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Abstract[1]

This paper attempts to evaluate the impact on crime of the restriction of late-night alcohol sales in Bogota and quantify the causal effect of problematic alcohol consumption on different crime categories. It is found that the restriction reduced deaths and injuries in car accidents and batteries. The results are stronger in areas where the restriction was actually binding and are highly heterogeneous depending on the number of liquor stores restricted at the block level. Finally, the paper measures the impact of the restriction on alcohol consumption (the first stage, or mechanism), and quantifies the causal pharmacological impact of alcohol consumption on crime using the restriction as an instrument for problematic alcohol consumption (the second stage). The results indicate that a one standard deviation (s.d.) increase in problematic alcohol consumption increases deaths and injuries in car accidents by 0.51 s.d and 0.82 s.d. respectively, and batteries by 1.27 s.d.

JEL classifications: C2, C54, D04
Keywords: Alcohol restrictions, Crime, Pharmacological channel

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1. Introduction

Alcohol consumption is associated with a variety of public health problems ranging from medical to social maladies. The latter include crime, and in particular violent crime. A growing body of evidence suggests that access to alcohol and alcohol consumption increase crime and disorder. However, estimating the causal impact of alcohol consumption on crime has proven to be particularly difficult, as unobserved heterogeneity might be responsible for a substantial part of the correlation between the two outcomes.

Our empirical setting is the implementation of restrictions on the business hours of stores selling alcohol for consumption *off-site*: liquor stores, supermarkets, *cigarrerías*, etc., henceforth liquor stores. Following a citywide surge in crime, on January 13, 2009 the mayor of Bogota issued a decree (Decree 013, 2009) restricting the sale of alcohol in liquor stores, supermarkets, *tiendas* and *cigarrerías*, from 11 p.m. to 10 a.m. the next morning. The decree did not affect premises for recreational consumption (bars, restaurants, nightclubs and discotheques), and it was limited to well-defined zones in nine of Bogota's 20 districts.

We take advantage of the time-series and cross block variation induced by this restriction to estimate the effect of restricting late-night sales of alcohol on crime categories that are mostly impacted by alcohol through the pharmacological channel. For example, homicides may be impacted by both the pharmacological channel but also by illegality itself (the systemic channel). We look at both the extensive margin (had or did not have restriction) and the intensive margin, allowing the impact of the law to be different according to the number of liquor stores at the block level. The restriction caused a reduction in battery, and deaths and injuries in car accidents dropped. We find no systematic impact on rapes and domestic violence.

We document the mechanism by looking at two misdemeanor categories: *exaggerated emotional state* and *walking drunk*, which are added up to construct a proxy for problematic alcohol consumption. We show that the restriction reduced problematic alcohol consumption. Establishing the mechanism has two purposes: it makes policy evaluation more credible and yields the first stage of an instrumented variable (IV) strategy to quantify the causal impact of problematic consumption on crime. We instrument our measure of problematic consumption with the location of the restriction and the density of liquor stores where the restriction was in place. The IV results show that problematic alcohol consumption has a strong impact on battery and on deaths and injuries in car accidents.

We make two contributions to the literature on the alcohol-crime nexus. First, we study the impact of a restriction on alcohol sales during specific hours of the day in some neighborhoods. The object of interest is a reduced-form estimate of the restriction on crime. It is important per se because imposing early closing hours is a relatively inexpensive policy.

Hour restrictions on the sales of alcohol are very common throughout the world. Few studies, however, can claim to have established that policy's causality, possibly because of lack of any variation in the restriction (let alone exogenous variation). The case of Bogota is an appropriate setting for studying the impact of hour-of-the-day restriction on the sales of alcohol for five reasons. First, we have both time-series and cross-section variation in hour-of-day restrictions of alcohol sales. Second, the time-series variation is of high frequency (months), and the cross-section variation is within a city, which allows us to compare similar blocks that are subject to common crime shocks. Third, Bogota is a city with relatively high levels of crime and thus the restriction has a chance of having a measurable impact. Fourth, we have high-quality data: geo-referenced information on crime and the presence of liquor stores. Fifth, because the intervention occurred in a small fraction of the localities restricted for a small period of time, it is highly unlikely that police deployment reacted to the policy, making it more credible to argue that cross-block difference in the restriction is indeed exogenous.

We additionally quantify the causal impact of alcohol consumption on crime. The empirical literature on the alcohol-crime nexus is extensive, but only a few papers have been successful in establishing causality. In a nutshell, we establish a causal link in two steps. First, we show that the restriction on alcohol sales reduced problematic consumption of alcohol, i.e., that associated with excessively drunken behavior. Assuming that the restriction only causes crime through its impact on alcohol consumption, one can establish causality by using the restrictions as an instrument for problematic alcohol consumption. As we shall see, this identifying assumption is more convincing for some types of crime than others. Knowledge about the alcohol-crime nexus is useful from a policy perspective even if one is unwilling to implement restrictions on alcohol sales. For example, the recent debate on the legalization of illegal drugs begs for answering two questions. First, how will drug consumption change with legalization? Second, how will this change impact public health *including* crime? Measuring the pharmacological channel sheds light on the second question.

The paper is organized as follows. Section 2 provides a general background of crime in Bogota, as well as the institutional setting of our empirical application; Section 2 also includes a description of the restriction we study. Section 3 provides a short introduction to the alcohol-crime nexus and to the theoretical mechanisms potentially behind our empirical results. Section 4 describes the data and provides summary statistics. In Section 5, we describe the empirical strategy, and results are presented in Section 6. Section 7 concludes.

2. Related Literature: The Alcohol-Crime Nexus[2]

There are two theoretical mechanisms through which the restriction we study may affect crime. The first is more traditional and corresponds roughly to Goldstein's statements on pharmacological and economically induced channels (Goldstein, 1985). The pharmacological channel refers to the fact that alcohol consumption impairs judgment and may induce violence. The economically induced channel argues that poor alcoholics will commit property crime to sustain their habit. Also, alcohol addiction may affect job market performance, augmenting the need to steal in order to sustain the habit. We expect the restriction to restrict crime through the alcohol-crime nexus.

Experimental studies in psychology suggest that there is a nexus between alcohol and violence (see McClelland et al., 1972 for the first convincing experimental evidence). Consumption of psychotropic substances affects behavior, sometimes exacerbating aggressiveness. McClelland et al. (1972), in their classic *The Drinking Man*, compared fantasies of sober and intoxicated men and found that intoxicated men were more likely than sober men to have fantasies involving power and domination. Extensive literature documents the causal impact of alcohol consumption on violent behavior in different settings (see Lipsey, Wilson and Cohen, 1997 for a survey). However, the literature has had difficulty documenting causal relationships from alcohol consumption to crime through the pharmacological and economically induced channels. For example, controlling for the omission of common determining factors such as child abuse and mental problems has proven to be an elusive task (see Currie and Terkin, 2006).

More recently, economists have contributed with more credible causal estimates. Carpenter and Dobkin (2009) exploit the exogenous variation provided by the 21-year-old legal

[2] This section is largely inspired by Biderman, De Mello and Schneider (2010).

drinking age in the United States. They show that alcohol consumption increases car accident fatalities and youth suicide. Similarly, Carpenter (2007) finds that youth drinking is associated with more property crime but has no impact on violent crime. Biderman, De Mello and Schneider et al. (2010) and Grönqvist and Niknami (2011) both study a similar yet different issue: how the social consumption of alcohol induces crime. Both papers find a significant impact of alcohol consumption on violent crimes. Heaton (2012) analyzes how crime changes when restrictions on Sunday alcohol sales are relaxed. The author finds an increase in minor crimes and in alcohol-related crimes. Similarly, Lovenheim and Steefel (2011) estimate the effect of restricting Sunday alcohol sales on crime, concluding that fatal vehicle accidents increase, especially for underage men. Finally, Kirabo and Owens (2010) use changes in train schedules to examine the relationship with drunk driving. The authors find little evidence on arrests for drunk driving and fatal vehicle accidents.

When seen through the lenses of the literature, our intervention is somewhat peculiar, and it relates to so-called "blue laws" that restrict the sale of alcohol on certain days and at certain times. Like closing bars in the evening, the restriction we study is operative only at specific hours of the day. It applies, however, to off-site alcohol sales. Thus, we are not measuring the pharmacological channel *together* with the social interactions that on-site consumption of alcohol implies. In fact, if the restriction implemented in Bogota reduces crime, it means that the immediate availability of alcohol in general has a detrimental effect on crime when it occurs during *specific* hours of the day. After all, one can always buy alcohol during non-restricted hours. Furthermore, we provide evidence that compulsive, not-anticipated and not time-consistent consumption of alcohol is criminogenic. Again, the restriction does not prevent drinkers from buying alcohol at other hours of the day or from stocking up to drink during restricted hours. In summary, the restriction we study prevents the following type of not-time consistent consumption: *ex ante,* the ideal consumption during the night is X, but once one starts drinking the optimal becomes Y > X, but now liquor stores are closed. Many cities restrict sales of alcohol at night, suggesting this type of consumption is relevant. Surprisingly, little evidence on the effects of such restrictions is available. In short, our paper studies whether this type of consumption is particularly criminogenic.

