ASOCIACION ARGENTINA
DE ECONOMIA POLITICA

ANALES | ASOCIACION ARGENTINA DE ECONOMIA POLITICA

XLIII Reunión Anual

Noviembre de 2008

ISSN 1852-0022
ISBN 978-987-99570-6-6

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May 29, 2008

Resumen

Este trabajo estudia el efecto de una ley que limita la venta de alcohol en kioscos y estaciones de servicio (ley seca) sobre las tasas de mortalidad asociadas a accidentes de tránsito (AT). El análisis explota el hecho de que cada provincia impuso la ley en distintos momentos del tiempo. Esta variación observada en el tiempo y en el espacio provee un instrumento potencial para identificar el efecto causal de la restricción sobre los muertos en AT. Luego de controlar por efectos fijos por provincia y por tiempo, se encuentra que la ley seca está asociada a una reducción del 14 por ciento en los muertos de AT. Este resultado es especialmente importante en el caso de Argentina, donde 8.000 personas mueren anualmente en AT y donde el 37 por ciento de las muertes son causadas por el abuso de alcohol.

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Abstract

This paper studies the effect of a law that prohibits the sale of alcohol at night for convenience stores and gas stations (usually called dry law) on car accident mortality rates. The analysis exploits the fact that each province decided to pass the law at different points in time. This observed variation in the implementation of the law across time and space provides a potential instrument to identify the causal effect of alcohol restriction in car mortality rates using the panel nature of the data. After controlling for province and year fixed effects, I found that the dry law is associated with reduction of 14 percent in car accident fatalities. This result is especially important in the case of Argentina where 8,000 people die annually in car accidents and where 37 percent of these deaths are caused by alcohol abuse.

I. Introduction

In the last years there has been an important concern in terms of public policy about reducing the risks associated with car accidents. According to the World Health Organization approximately 1,200,000 people die annually in the world in car accidents, 400,000 of whom are young people under the age of 25. Road traffic crashes rank as the 11th leading cause of death and account for 2.1 percent of all deaths globally. Motor-vehicle accidents are responsible for more annual deaths than AIDS, tuberculosis, and malaria. The most important feature is that 90 percent of these deaths have occurred in developing countries (see Table 1).[1]

One of the main causes of car accidents fatalities is alcohol abuse when driving. A report of the World Medical Association shows that in many countries where alcohol consumption is part of daily life, driving under the effects of alcohol is the cause of almost half of deaths and serious injuries in car accidents. In many high-income countries about 20 percent of fatally injured drivers have an excess of alcohol in their blood. In low-income countries the number is even worse, as alcohol is present in between 33 percent and 69 percent of fatally injured drivers. In the case of Argentina, where nearly 8,000 people die per year, 37 percent of motor-vehicle accidents are attributable to excessive alcohol consumption and these alcohol-related accidents are the main cause of young people deaths, leaving behind tumors and heart diseases.[2]

Table 1. Motor-Vehicle Fatalities in the World

	Number of motor-vehicle fatalities	Rate per 100,000 habitants	Proportion of the total
Countries with low and middle income	1,065,988	20.2	90
Countries with high income	117,504	12.6	10
Total	1,183,492	19.0	100

Source: World Health Organization (2002).

In addition to human losses there is an enormous economic cost, estimated as 1 percent of the National Gross Product (NPG) in low-income countries, 1.3 percent in those with middle income, and 2 percent in countries of high income.[3]

Many policies have been implemented in order to restrict alcohol consumption and reduce alcohol-related accidents such as higher beer taxes, driving laws, a minimum legal drinking age, and limited stores hours. There is strong evidence that alcohol consumption is sensitive to some of these policies. Using U.S. state level data, Cook and Tauchen (1982) find that excise taxes significantly reduce alcohol consumption and heavy drinking. Saffer and Chaloupka (1989), using a cross sectional time series data set for forty-eight U.S. states, verify that both alcohol taxes and minimum drinking-age laws reduce state level motor vehicle fatalities which are strongly correlated with drinking. In contrast, there is less agreement regarding the impact of other alcohol control regulations, such as limitations on selling hours.

In this study I test the effectiveness of a law that deals with the prohibition of alcohol selling during night-time hours at convenience stores and gas stations in Argentina. This law is often called dry law when it is applied in countries where alcohol is accepted as an allowed drug.[4] The purpose of this law is to reduce street alcohol consumption and therefore alcohol-

[1] World Bank & World Health Organization (2004). "World Report on road traffic injury prevention."
[2] Data provided by *Luchemos por la Vida* (Let's fight for life). This is a non-profit organization whose only purpose is to help prevent traffic accidents in Argentina.
[3] Report of the World Bank (January of 2000).
[4] Similar laws which restrict alcohol consumption are applied in Chile, Ecuador, the Dominican Republic, Spain, United States, Colombia and Costa Rica.

related car accidents and delinquency under the effects of alcohol. In this paper, I study whether the dry law has a causal effect on motor vehicle fatalities. It is especially important to study the potential effect in the case of Argentina which counts among the countries with the highest car mortality rates (see Table 2).

