Does State Monopolization of Alcohol Markets Save Lives?

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Today, nineteen states control alcohol sales via state monopolies at the wholesale and/or retail levels. Previous research has compared alcohol-related fatality rates to the National Alcohol Beverage Control Association's binary classification of states as "control" versus "license", and has done so by either examining a single state over time or a large number of states at a point in time. Absent from the literature is a robust panel analysis that looks at all states over a long period of time, employs panel data analytic techniques, and accounts for the degree of control states exercise over wholesale and retail alcohol markets. This paper presents a panel study of alcohol-related fatality rates for 49 states from 1982 to 2002 that, after controlling for related alcohol control policies, compares four measures of fatality rates to the degree of state monopoly control of alcohol markets. We find that the relationship between control and DUI fatalities varies based on the degree of control. Consistent with some previous literature, we find no difference in fatality rates for more stringent levels of control versus no control. Interestingly, we find higher fatality rates for lesser levels of control versus no control.

I. Alcohol-Control Policy and Policy Targets

Studies on the efficacy of alcohol controls have focused on the three broad categories of market intervention:

- Demand mitigation. Laws aimed at mitigating demand target by whom and the manner in which alcoholic beverages can be consumed. Among others, these laws include age restrictions, public consumption, and DUI/DWI laws. What the laws have in common is that they put a restrictive burden on the alcohol buyer.
- Supply mitigation. Laws aimed at mitigating supply target by whom and the manner in which alcoholic beverages can be sold. These laws include government ownership of retail or wholesale outlets, restrictions on outlet density, and restrictions on hours of operation. These laws and others like them place restrictive burdens on the alcohol seller.
- Transaction mitigation. Laws aimed at mitigating transactions target the act of buying and selling. These laws principally take the form of taxes and, in more rare cases, price controls.

While some laws may fall into more than one category (for example, depending on how they are enforced, keg registration laws could be considered demand mitigating or supply mitigating or both), the categories broadly reflect differences in policy efficacy noted in the literature. For example, in their meta studies, Carpenter and Dobkin (2010), Campbell et al. (2002), Grube and Nygaard (2001), and Her et al. (1999) find that studies report markedly different degrees of policy success depending on whether the policies are targeting alcohol demand, alcohol supply, or alcohol transactions.

In addition to various policy variables, studies have looked at various policy targets including:

- Sales/consumption mitigation. Studies that seek to focus on consumption typically take per-capita sales as a proxy. A reasonable counterargument to using this target is that alcohol consumption is not universally bad nor is a decline in alcohol consumption universally good.
- Underage drinking mitigation. Underage drinking data typically comes from surveys and so the data are less reliable. A major source of data in the United States is the National Survey on Drug Use and Health which asks children ages 12 and over to self-report their alcohol use.
- Underage binge drinking mitigation. As with underage drinking data, underage binge drinking data is also subject to self-report bias. Given the nature of the question (binge drinking), subjects' responses are likely to be even less reliable than responses to underage drinking. Particularly where younger teenagers are concerned, there may be incentives to lie in either direction (for detection avoidance or self-aggrandizement). If underage drinking and underage binge drinking data are subject to bias, so long as the bias is consistent, studies that look at changes in these measures are likely to obtain results that are at least directionally correct.
- Alcohol-related traffic accident mitigation. In the United States, alcohol-related traffic accidents are accidents in which at least one person involved in the accident, and who is not a vehicle passenger, has a blood alcohol content (BAC) above the statutory maximum. It is not necessary for an automobile driver to be the one with the high BAC. For example, if a car strikes a pedestrian and the

pedestrian's BAC is above the minimum, the accident will be classified as alcohol-related.

- Alcohol-related traffic fatality mitigation. An alcohol-related traffic fatality is an alcohol-related traffic accident in which at least one person died.
- Alcohol-involved traffic accident/fatality mitigation. An alcohol-involved traffic accident is an accident in which at least one automobile driver has a BAC above the statutory maximum.
- Crime. Carpenter and Dobkin (2010) offer a review of studies examining the causal effects of alcohol consumption on crime. Studies that examine crime focus on violent and property crimes as opposed to DUI and public intoxication.

The purpose of this paper is to provide an overview of research on the effects of alcohol policy variables on alcohol policy targets, and to add to the discussion by employing more sophisticated data techniques and more complex data sets than have been employed in previous research with a goal to examining the relationship between alcohol market privatization and social outcomes.

