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Dry Law and Homicides:  
Evidence from the  
São Paulo Metropolitan Area

Ciro Biderman  
João M P De Mello  
Alexandre A Schneider



DEPARTAMENTO DE ECONOMIA  
[www.econ.puc-rio.br](http://www.econ.puc-rio.br)

# Dry Law and Homicides: Evidence from the São Paulo Metropolitan Area\*

Ciro Biderman, João M P De Mello<sup>†</sup> and Alexandre A Schneider

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## Abstract

Following Bogotá in 1995, several Latin American cities have adopted “dry laws”, which restrict the sale of alcohol in bars and restaurants during specific hours of the week. Although in several cases a reduction in homicide followed adoption, this drop confounds other measures and/or previous decreasing trends. In this paper, we provide hard evidence on the effect of dry laws on homicide by using a feature of the adoption in the São Paulo Metropolitan Area. Between March 2001 and August 2004, 16 out of the 39 municipalities of the SPMA have adopted, at different dates, dry laws. By comparing the dynamics of murder between adopting and non-adopting cities, we estimate that dry laws reduce murder by at least 10%.

KEYWORDS: *Dry Law, Alcohol, Crime, Difference-in-differences.*

JEL CODES: I18, R58, Z00, K32

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\*Escola de Administração de Empresas de São Paulo, Fundação Getúlio Vargas. Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro (PUC-Rio). Secretary of Education, Mayorship of São Paulo. The authors would like to Lilia Konishe for excellent research assistance, and Flavia Chein for graciously helping with the map. We further thank Paulo Arvate, Paulina Achurra, Sergio Firpo, Claudio Ferraz, Miguel Foguel and Marcelo de Paiva Abreu, and seminar participants at PUC-Rio, IPEA, EPGE-FGV, and in the at 11<sup>th</sup> Annual Meeting of LACEA for comments and suggestions. Schneider stresses that opinions expressed here are solely his, and not the official position of the Mayorship of São Paulo.

<sup>†</sup>Corresponding author: jmpm@econ.puc-rio.br

*“Oh God, that men should put an enemy in their mouths to steal away their brains! that we should, with joy, pleasance, revel, and applause, transform ourselves into beasts!” Cassius, in William Shakespeare’s Othello*

*“Beer is proof that God loves us and wants us to be happy.” Benjamin Franklin*

## I. Introduction

As hinted by Cassius’ lament in Shakespeare’s Othello, there is a long tradition of anecdotal evidence that alcohol induces all sorts of social maladies. Poverty, unemployment, and family disruption are but a few examples. In this paper, we study the impact of alcohol consumption on the utmost form of social misbehavior: murder. More specifically, we estimate the causal effect on homicide rates of restrictions on the recreational sales of alcohol (dry laws, hereafter), which are mandatory night closing hours for restaurants and bars. To do so, we take advantage of an unique empirical opportunity: between March-2001 and August-2004, 16 out of 39 cities in the São Paulo Metropolitan Area (SPMA, hereafter) adopted dry laws. By comparing the dynamics of homicides in these cities with evolution of homicides in the remaining 23 cities that did not adopt dry laws, we are able to isolate simultaneous events that might spuriously produce a relationship between the adoption of a particular policy and some observed outcome. Our estimates suggest that dry laws cause, at a minimum, a 10% reduction in homicides.

The SPMA is not an isolated case. In fact, Bogotá, Colombia was the first Latin American metropolis to adopt such restrictions.<sup>1</sup> Despite numerous examples of adoption, hard evidence on their effectiveness is surprisingly scant. By comparing homicides before and after adoption, press articles attribute sharp reductions in murder rates to dry law adoption, although competing events or concurrently adopted policies could have produced the drop in

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<sup>1</sup>In the end of 1995 Bogotá adopted a law similar to the ones we study, the Ley Zana-  
horia, which restricted the sales of alcohol after 1am, which was subsequently relaxed to  
after 2am. There, however, the adoption was mostly uniform, with little cross-section  
variation. Another possible application is Brasília, Distrito Federal, the capital of the  
country. There, again, adoption was uniform..

murder rates.<sup>2</sup> Furthermore, homicides could have already been falling previous to adoption, as it is the case in the SPMA (see figure I). If restricting recreational alcohol consumption is socially costless, precise causal inference would not be so relevant as a matter of policy. However, Mr. Franklin, among others, would consider them costly.<sup>3</sup> Nevertheless, it is hard to exaggerate the social costs of crime, specially in high-crime environments such as large Latin American cities. Soares [2006], for example, calculates that crime represents some 38% of GDP in Brazil, if welfare costs are taken into account.<sup>4</sup> Restricting recreational sales of alcohol, if it works, might be a cost-effective way to reduce violent crime.

The presence of both time-series and cross-section variation in adoption within the same metropolitan area makes the SPMA case particularly attractive as an empirical application. Having both types of variation allows us to account for observed concurrent events, and for all time-invariant heterogeneity among cities. This is particularly important for interpreting the alcohol-crime relation as causal since factors that remain somewhat constant over short periods of time, such as child abuse, poverty, and psychological disturbances, may cause both alcohol (ab)use and crime. Additionally, it is possible to account for previous trends in homicides.

One can always find adopting and non-adopting cities if the search is wide enough geographically, but that hinders casual interpretation because it is difficult (if not impossible) to argue that unobserved heterogeneity is not

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<sup>2</sup>*The Economist*, 10/20/2005, reporting a story on Diadema (an adopting city in the SPMA) lists dry laws as an important factor contributing for the decline in murder rates starting in 2001. In the same story, Tulio Kahn, the head planning for the state of São Paulo Secretary of Public Security, lists several competing reasons: growth of protestant churches, which preach against drinking, the two-fold increase in prison population, and disarmament are only a few examples. In an interview to *O Globo*, the second largest circulation newspaper in the country, Barueri's (another SPMA city) Municipal Secretary of Communication reports that homicides "fell up to 70%" after the city implemented the dry law.

<sup>3</sup>Indeed, as we shall see in the next section, dry laws in the SPMA are less stringent on weekend than in weekday nights, despite the evidence that crime concentrates on weekends. See Finney [2004]. Since bar going is more common on weekends, this indicates a high perceived social and economic cost of such policies. Furthermore, in the city of São Paulo, a non-adopting one, where the night life is specially buoyant, adoption has always faced fiercer opposition. In Brasília, the law was adopted in August-2003, after over a year of judicial battles between restaurants and the local government.

<sup>4</sup>In comparison, violence seems relatively cheap in the United States, "only" 13% of GDP (Soares [2006, forthcoming]).

driving results. Cities that belong to the same metropolitan area are subject to roughly the same economic and sociopolitical shocks, which guarantees a minimum degree of homogeneity across observations, i.e., cities.

The SPMA case is an attractive empirical application for yet another reason. Dry laws were adopted non-simultaneously but over a short period of time (from March-2001 through August-2004). With non-simultaneous periods of adoption, some cities can "act" as both "control" and "treatment". If, however, adoption is too spread out in time, it would be hard to isolate the estimates from (other) unobserved factors.

From a methodological perspective, the SPMA case has, however, one challenging feature: adoption was not random but a choice of the cities. Difference-in-differences estimates can only be interpreted as causal under the assumption of unconfoundedness (Imbens [2000], Rosenbaum and Rubin [1983]), meaning that the treatment (adoption) is uncorrelated with unobservables factors that affect homicides, after controlling for observables. The unconfoundedness assumption would be satisfied by construction if adoption was randomly assigned to cities, which is not our case. Although we do not have exogenous variation to explain adoption, we address the question of non-random assignment with institutional knowledge and with the empirics of our application. Using a duration model we investigate the determinants of the timing of adoption, and conclude that adoption was not preceded by surges in homicides, which could potentially trigger other (unobserved) policy reactions, such as an increase in policing. In a nutshell, although the odds of adopting are higher in more violent cities, they did not adopt in periods in which homicides were particularly high. Second, the only observable factor that is systematically related to the timing of dry law adoption is the number of neighbors that have adopted the dry law, which indicates that randomness played a significant role in determining the timing of adoption. Third, the decision-making process is such that it is implausible that policing and adoption could be confounded, at least not within the time span of our sample. Third, we use modified versions of propensity score weighting and sample trimming (Imbens [2000], Crump et. al. [2006]), which boil down to "homogenizing" adopting and non-adopting cities with respect to observables, to correct for heterogeneity among adopting and non-adopting cities. Although these methods do not guarantee "homogeneity" among time-varying unobservables, if cities are similar in terms of observables, they are less likely to diverge meaningfully with respect to unobservables. Finally, as mentioned above, non-simultaneity of adoption helps to identify the effect of dry laws.

By focusing on late adopters before adoption, we construct a special “control” group that has revealed a very high propensity of adopting the dry law.

The paper is organized as follows. Section II contains an analysis of the economics and the criminology of the alcohol - crime *nexus*. Section III narrates the chronology of the events, and section IV describes the data. In Section V the empirical strategy is outlined and the main results are presented. Using a duration model we show that although more violent cities tended to adopt earlier, adoption did not occur as a response to recent surges in homicides. The effect of dry law adoption is estimated both parametrically for the mean and semi-parametrically for the whole distribution of homicides. Results are subject to an extensive robustness analysis, which include: different periods of analysis; propensity score weighting and trimming the sample according to the propensity score; excluding potential outliers; accounting for autocorrelation of the residuals; restricting the control group to late adopters before adoption; and accounting for general equilibrium effect that crime may shift from adopting to non-adopting cities. In all procedures we control for an extensive list “other suspects” in producing the results. This list contains: several lags of homicide; city fixed-effects and period specific effects; time-varying demographics such as income, age structure and educational attainment; and the simultaneous adoption of other policies such as the establishment of a municipal police force or a municipal secretary of justice. Finally, we run a placebo experiment with fictitious adopting periods and verify that results disappear. Section VI discusses the results, and section VII concludes the article.

