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

Ciro Biderman, João M P De Mello† 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. Several more have followed suit, or are likely to do so in the near future. Policy makers and the general press have argued that these measures reduce crime. In this paper, we use a particular feature of the adoption of laws in the São Paulo Metropolitan Area (SPMA) to estimate the effect of dry laws on the ultimate form of violent crime: murder. 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%, with an even larger effect in high crime cities. Results are robust to inclusion of a large set of controls, to propensity score weighting, to outliers, and to correction possible spillover effects from adopting to non-adopting cities.

KEYWORDS: *Dry Law, Alcohol, Crime*

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*Escola de Administrçăo 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 and 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.

†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!" Cassio, 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 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 assess the causal effect of restrictions on recreational sales of alcohol (dry laws, hereafter), which are mandatory night closing hours for restaurants and bars, on homicide rates. 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. This allows us to compare the dynamics of homicides between two groups of cities in the SPMA: one that adopted dry laws, and one that did not. With this strategy, it is possible 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 are associated with, at a minimum, a 10% reduction in homicides.

Besides the presence of adopting and non-adopting cities, restricting attention to the SPMA is particularly interesting for two additional reasons. First, adoption of dry law occurred over a short period of time, between March-2001 and August-2004, but was not simultaneous.Different periods of adoption allow for the same city to "act" as both "control" and "treatment". If, however, adoption is too spread out in time, the odds of capturing simultaneous concurring events increase. Thus, non-simultaneous adoption over a short period of time is valuable empirically. Second, cities that are part of the same metropolitan area are subject to roughly the same economic and socio-political shocks, which guarantees a minimum degree of guarantee of homogeneity across observations, i.e., cities.

From an empirical perspective, the SPMA are application has, however, one important disadvantage: adoption was a choice of the cities. For difference-in-differences methods to work one needs the assumption of unconfoundedness (Imbens [2000], Rosenbaum and Rubin [1983]), meaning 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 account for non-random assignment both by using institutional knowledge and the empirics of our application. First, we show that the timing of adoption does not seem related to observables that could potentially trigger other (unobserved) policy reactions, such as recent surges in homicides that might cause a adoption and more policing. In a nutshell, although more violent cities tend to adopt, they did not do so in response to increases in homicides. Second, we argue that decision-making is such that it is very unlikely that policing and adoption could be confounded. Third, we use a modified version of propensity score weighting and sample trimming (Imbens [2000], Crump et. al. [2006]), which boil down to "homogenizing" the adopting and non-adopting cities with respect to observables. Although these methods do not guarantee the "homogeneity" among time-varying unobservables, which is what we need, if cities are similar in terms of observables, they are less likely to differ significantly with respect to unobservables. Finally, but at less importantly, non-simultaneity of adoption helps to identify the effect of dry laws. By considering only periods before adoption by late adopters, they become a special "control" group that has, by construction, a revealed very high propensity of adopting the dry law.

The SPMA is not an isolated example of adoption of such policies. In fact, Bogotá, Colombia was the first Latin American metropolis to adopt such restrictions.[1] Following the examples of cities in the SPMA, several other Brazilian cities have followed suit. Large Latin American cities are of particular policy interest due to the high crime rates observed over the last

[1] In the end of 1995 Bogotá adopted a law similar to the ones we study, the Ley Zanahoria, 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..

twenty years, and it is hard to understate the social costs of crime. Soares [2006, forthcoming], for example, estimates welfare costs of crime of 38% of GDP in Brazil.[2]

Despite large welfare sums at stake, and several examples of dry law adoption, hard evidence on their effectiveness is surprisingly scant. General press cites examples of succesful crime reductions, and sometimes associate it with dry laws. This evidence always compares murder rates before and after adoption of dry laws, and infers the causal effect (or lack thereof) from decreasing (non-decreasing) rates. Homicides, however, decreased in general in the SPMA over the period, including in non-adopting cities (see figure I). Furthermore, several concurrent events might have caused this decline.[3] If restricting alcohol consumption is socially costless, precise causal inference would not be so relevant as a matter of policy. However, Mr. Franklin, among others, would tend to think they are costly.[4]

The paper is organized as follows. Section II presents the economics and the criminology of the alcohol - crime and recreational alcohol - crime *nexuses*. Section III presents the chronology of the events. Section IV describes the data used to implement the empirical strategy, which is presented at section V. Section VI has the results. Finally, section VII discusses and concludes the article.

II. The Economics and the Criminology of

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

[3] *The Economist*, 10/20/2005, reporting a story on Diadema (a 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 agains 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.

[4] 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 weedends, 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.

Alcohol Consumption

Our estimates and the economics of crime literature suggest that not only focused recreational restrictions are less costly but also more effective than outright prohibition and taxation. Studies on the American prohibition in the 1920s portrait hard interventionism very badly (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]). This evidence, and the conventional wisdom in the economics literature, provide further interest in studying the effects of dry laws. While the economics of addiction teaches us that price interventions and prohibition is ineffective in shifting consumption away from strong habit formation goods such as alcohol, 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. Given the option, it is not hard to guess which one Mr. Franklin would choose.