Table 2. Car Mortality Rates (per 1,000,000 vehicles)

China	2,033	Denmark	267
Rumania	1,414	Luxembourg	258
Argentina	**1,310**	Austria	257
Poland	890	Belgium	249
Turkey	752	Switzerland	212
Greece	650	United States	209
Slovak Republic	590	Holland	206
Bulgaria	519	Germany	188
Hungary	511	Italy	186
Portugal	466	Canada	180
Rep. Checa	444	Finlandia	179
Ireland	345	Australia	164
Israel	332	Great Britain	160
France	329	Japan	145
Spain	302	Sweden	132

Source: *Luchemos por la Vida.*

The expected effect of this law on car accident fatalities is not evident. One might expect a decrease in the number of car accidents fatalities by decreasing the availability of alcohol during the hours in which alcohol fatalities are substantially higher.[5] However, if people simply find an alternative location to purchase alcohol the impact of the law might be ameliorated. Furthermore, the existence of black markets could eliminate the positive effect of the law.

This paper attempts to find the causal effect of the law, using detailed data on alcohol laws in every province of Argentina between 1990 and 2005. Two important contributions are made.

First, to the best of my knowledge this paper is the initial study done for developing countries. There are many empirical studies for the United States that provide some evidence of the effect of dry laws in alcohol-related accidents but there are no studies for low and middle income countries, which account for 90 percent of car accident mortality rates. Nevertheless, these U.S. studies give an idea of the expected effect, suggesting that alcohol control regulations reduce fatalities. Brown, Jewell and Richer (1996), using a two-stage estimation and county-level data on the state of Texas find that county-level alcohol prohibition decreases alcohol-related motor vehicle accidents and fatalities. Winn and Giacopassi (1993) report that Kentucky counties that prohibit alcohol sales have significantly lowered alcohol-related motor vehicle accident rates. In the same line, Chaloupka et al. (1993) find a negative impact on state-level car mortality rates, and McCarthy (1999) concludes that bans on common site sale of alcohol and gasoline increases the number of alcohol-related fatalities outside metropolitan areas. The intuition behind this result is that citizens may react to the ban by traveling to other counties to purchase and consume alcohol, leading to the possibility of increased time on the road for drunk drivers and thus more accidents.[6]

[5] Dee (1999) suggests that alcohol involvement in fatal accidents is higher at night-time than at day-time. In the same line Grabowski et al. (2001) report that teen traffic fatalities are concentrated during night-time, between these hours teen driver death rates are nearly three times greater per trip than during the hours from 6 a.m. to 10 p.m.

[6] This may not be the case of Argentina where the restriction is applied only to convenience stores and gas stations.

Second, while the existing literature focused on cross-state comparisons of alcohol policies and car accident fatalities without controlling for unobserved variables that are correlated with cross-state variations in alcohol policies and thus producing biased estimates, in this study I provide evidence controlling for province time-invariant unobserved heterogeneity and common shock for all provinces. To do so I take advantage of the fact that in Argentina there is no national dry law and that each province decides to pass or not to pass the law. In addition, each province decided to set the law at different moments of time. The observed variation across time and space provides a potential instrument to identify the causal effect of alcohol restriction on motor vehicle fatalities using the panel nature of the data. It is important to note that none of the previous studies use matching methods and very few of them incorporate time-fixed effects and state-fixed effects. Furthermore, many previous studies have ignored that differences in car mortality rates are influenced by economic conditions, suffering from omitted variables bias. In attempt to address this issue I include controls for unemployment, public expenditure, and regional gross domestic product (GDP).[7] In the case of Argentina it is not necessary to control for other programs or laws which may be simultaneously operating to reduce drunk driving because most of them are all national, such as the National Traffic Law 24,449 and the Law 24,788, and therefore any effect arising from these laws would be captured by the time effect.[8] A major methodological concern is that the choice to pass the law may not be orthogonal to unobservable factors that also affect car mortality. I address this concern in a number of ways that lead me to believe in the existence of a causal effect between the law and the number of fatalities.

The main finding of this paper is that alcohol restriction on sales in convenience stores and gas stations is actually associated with a reduction of 14 percent in the number of car accident fatalities. This finding does not change as a result of different specifications, suggesting the existence of a causal effect. The validity of the causal interpretation of the estimates is tested using other causes of mortality. It is found that while the imposition of the law is correlated with car mortality rates, it is uncorrelated with deaths from causes unrelated to alcohol consumption.