## II. The Economics and the Criminology of Alcohol-Crime Nexus

Received theory and evidence on the criminology and economics literatures draw an ambiguous picture on the alcohol-crime *nexus*. In the criminology literature, studies that use individual data find a positive association between alcohol consumption and crime. McClelland et. ali. [1972], in their classic *The Drinking Man*, compare fantasies of sober and intoxicated men, and find that the later were more likely than the former to have fantasies that involved power and domination. Using British data, Hutchison

et. alli. [1998] found that 60% of people arrested for assault in city-center have consumed alcohol in the four hours prior to arrest. Greenfeld [1998], using data from several US sources, found a strong link between alcohol and crime. Particularly interesting for our purposes, inmate survey data shows that 75% of murder convicts were estimated to have a Blood Alcohol Concentration (BAC) over 0.08 at the time of the crime.<sup>5</sup> In our empirical setting, Gawryszewski et. alli. [2005] found, using toxicological blood exam data in the city of São Paulo, that 40.4% of homicide victims showed signs of alcohol drinking.

Causal interpretation, however, is problematic for two reasons: omitted variable and sample selection. It is rather conceivable that alcohol (ab)use and violent behavior (or being a victim of) are both determined by other factors such as child abuse and psychological disturbances. Since sober offenders (victims) are less likely to get caught (be victimized), sample selection would produce overestimation of the alcohol-crime *nexus* (see Martin [2001]). In this case, the policy implication could even be reversed: intoxication could help catching offenders.

While studies that use individual inmate and booking data find at least a strong *correlation* between alcohol consumption and crime, the association is tenuous in studies that use aggregate data. Take for example the question of whether the alcohol-violent crime *nexus* is magnified when consumed in some social settings, such as bars. The British Crime Survey 2001/02 (BCS 2001/02) found that 21% of all night-time violent incidents occurred in or around a pub. Stockwell et. alli. [2003], in a survey of Western Australian adults, found that bars were the preferred venue of alcohol consumption prior to committing violent crimes.<sup>6</sup> Gorman et. alli [1998], however, using data on New Jersey cities, cannot link outlet density and crime after they control for city demographics. This is not surprising since alcohol consumption correlates with other crime inducing factors, such as poverty and unemployment. In the end, the epidemiological literature has not been able to settle the issue of causality. In a comprehensive survey, Lipsey et. alli. [1997] concluded that “the causal issue is still cloudy and uncertain”.

Despite some evidence linking (at least as a correlation) alcohol consumption and criminal behavior in general, there is little work done on the effects

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<sup>5</sup>0.08 is the legal driving limit in most US states.

<sup>6</sup>See also Roncek and Maier [1991] and Scribner et. alli. [1995]. Martin [2001] provides an excellent and exhaustive survey on the literature.

of alcohol on the ultimate form of violence: murder. Our conjecture is that this lack of evidence is due to three facts. First, relative to robbery, murder is a rare occurrence, making aggregate murder measures (e.g., rates per 100th inhabitants) rather noisy. Second, it is hard to get enough independent variation in drinking behavior, both overtime and in a cross-section sense. Third, with individual level BAC data, is it hard to be completely convincing about the causal link between alcohol consumption and homicides for the omitted variable and selection problems outlined above.

Even if a causal link from alcohol to violence was well established, it is not clear-cut how should the policy maker intervene, if at all. The economics of crime literature has, in general, a rather negative view of outright prohibition and taxation. Studies on the American prohibition in the 1920s are a good example (Miron and Zweibel [1991, 1995]). Outright prohibition, while not reducing consumption meaningfully, induces crime associated with the impossibility of settling contracts through the formal judicial sector. Price oriented interventions (e.g., taxation) seem equality ineffective (Miron [1998]). Furthermore, there a substitution effect. Making alcohol illegal levels it with illicit psychotropics, and reduces the perceived cost of moving to “stronger” drugs (Thornton [1998]). In the light of this evidence, and of the wisdom in the economics literature, studying the effects of targeted sales restrictions such the SPMA dry laws becomes even more interesting from a policy perspective. The economics of addiction suggests that price interventions and prohibition are ineffective in shifting consumption away from strong habit formation goods such as alcohol. Nevertheless, restricting its consumption in recreational settings such as bars might be effective in reducing crime. Because less radical than prohibition, it may not trigger substitution effects, or contract-enforcement crime. Because specifically targeted at circumstances in which the effects of alcohol may be magnified by social interaction, it may be relatively economical from a welfare perspective.

Whether restricting recreational sales in the SMPA would have a first-order impact on murder is far from obvious. Enforcement could be lacking, particularly in fragile institutional settings such as the SPMA. Cross-section variation, which makes the SPMA attractive as case study in the first place, may produce a “beggar-thy-neighbor” general equilibrium effect: bar drinking shifts from one city to another, and the dry law becomes overall ineffective. Finally, the strongest association in the criminology literature is between *drinking* and crime, not necessarily between *bar drinking* and crime.

The SPMA is a particularly attractive setting for testing dry laws are

effective in reducing homicides for several reasons. Conceptually, one should be particularly interested in simple policies that can work in a weak law-and-order, high-crime cities.<sup>7</sup> Among the empirical reasons, we should emphasize the presence of both cross-city and time series variation. Studies such as Stockwell et. alli. [2003] and Gorman et. alli [1998] rely solely on cross-section variation, making it difficult to identify the causal effect of alcohol above and beyond demographics. Finally, because of the high frequency (monthly), short-term time series variation is alcohol sales' restrictions on the city level, our results are much less subject to other suspects criticisms. It is improbable that the average amount of child abuse and psychological disturbances changes significantly in short periods of time.

Our results support the hypothesis that alcohol and the social interaction of bars are complements in the production of homicides. In this sense, our paper reconciles the economics, the public health, and the criminology literatures. Previously attempted interventions (prohibition, taxation) are ineffective not because alcohol does not induce misbehavior, but because the either did failed to reduce alcohol consumption, or because they were poorly targeted at reducing consumption in particularly crime inducing settings, such as recreational consumption.<sup>8</sup>

### III. Brief Description of the Empirical Setting and the Chronology of Events

The São Paulo Metropolitan Area (SPMA) is largest contiguous urban area in South America, and the third largest worldwide. Politically, it is defined as a administrative region in the state of São Paulo. It has roughly 18

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<sup>7</sup>The homicide rate in São Paulo over the last ten years have been almost always higher than New York's in its 1990 peak. See section III. A.

<sup>8</sup>Evidently, there are competing theories to explain why restricting bars' operations would reduce homicides. It could be that bars are points of *rendezvous* for drug dealers, and closing them down hinders this crime inducing activity. Or, still, broken windows are the explanation, i.e., closing down run-down bars in the city periphery has a deterrence effect. New York's is often quoted as a case of succesful broken-windows policies, although empirical evidence has not been entirely supportive. See Wilson and Kelling [1982], Kelling and Sousa [2001] and Harcourt and Ludwig [2006] fail to do so in a five-city study. It is beyond our scope to horserace these alternative explanations.

million inhabitants.<sup>9</sup> It is composed of 39 cities, each independent administratively, with their own mayor and city council. City sizes vary widely, from Santa Isabel with a population of 11,000, to São Paulo, the largest, which had roughly 10 million inhabitants in 2005.

Although Brazil is much more politically centralized than the United States, cities have legislative jurisdiction over regulation of local commerce. This allowed the city of Barueri, in the São Paulo Metropolitan Area, to pass, in March 2001, a law mandating bars to close from 11PM to 6AM on weekdays, and from 2AM to 6AM, on weekends. The law allowed for exceptions. Bars not located near schools, outside “crime zones” and that have had any complaints for disturbances, were allowed to remain opened beyond the allowed time. As of September 2005, between 50 and 60 out of roughly 4,000 fell into this category.<sup>10</sup>

Soon after several cities followed suit, adopting laws very similar to Barueri’s. As of December 2004, 16 out the 39 cities in the SPMA have adopted dry laws, and the city of São Paulo itself is currently considering adopting a restricted version, operative only in high-crime areas.

City	Date - Dry Law
Barueri	Mar-01
Jandira	Aug-01
Itapevi	Jan-02
Diadema	Mar-02
Juquitiba	May-02
São Lourenço da Serra	Jun-02
Suzano	Jun-02
Itapeçerica	Jul-02
Mauá	Jul-02
Ferraz de Vasconcelos	Sep-02
Embu	Dec-02
Osasco	Dec-02
Embu - Guaçú	Apr-03
Vargem Grande Paulista	Dec-03
São Caetano	Jul-04
Poá	Aug-04

Table 1 Source: Kanh and Zanetic (2005), months alcohol laws passed in city council

<sup>9</sup>Projection for 2005 based on the 2000 census.

<sup>10</sup>See [www.propagandasembebida.org.br](http://www.propagandasembebida.org.br), in Portuguese.

## IV. Data

Homicides are from the Secretaria de Segurança do Estado de São Paulo.<sup>11</sup> Homicide data is based on all police reports, and include murders and non-negligent manslaughter, but exclude manslaughter and car accident deaths. This is important for our purposes since alcohol may trivially cause accidental deaths.