Seemingly opposite to the economics of crime literature, there is ample evidence in the criminology and public health literatures that alcohol is associated with misbehavior. McClelland et. alli. [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.[5] In our empirical setting, Gawryszewski et. alli. [2005] found, using toxicological blood exam data in the city of

[5] 0.08 is the legal driving limit in most US states.

São Paulo, that 40.4% of homicide victims showed signs of alcohol drinking. This evidence, however, has two major *caveats*: omitted variable and sample selection. The majority of the studies (Hutchison at. alli. [1998], Greenfeld [1998], and Gawryszewski et. alli. [2005] are but a few examples) use actual or inferred blood alcohol level of offenders or victims. Although strong, the evidence is not entirely convincing as a causal link. The main reason is the lack of controlling for factors that might determine, simultaneously, alcohol consumption *and* violent behavior (or being a victim of), such as child abuse and psychological disturbances. Sample selection implies overestimation of the alcohol-crime *nexus* since sober offenders (victims) are less likely to get caught (be victimized) (Martin [2001]).

While individual inmate and booking data studies document a strong correlation between alcohol and crime, studies that use aggregate data have more tenous positive association between results. Take for example the question of whether the alcohol-violent crime *nexus* is magnified when consumed is 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.[6] 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 most likely 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 of alcohol on the ultimate form of violence: murder. Our conjecture is that this lack of evidence is due to three facts. First, since murder, relatively to assault and robbery, is a rare occurrence, aggregate murder measures (e.g., rates per 100thd inhabitants) are 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 consump-

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

tion and homicides for the omitted variable and selection problems outlined above.

Even if one is convinced about the causal link from alcohol to violence, whether dry laws in the SMPA would have a impact on murder is far from obvious at a first glance. Enforcement could be lacking, specially in fragile institutional settings such as the SPMA. Also, cross-section variation, which makes the SPMA attractive as case study in the first place, may produce a "beggar-thy-neighbor effect": bar drinking shifts from one city to another, and the dry law becomes overall ineffective. Lastly, the strongest evidence is on the *drinking*-crime *nexus*, not necessarily *bar drinking*-crime *nexus*, specially since there could always be the case that bar habitués are more prone to violence than the average population. Although not self-evident, our estimates strongly indicates restricting bar drinking does reduce homicides.

The SPMA is a particularly attractive setting for testing dry laws are effective in reducing homicides for both empirical and conceptual reasons. Most prominent among the later, it is a weak law-and-order, high-homicide metropolis.[7] First, among the former, is presence of cross-section variation in how easy it is to consume alcohol in bars. Second, this cross-section variation varies over time. 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 not conceivable that the average amount of child abuse and psychological disturbances changes significantly in short periods of time.

Our results are 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.[8]

[7] 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.

[8] 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,

The paper is organized as follows. Section II presents the chronology of the events. Section III describes the data used to implement the empirical strategy, which is presented at section IV. Section V has the results. Finally, section VI discusses and concludes the article.

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 It has roughly 18 million inhabitants.[9] 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, for example, 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.[10]

Soon after several cities followed suit, adopting laws very similar to Baueri'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.

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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.

[9] Projection for 2005 based on the 2000 census.

[10] See www.propagandasembebida.org.br, in Portuguese.

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
Itapecerica	Jul-02
Mauá	Jul-02
Ferraz de Vasconcelos	Sep-02
Embu	Dec-02
Osasco	Dec-02
Embu – Guaçu	Apr-03
Vargem Grande Paulista	Dec-03
São Caetano	Jul-04
Poá	Aug-04

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

IV. Data

Homicides are from the INFOCRIM, a database from the Secretaria de Segurança do Estado de São Paulo. [11] Homicides data is based on all police reports, and include murders and nonnegligent manslaughter, but exclude manslaughter and car accident deaths. This is important for our purposes since alcohol may trivially cause accidental deaths.

Homicide data usually suffers very little from under-reporting, since investigation is mandatory. Evidently, there may be a discrepancy due to taxonomy, but 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.

[11] 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.

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.[12] 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].

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-section 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 a Metropolitan area are more or less subject to 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 2002-2003 biannual. These two features are desirable for two reasons. They attenuate the problem of unobserved heterogeneity between adopting and non-adopting cities,

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

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 town is viewed as a successful example of policy-induced crime reduction.[13] Diadema had, during the late 1990s, one of the highest crime rates in the country.[14] 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.[15] 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)

The case of the SPMA is an unique empirical opportunity. In very few instances there is both cross-section *and* time-series variation in alcohol restrictions. In Bogotá, for instance, adoption was uniform, and time-series variation alone seem doomed to capture competing factors, specially because alcohol adoption usually concurrently with other crime fighting measures. Short-term time-series variation could be a solution. For example, on elections' day in Brazil, a general dry law is operative, starting 12AM. In this sense, one could compare elections days with other days. This strategy would, however, fail because elections days are peculiar in several dimensions, increased policing to repress illegal advertising being but one dimension.

A. Summary statistics: Adopting and Non-Adopting

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

[14] See *The Economist*, 10/20/2005.