The rest of the paper proceeds as follows: Section II describes the data and Section III presents the specification strategy. Finally, Section IV concludes.

II. Data

The data is a panel of observations of 23 provinces of Argentina over the period 1990-2005. Three sets of data are used: province-level data on unintentional deaths in road accidents; information on the dates in which the law was passed; and province-level information on gross domestic product, unemployment rates, expenditure rates, citizen involvement, the number of registered vehicles, and weather conditions.

As there is no measure of alcohol-related car fatalities, the study uses car fatalities. This first dataset comes from two sources: Argentina's national office on crime statistics (*Registro Nacional de Reincidencia)*; and the National Crime Information System of the Ministry for Justice (*Sistema Nacional de Información Criminal del Ministerio de la Justicia)*. Information on car accident mortality from 1990 to 1997 was provided by the *Registro Nacional de Reincidencia* while the information from 1999 to 2005 was provided by the National Crime Information System.[9] These two sources compile the information registered by security forces, provincial police, federal police, gendarmerie, and naval prefecture based on the monthly number of

[7] Ruhm (1995) have pointed that only a few studies (Wagenaar and Maybee, 1986; Saffer and Chaloupka, 1989; Chaloupka *et al*., 1993; Evans, 1993) have accounted for differences in unemployment rates.
[8] The Law 24,788 was designed in 1997 to reduce alcohol consumption. One of it main regulation was to prohibit alcohol selling to people under the age of eighteen.
[9] There is no available data for the provinces in the year 1998.

criminal reports. The number of fatalities in road accidents registered by the police and law enforcement agencies does not include unreported casualties or those reported directly to some judicial court (Judge, Office of the Public Prosecutor, Chamber).[10]

It is important to note that the implementation of the dry law does not affect the procedure by which the observed outcome (the number of car accident fatalities) is measured and therefore there is no potential bias in the estimated effect coming from this source.[11] Car accident mortality rates per 100,000 inhabitants are presented in Table 3. A total of 336 observations are available for estimation and variability exists across provinces and time in car mortality rates.

Table 3. Car Accident Mortality Rates per 100,000 inhabitants

	Mean	Std. Dev.	Min	Max
Buenos Aires	12.23	5.02	6.63	22.75
Ciudad de Buenos Aires	7.79	6.34	2.94	27.90
Catamarca	14.49	5.97	6.20	29.36
Córdoba	10.49	2.02	5.71	14.60
Corrientes	13.42	4.34	9.59	26.59
Chaco	10.50	1.69	7.13	12.68
Chubut	7.95	6.78	1.42	29.13
Entre Rios	11.96	3.36	7.47	18.43
Formosa	7.54	2.56	2.70	12.50
Jujuy	13.82	5.25	6.55	21.70
La Pampa	19.90	9.40	9.95	4.13
La Rioja	16.08	5.15	1.05	2.51
Mendoza	14.29	2.06	9.80	16.56
Misiones	13.02	2.48	8.08	17.17
Río Negro	11.08	3.15	6.05	16.71
Salta	10.45	2.96	5.21	17.86
San Juan	17.86	5.37	12.16	33.64
San Luis	15.75	6.24	10.00	36.65
Santa Cruz	15.44	5.32	5.10	25.53
Santa Fe	14.03	2.61	11.18	18.52
Santiago del Estero	11.31	3.51	4.23	17.97
Tierra del Fuego	6.58	2.62	2.76	10.76
Tucumán	11.35	1.99	8.60	14.87

Note: Each mean was calculated taking into account the whole period (1990-2005).

[10] Although the data was provided by two different offices, they came from the same Ministry and thus, there was no methodological change.

[11] This type of bias might appear, for example, if a policy consisting of changes in blood alcohol content requirements is accompanied by increased efforts by police officers to find drunk drivers.

Table 4 presents average car mortality rates before and after the implementation law. In most provinces where the dry law was implemented, car mortality rates decreased.

Table 4. Before and After Comparisons of Car Accident Mortality Rates

"Dry Law" Jurisdictions	Pre-Treatment period	Post-Treatment period
Buenos Aires	16.13	9.64
Ciudad de Buenos Aires	8.34	4.18
Córdoba	10.18	12.48
Corrientes	15.51	11.04
Chaco	9.8	11.29
Chubut	9.33	2.4
La Pampa	25.54	16.15
La Rioja	17.68	11.69
Misiones	12.88	13.92
Río Negro	11.36	9.26
San Juan	18.94	13.9
Santa Cruz	14.99	18.39
Santa Fe	14.31	12.22
Tierra del Fuego	7.79	4.97
Tucumán	12.1	9.29
All	13.66	10.72

The information about the dates in which the law was passed was obtained from the legislatures of each province. In every province the law is very similar. In general it prohibits the sale of alcohol in the entire territory between 11:00 pm and 8:00 am, with the exception of bars, dance clubs, and restaurants.[12] The treatment variable is a dummy indicating if the province has passed the law or not. Table 5 describes the changes in the status of the alcohol law between 1995 and 2005. Over the analyzed period, most of the provinces have passed the law in different moments. This variation in time and in space is used to identify the causal effect in car accident deaths.