Homicide data normally should not suffer from under-reporting, since investigation is mandatory. There is, however, some discrepancy stemming from the fact that our data is from the original police report, and death may occur in the hospital, after the . previous work with the same data have cross-referenced police report with hospital data, and show an almost perfect match (De Mello and Zilberman [2006]). This increases our confidence that our measure of homicides do not have significant measurement error.

Demographic data is available publicly from SEADE, a state-government think-tank that compiles data for São Paulo from several sources. The two main original data sources are the 2000 census, and the 1999, 2001 and 2003 PNADs, the main household level national survey.<sup>12</sup> Data is available on several demographics that may affect crime. Income per capita is available annually for the 1999-2003. Population, based on projections, is available annually for the whole period. Educational attainment measures, such as high school drop out rates, and number of years of schooling, are only available for 2000 (census). They are only used in our propensity score procedure, since city dummies capture their effects in our main estimation procedure.

Finally, timing of adoption of dry laws, of the establishment of a municipal police force, and of the establishment of a municipal secretary of justice are from Kahn and Zanetic [2005].

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<sup>11</sup>The Secretaria de Segurança do Estado de São Paulo is a state level authority on policing. As we shall see in more detail, the main bulk of policing in Brazil is done at the state level.

<sup>12</sup>The PNAD is similar to the Panel Study of Income Dynamics (PSID) in the United States. It is not longitudinal, however, but has more survey families (100,000 against 8,000 in the PSID).

## V. The Empirical Strategy

Differences among cities in adoption of dry laws is the main source of variation used to estimate the effect of alcohol restrictions on crime. Both time-series and cross-city variation are a crucial fortunate feature of the data from the SPMA. Without *both* types of variation is it very difficult to identify the effect of alcohol restriction against other possible competing factors that might affect crime.

Attention is restricted to the SPMA, instead of the whole state or the whole country, to guarantee a minimal level of homogeneity across cities. Cities belonging to the same Metropolitan area are subject to roughly the same economic, social, and political shocks that affect crime in general, and homicides in particular. Moreover, the SPMA has the additional fortunate feature that adoption of dry laws, albeit not simultaneously, were concentrated in the period between 2001 and 2004, with the main bulk in the years 2002 and 2003. These two features are desirable for two reasons. They attenuate the problem of unobserved heterogeneity between adopting and non-adopting cities, and provide sufficient observations of adoption and non-adoption within an adequate period of time for the dynamics of homicide. Furthermore, differences in timings of adoption, conditional on being sufficiently close, help identifying the effect of dry laws, as it will be explained below.

The example of Diadema is illustrative. This city is viewed as a successful example of policy-induced crime reduction.<sup>13</sup> Diadema had, during the late 1990s, one of the highest crime rates in the country.<sup>14</sup> During the two-year period prior to the adoption of the alcohol law the murder rate was, on average, 7.52 per thousand inhabitants in Diadema. In the two years following adoption, on the other hand, it was 4.67, i.e., a decrease of 37.87%. For the SPMA, the numbers are 4.24 and 3.67, for the same two sub-periods, i.e., 13.46% drop.

While this evidence indicative, it is far from completely convincing. Several concurrent events also might explain the Diadema success. Improvements in community policing, and stricter gun control are but a couple.<sup>15</sup>

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<sup>13</sup> *The Economist*, on its 10/20/2005 edition, brings a story describing the recent case of Diadema.

<sup>14</sup> See *The Economist*, 10/20/2005.

<sup>15</sup> See *The Economist*, 10/20/2005.

Furthermore, crime could have been going down anyway. The SPMA, however, contains several cities that have adopted similar dry laws (adopting cities hereafter) and several cities that have not adopted (non-adopting cities). The fact that we have cities that *have not adopted* the law provides a natural candidate for comparison, that is, they are a reasonable control group for cities that *have* adopted the law (the treatment group). In this section we explore the cross-city and time-series variation in adoption to estimate the causal effect of dry laws on homicide.

## A. Summary statistics: Adopting and Non-Adopting Cities

The SPMA is a high homicide metropolis. In 2002, monthly homicides averaged 3.64 per 100thd inhabitants in the SPMA. For comparison, in this year the SPMA would rank second in the United States, slightly below Washington DC, the “murder capital”, with 3.81 monthly homicides per 100thd inhabitants. Chicago, the 5th worst city, had 1.85 monthly homicides per 100thd inhabitants.<sup>16</sup> In New York City at its peak (1990), the rate was 3.56.

In a snapshot comparison, table II shows that adoption of dry laws occurred, as expected, in cities where crime were high: adopting cities have an average rate of 4.22 homicides per thousand inhabitants, some 27% higher than non-adopting cities.

Adoption of dry laws seems indeed associated with a drop in homicides. In adopting cities, average monthly homicides in the 12 months subsequent to adoption were 3.71 per 100 thousand inhabitants, a 20% decrease from 4.62 in the previous 12 months. This, however, most likely includes a general tendency for decline, since homicides also dropped, albeit much less, in non-adopting cities, from 3.67 to 3.43 per 100thd inhabitants around the average period of adoption.<sup>17</sup>

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<sup>16</sup>This rank refers to cities that have a population over 500,000.

<sup>17</sup>For non-adopting cities the period of comparison is July 2002, the average adoption period, if late adopters (São Caetano and Poá) are excluded.

## Summary Statistics: Adopting and non-Adopting Cities

	Mean Adopting (16 cities)**	Mean non-Adopting (23 cities)
<b>Homicides per thd inhabitants</b>		
Jan-1997/Dec-2004	4.22 (0.92)	3.42 (0.61)
12 months before adoption†	4.62† (1.31)	3.67‡* (0.39)*
12 months after adoption†	3.71 (1.79)	3.43‡ (0.18)
Population < 50,000	2.30 (3.46)	1.44 (2.68)
100,000<Population< 150,000	3.42 (2.69)	2.78 (2.20)
Population > 150,000	4.21 (2.30)	3.51 (1.62)
<b>Demographics</b>		
Population (in thd) 1997-2004	186 (164)	652 (2100)
Population (in thd)* 1997-2004		208 (269)
Population	210	214
12 months before adoption†	(177)	(275)
Population	215	219
12 months after adoption†	(179)	(281)
% Male Population, age 15-30 1997-2004	14.5 (0.7)	13.7 (0.6)
% Male Population, age 15-30 12 months before adoption†	14.6 (0.4)	13.7 (0.5)
% Male Population, age 15-30 12 months after adoption†	14.5 (0.5)	13.5 (0.6)
<b>Educational Attainment</b>		
High school drop-out rate (2002)	8.28 (2.66)	7.52 (2.44)
Number of years of Schooling	6.97 (1.08)	7.00 (0.79)
<b>Income</b>		
Income per capita 1999-2004	8847 (7178)	8293 (4623)
Income per capita 12 months after adoption†	8484 (6651)	8641 (5086)
Income per capita 12 months after adoption†	9535 (6990)	9757 (6107)

Table II Source: Secretaria de Segurança do Estado de São Paulo, Fundação SEADE, and Kahn and Zanetic [2005]. Standard Errors in parentheses. †: for each city, average over the period (12 months before or after the adoption), then averaged over adopting cities. ‡ average homicides for non-adopting cities; period of reference is the average adoption period excluding Poá and São Caetano (May 2002). \* = excludes São Paulo. \*\* = São Caetano and Poá excluded for late adoption

As a “control group” for adopting cities, non-adopting cities are better the more alike (both in time-series and cross-sectional senses) these two groups are in terms of factors that may determine crime. Larger cities have more homicides and, apparently, adopting cities are smaller than non-adopting ones. This, however, is driven by São Paulo, which represents 58% of the

population of the SPMA. When São Paulo is excluded, adopting and non-adopting cities of about the same size. Population increases slowly, and similarly, in both groups around the adoption period. In terms of population in crime prime age, adopting cities have a slightly higher proportion of population in the 15-30 years interval (14.5% against 13.7%), which is expected given the higher crime rate in this group. This difference, however, is insignificant, both statistically and practically. It not surprising that these proportion barely changes during the period since changes in demographic structure take place over periods much longer than the time span in our sample. Adopting and non-adopting cities are also similar in terms of educational attainment both in terms of years of schooling and high school drop-out rates, although the later decreased faster in non-adopting cities over the 1999-2000 period.<sup>18</sup> Income also seems quite similar between adopting and non-adopting cities. When São Caetano, an outlier in terms of per capita income is excluded, the average through the Jan-99/Dec-2004 is R\$ 8847 and R\$8293 in adopting and non-adopting cities.<sup>19</sup> Even when São Caetano is included the difference is less than 20%. Finally, it does not seem that income was following a difference path in adopting and non-adopting cities around the adoption period. In summary, all observed characteristics indicate that adopting and non-adopting cities are quite similar, which is important for the estimation strategy to work.

## B. The Decision to Adopt the Law

Dry laws were not randomly assigned to cities but rather their own decision. Therefore, the unconfoundedness assumption (Rosenbaum and Rubin [1983], Imbens [2000]) is not satisfied by construction. Particularly dangerous for our purposes is adoption reacting to shocks to homicides. In this case, it would be conceivable that concurrent *unobserved* policy responding are confounded with adoption of dry laws. By estimate a duration model for the timing of adoption is estimated (see Kiefer [1988] and Jenkins [1995]), it is possible to assess whether this threat is empirically relevant .

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<sup>18</sup>Number of years of schooling is only available for 2000.