3. The Intervention

Following a citywide surge in crime in 2008, the city of Bogota implemented restrictions on the business hours of stores selling alcohol for consumption off-site (liquor stores, supermarkets, cigarrerías, etc.). The decision was motivated by the anecdotal observation that many crimes were being committed by young people drinking in the streets right in front of liquor stores and supermarkets, or in nearby places such as parks or plazas (squares). In fact, the decision to restrict alcohol sales on these specific premises was triggered by the highly publicized homicide of a journalist in a plaza right in front of a well-known supermarket in Bogota, where people gather at night to consume alcohol and, allegedly, other psychoactive drugs.

On January 13, 2009 the mayor of Bogota, Samuel Moreno, issued a decree (Decree 013, 2009) restricting the sale of alcohol in liquor stores, supermarkets, *tiendas* and *cigarrerías*, from 11 p.m. until 10 a.m. the next morning. The decree did not affect premises selling alcohol for on-site consumption, such as bars, restaurants, nightclubs and discotheques, and only affected well-defined zones in 9 of Bogota's 20 districts, which is crucial to our identification strategy. Restricted areas are shown in Map 1. In principle, the restricted areas were chosen due to their alleged high incidence of crime related to alcohol consumption and the high concentration of alcohol outlets. Restricted places were chosen based on both qualitative and quantitative observations of crime. The qualitative characteristics that influence the decision were the following: i) zones that presented an increase in noise, agglomeration and conflicts in public spaces, ii) zones with people in streets smoking and drinking alcohol in front of bars and discotheques, iii) advertisement on the streets for alcoholic beverages and establishments where alcohol is sold, iv) traffic jams, v) street vendors vi) drunk people walking on the streets, vii) taxis parked on the streets, and viii) garbage. Quantitatively, restricted zones had higher homicide rates, common batteries and thefts. Also, restricted zones had a higher density of liquor stores (LS), as shown in Table 1.

The restriction was initially put in place for a trial period of three months, during which the homicide rate appeared to have abated. The mayor subsequently decided to extend the restriction for another three months. On July 14, 2009 the restriction was finally lifted as a result of an agreement between the city's Chamber of Commerce and the local government in which the former pledged to self-regulate alcohol sales and prevent the consumption of alcohol in public areas, especially in front of liquor stores and supermarkets.

4. Data

The crime data we use for estimating the impact of the decree between January 13 and July 14, 2009 come from several sources. First, we use administrative police records for 25 different types of crimes (including homicide, theft, drug trafficking, prostitution, rapes and domestic violence, among others) and 25 types of misdemeanors (including street fights, noisy meetings, drug possession, exaggerated emotional state, walking drunk, etc.).[3] Those records include event-level data from January 2007 to mid-2011, where we have (for each event) the type of crime committed, the date and time of occurrence and, importantly, its exact location. Having the address of each crime and misdemeanor allows us to geo-code each event and assign it to a block in Bogota. The percentages of crimes and misdemeanors that we were able to geo-code were 82 percent and 54 percent, respectively.[4]

As in other countries and settings, crime data in Bogota suffers from under-reporting; however, homicides and deaths and injuries in car accidents are relatively well measured. For homicides and deaths in car accidents, as long as a body is produced, it has to be reported to the *Instituto Nacional de Medicina Legal y Ciencias Forenses* for the mandatory investigation. Injuries in car accidents are well measured, as it is mandatory to report to the police any type of accident or injuries caused when vehicles collide. Other types of crimes and misdemeanors, however, may suffer from under-reporting, which we take into account when interpreting our results on these types of criminal outcomes.

The information on the location of liquor stores, *cigarrerías* and other outlets where alcohol is sold for consumption off-premise, comes from a census undertaken by the city's Chamber of Commerce in 2008, the year *before* the restriction was imposed.[5] This census contains the type of economic activity of each firm in Bogota, its size (number of employees and assets) and its exact location. As with the case of crimes and misdemeanors, we geo-code each outlet and assign it to a block in Bogota, with a success rate of about 93 percent.

As mentioned above, Decree 013/2009 specifies the exact zones of Bogota where liquor stores and other similar outlets were restricted. We use the information contained in the decree to

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[3] We use crimes and misdemeanors that can potentially be affected by alcohol consumption only through the pharmacological channel and that are systematically well reported in police records.

[4] The crimes and misdemeanors that did not geo-code correspond to misspelled addresses, rural zones or those that have little information about the event.

[5] Since the decree was initially imposed for three months and then extended for three months more, the restriction is unlikely to have affected entry and exit decisions of liquor stores, *cigarrerías* or supermarkets.

precisely delimitate this zone geographically and identify the liquor stores and blocks affected by the restriction.

The richness of our data relies on two dimensions. The first is the spatial location of crimes and liquor stores where the restriction was put in place. The second is the temporal variation in crimes and misdemeanors before and after the restriction was implemented.

The socio-demographic data come from two main sources. Data from the *Secretaría Distrital de Planeación* (SDP) include population and the mean socioeconomic strata from 2005 to 2009;[6] this information is disaggregated at the Unidad de Planeamiento Zonal (UPZ) level.[7] Since these variables are reported annually, we interpolate them to obtain data at monthly levels. The second source, the National Census of 2005 conducted by the *Departamento Administrativo Nacional de Estadística* (DANE), the Colombian Bureau of Statistics, provides population data, male population aged 15 to 30 years, and the number of residential homes. These data are disaggregated at block level. As mean socioeconomic strata and population are disaggregated at the UPZ level, we associate UPZ information with blocks in order to obtain cross-block variation.

Information on enforcement comes from administrative police records. More precisely, we have information on confiscations and captures at the event level, which we also geo-code with success rates of 72 percent and 81 percent, respectively. We use these two measures as proxies for the intensity of police activity, as these two controls will be important for us to clean our estimations from a potential omitted variable bias. In other words, if the restriction generated higher (or lower) police activity, omitting this variable may generate a bias in our coefficient of interest. For instance, if police activity increased in restricted areas in order to enforce the decree, a negative coefficient on the intervention variable might just be capturing the positive correlation between the restriction and police enforcement and the negative effect of policing on crime. Controlling for police activity using our two proxy measures solves the potential omitted variable bias in our coefficient of interest.

[6] Bogota has six levels of socioeconomic strata, where 1 is the poorest and 6 the richest. The strata level is used for different decisions such as the pricing public utilities and the targeting of social programs.

[7] Bogota is administratively divided into districts (20) and *Unidades de Planeamiento Zonal* or UPZ (112). The latter are geographic zones that are smaller than districts but larger than blocks or neighborhoods. Their main function is to serve as areas for detailed urban development planning, as they differ within the city. UPZs are the intermediate scale for planning between districts and neighborhoods.

Table 1 presents the main summary statistics for our dataset. Means are shown for restricted and unrestricted blocks before and after we implement a Propensity Score Matching methodology to find an appropriate control group for the blocks restricted by the decree.

Before the Propensity Score Matching is implemented, restricted and unrestricted blocks display significant differences in demographic characteristics and, more importantly, in average socioeconomic strata. After the Propensity Score Matching is performed, however, these differences between treated and untreated blocks vanish and not statistically significant. This means that our treatment and control groups are comparable on observable characteristics. Statistically, this is shown by the differences in means test.

Regarding crime rates and liquor stores, restricted blocks have higher levels than unrestricted blocks both before and after Propensity Score Matching is performed. This confirms that the restricted areas were chosen due to higher crime rates and higher density of liquor stores, which is also shown by the difference in means test.

5. Empirical Strategy

This section describes the empirical strategy that we use for disentangling the impact of the restriction of alcohol sales in liquor stores on different types of crime in Bogota. The strategy has three pillars: computing the impact of the restriction on crime (the reduced form), measuring the effect of restriction on (problematic) alcohol consumption (the mechanism, or first stage), and estimating the causal impact of (problematic) alcohol consumption on crime (the structural form, or second stage).

5.1 The Impact of the Restriction on Crime (or The Reduced-Form)

We use the following specification to estimate the basic model, which measures the average direct effect of restricting late-night sales of alcohol on different types of crime:

$$Crime_{it} = \beta_0 + \beta_1 Adopt_{it} + \sum_{t=1}^{T} \omega_t Month_t + \sum_{i=1}^{I} \eta_i Block_i + \Phi Controls_{\mathrm{it}} + \varepsilon_{\mathrm{it}} \qquad (1)$$

where *i* indexes blocks and *t* months. We take the pre-adoption period to be between January 13 and July 14, 2008, since the decree was established between January 13 and July 14, 2009.[8]

[8] Information on crimes committed between 2010 and 2011 is not used, as the restriction was lifted on July 14, 2009. The mayor subsequently issued other decrees that aimed to reduce violent crime. For evaluation purposes, our post period is defined from January 13 to July 14, 2009.