II. Previous Research

Contrary to claims in the popular press from both sides of the discussion, research on the effects of controlling alcohol markets have not produced consistent results. Depending on the definition of "social outcome", more control has been shown to be associated with improved outcomes (Stockwell et al. 2009, Miller et al. 2006, Weitzman et al. 2003), worsened social outcomes (McCarthy 2003), no change in social outcomes (Trolldal 2005, Rees 1997, Wagenaar and Holder 1995, Mulford et al. 1992, MacDonald 1986), and both improved and worsened social outcomes (Holder and Wagenaar 1990).

by controlling outlet density (the number of retail establishments per square mile) is closely related to controlling markets by monopolization. States that monopolize retail alcohol sales control outlet density by deciding where to place the state's retail outlets. States with privatized (or partially privatized) retail markets employ density controls to restrict the density of private establishments. With respect to controlling retail prices, the types of beverages sold, hours of operation, and other non-location circumstances, monopolization provides the state with power that outlet density rules do not. However, if a state's goal is merely to control outlet density, monopolization is not necessary – state licensing within a privatized retail market serves the same purpose.

McCarthy (2003) performs a panel analysis of 111 non-metropolitan California cities over the period January 1981 through December 1989. He looks at changes in the densities (establishments per square-mile) of general off-site alcohol licenses (licenses to sell all types of alcohol for consumption elsewhere), general on-site alcohol licenses (licenses to sell all types of alcohol for consumption on the retailer's premises), and off-site and on-site beer/wine licenses

(licenses to sell only beer and wine), and compares these to changes in fatal, non-fatal, and total alcohol-related traffic accidents. McCarthy finds that an increase in the density of general off-site licenses is associated with decreases in fatal, non-fatal, and total alcohol-related traffic accidents, and that an increase in general on-site licenses is associated with increases in non-fatal, but not fatal, accidents. He also finds that, among beer/wine licenses, an increase in off-site license density is associated with a decline in total accidents, but that an increase in on-site density is associated with an increase in non-fatal accidents.

Stockwell et al. (2009) examined the effect of alcohol outlet density and the degree of privatization among retail alcohol stores on alcohol sales in British Columbia. British Columbia provides an interesting case study as the province has permitted a gradual increase in the number of private alcohol stores from 1988 to the present. Unlike McCarthy (2003), who defines density as number of outlets per square mile, Stockwell et al. define density as number of outlets per population. They look at 89 regions within British Columbia over the period April 2003 through March 2008 and claim to find that increased density and increased privatization is associated with increased per-capita alcohol sales. Their results, however, leave unaddressed the question of causality – is increased privatization causing increased sales of alcohol, or is increased demand for alcohol resulting in increased profit opportunities and therefore increased number of private outlets. Finally, the discussion of their statistical results leaves unaddressed potential technical errors that would, if present, render their estimates strongly biased in favor of their reported findings. Stockwell et al. do not discuss whether or not they tested for non-stationarity in their data. It is reasonable to assume that the time series data that they describe using would be non-stationary. Further, the results shown in their Table 4 (p. 1832) imply test statistics that are near-impossibly large for a correctly specified model (maximum = 83.5, average = 22.9).

Weitzman et al. (2003) examine data on drinking habits among college students at eight public universities and compare the self-reported measures to retail outlet densities. Each of the 3,421 students who participated in the study self-reported to which of the classifications their drinking behaviors belonged: *heavy drinking*, *frequent drinking*, *drinking-related problems*, *frequent drunkenness*, *non-binge drinking*, *binge drinking*, *drinks-to-get-drunk*, and *abstention*. Weitzman et al. find positive correlations between retail outlet density and heavy drinking,

frequent drinking, and drinking-related problems. As with related studies, the authors stress that their results are correlational and that neither causality nor the direction of causality is implied. [Insert Table 1]

These studies as well as others (Presley et al., 2002; Douglas et al., 1997; Gruenewald et al., 1996) point to a possible relationship between retail outlet density and alcohol consumption. As the studies are not experimental, they leave two important issues unaddressed: (1) Is the positive correlation between outlet density and alcohol consumption causal? While repeated correlational studies might suggest causality, there remains the possibility that there is no causality present. For example, it is possible that college students tend to drink more as a consequence of age, new-found freedom, and propensity to take risks, and it is possible that the density of alcohol retail outlets is higher near universities simply because the density of people is higher near universities. (2) Assuming causality is present, what is the direction of the causality? While there is a natural tendency to blame markets for people's behaviors, in fact, markets are merely the aggregation of people's behaviors. In other words, markets do not cause behavior; behavior causes markets. If the correlation between outlet density and alcohol consumption were shown to be causal, it would be tempting to blame increased consumption on the increased availability of alcohol. However, an equally compelling (some may argue, more compelling) argument is that the density of retail outlets is caused by the propensity of the nearby populace to consume alcohol. Finally, if the relationship between density and consumption is causal, it is possible that the causality is bi-directional. It may be the case that both increased density causes increased consumption and that increased consumption contributes to increased density.