<sup>19</sup>This corresponds to roughly 4250 and 4000 US dollars in 2004. São Caetano, an important automobile industry center, had a per capita income of R\$36,000 over in 2001, more than twice the second, São Bernardo do Campo, with R\$17,000.

The estimated model accounts for several factors that might affect the decision to adopt the law. Particularly important to our purposes is the recent dynamics of homicide, the first set of regressors. This allows us to test the hypothesis that adoption of the law was related to recent shocks to homicides. Additionally, we included average homicides in 2000 as a baseline measure of homicides to measure how overall violence affects the decision to adopt.

A set of city-level law enforcement variables (presence of a municipal secretary of justice and of a municipal police force) is included to assess whether the local-level attitude towards law and order influenced adoption.<sup>20</sup> Some demographic controls, such as income, population and male population between 15 and 30 are included because they can affect both homicides and, for political economy reasons, the adoption decision.

In two of our specifications time and time squared are included to account for time varying hazard rates. Adoption occurs over time and, as figure I makes clear, homicides follow a decreasing trend overall in the sample period. So time affects both adoption and homicides.

Finally, the number of neighboring cities that have adopted the law is included to capture neighbor emulation or adoption for fear of suffering from spillover effects. Table III presents the results.

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<sup>20</sup>Policing is mostly done at the state-level in Brazil, and this will be very important in interpreting our estimates as the causal effect of dry laws. See section ??.

Log Normal Duration Regression of Adoption of Dry Law			
	(1)	(2)	(3)
<b>Marginal Effects</b>			
<b>Dynamics of Homicides</b>			
<i>Homicides t - 1</i>	2.15E-05 (0.06)	9.78E-05 (0.68)	
<i>Homicides t - 2</i>	-9.06E-04 (-1.77)*	-2.20E-04 (-1.16)	
<i>Homicides t-3</i>	-4.93E-04 (-1.17)	-9.41E-05 (-0.57)	
<i>Homicides t-4</i>	1.28E-04 (-0.34)	2.60E-05 (0.16)	
<i>Average Homicides</i>			-3.96E-05 (-0.46)
<b>City-Level Enforcement</b>			
<i>Municipal Force?</i>	1.79E-03 (0.54)	-8.00E-04 (-0.48)	8.42E-04 (0.69)
<i>Secretary of Justice?</i>	3.61E-04 (0.10)	5.11E-04 (0.67)	-5.01E-05 (-0.45)
<b>Demographic controls</b>			
<i>City Level GDP †</i>	2.80E-03 (1.09)	7.25E-04 (0.70)	7.71E-04 (0.71)
<i>Population †</i>	1.17E-03 (0.04)	5.57E-03 (0.46)	6.03E-03 (0.48)
Male Population, 15 and 30 years†	-3.33E-03 (-0.10)	6.47E-03 (-0.52)	-6.91E-03 (-0.54)
<b>Time Trends</b>			
<i>Time</i>		1.70E-03 (2.35)**	1.72E-03 (2.36)**
<i>Time Squared</i>		-5.69E-06 (-2.27)**	-5.90E-06 (-1.16)
Number of Neighbors that Adopted	1.79E-03 (2.06)**	4.15E-04 (2.01)**	4.36E-04 (2.02)**
<b>Non-Time Varying Controls</b>			
Base Line Homicides	3.11E-03 (2.86)**	1.01E-03 (2.74)**	1.04E-03 (2.73)**
<i>Number of Observations</i>	2249	2249	2249
<i>Pseudo R-squared</i>	0.089	0.152	0.146

TABLE III: Source: Secretaria de Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE, and Kahn and Zanetic [2005]. Duration Log-Normal Regression. Marginal probability effects on hazard rate at month  $t$ . Robust  $t$ -statistics in parentheses. \*\*\* = significant at the 1% level, \*\* = significant 5% level. FGLS procedure using variance model for population. †: Variables in Logs.

In the first column, the model is estimated without the time trends. Results indicate that adoption is not systematically related to the recent dynamics of homicide. Individually, only the second lag of homicide seems to belong to the equation, and even then only marginally and with the wrong sign. As group, although one rejects the null hypothesis that all four lags can be excluded, we only do so marginally ( $p$ -value = 7.4%), and, again, the sum of the coefficients has the wrong sign. In summary, results in column (1) suggest that, if anything, a shock to homicides *lowers* the probability of adoption. These (weak) results in column (1), however, do not stand the inclusion of the time trends. In column (2) coefficients on the four lags of homicide are neither individually nor collectively significant.<sup>21</sup> Since homicides are rather noisy, in column (3), instead of the four lags, we include the

<sup>21</sup>The  $p$ -value of the  $F$ -test on joint significance is 56.2%.

average homicides over the previous four months. Again, recent surges in homicides do not seem to explain adoption.

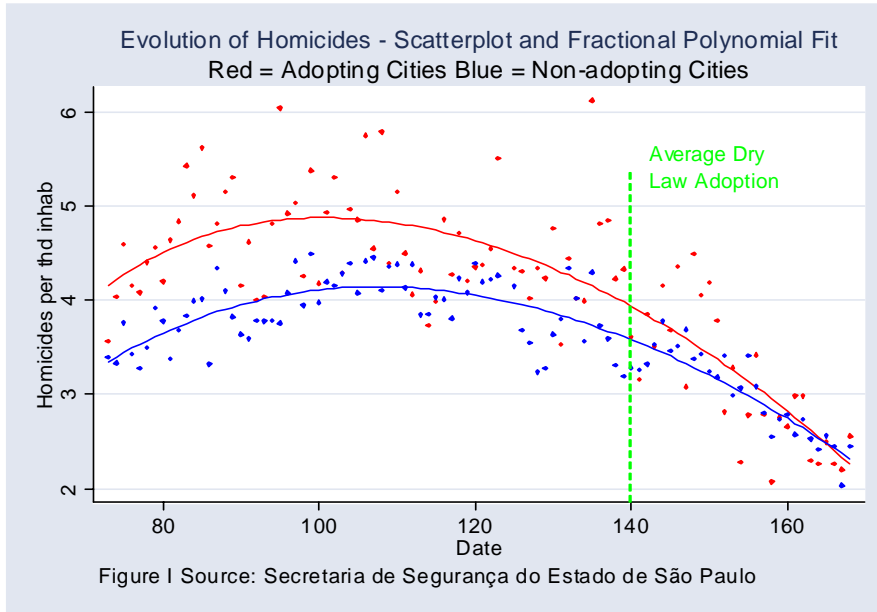
In contrast, base line homicides are related adoption as expected, given summary statistics in table II: cities that were more violent in 2000 had a greater tendency to adopt, although this difference is not vary large in practice. The largest coefficient on base line homicides (column (1)) implies that the most violent city in 2000 (Diadema, an adopting city, incidentally) was roughly 1.8% more likely to adopt than the least violent city (Biritiba Mirim, a non-adopting city, incidentally).

Adoption does not seem related to the presence of local law enforcement structures: the null hypothesis that coefficients on municipal secretary of justice and policing are zero is not rejected at any reasonable level in any of the three specification. Adoption does not seem related to the demographics of the cities: coefficients on local GDP, population, and male population between 15 and 30 years are all statistically zero. Finally, there is the interesting result that adoption by neighbors increases the odds of adopting, although again the magnitude is not terribly large in practice.

In summary, adoption seems unrelated to observables in general, and, rather important in our case, quite unrelated to shocks to homicide. In section VI, this empirical fact will be important in interpreting our results on the effect of the dry law as causal.

## C. Evolution of Homicides and the Choice of Period of Analysis

Figure I shows the evolution of homicides per 100thd inhabitants for adopting and non-adopting cities, from January 1997 to December 2004. Homicides, over our sample period, in the both groups peaked at an average of 5 and 4 per 100thd inhabitants for the adopting and non-adopting groups, respectively, in the first semester of 1999 (roughly period 100).



When the first law was adopted (Barueri, Jan-2001), homicides have been declining, in *both* groups, for approximately 18 months. Figure I also shows the “average adoption” period (136), which was May 2002.<sup>22</sup> While for non-adopting cities (blue) the scatterplots shows a stable decline, for adopting cities (red), the points present a steeper decline. Fractional Polynomial regression captures this by making the decreasing curve steeper for adopting cities after period 105.

Although very interesting, explaining what caused homicides to start falling in the SPMA in the first place is not our goal in this paper. Figure I motivates us to use observations between Jan-1999 and Dec-2004, so that we concentrate only on the period in which homicides are unambiguously falling.

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<sup>22</sup>As noted in section II, cities adopted at different points in time. May 2002 is the mean adoption period among adopting cities, not counting the late adopters, Poá and São Caetano.

## D. Controlling for Covariates: Difference-in-Differences Estimates

In this subsection we estimate a difference-in differences model to evaluate the impact of dry law adoption. The panel nature of the data is fully explored, with each city-period pair treated as an observation.

The main coefficient of interest is a difference-in-differences parameter, which measures the average difference, between cities that have adopted the law and have not adopted the law, in the change in the average crime rate between periods with and without the law. Let  $i$  denote a city in the SPMA and  $t$  denote a month. The main estimated model is:

$$Homicide_{it} = \gamma_0 + \gamma_1 AdoptLaw_{it} + \mathbf{Month}_t + \mathbf{City}_t + \mathbf{Controls}_{it} + \epsilon_{it} \quad (1)$$

$Homicide_{it}$  is the homicide per 100thd inhabitants in city  $i$  at month  $t$ .  $AdoptLaw_{it}$  assumes the value 1 if the alcohol law was city  $i$  at period  $t$  was operative. Hence for cities that have not adopted the law, it assumes only the value 0. If the adoption of alcohol laws induces a drop in crime, this coefficient should be *negative*.  $\mathbf{Month}_t$  and  $\mathbf{City}_t$  are month and city dummies, respectively.

