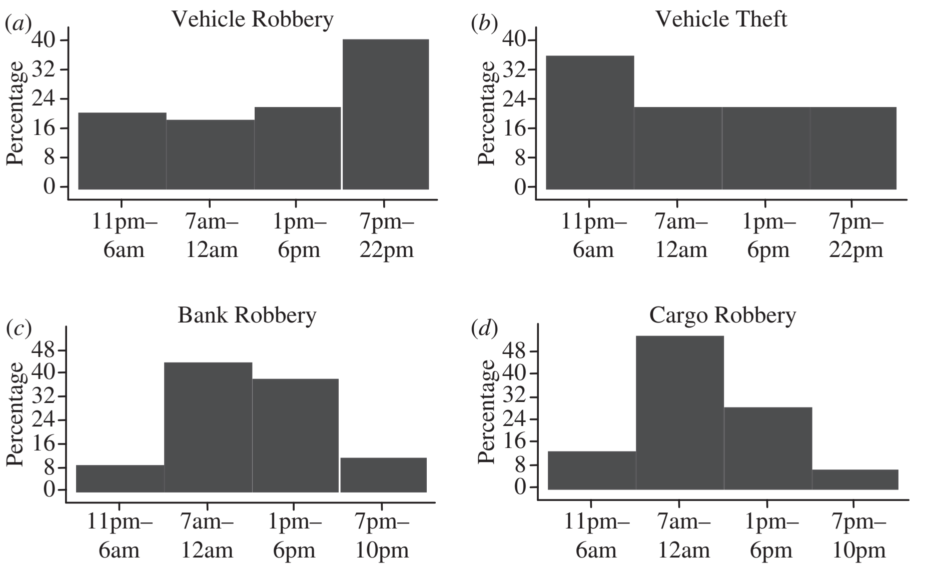

Fig. 5. *Distribution of Vehicle Theft and Bank/Cargo/Vehicle Robbery Over the Day*

Source. Secretaria de Segurança Pública do Estado de São Paulo, INFOCRIM. Sample is composed of all homicides committed in the SPMA and recorded by INFOCRIM between 1999 and 2003.

adds the lags of homicide and again we find no impact on vehicle robbery. Column (4) has the un-weighted OLS estimate, with similar results.

4.5.2. *Impact of dry laws on bank and cargo robbery*

Besides vehicle robberies, we have monthly data from January 2001 onwards on bank and cargo robbery, two good categories for falsification tests. Bank and cargo robbery should not be affected by dry laws because both are professional crimes. Bank robberies are complex ventures, which involve planning. Cargo robbers need a network of contacts to dispose of the merchandise in the market. Both bank and cargo robberies tend to be well measured because of insurance reasons. Finally, both categories occur mainly during the daytime. Panel (*c*) of Figure 4 shows that 92% of bank robberies occur between 7am and 10pm, and 82% between 7am and 6pm. This is expected because by definition robberies must involve threat and, thus, should almost always happen during bank opening hours. Cargo robberies have a similar distribution during the day (Figure 4, panel (*d*)). Relative to vehicle robbery, bank and (to lesser extent) cargo robbery have the disadvantage of being less frequent, which reduces the power of the test. Panels (*b*) and (*c*) in Table 11 have the estimates.

Start in panel (*b*). The impact of dry laws on bank robberies is never different from zero statistically and the estimated coefficient is erratic, with oscillating sign. Bank robberies are very infrequent and the failure to estimate the impact of dry laws on bank robbery may be due to the low power of the test. Panel (*c*) has the estimated impact of dry laws on cargo robbery, which are more frequent than bank robbery. Again, we never reject the null

Table 11

Falsification Tests

	WLS (1)	WLS (2)	WLS (3)	OLS (4)
(*a*) Dependent Variable: Vehicle Robbery per 100,000 inhabitants				
AdoptLaw	−0.260	1.896	0.735	0.125
	(1.781)	(1.774)	(0.753)	(0.854)
4 Lags of Vehicle Robbery?	No	No	Yes	Yes
Covariates?	No	Yes	Yes	Yes
Enforcement Variables?	No	Yes	Yes	Yes
(*b*) Dependent Variable: Bank Robbery per 100,000 inhabitants				
	(1)	(2)	(3)	(4)
AdoptLaw	−0.008	0.015	0.010	0.043
	(0.018)	(0.029)	(0.025)	(0.043)
4 Lags of Bank Robbery?	No	No	Yes	Yes
Covariates?	No	Yes	Yes	Yes
Enforcement Variables?	No	Yes	Yes	Yes
(*c*) Dependent Variable: Cargo Robbery per 100,000 inhabitants				
AdoptLaw	−0.205	0.046	0.035	0.137
	(0.243)	(0.166)	(0.114)	(0.135)
4 Lags of Cargo Robbery?	No	No	Yes	Yes
Covariates?	No	Yes	Yes	Yes
Enforcement Variables?	No	Yes	Yes	Yes

Source. Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE and Municipal Laws
*** = significant at the 1% level, ** = significant at the 5%, * = significant at the 10%. Observations are clustered at the city level. Covariates as defined in Table 5. For all specifications the number of observations is 1,716 in all specifications

hypothesis that the impact of dry laws is zero. The estimated coefficient in column (1), −0.205, is large when compared to the mean of cargo robbery in adopting cities before adoption (1.00 see Table 3) but it is not statistically significant. Furthermore, the estimates are not robust to the inclusion of controls: in all other three columns the impact of cargo robbery is insignificant in practice as well as statistically.

5. Conclusion

At our benchmark estimate, dry laws cause monthly homicide rates per 100,000 inhabitants to fall by almost 0.5, which means a 10% reduction. To the best of our knowledge, this is the first estimate of the impact of alcohol restrictions on bars and restaurants on violent crime accounting for endogeneity and that cannot be confounded with other policies or secular trends.

Restricting opening hours has the advantage of being easily enforceable. Consider the enforcement of the minimum drinking age: it is much harder to monitor whether a bar sells alcohol to minors then verifying whether it is opened at certain hours.

Our results provide a guarded support for policies that restrain the recreational consumption of alcohol. We use the word 'guarded' because in different institutional settings results may not arise. Furthermore, our results are silent with respect to the welfare cost of dry laws. Finally, we have no data to assess potentially perverse effects of the law. In the UK, for example, police report data suggest an increase in violent behaviour right after 11pm, as pubs were closing (Finey, 2004). A full cost–benefit analysis should be conducted in order to assert confidently that opening hour restrictions are worth implementing as a public policy.

Extrapolation to general alcohol consumption is not warranted. In fact, our results are not in contradiction with previous results in the economics of crime literature. Prohibition and taxation fail because they do not reduce consumption and may shift consumption to heavier 'psychotropic' substances. Restricting recreational consumption is less radical and more targeted than prohibition. The purpose is not to prevent people from drinking, but to make it difficult to do so in particularly dangerous settings.

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