From a policy perspective, the unanswered causality question is paramount. If the relationship between retail outlet density and alcohol consumption is not causal, or if it is causal

but the causality runs from consumption to density or is bi-directional, restrictions on outlet density will have no effect on alcohol consumption. Worse, as is the case with all social policies, implementing the policy may lead people to falsely believe that the government is judiciously spending its resources in pursuit of a valuable social goal, and to erroneously equate spending and regulation directed toward the goal with the achievement of the goal.

In an early review of literature on state monopolization of alcohol markets as a policy tool for reducing alcohol consumption, Holder (1993) looked at the use of state monopolization of alcohol markets as a means of combating alcohol consumption and, by extension, alcohol-related problems. Implicit in Holder's review, and in many subsequent studies, is the assumption that alcohol consumption causes, rather than is caused by (or unrelated to), social ills. If, in fact, the causality is reversed or not present, we would expect that reducing alcohol consumption would have no effect on social ills. Holder concludes that research demonstrates that limitations on the availability of alcohol can reduce the consumption of alcohol and that this effect is most pronounced when alternative, unrestricted forms of alcohol do not exist. However, this result seems to be tautological in that it is not possible to consume what does not exist.

MacDonald (1986) looked at privatization of wine sales in Idaho and Maine (both in 1971) and found that wine sales increased significantly following privatization, but that beer and spirits sales did not. In 1969, grocery stores in Washington were allowed to sell imported wines. Following this privatization, MacDonald detected an increase in wine sales – despite two mitigating factors: (1) grocery stores already sold domestic wines, and (2) grocery stores charged prices 25% higher than those in state-owned stores. As confirmed by later studies, MacDonald found that the wine privatization had no effect on beer and spirits sales. Virginia's privatization of fortified wine sales in 1974 was not associated with an increase in wine, beer, or spirits sales.

MacDonald suggests that this result may be due to the fact that fortified wine comprised a very small portion of the overall market for wine.

Holder and Wagenaar (1990) found that, following Iowa's privatization of liquor stores, sales of spirits rose significantly (9.5%), sales of wine fell significantly (13.7%), and sales of beer did not change. Wagenaar and Holder (1995) look at the privatization of wine sales in Alabama (1973 and 1980), Idaho (1971), Maine (1971), Montana (1979), and New Hampshire (1978) over the period 1968 through 1991. Employing a Box-Jenkins modeling technique to measure the relationship between privatization and alcohol sales, they find that each of the states experienced significant increases in wine sales following privatization. However, they found no significant change in beer and spirits sales following privatization. These results are consistent with their 1991 study in which they find similar results for privatization in Iowa (1985) and West Virginia (1981).

Conversely, Mulford, Ledolter, and Fitzgerald (1992), who also examined data before and after Iowa's privatization, found that the privatization effect on wine sales was temporary (dropping to insignificance by two years after privatization), and found no evidence of an increase in spirits sales following privatization. Mulford et al.'s analysis differs from Wagenaar and Holder's in several important respects. Mulford et al. have 29 more months of data following privatization. With the additional data, Mulford et al. are more likely than Wagenaar and Holder to detect the temporary nature of the privatization effect, if indeed the effect were temporary. Mulford et al. also express concern that Wagenaar and Holder's model was misspecified in that Wagenaar and Holder's model implicitly assumes that any change in baseline alcohol sales following privatization would be permanent. Thus, not only are Wagenaar and Holder's data less able to detect temporary effects, but their model expressly assumes that privatization effects are

permanent. Also, Wagenaar and Holder incorrectly include sales of wine coolers in their data. Because the privatization had no statutory effect on wine cooler distribution, sales of wine coolers should not be included in their measures of wine sales. Not only should wine cooler sales not have been included in the data, but, by coincidence, there was a surge in popularity of wine coolers that occurred around the time of Iowa's privatization. This coincidence, combined with the erroneous inclusion of wine cooler sales, causes Wagenaar and Holder's results to be biased toward showing a positive privatization effect.