[15] See *The Economist*, 10/20/2005.

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.[16] 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 cites 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 drop in non-adopting cities, albeit much less, from 3.67 to 3.43 per 100thd inhabitants around the average period of adoption.[17]

[16] This rank refers to cities that have a population over 500,000.

[17] 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)
Homicides per thd inhabitants		
Jan-1997/Dec-2004	4.22	3.42
	(0.92)	(0.61)
12 months before adoption†	4.62†	3.67‡*
	(1.31)	(0.39)*
12 months after adoption†	3.71	3.43‡
	(1.79)	(0.18)
Population < 50,000	2.30	1.44
	(3.46)	(2.68)
100,000<Population< 150,000	3.42	2.78
	(2.69)	(2.20)
Population > 150,000	4.21	3.51
	(2.30)	(1.62)
Demographics		
Population (in thd)	186	652
1997-2004	(164)	(2100)
Population (in thd)*		208
1997-2004		(269)
Population	210	214
12 months before adoption†	(177)	(275)
Population	215	219
12 months after adoption†	(179)	(281)
%Male Population, age 15-30	14.5	13.7
1997-2004	(0.7)	(0.6)
%Male Population, age 15-30	14.6	13.7
12 months before adoption†	(0.4)	(0.5)
%Male Population, age 15-30	14.5	13.5
12 months after adoption†	(0.5)	(0.6)
Educational Attainment		
High school drop-out rate (2002)	8.28	7.52
	(2.66)	(2.44)
Number of years of Schooling	6.97	7.00
	(1.08)	(0.79)
Income		
Income per capita	8847	8293
1999-2004	(7178)	(4623)
Income per capita	8484	8641
12 months after adoption†	(6651)	(5086)
Income per capita	9535	9757
12 months after adoption†	(6990)	(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 cites 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. It not surprising that these proportion barely changes during the period since changes in age distribution are slow. 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 more in non-adopting cities over the 1999-2000 period.[18] 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.[19] 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 our estimation strategy to work.

B. The Decision to Adopt the Law

In our application, adoption is not randomly assigned but rather a decision of the city. 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. In this sub-section we assess whether this threat is empirically relevant, and to do so we estimate

[18]Number of years of schooling is only availble for 2000.

[19]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.

a duration model for the timing of adoption is estimated (see Kiefer [1988] and Jenkins [1995]).

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, as a base line level measure, average homicides in 2000. This allows to account for whether overall violence affects the adoption decision.

We also include a set of city-level law enforcement variables (presence of a municipal secretary of justice and of a municipal police force) to account for local-level attitude towards law and order.[20] Some demographic controls, such as income, population and male population between 15 and 30 are included because they can both affect homicides and, for political economy reasons, the adoption of dry law.

In two of our specifications time and time squared are included to allow for time varying hazard rates. Adoption occurs over time and, ss 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. This accounts for the fact that adoption may occur as emulation, and because of fear of spillover effects. Table III presents the results.

[20] 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)
	Marginal Effects		
Dynamics of Homicides			
Homicides t - 1	2.15E-05	9.78E-05	
	(0.06)	(0.68)	
Homicides t - 2	-9.06E-04	-2.20E-04	
	(-1.77)*	(-1.16)	
Homicides t- 3	-4.93E-04	-9.41E-05	
	(-1.17)	(-0.57)	
Homicides t-4	1.28E-04	2.60E-05	
	(-0.34)	(0.16)	
Average Homicides			-3.96E-05
			(-0.46)
City-Level Enforcement			
Municipal Force?	1.79E-03	-8.00E-04	8.42E-04
	(0.54)	(-0.48)	(0.69)
Secretary of Justice?	3.61E-04	5.11E-04	-5.01E-05
	(0.10)	(0.67)	(-0.45)
Demographic controls			
City Level GDP †	2.80E-03	7.25E-04	7.71E-04
	(1.09)	(0.70)	(0.71)
Population †	1.17E-03	5.57E-03	6.03E-03
	(0.04)	(0.46)	(0.48)
Male Population, 15 and 30 years†	-3.33E-03	6.47E-03	-6.91E-03
	(-0.10)	(-0.52)	(-0.54)
Time Trends			
Time		1.70E-03	1.72E-03
		(2.35)**	(2.36)**
Time Squared		-5.69E-06	-5.90E-06
		(-2.27)**	(-1.16)
Number of Neighboors that Adopted	1.79E-03	4.15E-04	4.36E-04
	(2.06)**	(2.01)**	(2.02)**
Non-Time Varying Controls			
Base Line Homicides	3.11E-03	1.01E-03	1.04E-03
	(2.86)***	(2.74)***	(2.73)***
Number of Observations	2249	2249	2249
Pseudo R-squared	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 only marginally and with the wrong sign. As group, although one rejects the null hypothesis that all frou lags can be excluded, we only do so marginally (p-value = 7.4%), and 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.[21] Since homicides are rather noisy, in column (3), instead of the four lags, we include the 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 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 none of the three specification. Once again, 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

[21] The p-value of the F-test on joint significance is 56.2%.

of 5 and 4 per 100thd inhabitants for the adopting and non-adopting groups, respectively, in the first semester of 1999 (roughly period 100).