$\mathbf{Controls}_{it}$  include, depending on the specification, variables that may affect both homicides and adoption. In most specifications a set of  $\mathbf{City}$  dummies is included to control for all time-invariant city characteristics, such as average (overtime) differences in income, attitude towards crime, and demographic structure, to name just a few examples. Several lags of homicide are included since they might determine adoption of the law and future crime. Dry law adoption is a decision and, although results in table III suggest otherwise, there could still be that shocks to homicide induced adoption. In this case  $AdoptLaw_{it}$  could pick up the effect of other unobserved policy responses to surges in homicide. Inclusion of the lags of homicide, by controlling for the recent dynamics of murder, should attenuate these concerns. For example: if police enforcement does respond to shocks to homicides fast enough to cause us trouble, then including past homicides *proxies* for police responses. Most specifications also include a set of period specific dummies  $\mathbf{Month}$ , which control for time-specific shifts in crime, such as unemployment in the whole SPMA, seasonal factors, general trends in homicide, etc.<sup>23</sup>

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<sup>23</sup>observations on these variables. High school drop-outs, for example, are available

Demographic controls include city population, proportion of population in crime prime age (between 15 and 30 years), and per capita income.<sup>24</sup> All three variables could affect the homicide rate, and may also determine adoption of dry laws.<sup>25</sup> Two competing policies are accounted for: whether the city adopted local policy force, and the timing of establishment the local police force; and whether the city has a municipal secretary of justice, and the timing of establishment of the municipal secretary of justice.<sup>26</sup>

Finally, we control for the number of neighboring cities that have adopted the law at time  $t$ . Assuming the law actually works, there could be spillover effects, and if adoption anticipates that or is done by imitation, then adoption at neighboring cities affects both homicides and adoption.

Structure is imposed on the variance. The data is at the city level, and there is a large heterogeneity in city size in the data. Since homicides are not such a common occurrence, observations from small cities are much noisier than those from larger cities, i.e., the variance of  $\epsilon_{it}$  falls with city size. Indeed, standard errors of homicide per 100thd inhabitants decrease significantly with city size (table II). By appropriately weighting the data, the model becomes closer to homoskedastic. The Weighted Least Squares (WGLS) procedure assumes that:

$$Var(\epsilon_{it}) = \frac{\sigma^2}{\sqrt{population_{it}}} \quad (2)$$

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for 2002 only, and years of education only for 2000. These variables, however, should have a slow impact on crime, and variation over the period of analysis should not have a significant impact.

<sup>24</sup>These controls are only available annually, so we replicate the annual value for all months in the year.

<sup>25</sup>Variation in population overtime may simultaneously determine the adoption of the law and crime. Adoption is the outcome of collective action, and size and diversity of population may affect the process of social decision making. Age structure, beyond affecting crime, may determine collective decision, since age determines the preferences of the constituency. Since the period of analysis is short (four years), most variation in population, and age structure, is accounted for with the **City** dummies, but it is costless to control for time-series variation. Similarly for income.

<sup>26</sup>There is not a significant amount of variation in municipal police force and municipal secretary of justice. A significant proportion of cities did so before our period of analysis. Some however, did so concurrently.

## 1. Results with the whole sample

Main results are presented in table IV. In column (1), the simplest possible model is estimated, with fixed-effect and period-effects controls, but without lagged dependent variables, demographic variables, local enforcement presence or number of adopting neighbors. The coefficient on the variable *AdoptLaw* ( $\gamma_1$ ) is -0.632, and it is very well estimated ( $p$ -value  $< 0.001$ ). This coefficient is 81% of a standard deviation of homicides per 100th inhabitants in adopting cities, and is therefore practically significant. For a sense of practical significance, had the law been adopted in the city of São Paulo (10,000,000 inhabitants), this would mean roughly 758 homicides less annually, or roughly 22% of the homicides in São Paulo in 2004.<sup>27</sup>

Dependent Variable: Homicides per 100th inhabitants

	Jan-1999 - Dec-2004	May-1999 - Dec-2004	May-1999 - Dec-2004	May-1999 - Dec-2004	May-1999 - Dec-2004	Jan-2001 - Dec-2003
	(1)	(2)	(3)	(4)	(5)	(6)
<i>AdoptLaw</i>	<b>-0.632</b> (0.138)***	<b>-0.392</b> (0.136)***	<b>-0.454</b> (0.145)***	<b>-0.434</b> (0.159)***	<b>-0.432</b> (0.156)***	<b>-0.588</b> (0.258)***
<i>No Observations</i>	2808	2652	2652	2652	2652	1404
<i>R-squared</i>	0.530	0.556	0.557	0.557	0.558	0.491
<i>Homicides t-1/Homicides t-4</i>	No	Yes	Yes	Yes	Yes	Yes
<i>Municipal Force?</i>	No	No	Yes	Yes	Yes	Yes
<i>Secretary of Justice?</i>	No	No	Yes	Yes	Yes	Yes
<i>Demographic Controls?</i>	No	No	No	Yes	Yes	Yes
<i># Neighbors with Law</i>	No	No	No	No	Yes	Yes
<i>City Dummies?</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Period Dummies?</i>	Yes	Yes	Yes	Yes	Yes	Yes

TABLE IV: Source: Secretaria de Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE, and Kahn and Zanetic [2005]. Robust standard Errors in parentheses. \*\*\* = significant at the 1% level, \*\* = significant 5% level. FGLS procedure using variance model for population. Standard errors robust to heteroskedasticity.

Columns (2)-(5) show robustness to inclusion of controls. In column (2), four lags of homicide are included. The parameter of interest falls in absolute value to -0.392, but it is still significant both statistically and practically. In column (3), local enforcement presence is included and, if anything, results are stronger. Similarly when demographic controls and adopting neighbors are included (columns (4) and (5)).

In column (5), the sample is restricted to Jan-2001/December 2003, which accounts for two facts. First, São Caetano, Poá and Vargem Grande, adopted the alcohol late in the sample period, and it might be that the effects were

<sup>27</sup>This number corresponds to  $0.842 \times 100 \times 12$ . In 2004, there were 3431 homicides in the city of São Paulo.

not yet operative. In this case these three cities effectively become part of the control group. Second, with longer period, there is an increased change that the procedure will capture competing factors. Results, if anything, are stronger.

Table V shows further sample robustness checks. The city of São Paulo represents 55% of the population of the SPMA. Since the regression is weighted by population, São Paulo alone could be driving results. Column (1) shows the same model in column (5), table IV excluding the city of São Paulo. Results are again stronger.

Dependent Variable: Homicides per 100th inhabitants

	May-1999 - Dec-2004	May-1999 - Dec-2004	May-1999 - Dec-2004	May-2001 - Dec-2003	May-2001 - Dec-2003	Jan-2001 - Dec-2003	Jan-2001 - Dec-2003
	(1)¥	(2)†	(3)‡	(4)€	(5)§	(6)‡	(7)§
<i>AdoptLaw</i>	<b>-0.561</b> (0.180)***	<b>-0.425</b> (0.171)**	<b>-0.425</b> (0.159)***	<b>-0.425</b> (0.166)***	<b>-0.431</b> (0.120)***	<b>-0.583</b> (0.242)**	<b>-0.633</b> (0.177)***
<i>No Observations</i>	2584	2652	2652	2652	2613	1404	1365
<i>R-squared</i>	0.499	0.394	0.489	0.488	0.005	0.374	0.009
<i>Homicides t-1/Homicides t-4</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Municipal Force?</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Secretary of Justice?</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Demographic controls?</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i># Neighbors with Law</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>City Dummies?</i>	Yes	Yes	Yes	Yes	No	Yes	No
<i>Period Dummies?</i>	Yes	Yes	Yes	Yes	Yes	Yes	Yes

TABLE V: Source: Secretaria de Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE, and Kahn and Zanetic [2005]. Robust standard Errors in parentheses. \*\*\* = significant at the 1% level, \*\* = significant 5% level. FGLS procedure using variance model for population. Standard error robust to heteroskedasticity. ¥: Excludes São Paulo. †: No model for the variance: all cities with the same weight. ‡: standard errors robust to AR(1) correlation in the error term. € standard errors robust to panel-specific AR(1) correlation in the error term. §: Fixed -effect model with AR(1) model for the error term.

Since there might skepticism about the model for variance, coefficients are re-estimated only correcting standard errors for heteroskedasticity (column (2)). Comparing results in table IV, column (5) and Table V, column (2), one verifies that modelling the variance as a function of population mainly decreases the standard error of estimation, not the estimated coefficient. Columns (3)-(7) show estimates of different specifications for the error term: standard error robustness to 1<sup>st</sup> order autocorrelation in error term (column (3)), standard error robustness to panel specific 1<sup>st</sup> order autocorrelation in error term (column (4)), and a FGLS with the error term as an AR(1) process (column (5)). These robustness checks are important in the light of results in Bertrand, Duflo and Mullainathan [2004], who show that estimated standard errors of difference-in-differences estimates may be underestimated when the error term presents autocorrelation. The fact that the estimated coefficient is still precisely estimated after accounting (or modelling ) for

1<sup>st</sup> order autocorrelation is rather reassuring. Columns (6) and (7) present the same robustness checks for sample restricted to Jan-2001 and Dec-2003. Results are again stronger.