Finally, it is worth noting that Wagenaar and Holder do not mention testing for non-stationarity – an anomaly that frequently plagues time series data. The presence of non-stationarity in a time series data set results in spurious parameter estimates – results that are biased toward significance. In their 1991 paper, Wagenaar and Holder report that they made their data stationary by using first differences, and, in their 1995 paper, do not mention stationarity at all. In 1991, they do not report the results for stationarity tests nor discuss performing post-regression tests for stationarity in the residuals. Were it not for their reported regression results, this would be less of an issue. However, their regression results (both in the 1991 and 1995 papers) exhibit extremely high multiple correlation coefficients and extremely large test statistics – both of which can indicate the presence of unaddressed non-stationarity. As discussed later, unaddressed non-stationarity frequently results in erroneous findings of significant relationships. Rehm and Gmel (2001) raise concerns about this problem, particularly as it relates to published research investigating alcohol use.

Trolldal (2005a) looked at alcohol sales and vehicle fatalities in Alberta over the period of 1950 to 2000. From 1993 to 1994, Alberta privatized all of its retail liquor stores resulting in an almost tripling of the number of retail outlets selling wine or spirits. Controlling for changes

in after-tax income and alcohol prices, Trolldal finds that sales of spirits increased following privatization, that sales of beer and wine were unchanged, that the change in total sales of alcohol were insignificant, and that privatization had no significant effect on vehicle fatalities. Trolldal notes that these results contradict previous studies and suggests that part of the difference may lie in the fact that Alberta privatized retail, but not wholesale, markets. Because the state maintained monopolization of wholesale markets, it is possible that wholesale restrictions limited the extent to which retail markets could grow following privatization. Trolldal also notes that total sales did not increase despite an increase in the number of retail outlets. This, he suggests, can imply the existence of a saturation point beyond which further increases in the number of outlets has no effect on sales. This result suggests that previous findings that outlet density affects sales may only hold for lower density geographical areas. [Insert Table 2]

The National Alcohol Beverage Control Association (NABCA) classifies states that maintain monopoly control of alcohol sales, whether at the wholesale or retail level, as "control," and states that impose no regulations as "license". Like Trolldal, Rees (1997) looks at traffic fatalities in Iowa, West Virginia, and Pennsylvania between 1985 and 1995, and finds no significant difference in alcohol-related traffic fatalities between control and license states.

In a study of alcohol-control policies and the driving under the influence (DUI) fatality rate, Kenkel (1993) hypothesized that the DUI fatality rate would be lower in control states than in license states. He fits data from the 1985 Health Interview Survey to a consumer behavior model but finds no evidence of a relationship between DUI fatality rates and whether a state is control or license. Kenkel's (1993) model is based on Becker's (1969) which assumes that an individual will only commit a DUI offense when the expected utility from the offense outweighs

his utility from any other activity. Becker finds that the number of criminal offenses committed by any individual is a function of the probability of conviction, the punishment if convicted, and other personal factors such as a willingness to commit illegal acts. As does Becker, Kenkel finds that the discounted expected cost of drunk driving is predominantly a function of the probability of arrest and conviction rather than a function of alcohol availability.

[Insert Table 3]

This finding is consistent with Chaloupka and Saffer (1989) who compare DUI fatality rates before and after laws that allowed police to administer blood alcohol content (BAC) tests. Chaloupka and Saffer find a statistical decrease in the number of DUI fatalities in states with preliminary BAC test laws. Results from Chaloupka, Saffer, and Grossman (1993) suggest that, if every state had a preliminary breath test law, average annual DUI fatalities would decrease by approximately 3.4 percent, and that 18 to 20 year-olds would account for 20 percent of this reduction.

In October 2000, the U.S. Congress passed the Department of Transportation's 2001 Appropriations Act which mandated that states consider driving to be illegal if the driver's BAC is at least 0.08 g/dl (NHTSA, 2001b). By 2005, every state had enacted a 0.08 g/dl BAC law. Zador et al. (2000), Dee (2001), and Lund et al. (2007) found that setting a BAC of 0.08 g/dl resulted in a reduction in DUI fatality rates. Lund et al.'s (2007) results suggest that if all drivers had a BAC less than 0.08 g/dl in 2005, nearly 9,000 deaths could have been avoided. Similarly, Dee (2001) finds that 0.08 g/dl BAC laws have reduced traffic fatality rates by approximately 16.5 percent, and have reduced weekend and weekday fatality rates by 8.6 percent and 5.8 percent, respectively. Confounding this apparent effect, however, is the increasing prevalence of airbags and primary seatbelt laws (McCartt et al., 2008).