Table 5. Jurisdictions with Alcohol Restrictions

"Dry Law" Jurisdictions	Date	Department
Buenos Aires	1996	All
Ciudad de Buenos Aires	2004	All
Córdoba	2004	*Ciudad de Córdoba*
Corrientes	1999	*Ciudad de Corrientes*
Chaco	1999	All
Chubut	2003	All
La Pampa	1996	All
La Rioja	2001	All
Misiones	2004	*Posadas*
Río Negro	2004	All
San Juan	2003	All
Santa Cruz	2004	All
Santa Fé	2004	*Ciudad de Santa Fé, Rosario*
Tierra del Fuego	2000	All
Tucumán	2002	All

[12] In the case of *Buenos Aires*, *Santa Fe*, and *Chubut,* there is a more restricted law which prohibits alcohol sale in convenience stores and gas stations the whole day. No separation into two different treatments was done due to lack of statistical power.

The third dataset contains province-level information on control variables. The unemployment rate, the gross domestic product, and the alcohol expenditure rate were obtained from the National Institute of Statistics and Census (*Instituto Nacional de Estadística y Censos*) and the Bureau of Federal Investments (*Consejo Federal de Inversiones*). For the population and the gross domestic product variables, projections are made for every year based on information in 1990, 1991, 1993 and 2001. Data corresponding to public expenditure and percentage of paved roads was collected from the Ministry of the Economy. I use the precipitation rate as a proxy for weather conditions. This variable was obtained from the National Meteorological Service (*Servicio Nacional Meteorológico*). The number of registered vehicles was provided by the National Registry of Motor-Vehicle Property (*Dirección Nacional del Registro Automotor*). Citizen involvement was calculated as the ratio of voters that actually vote in presidential elections and was obtained from the *Ministerio del Interior de la República Argentina*.[13] Finally, the number of doctors and life expectancy was obtained from the Ministry for Health.

Table 6 compares some characteristics related to car mortality across all provinces in the pre-treatment period. In all cases I cannot reject the hypothesis of equality between the control and the treated group in the pre-intervention period.[14] In particular, the treated and the control group showed similar levels of car mortality rates before the implementation of the law.

Table 6. Pre-Treatment Characteristics

	Mean for the Control Group	Mean for the Treated Group	Difference
Car mortality rate	13.375	13.812	-.217
	(.836238)	(.836238)	(1.17)
Population	668395	1771208	-1102813
	(140325.5)	(807348.6)	(1122217)
Gross domestic product	7364.986	6191.364	1173.622
	(3611.714)	(1072.775)	(2980.195)
Unemployment rate	.055	.070	-.001
	(.009)	(.006)	(.011)
Public Expenditure	109.625	133.666	-24.041
	(9.494)	(21.738)	(30.851)
Citizen involvement	82.125	83.06667	-.9416667
	(1.652)	(1.232)	(2.075)
Number of registered cars	13690.63	20739.27	-7048.642
	(2381.666)	(2069.292)	(3332.612)**
Precipitation	783.25	834.733	-51.483
	(117.602)	(130.224)	(198.928)
Percentage of paved road	73.375	75.866	-2.491
	(7.485)	(5.991)	(9.870)
Life expectance	70.281	70.984	-.703
	(.509)	(.287)	(.539)
Alcohol risk consumption rate	1.046	1.2	-.153
	(.078)	(.068)	(.109)
Number of doctors	237.600	310.929	-73.329
	(19.940)	(57.810)	(81.295)

Note: Standard errors are in parentheses.

[13] For the variable citizen involvement I have data only for the years 1989, 1995, 1999 and 2003. For the missing years, I suppose the same citizen involvement (for example if the citizen involvement was about an 87 percent in 1990 for Buenos Aires, I take the same percentage for the following years 1991,1992,1993,and 1994).

[14] I can only reject the hypothesis null of equality between the control and the treated group for the number of registered cars. Nevertheless this might not be a concern because I will include this variable in my regression model.

These results can be interpreted as evidence that the pre-intervention characteristics of the treatment and control group are similar and suggests that the control group may provide a good counterfactual of what would have happened to the treated group in the absence of the intervention. Also, it is shown not to be the case that the provinces where this law was implemented were those with higher rates of car accidents and thus where citizens pressured for this type of law.