$Crime_{it}$ is the crime or misdemeanor rate per 100,000 inhabitants for block *i* in month *t*. $Adopt_{it}$ is a dummy variable that takes the value 1 if the restriction covered block i in month t, and 0 otherwise. For non-treated blocks, this dummy variable is always 0. $Month_t$ is a set of time dummy variables which assume different values depending on the month. These time dummies capture different crime trends in Bogota over the period studied.[9] $Block_i$ is a set of dummy variables that control for block fixed effects. In $Controls_{\mathrm{it}}$ we include variables that change over time at the block level as a way to capture time-varying heterogeneity. In some specifications we include police confiscations and capture rates per 100,000 inhabitants at the block level. Including enforcement variables is important, as the literature has established a clear relationship between levels of police activity and crime (Marvell and Moody, 1996; Corman and Mocan, 2000; Di Tella and Schargrodsky, 2004; Levitt, 2002). This inclusion comes at a cost, however, because enforcement can itself be endogenous. The error term, $\varepsilon_{\mathrm{it}}$, is clustered at the district level. β_1 is our coefficient of interest in specification (1), and it measures the average change in crime after the restriction was adopted in treated blocks relative to untreated ones. In other words, this parameter captures the average effect of restricting late-night alcohol sales on crime rates. We estimate equation (1) for crime categories that are conceivably related to the pharmacological channel: deaths in car accidents, battery, injuries in car accidents, domestic violence and rape.

A digression on why we choose to overlook several important crime categories is warranted. As shall be seen, when estimating the causal impact of alcohol on crime *through the pharmacological channel*, we assume that the restriction does not cause crime directly, only through its impact on the problematic consumption of alcohol. For many crime categories, this assumption is not credible. One such category is homicide. The restriction many cause violence directly, through the systemic channel (Goldstein, 1985). That is, the illegality of selling alcohol after hours may create an illegal alcohol market, and violence is commonly associated with illegal markets in Latin America (see Mejía and Restrepo, 2012; Chimeli and Soares, 2011; De Mello, 2012).

As the decree established the restriction in specific zones in 9 of the Bogota's 20 districts, our best unit of analysis is the block (*manzana*). Due to their geographic proximity, restricted blocks may have contaminated "non-treated" blocks. After consuming alcohol, people can

[9] These can also be understood as time fixed effects.

circulate between treated and non-treated bordering blocks when committing crimes. In order to have a “clean” group of possible controls in the analysis, we remove non-treated blocks that are within the 10 percent lowest distance to blocks that were covered by the restriction.[10] More precisely, the decontamination process removes from the potential control group those blocks in non-treated areas that are “close” to treated areas. Map 2 shows the blocks covered by the restriction (red), the blocks that were removed from the analysis for decontamination purposes (yellow) and the blocks not covered by the restriction (green). It is from the latter group of blocks that we will construct our control group using a Propensity Score Matching methodology.

When the mayor of Bogota announced the decree, he argued that those areas affected by the restriction had the highest crimes rates in the city. For this reason, we construct a control group similar in observable characteristics to the restricted blocks. We use a Propensity Score Matching (PSM) technique that matches untreated blocks to the treated blocks by average socioeconomic stratum, percentage of men between 20 to 24 years, 25 to 29 years and 30 to 34 years.[11] Using the mean socioeconomic strata helps us capture many unobservable characteristics at the block level. With this variable, we validate the PSM without including other variables taking away degrees of freedom.

Once we have the treatment and control groups we check the common trends assumption required for the Diff-in-Diff strategy that we use to estimate the causal effect of the restriction of alcohol sales on crime. Crime and misdemeanors in the treated and control groups should follow similar trends before the restriction was put in place, so that observed changes between both groups after the implementation of the decree can actually be attributed (in a causal sense) to the restriction. Unobserved differences remain constant over time.

Model (1) is the extensive margin. We also measure the intensive margin and allow the effects of restriction to be heterogeneous over the intensity of the restriction in the following way:

[10] Appendix 1 shows that all results are robust to using different cut-off distances for “decontaminating” the blocks in the control group and to including bordering blocks in the analysis (e.g., the results are robust to using “contaminated” blocks in the control group).

[11] Estimations of the p-score and common support are shown in Appendix 2.

$$Crime_{it} = \beta_0 + \beta_1 Adopt_{it} + \beta_2 LS_i + \beta_3 LS_i * post_t + \beta_4 LS_i * Adopt_i + \beta_5 LS_i * Adopt_{it} + \sum_{t=1}^{T} \omega_t Month_t + \sum_{i=1}^{I} \eta_i Block_i + \Phi Controls_{\text{it}} + \varepsilon_{\text{it}}. \quad (2)$$

LS_i denotes the density of liquor stores per 100,000 inhabitants (*LS*) in block *i* in 2008, the year before the implementation of the restriction. The variable $post_t$ takes the value of 1 for the period between January 13 and July 14, 2009 and 0 otherwise. $LS_i * Adopt_i$ captures the density of *LS* per 100,000 inhabitants interacted with the treatment variable. Finally, $LS_i * Adopt_{it}$ refers to the density of LS per 100,000 inhabitants for block *i* in month *t* in order to capture the differentiated effect of the decree depending on the density of liquor stores. In this specification, β_1 and β_5 are our coefficients of interest as they measure the effect of the restriction and its intensity at block level, comparing treated and untreated blocks. The net effect of the restriction under the specification in equation (2) is $\beta_1 + \beta_5 LS_i$. We estimate equation (2) using the same crime categories as in equation (1).

As we will show, our measure of *LS* is important not only to capture the intensity of the restriction at the block level, but also because it allows us to conduct an important falsification test. More precisely, if the true effect of the restriction is to restrict late-night alcohol sales, we should not find any effect in those blocks covered by the decree that do not have liquor stores (after conducting the appropriate decontamination procedure to clean the estimations from potential spillovers from restricted, close-by, blocks with positive levels of LS).

5.2 The Mechanism (or First Stage): The Effect of the Restriction on (Problematic) Alcohol Consumption

We do not have alcohol sales, but we have something arguably superior for our purposes: *exaggerated emotional state* and *walking drunk,* two categories of misdemeanors that are proxies for problematic alcohol consumption. If the restriction affected alcohol consumption, we should observe a disproportionate decrease in problematic alcohol consumption in blocks affected by the decree. In addition, the reduction should be larger in blocks with more liquor stores (the intensive margin).

The first-stage equation that we estimate is:

$$Alcohol_{it} = \beta_0 + \beta_1 Adopt_{it} + \beta_2 LS_i + \beta_3 LS_i * post_t + \beta_4 LS_i * Adopt_i + \beta_5 LS_i * Adopt_{it} + \sum_{t=1}^{T} \omega_t Month_t + \sum_{i=1}^{I} \eta_i Block_i + \Phi Controls_{\text{it}} + \varepsilon_{\text{it,}} \tag{3}$$

where all variables are defined as in equation (2). $Alcohol_{it}$ is the sum of *exaggerated emotional state* and *walking drunk* rate per 100,000 inhabitants.[12]

5.3 The Structural Form: The Causal Impact of Problematic Alcohol Consumption on Crime, or the Pharmacological Channel

We use the predicted values of equation (3) to measure and analyze the causal effect of problematic alcohol consumption on crime. We estimate the following equation:

$$Crime_{it} = \beta_0 + \beta_1 \widehat{Alcohol}_{it} + \beta_2 LS_i + \beta_3 LS_i * post_t + \beta_4 LS_i * Adopt_i + \sum_{t=1}^{T} \omega_t Month_t + \sum_{i=1}^{I} \eta_i Block_i + \Phi Controls_{\text{it}} + \varepsilon_{\text{it}}, \tag{4}$$

where all variables are defined as in equation (1) and $\widehat{Alcohol}_{it}$ is obtained from the predicted values of equation (3).

5.4 Robustness Checks

We perform three additional exercises in order to examine the robustness of our results. In the first one, we eliminate treated blocks without liquor stores. This means that we only use restricted blocks where the restriction was really binding (e.g., blocks with positive levels of *LS*). Under the second exercise we eliminate blocks that are within the lowest 90 percent of the distribution of LS.[13] We call this exercise binding at 90 percent, and we try to evaluate the heterogeneous effect of the restriction where the concentration of *LS* is higher.