Cook and Tauchen (1984) find that states that reduced the minimum drinking age from 21 to 18 experienced an 11.1 percent increase in the traffic fatality rate among 18 to 21 year-olds. Dee and Evans (2001) hypothesized that this change in the minimum drinking age would only shift driving fatalities from teens to adult drivers. Instead, they find that the movement significantly decreased alcohol-related traffic fatalities for teen drivers. This finding is similar to Dee (1990) who finds that changing the minimum drinking age from 18 to 21 substantially reduced abusive teen drinking by 8 percent and teen driving fatalities by 9 percent. In a meta study, Wagenaar and Toomey (2002) reviewed 241 empirical analyses on the minimum legal drinking age and conclude that the body of research suggests an inverse relationship between the mandatory legal drinking age and both per-capita alcohol consumption and the rate of traffic accidents.

Kahane (2000) finds that seatbelt laws are 45 percent and 60 percent effective in reducing traffic fatalities in passenger cars and light trucks, respectively. Chaloupka et al. (1993) and Eisenberg (2003) find similar results. Sobel and Nesbit (2007) and Risa (1994) look at the relationship between seat belt laws and traffic fatality rates and find that the safety gain from wearing a seat belt is offset by the driver's willingness to drive faster and more recklessly.

Other factors that have been identified as being related to traffic fatalities include the price of unleaded gasoline (Sivak 2009; Grabowski and Morrisey 2004), speed limits and vehicle miles traveled (Park et al. 2008), and rainfall, temperature, and terrain (Yakovlev and Inden 2009).

What has not been explored is the relationship between alcohol-related fatalities and the *degree* of control states exercise over wholesale and retail alcohol markets. We perform this analysis in a panel data setting. Whereas studies using NABCA's binary classifications of control

versus license found no relationship between control and alcohol-related fatalities, we find a significant relationship that varies based on the degree of market control.

Trolldal (2005b) looked at the effect of two rounds of privatization of retail alcohol markets in Quebec. Starting in 1978, grocery stores were permitted to sell wines that were either produced in Canada or that were imported and bottled by the Quebec Liquor Board. Starting in 1983, grocery stores were allowed to sell wines that were imported and bottled by private Quebec manufacturers. Using annual data from Quebec over the period 1950 through 2000, he finds that wine sales increased by 10% following the 1978 privatization, but that the change was small enough such that there was no discernable effect on total alcohol sales. He also finds that the 1983 privatization had no effect on sales of beer, wine, or spirits.

In addition to looking at outlet density, Stockwell et al. (2009) look at the relationship between privatization and alcohol sales after controlling for the possible effect of outlet density on sales. They find that increases in the proportion of privately owned retail stores were associated with increases in total alcohol sales.

Miller et al. (2006) examine the relationship between alcohol retail monopolies and the incidences of underage drinking, underage binge drinking, and DUI involved fatalities. Like Pulito and Davies (2009), Miller et al. perform difference of means tests for privatized versus non-privatized states. Among other things, this has the effect of side-stepping the possible stationarity issues that plague time-series studies. Miller et al. find that the average incidences of underage drinking, underage binge drinking, and DUI fatalities were higher for states with privatized alcohol markets. In contrast to Pulito and Davies, Miller et al. look only at a single year for each state (data for most of the states are from 2001, though some are from as far back as 1997 or from 2002). Miller et al. perform weighted difference of means tests where the

observations are weighted for each state's population size. This is odd given that they are analyzing *incidences* of drinking. Weighting incidences of underage drinking by a state's population size counteracts the division by population size necessary for calculating the incidence in the first place. This results in an analysis that, in effect, compares the *number* of underage drinkers across states rather than the *incidence* of underage drinking across states, thereby biasing results in the direction of the largest states. Employing the correct procedure of comparing the unweighted incidences of underage drinking yields results that are directionally similar to those found by Miller et al., but which are (marginally) statistically insignificant (p = 0.076 for the difference in underage drinking, $p = 0.053$ for the difference in underage binge drinking). The insignificance ceases to be marginal if we remove Utah from the data set – a reasonable adjustment given the number of people with strong religious views against alcohol consumption. Among non-privatized states, Utah's incidence of underage drinking is a remarkable 7 standard deviations below the mean while Utah's incidence of underage binge drinking is 5.7 standard deviations below the mean! Removing Utah yields results of $p = 0.124$ for the difference in underage drinking and $p = 0.096$ for the difference in underage binge drinking, indicating that states with privatized alcohol markets exhibit incidences of underage drinking and underage binge drinking that are not significantly different from those of states with state-controlled alcohol markets.