III. The Effect of the Dry Law on Car Accident Mortality

A. Empirical Strategy

The purpose of this paper is to identify the average effect of the dry law on car mortality rates, comparing car accident fatalities of provinces that passed the law to provinces that did not pass the law. As there is no experimental design, it cannot be asserted that the decision to implement the legislation was random in each province. Different particular characteristics in the provinces may have affected the decision of whether or not to implement the law. It could be the case that some provinces—for example those whose governors are more concerned about alcohol consumption—have implemented this type of laws earlier than other provinces. In this sense, correlation between the dry law and certain factors that may influence fatalities may lead to a biased estimate. Many variables that potentially confound the identification are those that vary across provinces but are fixed over time, such as the number of highways, customs, and attitudes of each society towards alcohol consumption.[15] In order to control these time-invariant unobserved variables, I use panel data and estimate a difference-in-differences model. This model compares the evolution in outcomes in the treatment group before and after the intervention to the evolution in outcomes in the control group. The evolution in the control group is an estimate of what would have happened to the treatment group with no intervention.

The difference-in-differences estimator includes year fixed effects that control for any common shocks for all provinces and province fixed effects that control for time-invariant influences. Formally, the difference-in-differences model can be represented by the following equation:

$$CarMortality_{it} = \alpha DryLaw_{it} + \beta X_{it} + \gamma_i + \mu_t + \varepsilon_{it} \qquad (1)$$

where $CarMortality_{it}$ is the car mortality rate in province *i* in year *t* (car fatalities of province *i* in year *t* per 100,000 inhabitants); $DryLaw_{it}$ is a dummy variable that takes the value of one if the province *i* has a dry law in year *t* and zero otherwise; X_{it} is a vector of control variables that varies across provinces and time; μ_t is a time effect common to all provinces in period *t;* and γ_i is a province fixed effect.

The inclusion of province and time fixed effects is crucial for the identification of the parameters of interest as it guarantees that there will not be a bias coming from differences in car mortality rates across provinces that are time-invariant and differences in car mortality rates across time that are common to all provinces.

The X_{it} vector includes variables like regional gross domestic product, unemployment rate, public spending, citizen involvement, number of registered vehicles and precipitation.

GDP and the unemployment rate are used as proxies for macroeconomic conditions. GDP may be positively correlated with the implementation of the law in two ways. First, provinces with higher gross domestic product may tend to have better institutions and hence more laws. Second, provinces with higher GDP can have more alcohol related accidents since alcohol consumption increases in better economic times, therefore leading to more car accident fatalities and thus a higher likelihood of passing the law. The possible level and direction of correlation between unemployment rates and car accident mortality rates is unclear. On the one

[15] While many highways were constructed in Buenos Aires in the 1990's, there is no variability across other provinces.

hand, the two might be negatively correlated since total alcohol consumption and the proportion of drinking that occurs in bars and restaurants is likely to fall during recessions.[16] On the other hand, the unemployment rate may be positively correlated with car accident mortality rates since unemployed workers could drink more due to depression.

The public spending variable gives an idea of the expected probability of detection of drunk drivers and it controls for road conditions through expenditure on urban improvement. It is important to note that I did not include whether or not the roads are paved because these variables are time invariant and therefore it is captured by the province fixed effect.[17]

The number of registered vehicles captures the traffic density; the expected sign of this variable is not evident. On the one hand, traffic density is positively correlated with car accident frequency while on the other hand it may decrease fatal accidents as driver speed is lower when traffic density is higher.

The vector of control variables also includes a proxy for citizen involvement represented by voter participation. The inclusion of this variable is important for two reasons. First, as the vote in Argentina is mandatory, it represents citizens' willingness to obey laws. In provinces where citizen involvement is higher, laws are more likely to be enforced and people might be more respectful of laws. Furthermore, in these provinces citizens may conduct themselves more responsibly and partake less frequently in drunk driving. Second, citizen involvement may be correlated with the number of laws passed in each province.

Finally, as the correlation between the alcohol law and car accident deaths may be confounded with a weather effect, I include the amount of precipitation. Provinces with rainy weather and thus a higher frequency of car accidents might be more likely to pass the law.