Finally, in the third exercise we eliminate from the treatment group those blocks with *LS*>0 and only use crimes committed between 10 a.m. and 11 p.m. In these blocks, there is no presence of *LS* and the restriction is not active.

[12] Our results are robust to using the two misdemeanors separately.

[13] Appendix 3 shows the distribution of LS before and after removing the lowest 90 percent.

We expect to find a stronger effect when the restriction is binding at 90 percent than when the restriction is just binding, as the effect of the restriction increases when there is a greater presence of *LS*. We also expect our results when the restriction is binding to be stronger than in the baseline estimations.

We compare the binding results with the non-binding results. In the latter estimations we should not find any effect of the restriction on crime (unless, of course, the restriction led to other changes different from a reduction in alcohol sales and consumption). If the restriction had a stronger effect on crime in those blocks where $LS>0$ or when the block is in the 90th percentile or above in the distribution of *LS* and did not have an effect in blocks where $LS=0$, then this can be interpreted as yet another piece of evidence demonstrating that the restriction had an effect on crime *through* the reduction of alcohol consumption.

6. Results

6.1 Propensity Score Matching Results

The restricted zones were not chosen randomly, and we use a Propensity Score (PS) method in order to find a non-experimental control group among the blocks not covered by the restriction. The basic idea of PS is to find a group of blocks not affected by the restriction that is similar to restricted blocks in all relevant pretreatment characteristics (see Dehejia and Wahba, 1998, and Caliendo and Kopeining, 2008). The identifying assumption behind the PS method is that assignment to treatment (e.g., the restriction) depends only on observable pre-intervention variables that are unaffected by participation in the program (e.g., the restriction) or the anticipation of it (Caliendo and Kopeinig, 2008). If equation (1) is estimated with all unrestricted blocks as the control group, the true effect of the restriction can be confounded with preexisting differences between the treatment and control groups. The difference in means test on observable characteristics reveals that it is indeed the case that restricted blocks are significantly different from non-restricted blocks in all pre-intervention observable characteristics (socioeconomic strata and demographic structure). Table 1 (fifth column) shows the results of this test. This table reveals that the differences in means for all observable demographic characteristics are statistically significant, which means that the groups are not comparable in any observable dimensions. Thus, we implement a PS method in order to find a control group that is observationally similar to the group of blocks covered by the restriction. Before implementing the PS, however, we exclude from the potential control group all blocks that are within the 10 percent closest distance to the treated group of blocks. We undertake this "decontamination" process in order to isolate potential spillover effects from restricted blocks to unrestricted blocks. The results of the p-score, using average socioeconomic strata, percentage of men between 20 to 24 years, 25 to 29 years and 30 to 35 as determinants, are shown in Appendix 2. Only socioeconomic variables that are statistically significant are included in the probit model.

As Table 1 (sixth column) reveals, the difference in means in observable characteristics included as determinants is not significant once PS is implemented. As such, our treatment and non-experimental control groups are comparable in terms of observable characteristics, and differences in crime rates after the restriction was implemented can, at first sight, be attributed to the treatment effect. Table 1 also shows that before and after implementing the Propensity Score Matching, the difference of means for crime rates and liquor stores, is statistically significant. Specifically, restricted blocks have higher means than unrestricted blocks. However, the necessary condition for the estimation of the Diff-in-Diff model is not that restricted and unrestricted blocks have the same crime rate, but rather that crime rates that will be used as dependent variables follow common trends before the restriction was implemented. This is analyzed in the following subsection.

6.2 Common Trends before the Restriction

As mentioned before, the validity of using a differences-in-differences estimator in equation (1) relies on the assumption that the underlying trends in the outcome variable (crime and misdemeanor rates), is the same for both treatment and control groups *before* the restriction was put in place. Figures 1.1-1.5 show that all crime and misdemeanors follow a similar trend (e.g., they have common trends) for the restricted and the non-experimental control group. These figures capture the difference in the growth rate of crimes between treatment and control groups during the pre-adoption period. This means that if the outcomes follow similar trends, the confidence interval should fluctuate around zero.

6.3 The Reduced Form: The Direct Impact of the Restriction of Alcohol Sales on Crime

Table 2 shows the results of the estimations of equations (1) and (2) for different types of crimes and misdemeanors. For conciseness, only β_0, β_1 and β_5 are reported. The model includes block and period fixed effects, and errors are clustered at the district level. The controls included are confiscations and captures rates per 100,000 inhabitants. In panel (a) we present the results of estimating equation (1) without the inclusion of the density of liquor stores at the block level. These estimations capture the average effect of the restriction independent of the level of liquor stores. The coefficient of interest in this case, β_1, is shown in panel (a). We only find a statistically significant average effect of the restriction on batteries and rapes. With the implementation of the restriction, batteries decreased in 0.143 cases per 100,000 inhabitants. On the other hand, the results presented in panel (a) also reveal that the restriction induced an *increase* in rapes. This increase corresponds to 0.132 rapes per 100,000 inhabitants.

In panel (b) we present the results of the estimation of equation (2), which includes the density of LS per 100,000 inhabitants as a control and interact it with the variable that captures the restriction. The inclusion of LS in the estimation of equation (2) is important in order to account for the intensity of the restriction at the block level. It should be noted that our measure of LS is for 2008, one year *before* the restriction was announced and implemented, thus reducing concerns about the potential endogeneity of this variable. The estimated coefficients of $Adopt_{it}$ (β_1) and $LS_i * Adopt_{it}$ (β_5) are shown in panel (b). All estimates include controls, as well as month and block fixed effects. Our estimates reveal that once the density of liquor stores is included, the restriction appears to have a more significant effect precisely on those blocks that were more affected by it (i.e., in blocks with a higher density of LS). More precisely, our estimates indicate that the effect of the restriction on crime (negative or positive), as expected, increases with the intensity of the restriction. Figures 2.1-2.4 show the net impact of the restriction on different types of crime and misdemeanors as a function of the density of LS. These figures are obtained from the results in panel (b) of Table 2. Specifically, these figures show how the derivative of crime with respect to $Adopt_{it}$ changes as a function of LS. This means that the impact of the restriction on crimes and misdemeanors becomes stronger as the density of LS increases. On the one hand, the figures indicate that the restriction caused a decrease in deaths and injuries in car accidents, and in batteries. Again, this effect is stronger in blocks with a higher density of LS. On the other hand, however, other figures reveal that the restriction *increased* rapes, and that the increase was larger precisely in those blocks with a higher initial density of LS. The results in panel (b) confirm the results in panel (a) and provide further evidence of the heterogeneous effects of the restrictions on different types of crimes and misdemeanors depending on the intensity of the restriction at the block level.

It is important to notice that the result on rapes and (in some cases) domestic violence is the opposite of what was expected by the authorities when they decided to implement the restriction. We conjecture that the restriction might have created behavioral changes with respect to the preferred location for alcohol consumption. More precisely, given that doorstep sales were not affected by the restriction, alcohol consumption may have shifted from parks, streets and plazas to home consumption, thus possibly increasing rapes and cases of violence against women and children, which are in most cases perpetrated at home.

6.4 The First Stage or Mechanism: The Impact of the Restriction on Problematic Alcohol Consumption

Table 4 presents the results for the first stage. Significance in the first stage estimations actually improves when the restriction is binding. In this case, it is important to check the f-statistic and the weak-id f-test. The f-statistic shows the joint significance of the first stage estimation. Both the baseline and binding restriction models are statistically significant. The weak-id coefficient shows the strength or weakness of the instrumental variables included in the model. When we compare the baseline estimations and the binding restriction estimations, the weak-id coefficient is actually stronger for the case when the restriction was more binding (i.e., in the model where we use the blocks above the 90^{th} percentile in the distribution of *LS*).

6.5 The Structural Form: The Impact of Problematic Alcohol Consumption on Crime, or the Pharmacological Channel

Results for the second-stage estimations are presented in Table 5. We use the sum of *exaggerated emotional state* and *walking drunk* as a proxy for problematic alcohol consumption and instrument it using the restriction and the density of *LS*. The results show that problematic alcohol consumption has an impact on the crime categories most associated with the pharmacological channel: deaths and injuries in car accidents, and batteries. For the case of rapes and domestic violence, the restriction (as explained before) might have induced a behavioral change with respect to the preferred location for alcohol consumption (at home instead of public spaces). Because *exaggerated emotional state* and *walking drunk* capture *outdoors* intoxication, the reduced form showed that the restriction increases rapes (and sometimes domestic violence), and our measure of problematic alcohol consumption is for *outdoors* problematic intoxication, which was reduced by the policy. Thus, the structural form will mechanically say that *outdoors* problematic consumption reduces rapes, and for this reason we exclude rapes and domestic violence from the structural (second stage) estimations.