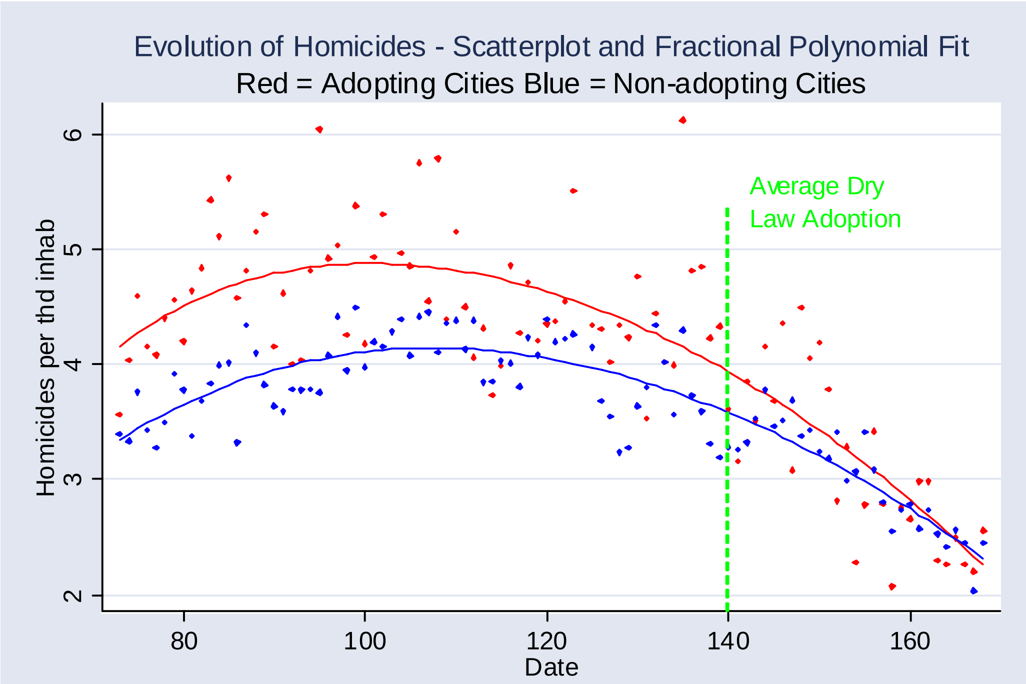


Figure I Source: Secretaria de Segurança do Estado de São Paulo

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.[22] While for non-adopting cities (blue) the scatterplot 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

[22] 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.

that we concentrate only on the period in which homicides are unambiguously falling.

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

In this section we estimate a difference-in differences model to evaluate the impact of dry law adoption. The model allows us to control for considerable competing variation that may cause both adoption and homicides, which is crucial for two reasons. First, there is heterogeneity both between adopting and non-adopting cities, and within both groups. Second, adoption is not exogenously given, but cities self-select into it. With only time-series variation, there is just not enough variation to account for the dynamics of homicide that might cause both adoption of dry laws, i.e., high crime today causes adoption tomorrow.

In this subsection, the panel nature of the data is fully explored, treating each city-period pair as an observation. The full use of the panel structure of the data permits a higher level of confidence in the estimates for several reasons. One, which is explained below, is that variation in adoption time mitigates the possibility of unobserved heterogeneity driving results. Second, it provides sufficient variation to estimate the effect of dry laws above and beyond covariates such as adoption of other policies, such as establishment of a Municipal Police Force, establishment of a Municipal Secretary of Justice, and for population changes. Third, With only time-series variation, there is just not enough variation to account for the dynamics of homicide that might cause both adoption of dry laws, i.e., high crime today causes adoption tomorrow. By introducing cross-section variation in adoption and timing of adopting, enough variation permits us to estimate a model that is richer is terms of homicide dynamics. In this case, we can be more confident that the "endogeneity" of adoption is at least partially accounted for when the dynamics of homicides are controlled for. Fourth, using cross-section information allows us to "homogenize" adopting and non-adopting cites according to observables, and therefore further mitigate the non-exogenous adoption problem. Lastly, we are now able to assess whether dry laws work because they shift crime to their neighbors (the "Beggar-thy-Neighbor" effect), and evaluate whether dry laws are particularly valuable in high homicide cites.

The main coefficient of interest is a difference-in-difference Parameter, and 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 1. If the adoption of alcohol laws induces a drop in crime, this coefficient should be *negative*. $\mathbf{Month}_t$ an $\mathbf{City}_t$ are month and city dummies, respectively.

$\mathbf{Controls}_{it}$ include, depending on the specification, variables that may affect both homicides and adoption. In almost all specifications a set of **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. Law adoption is a decision and, although results in table III suggest otherwise, one would 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 **Month**. They control for time-specific shifts in crime, such as unemployment in the whole SPMA, seasonal factors, general trends in homicide, etc.[23]

Demographic controls include city population, proportion of population in crime prime age (between 15 and 30 years), and per capita income.[24]

[23]observations on these variables. High school drop-outs, for example, are available 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.