II. Data

We use a panel of the fifty U.S. States over the period 1982 through 2002. We divide alcohol control variables into two categories: *alcohol sales and use controls* and *alcohol market controls*. The former category includes laws that regulate the use of alcohol (e.g., DUI laws), the

sales or purchase of alcohol (e.g., keg registration laws, dram shop laws), or are tangentially related to DUI fatality rates (e.g., seat belt laws). The latter category is the focus of our study and reflects whether wholesale and retail markets are state run or privatized. Variable definitions and summary statistics are shown in Table 4.

[Insert Table 4]

The Fatality Analysis Reporting System (FARS) defines an *alcohol-impaired* traffic fatality as one in which a person, who is either a driver or a vehicle occupant, is killed within 30 days of a motor vehicle accident in which at least one driver had a BAC of at least 0.08 g/dl. An *alcohol-involved* traffic fatality is one in which a person, who is either a driver, a vehicle occupant, or a non-motorist, is killed within 30 days of a motor vehicle accident in which at least one person (driver, passenger, or non-motorist) had a BAC of at least 0.01 g/dl (ANSI, 1996; NHTSA, 2007). The definition of an alcohol-impaired fatality is narrower than the definition of an alcohol-involved fatality in that the former requires a minimum BAC of 0.08 g/dl (versus 0.01 g/dl) and requires that the person with the BAC be a driver.

Prior to the National Minimum Legal Drinking Age Act of 1984, drinking ages varied across states and by beverage type. Due to high multicollinearity among the minimum drinking ages for the various forms of alcoholic beverages (beer, wine, spirits), we create a variable that reflects the youngest age one can purchase and consume alcohol of any form. Due to high multicollinearity between primary and secondary seat belt laws (Yakovlev and Inden 2009), the mandatory seat belt variable reflects the first year in which any mandatory seat belt law (either primary *or* secondary) was adopted. The BAC limit variable reflects the year in which a state enacted a law making it illegal for an automobile operator to drive with a BAC greater than or equal to 0.08 g/dl. This zero tolerance law represents the year in which a state enacted a BAC

limit of 0.02 g/dl or less for drivers under the age of 21. The tolerance of so called "zero tolerance" laws vary by state. For example, for underage drivers, Texas mandates a BAC limit of 0.00 g/dl, while Pennsylvania mandates a BAC limit of 0.02 g/dl (NHTSA, 1997).

Keg registration laws require retailers to record the serial numbers of kegs along with personal identifications of people who purchase the kegs. Preliminary breath test laws allow police officers to administer a BAC test if the driver is suspected of being intoxicated. Open container laws prohibit alcohol from being readily accessible by either the driver or passenger of a moving vehicle. Dram shop laws establish liability for any establishment that serves alcohol to an obviously intoxicated person who is subsequently involved in an alcohol-related traffic fatality.

We base our classifications according to degree of alcohol market control on Pulito and Davies (2009). These classifications are shown in Table 5.

[Insert Table 5]

In Maryland, only Montgomery County regulates alcohol sales. Due to the inability to differentiate between the sale of alcohol in Montgomery County and sales in the rest of the state, we drop Maryland from the data set. Table 3 shows the states according to their control classifications. With the exception of Maryland, states not appearing in Table 6 are classified as *No Control* states.

[Insert Table 6]

III. Results and Discussion

For each of four dependent variables, we estimate the following model in a fixed-effect panel framework correcting for heteroskedasticity and first-order autocorrelation:[1]

$$Y_{it} = \alpha + \sum_{j=1}^{3} \gamma_j C_{it}^j + \sum_{j=1}^{8} \beta_j X_{it}^j + h_t + \varepsilon_{it} \quad (5)$$

where the h_i are year-specific fixed effects and ε_{it} is the error term.[2] We estimate this model for each of four outcomes, Y_{it}: the alcohol-involved traffic fatality rate, the alcohol-impaired traffic fatality rate, the underage alcohol-involved traffic fatality rate, and the underage alcohol-impaired traffic fatality rate. The variables X_{it}^j represent the following alcohol sales and use control policies: the minimum legal drinking age, mandatory seat belt law, 0.08 g/dl BAC limit, zero tolerance law, keg registration law, preliminary breath test, open container law, and the dram shop law. The variables C_{it}^j are dummies representing the alcohol market control classification for state *i* in year *t*. Results are shown in Table 7.