In the 2SLS estimations we find a positive and significant effect of higher problematic alcohol consumption on deaths and injuries in car accidents and on batteries under most specifications. In particular, the three panels in Table 5 show that deaths and injuries in car accidents and batteries are positively affected by (problematic) alcohol consumption. With the exception of injuries in car accidents for the estimations for the binding restriction, the (causal) effect of problematic alcohol consumption on these crimes is always positive and statistically significant. According the results for the baseline estimations presented in panel (a) of Table 5 an increase of one (1) standard deviation (s.d.) in our measure of problematic alcohol consumption causes an increase of 0.51 s.d. in deaths in car accidents, 0.82 s.d. in injuries in car accidents and 1.27 s.d. in batteries. Conversely, a 10 percent increase in our measure of problematic alcohol consumption leads to an increase of about 12.6 percent in both deaths and injuries in car accidents and a 14.7 percent increase in batteries.

6.6 Robustness Checks and Falsification Test

In Table 3 we present several robustness checks and a falsification test. Panel (a) of Table 3 shows a robustness check where we compare our baseline estimations with the results when we restrict our sample to blocks with a positive level of *LS*. The results, if anything, corroborate the results obtained before. In this case, we find that domestic violence also increases with the restriction. This result is consistent with the observed increase in rapes, and it turns out to be a worrying unintended consequence of the restriction. Our results obtained for homicides and injuries in car accidents are not robust to the removal of blocks without *LS*. Compared to the baseline, the results are smaller for homicides in car accidents and battery when the restriction is binding. For the case of rapes, the results are larger. Figures 3.1-3.4 show the net impact of the restriction on different types of crime. Again, these figures show how crimes and contraventions behave when the restriction was imposed, depending on the density of *LS*.

Panel (b) in Table 3 presents the results of the estimations of equation (2) when we only take those blocks above the 90^{th} percentile in the distribution of LS. In other words, these estimations use those blocks most affected by the restriction. Figures 4.1-4.4 again show how crime changes with the restriction for different levels of *LS*. These results again corroborate the results obtained in panel (a): an increase in homicides and rapes, and a decrease in deaths and injuries in car accidents, and batteries. Compared to the binding restriction estimates, these results are stronger for homicides, batteries, and injuries in car accidents. Results are smaller for rapes and deaths in car accidents. The result obtained before for domestic violence is not robust to this exercise. Again, results obtained are stronger when the density of *LS* increases.

Finally, panel (c) presents a falsification test. In this panel we present the results of estimating equation (1) restricting our sample to restricted block without *LS* and to crimes and misdemeanors that took place between 10 am and 11 pm, when the restriction on liquor stores was not active. This falsification test is aimed at showing that crimes perpetrated at times of the day when the restriction was not binding should not be affected by the restriction. Our results are in general consistent with this hypothesis. We take this as further evidence suggesting that pharmacologically-induced violence might have been reduced as a result of the restriction.

7. Concluding Remarks

Alcohol markets and alcohol consumption can affect crime and violence through different channels: the pharmacological, the economic and the systemic channels. This paper evaluates the effect of a restriction imposed on late-night alcohol sales during the first semester of 2009 in Bogota on crime through the pharmacological channel. The restriction did not affect bars, discotheques and other stores for alcohol consumption *on-site*, but it did restrict those stores selling alcohol for consumption *off-site*: liquor stores, supermarkets, *cigarrerías*, etc. We exploit time-series and cross-block variation in the restriction in order to measure its causal effects on several crime categories. We have data at the block level on the number of outlets restricted, which allows us to estimate heterogeneous effects with respect to how binding the restriction was at the block level.

Our findings indicate that some crime categories were reduced as a result of the restriction (deaths and injuries in car accidents, and batteries). This is evidence of the pharmacological channel through which alcohol consumption affects crime. As for rapes and domestic violence, our conjecture is that, given that doorstep sales were not affected, the restriction might have induced behavioral changes with respect to the preferred location for alcohol consumption. More precisely, the restriction might have induced people to stay at home and consume alcohol there, which might have increased the cases of rapes and domestic violence.

Although we do not have data on alcohol sales, we use the sum of the misdemeanors *exaggerated emotional state* and *walking drunk* as proxies for (problematic) alcohol consumption in a 2SLS framework in order to further test the pharmacological channel. The results of these estimations corroborate the results obtained in the baseline estimations. First, the restriction indeed reduced (problematic) alcohol consumption (e.g., the number of cases reported for the two misdemeanors, *exaggerated emotional state* and *walking drunk*) and second, the lowering of these misdemeanors as a result of the restriction, reduced deaths and injuries in car accidents.

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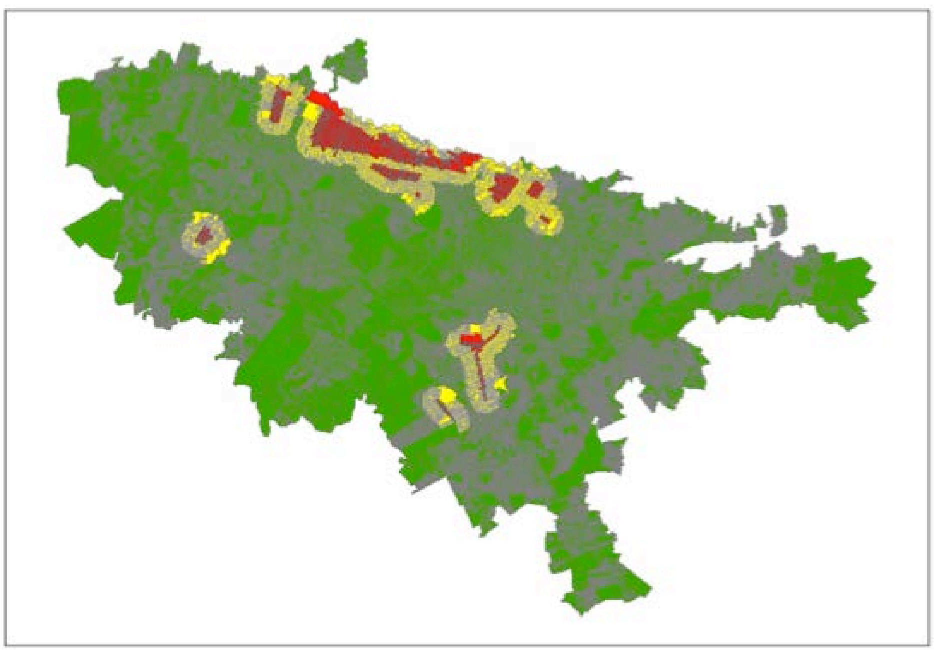
Map 1. Areas Restricted by Decree 013/2009

Map 2. Restricted and Unrestricted Blocks Obtained after Performing the "Decontamination" Method



Note: Restricted blocks are shown in red, blocks that were removed are yellow and untreated blocks are green.

Table 1

Summary statistics

	Restricted Blocks		Unrestricted Blocks		Difference of Means	
	Before PSM	After PSM	Before PSM	After PSM	Before PSM	After PSM
Demographics						
Population	175.040	175.727	186.687	208.285	11.647*	32.557***
	(173.597)	(173.949)	(204.232)	(277.197)	(6.910)	(10.982)
% Male Population, age 20-24	0.049	0.049	0.046	0.050	-0.003***	0.000
	(0.033)	(0.033)	(0.025)	(0.047)	(0.001)	(0.002)
% Male Population, age 25-29	0.051	0.051	0.043	0.053	-0.008***	0.002
	(0.035)	(0.034)	(0.023)	(0.043)	(0.001)	(0.002)
% Male Population, age 30-34	0.042	0.042	0.036	0.041	-0.006***	-0.001
	(0.028)	(0.028)	(0.021)	(0.031)	(0.001)	(0.001)
Medium Strata	3.939	3.923	2.421	3.836	-1.512***	-0.092
	(1.294)	(1.291)	(0.844)	(1.146)	(0.029)	(0.058)
Monthly Crime Rate per 100,000 inhabitants						
Deaths in Car Accidents	0.036	0.042	0.006	0.044	-0.029***	0.002
	(0.783)	(0.784)	(0.826)	(3.060)	(0.010)	(0.043)
Battery	0.310	0.367	0.041	0.026	-0.268***	-0.341***
	(2.923)	(3.186)	(1.425)	(0.430)	(0.018)	(0.044)
Injuries in Car Accidents	0.254	0.244	0.058	0.066	-0.197***	-0.178***
	(3.001)	(2.823)	(5.533)	(1.887)	(0.065)	(0.047)
Domestic Violence	0.188	0.186	0.120	0.089	-0.068	-0.097
	(4.292)	(4.184)	(4.271)	(2.494)	(0.051)	(0.067)
Rapes	0.023	0.031	0.006	0.002	-0.016*	-0.029
	(1.433)	(1.667)	(0.803)	(0.151)	(0.010)	(0.023)
Problematic alcohol consumption	9.185	10.622	0.780	0.360	-8.410***	-10.263***
	(75.956)	(79.597)	(21.377)	(7.114)	(0.292)	(1.095)
Confiscations	260.186	336.645	18.359	3.303	-241.827***	-333.342
	(1,3637.59)	(1,5862.82)	(1,283.667)	(48.322)	(30.637)	(217.320)
Captures	0.538	0.614	0.087	0.101	-0.451***	-0.514***
	(2.839)	(3.058)	(2.464)	(1.452)	(0.030)	(0.046)
Liquor Stores per 100,000 inhabitants	2.184	2.078	0.386	0.393	-1.798***	-1.684***
	(8.165)	(6.285)	(4.390)	(2.332)	(0.133)	(0.225)
Observations	5,364	5,328	165,756	5,328	171,120	10,656
Number of blocks	894	888	27,626	888	28,520	1,776

Note: Standard deviations in parentheses. For time varying variables, means are computed for the pre-adoption period between January 2008 to July 2008.