[24]These controls are only available annually, so we replicate the annual value for all months in the year.

All three variables could affect the homicide rate, and may also determine adoption of dry laws.[25] Finally, two possible concurring policy changes 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.[26]

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. Our data is at the city level, and there is large heterogeneity in city size in the data. Since homicides are a not so common occurrence, observations from small cities are much noisier than those from larger cities, i.e., the variance of ϵ_{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 Feasible Generalized Least Squares (FGLS) procedure assumes that:

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

1. *Results with all adopting cities*

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 neighboors. The coefficient on the

[25] 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.

[26] 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.

variable *AdoptLaw* (γ_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.[27]

Dependent Variable: Homicides per 100thd 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)
AdoptLaw	**-0.632**	**-0.392**	**-0.454**	**-0.434**	**-0.432**	**-0.588**
	(0.138)* **	**(0.136)*** **	**(0.145)*** **	**(0.159)*** **	**(0.156)*** **	**(0.258)*** **
No Observations	2808	2652	2652	2652	2652	1404
R-squared	0.530	0.556	0.557	0.557	0.558	0.491
Homicides t-1/Homicides t-4	No	Yes	Yes	Yes	Yes	Yes
Municipal Force?	No	No	Yes	Yes	Yes	Yes
Secreatry of Justice?	No	No	Yes	Yes	Yes	Yes
Demographic Controls?	No	No	No	Yes	Yes	Yes
Number of Neighboors with Law	No	No	No	No	Yes	Yes
City Dummies?	Yes	Yes	Yes	Yes	Yes	Yes
Period Dummies?	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) shows 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 neighboors are included (columns (4) and (5)).

In column (5), the sample is restricted to Jan-2001/December 2003. Restrciting the sample 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 not yet operative. In this case these three cities

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

effectively become part of the control group. Second, longer period are more likely to competing factors. Again, results, if anything, are stronger.

Table V shows further sample robustsness 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 as (5) in table IV but in which São Paulo is excluded. Results are stronger.

Dependent Variable: Homicides per 100thd 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)§
AdoptLaw	**-0.561** **(0.180)*****	**-0.425** **(0.171)****	**-0.425** **(0.159)*****	**-0.425** **(0.166)*****	**-0.431** **(0.120)*****	**-0.583** **(0.242)****	**-0.633** **(0.177)*****
No Observations	2584	2652	2652	2652	2613	1404	1365
R-squared	0.499	0.394	0.489	0.488	0.005	0.374	0.009
Homicides t- 1/Homicides t-4	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Force?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Secretary of Justice?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Dummies?	Yes	Yes	No	Yes	No	Yes	No
Period Dummies?	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^{st} ordem autocorrelation in error term (column (3)), standard error robustness to panel specific 1^{st} ordem 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 face of the 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^{st} ordem 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 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 likely. 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 characteristics of adopting and non-adopting cities are not significantly different, but distributions may still be very different.

To correct biases due to "lack of common support" and hetorogeneity in the distribution of observables, we use two methods : propensity score weighting (Rosenbaum and Rubin [1983], RB 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 we summarized by function $b(\mathbf{X})$. In particular, the use the propensity score fucntion $p(\mathbf{X})$, 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 "homigeneized", and the bias eliminated.

We follow RR and use a slightly modified version of their method. The propensity score fuction $p(\mathbf{X})$ is almost never directly observed, so estimated versions (by logit, for instance) are used. In our case, however, we cannot estimate it with any precision due to micronumerosity (in 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:

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

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

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

Results are in table VI.

Dependent Variable: Homicides per 100thd inhabitants

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

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 (??). Following Crump et al [2006], we use the 10/90 rule: all observations with estimated propensity scores below 10Although 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 gives up external validity to increase internal validity. Table VII shows the results.

Dependent Variable: Homicides per 100thd inhabitants

	May-1999 - Dec-2004	Jan-2001 - Dec-2003	May-1999 - Dec-2004
	(1)	(2)	(3)†
AdoptLaw	**-0.556** **(0.295)***	**-0.853** **(0.387)****	**-0.560** **(0.294)***
No Observations	838	519	799
R-squared	0.559	0.489	0.465
Homicides t- 1/*Homicides t-* 4	Yes	Yes	Yes
Municipal Force?	Yes	Yes	Yes
Secretary of Justice?	Yes	Yes	Yes
Demographic controls?	Yes	Yes	Yes
Number of Neighboor with Law	Yes	Yes	Yes
City Dummies?	Yes	Yes	Yes
Period Dummies?	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. All city-month pairs with propensity smaller than 10% and larger than 90% are excluded. Standard errors robust to heteroskedasticity. † = Excludes São Paulo

The estimated coefficients are, if anything, stronger than in tables IV. Not surprisingly, precision is somewhat lost since the number of observations falls dramatically, but all null hypothesis are rejected at, at least, the 5.5

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-2003	**May 1999 - Jul-2003**
	(1)†	(2)‡
AdoptLaw	**-0.755** **(0.321)****	**-0.957** **(0.356)*****
No Observations	957	888
R-squared	0.529	0.538
Homicides t-1/Homicides t-4	Yes	Yes
Municipal Force?	Yes	Yes
Secretary of Justice?	Yes	Yes
Demographic controls?	Yes	Yes
Number of Neighboor with Law	Yes	Yes
City Dummies?	Yes	Yes
Period Dummies?	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 in reduced significantly, the null is rejected at the 1.9is 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, most likely something else drove the estimated difference in the dynamics of homicides between adopting and non-adopting cites. So we made the coding on purpose and defined the adoption period twelve months before actual adoption. Table VIII shows the results.