Results also suggest that homicides have persistence: in column (6), when the whole sample is used, all coefficients on the 4 lags of homicides are significant. In this case, the coefficient on *Adoplaw* fall abruptly but we are still able to estimate it precisely ( $p$ -value = 0.019). This suggests that part of the effect of dry law estimated in the previous models is due to reverse causality: past homicide causes adoption of law, and both cause current homicides. Still, the coefficient, albeit smaller, suggests the law has a meaningful practical effect: the counterfactual is that, had the city of São Paulo adopted the law, there would have been 480 homicides less annually. In column (7), the same model as in column (6) is estimated for the sub-sample Jan-2001/December 2003, and results are again stronger.

As with any difference-in-differences procedure, the objection that systematic, time-varying unobserved heterogeneity between the adopting and non-adopting is driving the results can be raised. However, the fact that cities that have adopted the law did so at different points in times takes the time-varying unobserved heterogeneity story to a higher level of difficulty. In order to drive results on the average, it would be necessary that this time-varying unobserved heterogeneity would have occurred at different points in time, in general coinciding with the different dates of adoption in different cities. Although this possibility remains conceivable, it is highly improbable. In the next subsections, we present additional evidence that the suspicion that unobserved heterogeneity may drive results is not warranted.

## *2. Propensity Score Weighting and Sample Trimming*

Even under unconfoundedness, results in tables IV and V could still suffer from biases when the effect of adoption is heterogeneous across cities. This bias arises because, for some adopting cities, there may not be a comparable non-adopting city, or because the distributions of observables are very different between the two groups of cities. Table II suggests that the average observed characteristics of adopting and non-adopting cities are not significantly different, but distributions of unobservables may still be very dissimilar.

To correct biases due to “lack of common support” and heterogeneity in the distribution of observables, we use two methods: propensity score weighting (Rosenbaum and Rubin [1983], RR henceforth) and sample trimming (Crump et al [2006]).

RR show that, under weak conditions, differences among cities in the vector  $\mathbf{X}$  of observables can be summarized by function  $b(\mathbf{X})$ . In particular, we use the propensity score function  $p(\mathbf{X})$ , which is the probability of adoption given observables. By weighting observations by the inverse of the (estimated) predicted probability the “treatment” received (adoption and non-adoption in our case), RR show that observations in the two groups are “homogenized”, and the bias in estimating the Average Treatment Effect (ATE) is eliminated.

We use a slightly modified version of RR method. The propensity score function  $p(\mathbf{X})$  is almost never directly observed, so estimated versions (by logit, for instance) are used. In our case, however, we could estimate it with any precision due to a micronumerosity problem (in the Golberger [1990] sense): there are only 39 observations overall.

To circumvent this problem, we treat every pair month-city as a potential treatment unit, and estimate a probability of treatment by regressing the difference-in-differences dummy  $AdoptLaw_{it}$  on time-invariant and time-varying observables to obtain a predicted probability that a city is treated (has the law in place) at any given month. Clearly, treatment for us is not only adopting the law but also not repelling it. We estimate the following model:

$$AdoptLaw_{it} = p(\mathbf{X}_{it}) + \varepsilon_{it} \tag{3}$$

assuming  $\varepsilon_{it}$  follows a logistic distribution. We get predicted probabilities of adoption given observables,  $\hat{p}(\mathbf{X}_{it})$  and weight observations according to the following weight function

$$weight_{it} = \begin{cases} \frac{1}{\hat{p}(\mathbf{X}_{it})}, & \text{if } AdoptLaw_{it} = 1 \\ \frac{1}{1-\hat{p}(\mathbf{X}_{it})}, & \text{if } AdoptLaw_{it} = 0 \end{cases}$$

Results are in table VI.

Dependent Variable: Homicides per 100th inhabitants

	May-1999 - Dec-2004	Jan-2001 - Dec- 2003	May-1999 - Dec-2004
	(1)	(2)	(3) <sup>†</sup>
<i>AdoptLaw</i>	<b>-0.293</b> <b>(0.167)*</b>	<b>-0.611</b> <b>(0.245)**</b>	<b>-0.437</b> <b>(0.195)**</b>
<i>No Observations</i>	2652	1404	2584
<i>R-squared</i>	0.539	0.431	0.465
<i>Homicides t-1/Homicides t-4</i>	Yes	Yes	Yes
<i>Municipal Force?</i>	Yes	Yes	Yes
<i>Secretary of Justice?</i>	Yes	Yes	Yes
<i>Demographic controls?</i>	Yes	Yes	Yes
<i>Number of Neighbor with Law</i>	Yes	Yes	Yes
<i>City Dummies?</i>	Yes	Yes	Yes
<i>Period Dummies?</i>	Yes	Yes	Yes

TABLE VI: Source: Secretaria de Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE, and Kahn and Zanetic [2005]. Robust standard Errors in parentheses. \*\*\* = significant at the 1% level, \*\* = significant 5% level. FGLS procedure using variance model for population. Additional weighting with inverse of propensity score (for adopting city at adopting periods) and with the inverse of 1 minus propensity score (for non-Adopting cities and adopting cities at non-adopting periods). Standard errors robust to heteroskedasticity. <sup>†</sup> = Excludes São Paulo

The estimated coefficient is still lower but it still arises. In column (1) (comparable to table IV, column (5)), our lowest estimate, the effect of dry law adoption is -0.293, which means 351 homicides in the city of São Paulo or roughly 11% of homicides in 2004. We reject the null hypothesis that this coefficient is zero at the 7.6% level. When the sample is restricted to the Jan-2000/Dec-2003 (and late adopters become controls), the effect is again stronger (20% of homicides in the city of São Paulo), and we reject the null at the 1.6% level. Results are stronger when São Paulo is excluded.

The second method is sample trimming as proposed by Crump et al [2006]. The sample is homogenized by excluding cities too dissimilar, where similarity which is measured by the propensity score estimated in (3). Following Crump et al [2006], we use the 10/90 rule: all observations with estimated propensity scores below 10% and above 90% are excluded, which drastically reduces the sample size. Although the object estimated is no longer the average effect of dry law, but only the effect of dry law for sufficiently similar cities, this procedure attenuates the problem of unobserved heterogeneity. In other words, the procedure trades external validity for increased internal validity. Table VII shows the results.

Dependent Variable: Homicides per 100th inhabitants

	May-1999 - Dec- 2004	Jan-2001 - Dec-2003	May-1999 - Dec- 2004
	(1)	(2)	(3)†
<i>AdoptLaw</i>	<b>-0.556</b> <b>(0.295)*</b>	<b>-0.853</b> <b>(0.387)**</b>	<b>-0.560</b> <b>(0.294)*</b>
<i>No Observations</i>	838	519	799
<i>R-squared</i>	0.559	0.489	0.465
<i>Homicides t-1/Homicides t-4</i>	Yes	Yes	Yes
<i>Municipal Force?</i>	Yes	Yes	Yes
<i>Secretary of Justice?</i>	Yes	Yes	Yes
<i>Demographic controls?</i>	Yes	Yes	Yes
<i>Number of Neighbor with Law</i>	Yes	Yes	Yes
<i>City Dummies?</i>	Yes	Yes	Yes
<i>Period Dummies?</i>	Yes	Yes	Yes

TABLE VII: Source: Secretaria de Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE, and Kahn and Zanetic [2005]. Robust standard Errors in parentheses. \*\*\* = significant at the 1% level, \*\* = significant 5% level. FGLS procedure using variance model for population. Additional weighting with inverse of propensity score (for adopting city at adopting periods) and with the inverse of 1 minus propensity score (for non-Adopting cities and adopting cities at non-adopting periods). Standard errors robust to heteroskedasticity. † = Excludes São Paulo

The estimated effect of the dry law adoption is, if anything, stronger than in table IV. Not surprisingly, some precision is lost since the number of observations falls dramatically, but all null hypotheses are still rejected at the 5.5% level, at a minimum.

### 3. Late Adopters as Controls

In this subsection we take advantage of the fact that different cities adopted at different dates to identify the effect of adoption. Embu-Guaçu, Vargem Grande, São Caetano and Poá adopted dry laws in Jul-03, Dec-03, Jul-04 and Aug-04, respectively. Therefore, if the sample is restricted to end in December 2003, then these three cities become non-adopting cities, which already happened with the estimate in Table IV, column (6). Here, we use by these three cities as the control group. By revealed preference, they are had a very high “propensity” to adopt, given that they eventually did adopt. Results are in table VIII.

Dependent Variable: Homicides per 100thd inhabitants

	May-1999 - Dec-	May 1999 -
	2003	Jul-2003
	(1)†	(2)‡
<i>AdoptLaw</i>	<b>-0.755</b> <b>(0.321)**</b>	<b>-0.957</b> <b>(0.356)***</b>
<i>No Observations</i>	957	888
<i>R-squared</i>	0.529	0.538
<i>Homicides t- 1/Homicides t- 4</i>	Yes	Yes
<i>Municipal Force?</i>	Yes	Yes
<i>Secretary of Justice?</i>	Yes	Yes
<i>Demographic controls?</i>	Yes	Yes
<i>Number of Neighbor with Law</i>	Yes	Yes
<i>City Dummies?</i>	Yes	Yes
<i>Period Dummies?</i>	Yes	Yes

TABLE VIII: Source: Secretaria de Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE, and Kahn and Zanetic [2005]. Robust standard Errors in parentheses. \*\*\* = significant at the 1% level, \*\* = significant 5% level. FGLS procedure using variance model for population. †: Only three late adopters as the controls group: São Caetano, Poá, Vargem Grande. ‡: Only four late adopters as the controls group: Embu-Guaçu, São Caetano, Poá, Vargem Grande. Standard errors robust to heteroskedasticity.