The error ε_{it} is a province time-varying error which is generally assumed to be independent across time and space; however, as the analysis uses panel data, the errors could be correlated across time in the same province. In the case of a positive correlation, the standard errors could be computed smaller and the null hypothesis could be over rejected. To avoid potential biases in their estimation, standard errors are clustered at the province level, allowing an arbitrary covariance structure within provinces over time.[18] It is important to note that if the province errors are highly correlated, clustering standard errors may reduce the statistical power of the estimation which is the probability of detecting a causal effect when the effect really exists.[19]

In this model, α is the parameter of interest, as it estimates the average treatment effect on the treated. The key identifying assumption of this model is that the change in mortality in the control group is an unbiased estimator of what would have happened to the treated group if there had been no intervention. Although this assumption cannot be tested directly, it is possible to test if trends in the pre-treatment period are similar for the treated and the control groups. If in the pre-intervention period the trends are not different then it is possible to assume that in the absence of the treatment the trends would remain the same in the post-intervention period. Formally, this assumption is tested estimating a modified version of equation (1).[20] In this model, the null hypothesis that the pre-intervention year dummies are the same for the eventually treated provinces and the control provinces cannot be rejected. This result suggests

[16] Ruhm (1996) has shown that individuals may shift drinking away from bars where alcohol is relatively expensive during downturns.
[17] See Statistical yearbooks of the National Institute of Statistics and Census (INDEC, 1990-2005).
[18] See Bertrand *et al.* (2004).
[19] This could be a problem in the case of not finding any effect of the law. Unfortunately, in the case of not finding an effect, nothing can be done to solve this problem because the number of provinces cannot be increased, which is one possible solution to the power problem. One possible solution to the power problem is to increase the sample size, including more groups. In this study, each province conforms to a group (so there are 23 groups). Therefore, the sample size cannot be increased because the analysis uses all the provinces and there are no more groups (provinces) to add.
[20] As in Galiani et al. (2006) the estimation only considers the observations of the pre-intervention period. The control group has 16 observations and the treated group only consists of pre-treatment observations. Therefore, a dummy is generated, which takes the value of 1 if the province will be eventually treated and 0 if the province will never be treated. Finally, an interaction is made between this dummy and the year dummy; and the model (1) is estimated without the dry law dummy.

that car mortality rates in the treated and control provinces could have equal trends in the pre-intervention period, giving validity to the difference-in-differences approach.[21]

B. Results

Column 1 in table 7 presents the results for a model including only the dry law dummy, the fixed effects for each province, and the year dummies. Provinces that have passed the alcohol-related law experience a statistically significant reduction in car mortality rate of about 1.57, which amounts to an 11.4 percent reduction of the baseline rate.

One concern regarding the difference-in-differences model is that some characteristics that vary across time and across provinces can be correlated with car mortality and the implementation of the alcohol law. To address this issue, column 2 includes a set of socioeconomic characteristics such as the unemployment rate, public spending, and GDP. The variable public spending includes road infrastructure investment and police expenditure. It could happen that provinces which passed the law were also spending more budgets on road improvements, so the effect of the dry law could be coming from public expenditure. Nevertheless, the estimated impact of the law is unchanged. It is important to note that the population is not included as a control variable since all the variables are calculated on a per capita basis. The only significant control variable is unemployment and its coefficient suggests that during recessions alcohol consumption decrease and thus car accidents.[22]

In the fourth column, I add controls for citizen involvement, number of registered vehicles and precipitation. Little change in the policy's effect is observed (16 percent with five percent of statistical significance).[23] Furthermore, the results are unaffected when standard errors are computed by clustering at the provincial level.

Overall, these results suggest that the introduction of a law that restricts the sale of alcohol for convenience stores and gas stations generates a significant decline in car mortality rates.[24] The rest of the paper will focus on the specification of the model.

C. Heterogeneous Provinces

The difference-in-differences approach does not take into account the possibility that some provinces may not serve as good controls. In this section, I use matching methods to attempt to solve this problem.

Matching methods eliminate the aforementioned bias by comparing treated provinces with control provinces that have similar characteristics. The assumption behind matching method is that conditional on some observable variables, the counterfactual outcome distribution of the treated provinces is the same as the observed outcome distribution of the units in the control group. Formally, a propensity score for each province is estimated using a logit model, which estimates the probability of being treated conditional on a set of pre-intervention characteristics.[25] In this case, the probability of passing the law is conditioned on GDP, the public expenditure rate, the unemployment rate, the citizen involvement rate, the car mortality rate, the amount of precipitation, the number of registered vehicles, the proportion of doctors, life expectancy, the alcohol expenditure rate, and the percentage of paved roads. Based on the propensity scores, I keep only the observations which are on the common support. All control provinces whose propensity scores are less than the minimum propensity score of the treated

[21] See results in the appendix (Table A1).
[22] Chaloupka *et al.* (1993) has also found a similar result.
[23] For robustness purpose, all the models were also estimated without the control citizen involvement and results did not change.