Dependent Variable: Homicides per 100thd 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)¥
AdoptLaw	**-0.084**	**0.064**	**-0.128**	**-0.067**	**0.239**	**-0.033**
	(0.155)	**(0.215)**	**(0.171)**	**(0.112)**	**(0.462)**	**(0.163)**
No Observations	2652	1404	2584	2613	957	2652
R-squared	0.555	0.486	0.496	0.0001	0.525	0.539
Homicides t-1/Homicides t-4	Yes	Yes	Yes	Yes	Yes	Yes
Municipal Force?	Yes	Yes	Yes	Yes	Yes	Yes
Secreatry of Justice?	Yes	Yes	Yes	Yes	Yes	Yes
Demographic Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Number of Neighboors with Law	Yes	Yes	Yes	Yes	Yes	Yes
City Dummies?	Yes	Yes	Yes	No	Yes	Yes
Period Dummies?	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 undistinguishable from zero because their magnitude (was still negative) falls by an order of magnitude. 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 is positive but again statistically undistinguishable from zero. 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)).

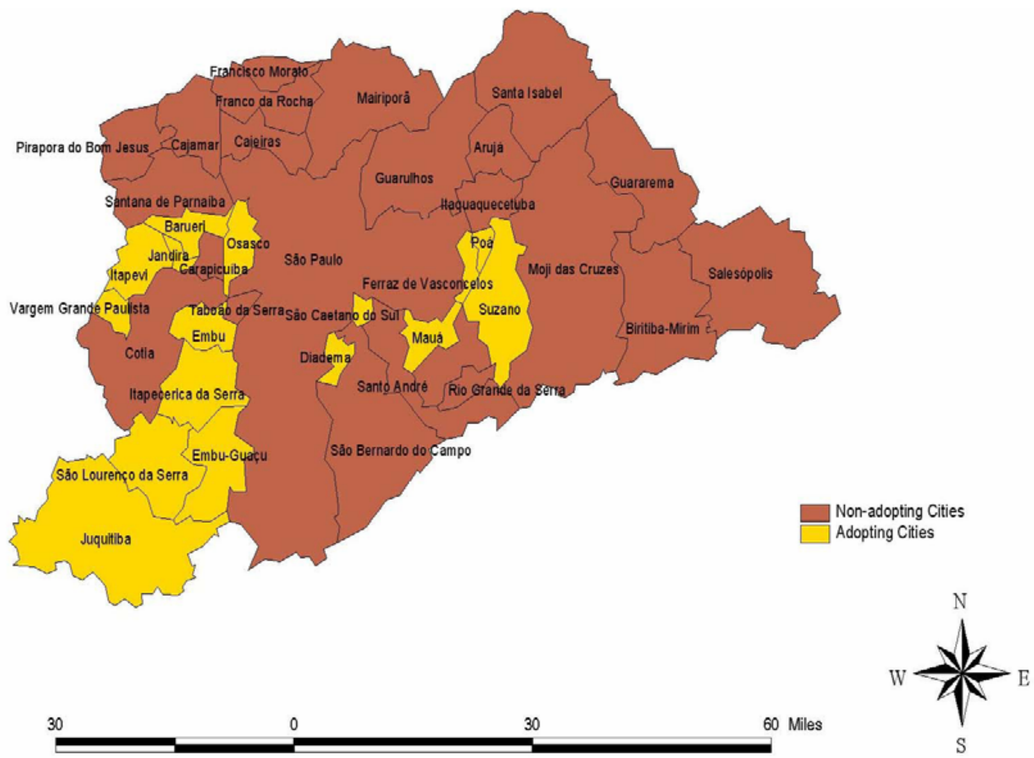
5. Results: Beggar-thy-Neighbor?

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.[28]. Results are in table X and figure III shows a map of the SPMA, with adopting and non-adopting cities identified geographically.



[28]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)†
AdoptLaw	**-0.408** (0.150)***	**-0.561** (0.247)**	**-0.599** (0.173)***
No Observations	2652	1404	2584
R-squared	0.565	0.521	0.512
Homicides t- 1/Homicides t-4	Yes	Yes	Yes
Municipal Force?	Yes	Yes	Yes
Secretary of Justice?	Yes	Yes	Yes
Demographic controls?	Yes	Yes	Yes
Number of Neighboor with Law	Yes	Yes	Yes
City Dummies?	Yes	Yes	Yes
Period Dummies?	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 ofadopting 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 fot the absence of the difference-in-differences regressor $AdoptLaw$, an equation very similar to (1) is estimated:

$$Homicide_{it} = \gamma_0 + \mathbf{Month}_t + \mathbf{City}_t + \mathbf{Controls}_{it} + \epsilon_{it} \qquad (2)$$

with the same model for the variance.