Table 2

	Deaths in Car Accidents	Battery	Injuries in Car Accidents	Domestic Violence	Rapes
Baseline regressions					
(a) Basic model					
Restriction	0,01	-0.143*	-0,013	0,014	0.132*
	(0.042)	(0.078)	(0.02)	(0.056)	(0.066)
Constant	0,013	0.2***	0.146**	0.2***	0,008
	(0.015)	(0.054)	(0.051)	(0.065)	(0.018)
R-squared	0,001	0,001	0,004	0,001	0,002
Observations	21,312	21,312	21,312	21,312	21,312
Number of blocks	1,776	1,776	1,776	1,776	1,776
(b) Including Liquor Stores					
Restriction	0,039	-0,05	0,085	-0,056	0,066
	(0.036)	(0.06)	(0.057)	(0.059)	(0.04)
Restriction*LS	-0.018***	-0.184***	-0.103**	0,058	0.031***
	(0.005)	(0.062)	(0.046)	(0.065)	(0.007)
Constant	0,013	0.201***	0.146**	0.199***	0,007
	(0.016)	(0.052)	(0.056)	(0.064)	(0.014)
R-squared	0,001	0,003	0,007	0,001	0,003
Observations	21,312	21,312	21,312	21,312	21,312
Number of blocks	1,776	1,776	1,776	1,776	1,776

Note: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors in parenthesis are clustered at district level. Pre-adoption period is January 2008 to July 2008; post-adoption period is January 2009 to July 2009. All regressions include time and block fixed effects. Controls included are confiscations and captures rates per 100,000 inhabitants.

Table 3

Robustness checks and Falsification test	Deaths in Car Accidents	Battery	Injuries in Car Accidents	Domestic Violence	Rapes
(a) Robustness check: Binding restriction					
Restriction	0.022***	0,042	-0,053	0,012	0,151
	(0.006)	(0.195)	(0.067)	(0.217)	(0.121)
Restriction*LS	-0.016***	-0.084**	-0,002	0.045*	0.036***
	(0.003)	(0.037)	(0.018)	(0.025)	(0.005)
Constant	0,032	0.442***	0.164**	0,277	0,055
	(0.029)	(0.13)	(0.063)	(0.198)	(0.032)
R-squared	0,018	0,013	0,012	0,004	0,006
Observations	6,576	6,576	6,576	6,576	6,576
Number of blocks	548	548	548	548	548
(b) Robustness check: Binding at 90% of LS					
Restriction	-0,074	-0,612	0,093	-0,216	0.461*
	0,07	1,985	0,76	1,626	0,222
Restriction*LS	-0.012***	-0.177***	-0.111***	0,043	0.016**
	0,003	0,031	0,032	0,032	0,006
Constant	0,201	1,489	1.641**	0,067	-0,023
	0,156	1,012	0,73	0,21	0,184
R-squared	0,019	0,018	0,018	0,005	0,009
Observations	2,148	2,148	2,148	2,148	2,148
Number of blocks	191	191	191	191	191
(c) Falsification test: Non-Binding restriction, 10am-11pm					
Restriction	-0,012	0,006	-0,01	-0,047	0.025**
	(0.014)	(0.037)	(0.021)	(0.059)	(0.011)
Constant	0,002	0,014	0.054*	0,046	0,001
	(0.004)	(0.017)	(0.029)	(0.047)	(0.002)
R-squared	0,003	0,001	0,003	0,001	0,001
Observations	14,208	14,208	14,208	14,208	14,208
Number of blocks	1,184	1,184	1,184	1,184	1,184

Note: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors in parenthesis are clustered at district level. Pre-adoption period is January 2008 to July 2008; post-adoption period is January 2009 to July 2009. All regressions include time and block fixed effects. Controls included are confiscations and captures rates per 100,00 inhabitants. Binding restrictions refers to blocks where LS>0. Non-binding restriction refers to blocks where LS=0

Table 4

First stage regressions (Exaggerated Emotional State and Walking Drunk)

	Baseline with Liquor Stores	Robustness check: Binding restriction	Robustness check: Binding at 90% of LS
Restriction	0.727	17.447**	23.585*
	(2.518)	(6.693)	(11.088)
Restriction*LS	-3.659***	-4.942***	-4.627***
	(0.834)	(0.770)	(0.492)
Constant	5.397***	12.995**	22.568
	(1.612)	(5.395)	(21.235)
R-squared	0.029	0.040	0.1413
Observations	21,312	6,576	2,148
Number of blocks	1,776	548	191
Angrist-Pischke F p-value	0,001	0.000	0.000
Weak-id Angrist-Pischke statistic	10,34	22,31	44.30
Cragg-Donald Wald F statistic	25,82	34,88	12.87

Note: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors in parenthesis are clustered at district level. Pre-adoption period is January 2008 to July 2008; post-adoption period is January 2009 to July 2009. All regressions include time and block fixed effects. Controls included are confiscations and captures rates per 100,00 inhabitants. Binding restrictions refers to blocks where LS>0. Non-binding restriction refers to blocks where LS=0

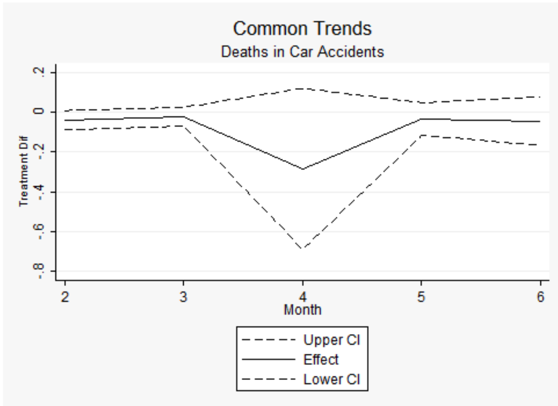
Table 5

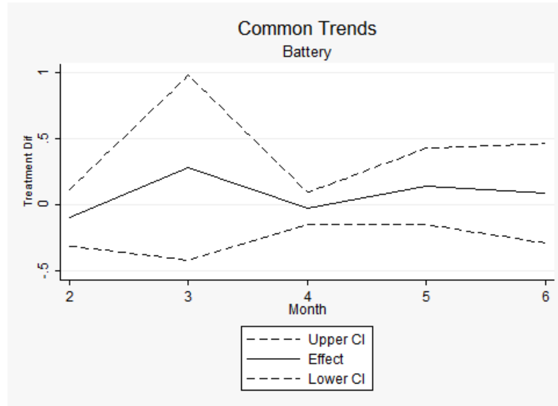
Second stage regressions

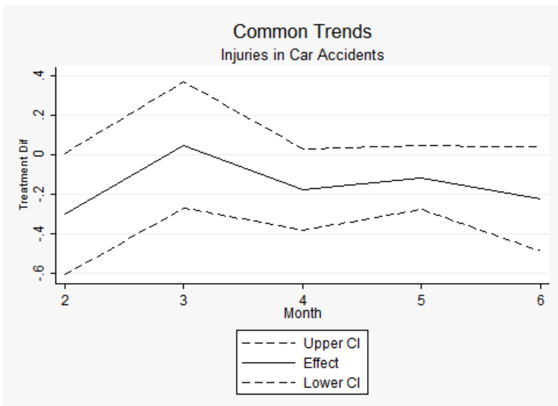
	Deaths in Car Accidents	Injuries in Car Accidents	Batteries
	(a) Baseline with Liquor Stores		
Alcohol	0.005***	0.029**	0.051***
	(0.001)	(0.011)	(0.019)
Constant	-0,005	-0.040	-0,154
	(0.045)	(0.076)	(0.121)
R-squared	-0,026	-0.520	-0,908
Observations	21,312	21,312	21,312
Number of blocks	1,776	1,776	1,776
	(b) Robustness check: Binding restriction		
Alcohol	0.003***	0,001	0.018**
	(0.000)	(0.004)	(0.008)
Constant	-0,028	0,201	-0,148
	(0.033)	(0.145)	(-0.200)
R-squared	-0,401	0,013	-0,286
Observations	6,576	6,576	6,576
Number of blocks	548	548	548
	(c) Robustness check: Binding at 90% of LS		
Alcohol	0.028***	0.004***	0.051***
	(0.010)	(0.001)	(0.011)
Constant	-0,088	-0,098	-1.654*
	(0.125)	(0.809)	(0.957)
R-squared	-0,135	-0,092	-0,583
Observations	2,148	2,148	2,148
Number of blocks	191	191	191