In column (1), the sample is restricted to Dec-03 and only Vargem Grande, São Caetano and Poá are used as a control group. Results are quite strong: -0.755, or 906 homicides in the city of São Paulo. Although the sample size is reduced significantly, the null is rejected at the 1.9% level. In column (2), the sample is further restricted to Jul-03, and Embu-Guaçu is included as a non-adopting city. Results are again stronger.

#### 4. A Placebo Experiment

Suppose we made a coding error and defined the adoption periods in adopting cities a year before the law was actually passed, and results still arose. In this case one would have reason to suspect that something else drove the results found so far. In table VIII some of the models on tables

IV through VIII were estimated with the adoption period defined 12 months before actual adoption.

Dependent Variable: Homicides per 100th inhabitants

	May-1999 - Dec-2004	Jan 2001 - Dec-2003	May-1999 - Dec-2004	May-1999 - Dec-2004	May-1999 - Dec-2003	May 1999 - Dec-2004
	(1)	(2)	(3)†	(4)‡	(5)§	(6)¶
<i>AdoptLaw</i>	<b>-0.084</b> (0.155)	<b>0.064</b> (0.215)	<b>-0.128</b> (0.171)	<b>-0.067</b> (0.112)	<b>0.239</b> (0.462)	<b>-0.033</b> (0.163)
<i>No Observations</i>	2652	1404	2584	2613	957	2652
<i>R-squared</i>	0.555	0.486	0.496	0.0001	0.525	0.539
<i>Homicides t-1/Homicides t-4</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Municipal Force?</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Secretary of Justice?</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Demographic Controls?</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Number of Neighbors with Law</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>City Dummies?</i>	Yes	Yes	Yes	No	Yes	Yes
<i>Period Dummies?</i>	Yes	Yes	Yes	Yes	Yes	Yes

TABLE IX: Source: Secretaria de Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE, and Kahn and Zanetic [2005]. Robust standard Errors in parentheses. \*\*\* = significant at the 1% level, \*\* = significant 5% level. FGLS procedure using variance model for population. Standard errors robust to heteroskedasticity. †: excludes São Paulo. ‡: Fixed-effect model with AR(1) disturbances. §: Only three late adopters as controls: Poá, São Caetano e Vargem Grande. ¶: propensity score weighting as in table VI.

All estimated coefficients are now indistinguishable from zero. When it is still negative, the null hypothesis that it is zero is not rejected because the estimated coefficient falls by one order of magnitude relative to those estimated so far. When the sample is restricted to the Jan-2001/Dec-2003 period (column (2)), and when only late adopters are used as controls (column (5)), the estimated effect becomes positive but again statistically insignificant. None of following work: Excluding São Paulo (column (3)), FGLS fixed-effect model with an AR(1) model for error term (column (4)), and propensity score weighting (column (5)).

### 5. Results: Beggar-thy-Neighbor?

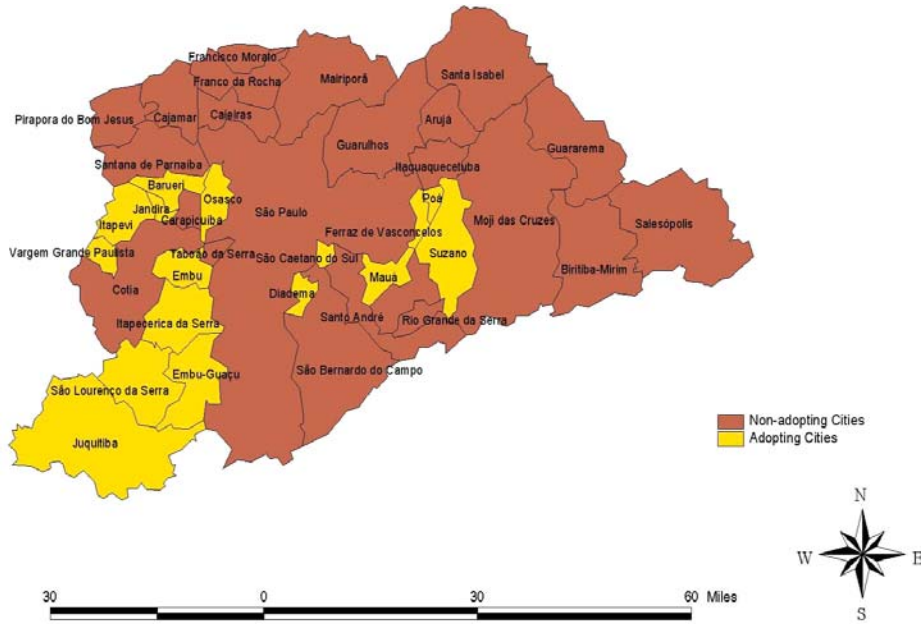
Adopting the dry law in one city might just shift drinking to non-adopting neighbors. In this case, homicides might actually fall in adopting cities, but would increase in neighboring non-adopting cities. They would be overall ineffective but we would still estimate the a significant difference in the dynamics of homicides between adopting and non-adopting cities associated with the adoption of dry laws.

To account for spillover effects, we propose a weighting scheme that underweights observations from adopting city-pairs with more of non-adopting

neighbors and, conversely, over-weights observations from non-adopting city-month pairs with more of non-adopting neighbors. More specifically:

$$weightNEI_{it} = \begin{cases} \frac{\text{Number of adopting neighbors} + 1}{\text{Number of neighbors}}, & \text{if } AdoptLaw_{it} = 1 \\ 1 - \frac{\text{Number of adopting neighbors} + 1}{\text{Number of neighbors}}, & \text{if } AdoptLaw_{it} = 0 \end{cases}$$

With this weighting scheme, the same adopting city receives different weights overtime if, after her adoption, her neighbors adopt the law. In this example, later observations receive more weight.<sup>28</sup> Results are in table X and figure III shows a map of the SPMA, with adopting and non-adopting cities identified geographically.



<sup>28</sup>The number 1 is added to the number of adopting neighbors to avoid giving zero weight to all those observations from adopting cities (at adopting periods) with zero adopting neighbors

Dependent Variable: Homicides per 100thd inhabitants

	May-1999 - Dec-2004	Jan-2001 - Dec-2003	May-1999 - Dec-2004
	(1)	(2)	(3)†
<i>AdoptLaw</i>	<b>-0.408</b> (0.150)***	<b>-0.561</b> (0.247)**	<b>-0.599</b> (0.173)***
<i>No Observations</i>	2652	1404	2584
<i>R-squared</i>	0.565	0.521	0.512
<i>Homicides t-1/Homicides t-4</i>	Yes	Yes	Yes
<i>Municipal Force?</i>	Yes	Yes	Yes
<i>Secretary of Justice?</i>	Yes	Yes	Yes
<i>Demographic controls?</i>	Yes	Yes	Yes
<i>Number of Neighbor with Law</i>	Yes	Yes	Yes
<i>City Dummies?</i>	Yes	Yes	Yes
<i>Period Dummies?</i>	Yes	Yes	Yes

TABLE X: Source: Secretaria de Secretaria Estadual de Segurança Pública de São Paulo, Fundação SEADE, and Kahn and Zanetic [2005]. Robust standard Errors in parentheses. \*\*\* = significant at the 1% level, \*\* = significant 5% level. FGLS procedure using variance model for population. Additional weighting with corrected percentage of adopting neighbors (for adopting city at adopting periods) and with 1 - minus corrected percentage of adopting neighbors (for non-adopting cities and adopting cities at non-adopting periods). Standard errors robust to heteroskedasticity. † = Excludes São Paulo

Relative to table IV, results are very much unchanged. Again the same patterns arise: the effect of dry law adoption is well estimated and have practical significance; results are even stronger when the sample is restricted to Jan-01/Dec-03, and when São Paulo is excluded.

## D. Semi-Parametric Evidence

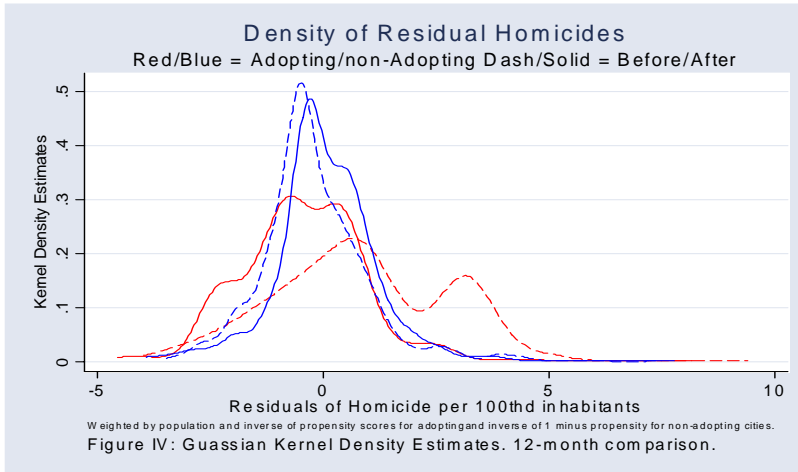
Results in tables IV to IX, although strongly suggestive, refer only to the average of the distribution of homicides. More generally, one would like to be certain that the distribution of homicides in adopting and non-adopting cities shifted in a compatible way with the shift in mean. This sub-section provides semi-parametric evidence in this direction.