We recover the residuals of (**??**), and estimate the densities of residuals, before and after adoption, for adopting and non-adopting cities, using kernel methods, using the propensity score weighting as in table VI. 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.[29] Figures III and IV show the results.

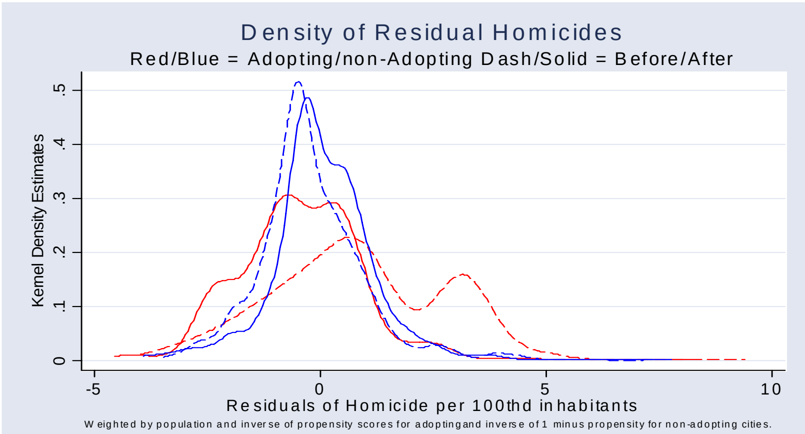


Figure IV: Guassian Kernel Density Estimates. 12-month comparison.

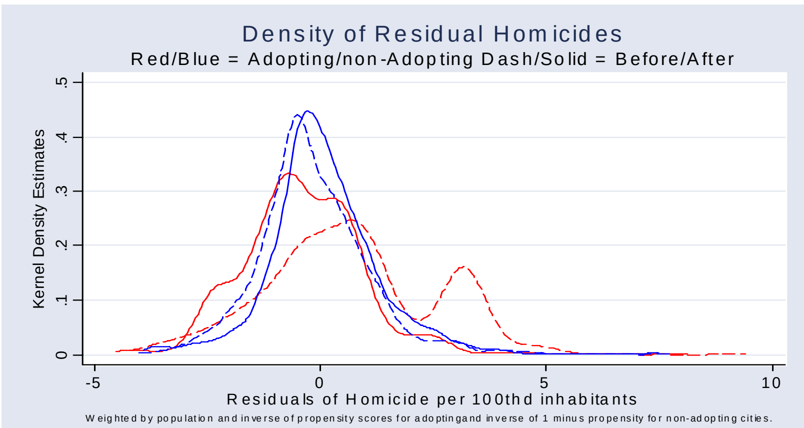


Figure III: Guassian Kernel Density Estimates. 18-month comparison.

[29] 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 one would expect, that is, towards less homicides. For non-adopting cities, if anything, the density of homicides changes towards more homicides. Results are similar for whether the comparison period is 12 and 18 months.

VI. Discussion

In this section we discuss potential pitfalls of our estimation strategy.

A. Omitted determinants and unobserved heterogeneity in trends

The error term in (1) contains all omitted factors that determine homicides. There is always the possibility that ϵ_{it} is decreasing (or increasing less) for those cities that adopted dry laws, relative to cities that did non-adopting cities. Since ϵ_{it} is by construction non-observed, one cannot dismiss this possibility completely, despite 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).

Police, an element of ϵ_{it}, is a candidate for producing our results.[30] Police should respond to crime, as the economics of crime literature suggests, and crime reduces with police (Marvell and Moody [1996], Corman and Mocan [2000], Di Tella and Schargrodsky [2004], and Levitt [2002]). It is conceivable, as we will see below, that adoption of dry law also responds to crime. Therefore, if laws were adopted at times of high crime, there is a strong possibility that policing was also increasing at the same time. There are two reasons why omitting police is not a serious problem for our purposes. The first is institutional, and the second purely data driven.

[30] Unfortunately, we do not have access to police data on the city level, only the SPMA level.

By constitutional appointment, the vast majority of enforcement is done at the state, not the city level. [31] This, by itself, alleviates but does not solve the problem. There is however one important institutional arrangement that does. Legally, police force size is determined annually based on the population, so that every city has roughly the same number of policemen per capita. Police districts are the smallest geographical unit, and are part of battalions. 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 reallocation of police in response to crime.

Although omission of police may not be a serious problem, other omitted competing factors exist. Investment in community centers in crime ridden places, investment in public lighting, neighborhood watch programs, municipal level advertising of DISQUE DENÚNCIA are among them.[32] Although past crime is controlled for in the (1) it is still conceivable that these measures were taken as whole concurrently with dry law in response to high rates of crime in general and homicides in particular.