[Insert Table 7]

In all but two cases (keg registration laws affecting underage involved and underage impaired fatalities), alcohol sales and use controls show an inverse relationship DUI fatalities: more stringent policies are associated with reduced fatality rates. However, the relationship is statistically significant only for zero tolerance laws (affecting underage involved and underage impaired fatalities) and preliminary breath test and open container laws (affecting adult involed and adult impaired fatalities). Whereas some studies using NABCA classifications of "control"

[1] The Hausman and Breusch-Pagan random effects test supports the choice for a fixed effects model. The residuals from each of the regressions are stationary.

[2] Due to multicollinearity between some of the state-specific dummies and the alcohol control dummies, it is not possible to allow for state-specific fixed effects.

versus “license” states have found no difference in DUI fatality rates while others have found higher fatalities absent control, our results show (1) that the relationship varies depending on the degree of control, and (2) some degrees of control are actually associated with increased fatalities (versus no control). In only one case (moderate control as affecting adult impaired fatality rates) do we find that any degree of control is associated with significantly reduced fatality rates. In all cases (adult involved, adult impaired, underage involved, and underage impaired), we find significantly higher fatality rates under light control than under no control. In one case (underage involved), we find fatality rates are (weakly) significantly higher under heavy control versus no control. In all other cases, we find no difference in fatality rates for any of the cases under any degree of control versus no control.

IV. Conclusion

This study utilizes a panel of 49 states from 1982 to 2002 in an attempt to measure the relationship between privatization of alcohol sales and alcohol-related traffic fatalities. Controlling for other alcohol-control policies, the bulk of our results confirm previous research showing no difference in fatality rates for control versus no control. Interestingly, by classifying control by degree, we find light control to be associated with significantly elevated DUI fatality rates both among adults and the underage.

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Table 1. Sample of Studies Examining the Relationship Between Outlet Density and Consumption, Binge Drinking, and DUI.

McCarthy (2003) (density = outlets per square mile)			
	Alcohol-Related Traffic Accidents		
	Non-Fatal	**Fatal**	**Total**
Increase in density of off-site general licenses	Decrease	Decrease	Decrease
Increase in density of on-site general licenses	Increase	No change	No change
Increase in density of off-site beer/wine licenses	No change	No change	Decrease
Increase in density of on-site beer/wine licenses	Increase	No change	No change

Weitzman et al. (2003) results (density = outlets per square mile)			
	Correlation with Self-Reported Behaviors		
	Heavy Drinking	**Frequent Drinking**	**Drinking-Related Problems**
Outlet density	Positive	Positive	Positive

Stockwell et al. (2009) (density = outlets per population 15 and older)	
	Per-Capita Alcohol Consumption
Increase in density of beer outlets	Increase
Increase in density of wine outlets	Increase
Increase in density of spirits outlets	Increase
Increase in density of on-site beer/wine licenses	Increase

Table 2. Sample of Studies Examining the Relationship Between Privatization and Alcohol Sales

MacDonald (1986)				
		Beer Sales	**Wine Sales**	**Spirits Sales**
Privatization of retail wine stores (Idaho, Maine)		No change	Increase	No change
Privatization of retail wine stores (Washington)		No change	Increase	No change
Privatization of retail fortified wine stores (Virginia)		No change	No change	No change
Holder and Wagenaar (1990)				
		Beer Sales	**Wine Sales**	**Spirits Sales**
Privatization of retail spirits stores (Iowa)		No change	Decrease	Increase
Wagenaar and Holder (1991)				
	Beer Sales	**Wine Sales**	**Spirits Sales**	**Total Sales**
Privatization of retail wine stores (Iowa, West Virginia)	No change	Increase	No change	Increase
Mulford, Ledolter, Fitzgerald (1992)				
		Beer Sales	**Wine Sales**	**Spirits Sales**
Privatization of retail wine stores (Iowa)		No change	Temporary increase; No change after two years	No change
Wagenaar and Holder (1995)				
		Beer Sales	**Wine Sales**	**Spirits Sales**
Privatization of retail wine stores (Alabama, Idaho, Maine, Montana, New Hampshire)		No change	Increase	No change
Trolldal (2005a)				
	Beer Sales	**Wine Sales**	**Spirits Sales**	**Total Sales**
Privatization of retail spirits stores (Alberta)	No change	No change	Increase	No change
Trolldal (2005b)				
	Beer Sales	**Wine Sales**	**Spirits Sales**	**Total Sales**
Privatization of retail wine stores (Quebec, 1978)	No change	Increase	No change	No change
Privatization of retail wine stores (Quebec, 1983)	No change	No change	No change	No change