[25] Rosembaum and Rubin (1983) show that if it is valid to conduct matching on X´s, it is equally valid to do so for the propensity score. The advantage of using the propensity score is that it has one dimension. The propensity score is P (T=1/X) which, of course, has one dimension.

provinces and all treated provinces whose propensity scores are higher than the maximum propensity score of the control provinces are excluded. Therefore, with these observations, I estimate a difference-in-differences model on the common support.[26]

Finally, I use the kernel density weighting procedure to obtain a fixed effect matching estimator similar to the difference-in-differences matching estimator.[27] This procedure estimates the average treatment effect on the treated after using propensity score matching to minimize the differences between the treated and control group. This estimator combines the advantages of using matching methods with the benefits of estimation using the difference-in-differences model.

The results of these models are reported in Table 7. The results do not change when the observations are restricted to the common support. Column 9 presents the results of the kernel matching model in the common support. A reduction of about 14 percent is found and is significant at the 5 percent level.[28]

[26] The region of common support is [.14341644, 1] and the balancing property is satisfied.

[27] The kernel matching procedure gives a positive weighting to all provinces in the control group but the weighting is inversely proportional to the distance in propensity scores.

[28] For robustness purpose, all the models were also estimated in logs with similar results. All regressions mention and not show are available.

Table 7. Impact of the Dry Law on Car Accident Mortality Rates

	FULL SAMPLE				USING OBSERVATIONS ON COMMON SUPPORT				KERNEL MATCHING[a]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
DryLaw(=1)	-1.571208	-2.062532	-2.201858	-1.902302	-1.599648	-2.077977	-2.330818	-1.986002	-1.832
	(.8852299)*	(.9312431)**	(.9412235)**	(.9564898)**	(.8994801)*	(.9422438)**	(.9555845)**	(.9739893)**	(0.740)**
	[.8614502]*	[.9036023]**	[.9430633]**	[.9665846]**	[.894417]*	[.079158]**	[.9871087]**	[1.00801]**	
	{.9618942}	{.6616625}***	{.7176608}***	{ .7882377}**	{.96953}	{.6657079}***	{.7353158}***	{.8072872}**	
Δ% in mortality rate	-11.4	-15	-16	-13.8	-11.6	-15.1	-16.9	-14.4	-13.3
GDP per capita		-.0001796	-.0002149	-.0001186		-.0001924	-.00025	-.0001489	
		(.0002242)	(.0002269)	(.0002361)		(.0002242)	(.0002271)	(.0002365)	
		[.000245]	[.0002433]	[.0002575]		[.0000201]	[.0002442]	[.0002563]	
		{.0004567}	{.000435}	{.0004311}		{ .000461}	{.0004278}	{.0004198}	
Unemployment rate		-30.72676	-30.31064	-31.96489		-27.31604	-25.81227	-27.91007	
		(12.24095)**	(12.24698)**	(12.41652)**		(12.68169)**	(12.69179)**	(12.84406)**	
		[12.23258]**	[12.14504]**	[12.37045]***		[1.120588]**	[12.47874]**	[12.63554]**	
		{12.83959}**	{13.04981}**	{12.62346}**		{13.67664}*	{13.60569}*	{13.45073}**	
Public Expenditure rate		-.0003859	-.0005679	-.0003878		-.0003666	-.0006457	-.0004637	
		(.0007709)	(.0007914)	(.0008079)		(.0007731)	(.0007943)	(.0008101)	
		[.0007667]	[.0008349]	[.0008281]		[.0000668]	[.0008555]	[.0008454]	
		{.000805}	{.0008836}	{.0008374}		{.0008344}	{.0009161}	{.0008573}	
Citizen Involvement			.1634018	.1432987			.2437653	.2160122	
			(.1608231)	(.1624297)			(.1661662)	(.1675703)	
			[.1650702]	[.1664365]			[.1656662]	[.1646803]	
			{.1878166}	{.1996894}			{.1741906}	{.180947}	
Number of registered vehicles				-1526576				-1513959	
				-107662				-1074202	
				[14.60204]				[14.61882]	
				{10.59999}				{10.19451}	
Precipitation				.0011331				.001282	
				(.0009906)				(.0010119)	
				[.0010849]				[.0011099]	
				{.001523}				{.0015558}	
R^2	0.12	0.22	0.26	0.20	0.11	0.21	0.26	0.22	
Observations	336	282	282	282	322	271	271	271	

Note: Each column reports the estimated coefficients of a regression model in which the dependent variable is the car mortality rate, whose mean is 13.73 during 1990-1995. Standard errors are in parentheses. Robust standard errors are in brackets. Standard errors clustered at the province level are in braces. All the regressions include year and provinces fixed effects. a- Standard errors for the kernel matching estimate are bootstrapped standard errors.

* Statistically different from zero at the .1 level of significance.