For this story to be relevant, adopting cities would be in specially high crime periods, both historically and compared with non-adopting cities. Although the procedure in the procedure in table V homogenizes adopting and non-adopting cities, it does so selecting on all observables. Therefore it is informative to take a closer look at the dynamics homicides prior to the adoption of dry laws. If adopting and non-adopting cities were at very different points in the crime cycle, there might be reason for concern.

Figure I already shows that adoption of dry laws was not at peak of homicides when adopting cities are taken as whole. Table VIII shows mean homicides in prior to law adoption and over the whole period for adopting cities.

[31] 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.

[32] DISQUE DENÚNCIA is a type 109 number specifically for denouncing crimes anonimously. 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.

Homicides per 100thd inhabitants

	12 previous months	6 previous months	Jan-1997 to adoption
All non-adopting cities	3.67	3.62	4.02
	(0.39)	(0.39)	(0.36)
Barueri	4.72	4.01	4.55
	(1.51)	(1.44)	(1.33)
Diadema	5.16	5.60	6.83
	(1.46)	(2.03)	(2.23)
Embu	6.12	4.94	6.58
	(2.09)	(1.76)	(2.04)
Embu – Guaçu	5.86	5.79	5.13
	(2.67)	(3.45)	(2.74)
Ferraz de Vasconcelos	3.49	3.50	3.83
	(1.22)	(1.92)	(1.97)
Itapecerica	6.55	6.36	6.94
	(1.30)	(1.00)	(2.10)
Itapevi	5.44	5.75	6.34
	(1.80)	(1.52)	(2.63)
Jandira	4.63	5.09	4.62
	(1.82)	(2.25)	(1.86)
Juquitiba	1.52	1.21	2.23
	(3.31)	(1.87)	(3.19)
Mauá	4.28	4.64	4.28
	(1.28)	(1.53)	(1.19)
Osasco	4.43	4.31	3.64
	(0.66)	(0.49)	(1.03)
Poá	1.14	0.48	1.37
	(1.02)	(0.53)	(0.92)
São Caetano	0.60	0.60	0.93
	(0.52)	(0.55)	(0.82)
Suzano	2.43	2.39	2.87
	(1.25)	(1.40)	(1.27)
Vargem Grande Paulista	0.64	0	2.39
	(1.16)		(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 VIII does not bring any surprise. As indicated by figure I, homicides in adopting cities were not specially high in the year or the six month period prior to the adoption 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.

Although the issue is whether *unobserved* factors are driving results, it is still auspicious if observed factor seem homogenous between adopting and non-adopting cities. In table II, except for the *level* of homicides, the two groups look remarkably similar, both in a snapshot and overtime. Educational attainment, income (??), population are all similar between the two groups. Trends in population, and income as well. The only difference in trends, if any, is in high-school drop-out rates, which dropped more in non-adopting cities, which, if anything, should have produced a steeper drop in homicides in *non-adopting* cities.

B. Endogeneity of Dry Law Adoption, Again

As it was noted several times, adoption, a city level decision, is not exogenously given. In the case of Diadema, for example, crime as perceived to have reached uncontrolled unprecedented levels in 2001, a year before the adoption of the law How can endogeneity affect our results? The most conceivable story is that dry laws were adopted at periods of specially high crime, *and* crime is a mean-reversing process. This, however, does not pose a serious threat since past crime is controlled for in (1). The identification assumption is that lawmakers react to current or recent past crimes when deciding to adopt the dry law. This would not be true if, anticipating a drop in crime, lawmakers adopted the law in order to capitalize on the eventual drop. This amount of lawmaker forward looking behavior seems unlikely. An option would be to find exogenous variation in adoption. We do not pursue this strategy because what determines crime also determines the adoption of the dry law

.

V. Conclusion

Our results indicate that dry laws are associated with a decrease in homicides. Adoption of dry law would have induced a decrease in homicides between 10% and 29% in São Paulo, from the most to the least conservative estimates. These results are robust to a wide range of controls, to the presence of outliers, and dot not seem produced by unobserved heterogeneity. It seems warranted to say that the economic benefits of dry laws are substantial. A very rough computation, using the per capita income at the city of São Paulo in 2003, implies gains between 1.9 and 5.5 million dollars annually in the city of São Paulo. These costs, however, are way underestimated since they not include medical expenses, costs associated with family disintegration, and psychological costs. Furthermore, they do not account not the fact that homicide victims are heavily in the 15-30 years old, a high net present value age group.

These findings support both the alcohol-crime hypothesis,and its magnification through alcohol consumed socially in bars, even though our estimates are not direct behavioral parameters of the precise transmission mechanism. The high frequency - the short period nature of the data, and the cross-city variation in adoption of dry law are both strongly suggest no alternative to dry law can rationalize the results. While other theories may rationalize why restricting bar activities reduces homicides, they seem less appealing theoretically, and have less empirical support in the literature.

This result, however, does not warrant extrapolations such as banning alcohol sales in bars altogether. Results here as not relative to other forms of restrictions, such as taxation and regulation, and the economic and social costs of banning alcohol in recreational outlets are not accounted for.

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