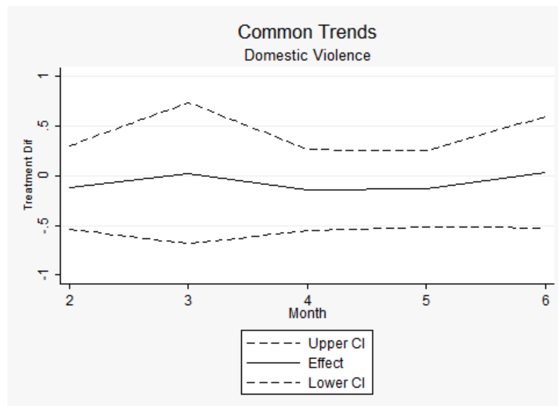
Note: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level. Standard errors in parenthesis are clustered at district level. Pre-adoption period is January 2008 to July 2008; post-adoption period is January 2009 to July 2009. All regressions include time and block fixed effects. Controls included are confiscations and captures rates per 100,00 inhabitants. Binding restrictions refers to blocks where LS>0. Non-binding restriction refers to blocks where LS=0

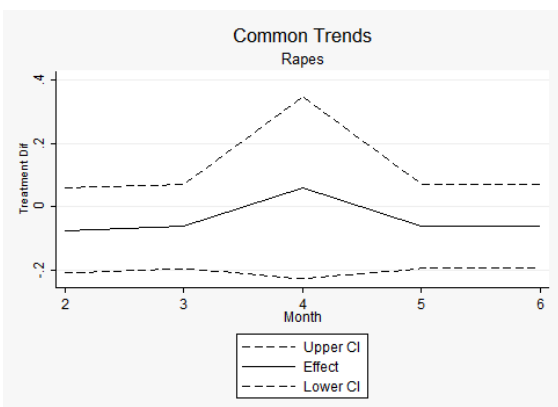
Figures 1.1-1.5. Common Trends Test for Crimes and Misdemeanors