Except for the absence of the difference-in-differences regressor *AdoptLaw*, an equation very similar to (1) is estimated:

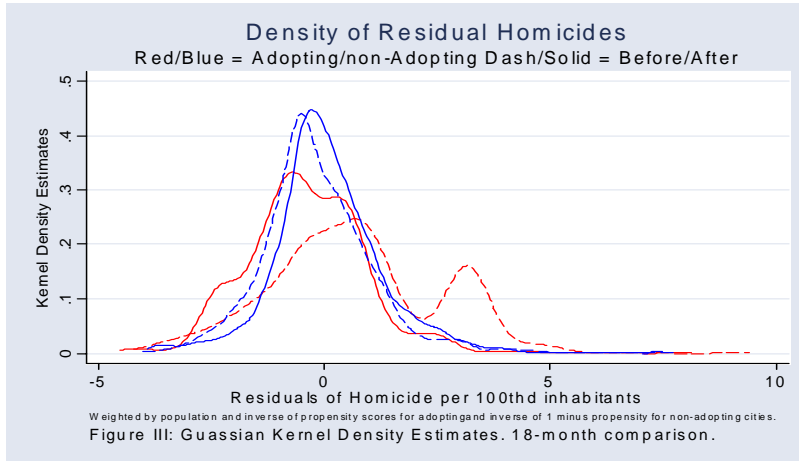
$$Homicide_{it} = \gamma_0 + \mathbf{Month}_t + \mathbf{City}_t + \mathbf{Controls}_{it} + \epsilon_{it} \quad (4)$$

using the model for the variance defined in (2).

The residuals of (4) are constructed and, using kernel methods, four densities of  $\hat{\epsilon}_{it}$  are estimated: before and after adoption, for adopting and non-adopting cities. Since adoption is non-simultaneous, it is not clear what the relevant comparison periods are. We follow the same procedure as we did to compute the summary statistics. For adopting cities, an observation month-city pair 18 or 12 months before and after adoption. For non-adopting cities, we compare the 12 or 18-month period before and after the average adoption, May 2002.<sup>29</sup> The kernel estimation uses the propensity scores as weights. Figures III and IV show the results.



<sup>29</sup>The procedure excludes the two late adopters, Poá and São Caetano.



Visual inspection of figures III and IV show that the results for the mean also arise for the whole distribution of homicides. After controlling for city, month specific effects, and for demographics, the density of homicides changes drastically in adopting cities, in the direction of a reduction homicides, as one would expect given the results so far. For non-adopting cities, if anything, the density of homicides changes towards more homicides. Results are similar regardless of the whether the comparison period is 12 or 18 months.

## VI. Discussion

The error term in (1) contains all factors that determine homicides other than those included as regressors. There is always the possibility that  $\epsilon_{it}$  is decreasing (or increasing less) for those cities that adopted dry laws, relative to cities that did non-adopting cities, and therefore this unobserved heterogeneity would have driven results. Since  $\epsilon_{it}$  is by construction non-observed, one cannot dismiss this possibility completely in spite of the fact that, when we restrict the sample to “similar” adopting and non-adopting cities (table V), results do not differ meaningfully from when the full sample is used (table IV versus table VI).

Police, which belongs to  $\epsilon_{it}$ , is a candidate for spuriously producing the results.<sup>30</sup> Police could respond to crime, as the economics of crime literature suggests, and crime falls with police (Marvell and Moody [1996], Corman and Mocan [2000], Di Tella and Schargrotsky [2004], and Levitt [2002]). Although summary statistics in table II and results in table III suggest that cities did adoption was not a reaction to spikes in homicide, it is still conceivable that, for unknown reasons, police increased in adopting relative to non-adopting cities precisely around the adoption periods. In this case, omitting police in (1) would be problematic. The institutions of police enforcement in the state of São Paulo, however, suggest otherwise.

By constitutional mandate, the vast majority of enforcement is done at the state, not the city level, which mitigates but does not solve problems caused by the omitting polices: state enforcement authority itself could have put more police in the streets in adopting cities around the time of adoption.<sup>31</sup> However, the state constitution also mandates that local police force size is determined quinquennially, based on the population, so that every city has roughly the same number of policemen per capita over long periods of time. Some reallocation could occur when several cities are covered by the same batallion.<sup>32</sup> There is some flexibility of allocating policemen among battalions, but there is usually one battalion per city, which pretty much excludes the possibility of large deployment of police in given one city in response to crime, at least not within the span of our sample.

Although omission of police does not seem to be a serious threat to our estimation strategy, estimates could capture other omitted competing policy reactions. Investment in community centers in crime ridden places, investment in public lighting, neighborhood watch programs, municipal level advertising of DISQUE DENÚNCIA are among them.<sup>33</sup> Although past crime

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<sup>30</sup>Unfortunately, we do not have access to police data on the city level, only the SPMA level.

<sup>31</sup>In all Brazilian states there are two police forces. The Civil Police, which is investigative and preventive, and the Military Police, which does deterrence policing.

<sup>32</sup>Police's organizational structure is as follows. The smallest unit is the police districts, which are part of a companies that, in turn, are part of batallions, the largest unit. While large cities have more than one batallion, some batallions cover more than one city. It is very rare that more than three cities are covered by the same batallion

<sup>33</sup>DISQUE DENÚNCIA is a type 109 number specifically for denouncing crimes anonymously. Three adopting cities, Diadema, Jandira and Itapevi passed a law imposing advertising of DISQUE DENÚNCIA on several municipal medias. The adoption was, however, always at least a year prior to adoption of dry laws. It is arguable that some

is controlled for in the (1) it is still conceivable that these measures could produce the results if either they were adopted simultaneously with dry laws, or before dry law adoption and these other policies take a longer to produce results.

Regardless, competing policy reactions (police, community policing, etc) are more problematic if dry laws were adopted in periods in which crime was historically high. Figure I already shows that adoption of dry laws was not at peak of homicides when adopting cities are taken as whole. Tables II and III suggest that adoptions did not occur in particularly high-crime periods. Table XI takes a further and closer look at homicides by city, 6 and 12 months prior to law adoption and over the period from Jan/1997 to adoption.

Homicides per 100thd inhabitants			
	12 previous months	6 previous months	Jan-1997 to adoption
<i>All non-adopting cities</i>	3.67 (0.39)	3.62 (0.39)	4.02 (0.36)
<i>Barueri</i>	4.72 (1.51)	4.01 (1.44)	4.55 (1.33)
<i>Diadema</i>	5.16 (1.46)	5.60 (2.03)	6.83 (2.23)
<i>Embu</i>	6.12 (2.09)	4.94 (1.76)	6.58 (2.04)
<i>Embu – Guaçu</i>	5.86 (2.67)	5.79 (3.45)	5.13 (2.74)
<i>Ferraz de Vasconcelos</i>	3.49 (1.22)	3.50 (1.92)	3.83 (1.97)
<i>Itapecerica</i>	6.55 (1.30)	6.36 (1.00)	6.94 (2.10)
<i>Itapevi</i>	5.44 (1.80)	5.75 (1.52)	6.34 (2.63)
<i>Jandira</i>	4.63 (1.82)	5.09 (2.25)	4.62 (1.86)
<i>Juquitiba</i>	1.52 (3.31)	1.21 (1.87)	2.23 (3.19)
<i>Mauá</i>	4.28 (1.28)	4.64 (1.53)	4.28 (1.19)
<i>Osasco</i>	4.43 (0.66)	4.31 (0.49)	3.64 (1.03)
<i>Poá</i>	1.14 (1.02)	0.48 (0.53)	1.37 (0.92)
<i>São Caetano</i>	0.60 (0.52)	0.60 (0.55)	0.93 (0.82)
<i>Suzano</i>	2.43 (1.25)	2.39 (1.40)	2.87 (1.27)
<i>Vargem Grande Paulista</i>	0.64 (1.16)	0	2.39 (3.13)

Table VIII Source Secretaria de Segurança Pública de São Paulo, Kahn and Zanetic [2005]. For non-adopting cities, period of reference is July 2002. For adopting cities, date of adoption as in table I

Table X does not bring any surprise. Homicides in adopting cities were not especially high in the year or the six-month period prior to the adoption measures adopted in the state of São Paulo over the period improved the efficacy of police work. The most noticeable is the creation of a large database of local crime, INFOCRIM, similar to compstats used in many other cities. Again, this affected all cities in the SPMA uniformly.

of the dry law. Only Osasco out of the 16 adopting cities crime was higher than historical levels in a significant way. In non-adopting cities crime was also lower than the historical level. This suggests that cities in the SPMA follows quite uniformly general trends in homicide. It does not seem that adopting cities were at much different point in the cycle of homicides around the period that dry laws were adopted.

## VII. Conclusion

At the most conservative estimate, homicides fell by 8% in the SPMA due to the adoption of dry laws. Evaluating at an “average” estimate, the reduction was 20%. This means some 429 to 936 additional lives would be spared were dry laws adopted in the 23 non-adopting cities. These results are robust to a wide range of controls, to the presence of outliers, and do not seem produced by unobserved heterogeneity. Although a welfare costs of adopting the law were not computed, this paper suggests that benefits from targeted restriction of recreational alcohol consumption could be substantial, at least in high-crime areas such as the SPMA.

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Departamento de Economia PUC-Rio  
Pontificia Universidade Católica do Rio de Janeiro  
Rua Marques de São Vicente 225 - Rio de Janeiro 22453-900, RJ  
Tel.(21) 35271078 Fax (21) 35271084  
[www.econ.puc-rio.br](http://www.econ.puc-rio.br)  
[flavia@econ.puc-rio.br](mailto:flavia@econ.puc-rio.br)