** Statistically different from zero at the .05 level of significance.

*** Statistically different from zero at the .01 level of significance.

D. Impact of the Dry Law in Other Causes of Mortality

In spite of the robustness of the estimates to different estimation methods, one could still argue that when the dry law was passed other unobserved changes correlated with mortality in general may have an effect in provinces where the law was implemented. For example, it could be the case that provinces that pass the law are more active in terms of public policy. Thus, the governments who pass the law may be more concerned about mortality in general and not only on car accident mortality. In this case, there may be unobserved variables which are correlated with the passing of the law. To address this issue, a difference-in-differences model is estimated for other types of mortality, such as infections, tumors, and cardiovascular diseases.[29] If the estimation presented in the previous section is correct, the dry law should not operate affecting mortality rates which are not correlated with alcohol abuse.

Table 8. Impact of the Dry Law in other Causes of Mortality

	1990-1995 Mean Mortality Rate	Estimated Impact Coefficients	Δ%in Mortality Rate
Mortality in General	6.65942	0.0863804	12.9
		(.1033147)	
		[.1132787]	
		{.1491665}	
Infectious Diseases	64.68841	1.547875	2.3
		(8.63611)	
		[4.600039]	
		{6.041139}	
Tumors	128.8043	7.654269	5.9
		(20.54315)	
		[8.092138]	
		{7.306383}	
Cardiovascular diseases	236.1159	2.279264	0.96
		(36.26529)	
		[15.30938]	
		{16.17147}	

Note: Each cell reports the estimated coefficient from a different difference in-differences regression. Standard errors are in parentheses. Robust standard errors are in brackets. Standard errors clustered at the province level are in braces. All the regressions include year and province fixed effects.

As shown in Table 8, the null hypothesis cannot be rejected. It is not statistically significant, which suggest that the dry law has no influence on other sources of mortality and that it has only affected alcohol-related casualties such as car accident mortality rates. This result provides evidence that the law has only affected car accident mortality rates through a reduction of alcohol consumption when driving. Furthermore, this result suggests that any other plausible explanation of the main estimates can be eliminated and strengthens the causal interpretation.

[29] The impact of the dry law is only estimated for these particular alternative causes of mortality because there was no data available on other causes.

IV. Conclusions

This paper shows that alcohol-related laws in Argentina have reduced car accident casualties. It provides evidence that decreasing the availability of alcohol during the hours in which fatalities are substantially higher is a policy that reduces car accident casualties. Using a combination of methods, I found that provinces which passed the law experienced, on average, a reduction of about 14 percent in car accident mortality rates. This study suggests one way to reduce alcohol consumption and hence car accidents fatalities in the context of developing countries, where the number of car accident deaths have been substantially increasing in the last years. While the previous literature has focused on the effect of dry laws in developed countries, this paper provides evidence for a country which belongs to the group of developing countries, which together account for 90 percent of car accident mortality rates.

Many factors suggest that the relation between the dry law and car accident fatalities might be causal. First, the treatment and the control group exhibited similar time trends in the pre-intervention period, validating the difference-in-differences identification strategy. Second, the model includes provinces and time fixed effect and the conclusions are robust to the inclusion of variables that may affect drunk driving behavior and may also influence car accidents casualties. It also remains robust after controlling for heterogeneous provinces, redefining the control and treatment groups according to each province's propensity score. Furthermore, the results remain unchanged when the control group is re-weighted with the kernel density procedure. Third, it demonstrates that the dry law only affected car mortality rates and not other types of deaths where alcohol is not involved.

The results shed light on a number of important policy debates. This result is very important in Argentina where the dry law has been criticized frequently, mainly by consumers and retailers who have argued that the dry law restricts personal liberties and generates losses as it limits the sale of alcohol to fewer hours of the day. Moreover, a commonly held opinion was that this type of law could not be enforceable and that it was only a strategy of politicians to show the society a commitment to alcohol concerns. This work, however, shows that the law was enforceable and with significant results in car accidents fatalities. As Hobbes said "man is a wolf to man," the society needs this kind of laws to protect people from others and from themselves. In this sense, more attention ought to be paid to policies regarding alcohol consumption because they may have highly beneficial effects.

Appendix

Table A1. Test of trends

Inter1990	.1379642
	(.2310363)
Inter1991	-.1886652
	(.2322094)
Inter1992	.2108176
	(.2310363)
Inter1993	-.0683793
	(.2310363)
Inter1994	.0337442
	(.2326525)
Inter1995	-.01365
	(.2310363)
Inter1996	.2234399
	(.2452311)
Inter1997	.1522996
	(.2452311)
Inter 1999	.2697993
	(.2366806)
Inter2001	-.0120813
	(.2420228)
Inter2002	-.0711589
	(.2459588)
Inter2003	.0436593
	(.2620646)
Year fixed effect	Yes
Province fixed effect	Yes
R	271
Observations	0.10

Note: standard errors are in parenthesis

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