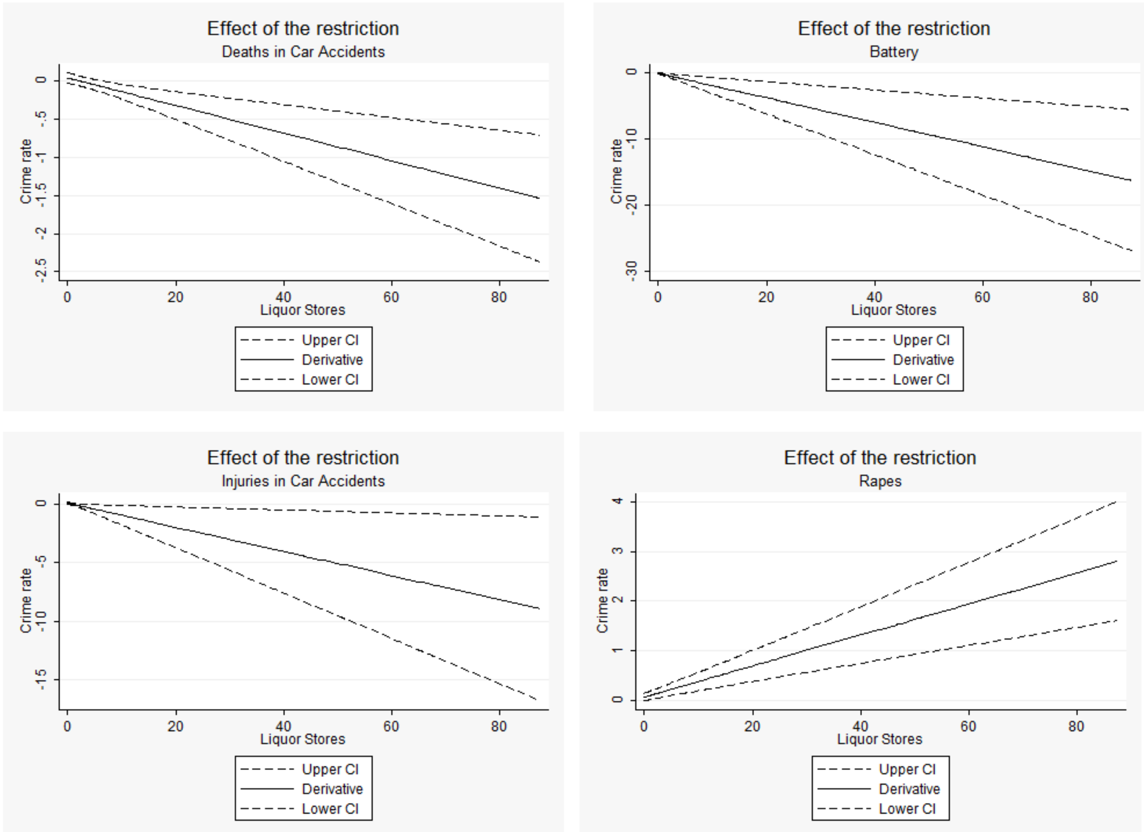




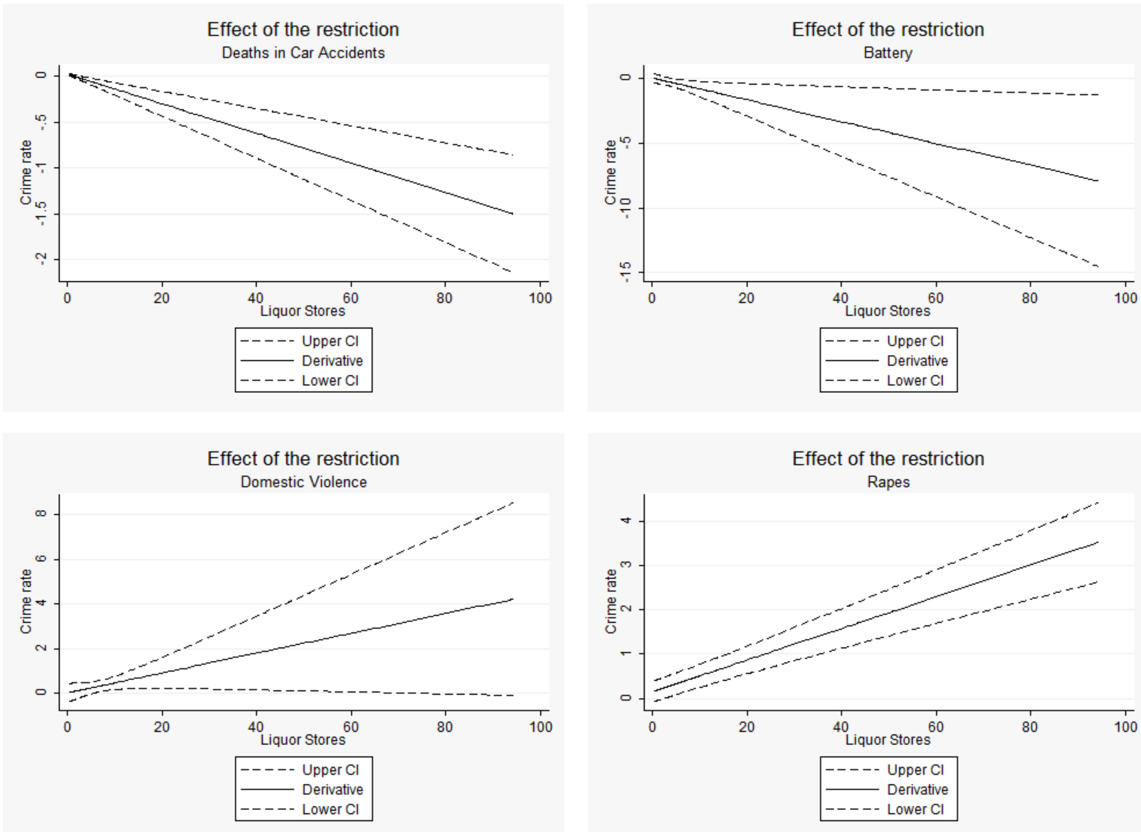




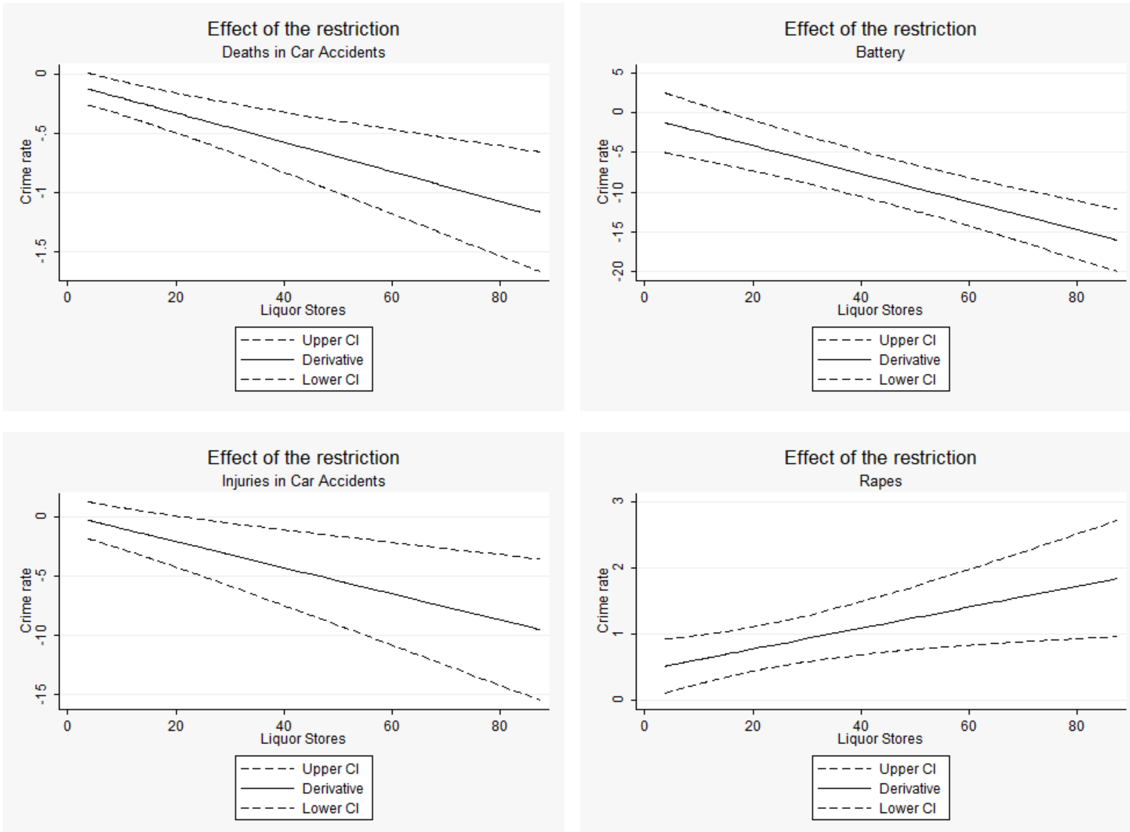
Figures 2.1-2.4 Baseline Estimations: Effect of Restriction on Crimes and Misdemeanors



Figures 3.1-3.4 Binding Restriction: Effect of Restriction on Crimes and Misdemeanors



Figures 4.1-4.4 Binding Restriction at 90 Percent: Effect of Restriction on Crimes and Misdemeanors



Appendix 1. Spillover Effects: Contaminated Estimation and Decontaminated Estimations with Different "Cut-Offs"

Appendix 1

Robustness checks for decontamination process

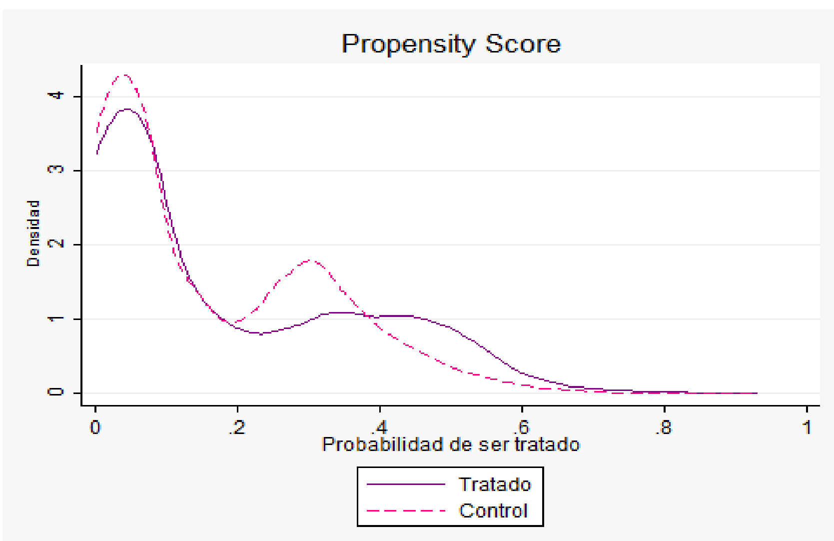
	Deaths in Car Accidents	Battery	Injuries in Car Accidents	Domestic Violence	Rapes
(a) Contaminated					
Restriction	-0,007	-0,061	0,047	-0,082	0.075*
	0,012	0,043	0,052	0,049	0,042
Restriction*LS	-0.016***	0,013	-0,032	0,097	0.042***
	0,004	0,091	0,019	0,112	0,009
Constant	0,011	0.178***	0.095***	0.208**	0,02
	0,012	0,058	0,027	0,083	0,014
R-squared	0,006	0,004	0,009	0,001	0,004
Observations	21,432	21,432	21,432	21,432	21,432
Number of blocks	1,786	1,786	1,786	1,786	1,786
(b) Decontaminated 5%					
Restriction	0,033	-0,026	0,049	-0.096*	0,076
	0,043	0,033	0,046	0,051	0,044
Restriction*LS	-0.026**	-0.071**	-0.041*	-0,043	0.026**
	0,01	0,032	0,019	0,081	0,01
Constant	0,015	0.187***	0.092**	0.168***	0,02
	0,016	0,054	0,042	0,057	0,015
R-squared	0,001	0,003	0,01	0,001	0,004
Observations	21,384	21,384	21,384	21,384	21,384
Number of blocks	1,782	1,782	1,782	1,782	1,782
(c) Decontaminated 10%					
Restriction	0,039	-0,05	0,085	-0,056	0,066
	(0.036)	(0.06)	(0.057)	(0.059)	(0.04)
Restriction*LS	-0.018***	-0.184***	-0.103**	0,058	0.031***
	(0.005)	(0.062)	(0.046)	(0.065)	(0.007)
Constant	0,013	0.201***	0.146**	0.199***	0,007
	(0.016)	(0.052)	(0.056)	(0.064)	(0.014)
R-squared	0,001	0,003	0,007	0,001	0,003
Observations	21,312	21,312	21,312	21,312	21,312
Number of blocks	1,776	1,776	1,776	1,776	1,776
(d) Decontaminated 15%					
Restriction	0,038	-0.115*	0,035	-0,053	0,089
	0,037	0,058	0,046	0,053	0,053
Restriction*LS	-0.016***	-0,035	-0,011	0,088	0.039***
	0,005	0,023	0,025	0,068	0,013
Constant	0,003	0.199***	0,063	0.169**	0,023
	0,016	0,066	0,047	0,067	0,017
R-squared	0,001	0,002	0,011	0,001	0,004
Observations	19,200	19,200	19,200	19,200	19,200
Number of blocks	1,600	1,600	1,600	1,600	1,600
(e) Decontaminated 20%					
Restriction	0,044	-0,035	0,083	0,061	0,087
	0,035	0,052	0,058	0,144	0,053
Restriction*LS	0.001**	-0,269	-0.037***	-0,396	0.033***
	0,001	0,163	0,013	0,348	0,008
Constant	-0.013***	0.213***	0.156*	0.192**	0,018
	0,003	0,061	0,075	0,072	0,02
R-squared	0,001	0,017	0,009	0,024	0,003
Observations	18,576	18,576	18,576	18,576	18,576
Number of blocks	1,548	1,548	1,548	1,548	1,548

Appendix 2. Estimation of Propensity Score Matching

Appendix 2

Propensity Score Matching: probit regression

% men 20-24	3.007***
	(0.553)
% men 25-29	5.077***
	(0.579)
% men 30-34	5.779***
	(0.689)
Mean socioeconomic strata	0.665***
	(0.017)
Constant	-4.462***
	(0.079)
Pseudo R-squared	0.253
Log likelihood	-2,968.435
Restricted log likelihood (LR)	2,014.04
Observations	28,520
Number of blocks	28,520



Appendix 3. Distribution of LS across Blocks in Bogota