Table 3. Sample of Studies Examining the Relationship Between Privatization and DUI

Kenkel (1993)	
	Alcohol-Related Vehicle Fatalities
Privatization of retail wine and spirits (1985 Health Interview Survey)	No change
Rees (1997)	
	Alcohol-Related Vehicle Fatalities
Privatization of retail spirits (West Virginia and Iowa)	No change
Trolldal (2005a)	
	Alcohol-Related Vehicle Fatalities
Privatization of retail spirits stores (Alberta)	No change
Miller et al. (2006)	
	Alcohol-Related Vehicle Fatalities
Privatization of retail wine or spirits stores (50 U.S. states)	Increase

Table 4. Variable Descriptions and Summary Statistics

Variable Description	*Mean (Stdev)*
Outcome Variables	
Alcohol-involved traffic fatalities per 1,000 people among 21+ year-olds	0.09 (0.04)
Alcohol-involved traffic fatalities per 1,000 people among 15 to 20 year-olds	0.04 (0.02)
Alcohol-impaired traffic fatalities per 1,000 people among 21+ year-olds	0.08 (0.03)
Alcohol-impaired traffic fatalities per 1,000 people among 15 to 20 year-olds	0.03 (0.02)
Alcohol Market Controls	
Dummy variable for the classification of a state as Heavy Control (1=State is classified as Heavy Control, 0=Otherwise)	0.11 (0.31)
Dummy variable for the classification of a state as Moderate Control (1=State is classified as Moderate Control, 0=Otherwise)	0.18 (0.39)
Dummy variable for the classification of a state as Light Control (1=State is classified as Light Control, 0=Otherwise)	0.07 (0.26)
Alcohol Sales and Use Controls	
Lowest minimum legal drinking age among all alcoholic beverage types	20.69 (0.79)
Dummy variable for Enforcement/Adoption of a mandatory seat belt law (1=Enforcement/Adoption, 0=Otherwise)	0.66 (0.48)
Dummy variable for Enforcement/Adoption of a 0.08 g/dl BAC limit for drivers (1=Enforcement/Adoption, 0=Otherwise)	0.19 (0.39)
Dummy variable for Enforcement/Adoption of a 0.02 g/dl BAC limit for drivers under the age of 21 (1=Enforcement/Adoption, 0=Otherwise)	0.37 (0.48)
Dummy variable for Enforcement/Adoption of a keg registration law (1=Enforcement/Adoption, 0=Otherwise)	0.17 (0.38)
Dummy variable for Enforcement/Adoption of a preliminary breath test law (1=Enforcement/Adoption, 0=Otherwise)	0.51 (0.50)
Dummy variable for Enforcement/Adoption of a open container law (1=Enforcement/Adoption, 0=Otherwise)	0.49 (0.50)
Dummy variable for Enforcement/Adoption of a dram shop law (1=Enforcement/Adoption, 0=Otherwise)	0.83 (0.38)

Outcome data come from the Fatality Analysis Reporting System (2009). Data on alcohol sales and use controls come from Ponicki (2009). Alcohol market control classifications come from Pulito and Davies (2009). Data are annual for 49 states over the period 1982 to 2002.

Table 5. Classifications According to the Degree of State Monopolization of Alcohol Markets

Heavy Control	State maintains monopoly control over sales of at least two types of alcohol (beer, wine, and liquor) at the retail level and at least one type of alcohol at the wholesale levels.
Moderate Control	State maintains monopoly control over sales of one type of alcohol (beer, wine, or liquor) at the retail level and at least one type of alcohol at the wholesale level.*
Light Control	State maintains monopoly control over no sales at the retail level and at least one type of alcohol at the wholesale level.
No Control	State does not maintain monopoly control over sales of any alcohol at either the wholesale or retail level. This is NABCA's definition of "license".

* At the retail level, Idaho regulates all beverages that exceed 16 percent alcohol, and Ohio regulates all beverages that exceed 21 percent alcohol. Wine and Spirits have an average alcohol content of 12 percent and 40 percent, respectively (U.S. Department of Health and Human Services, 2005). Based on these content levels, we classify Ohio and Idaho as moderate control states.

Table 6. States According to Alcohol Market Control Classification

Heavy Control	Moderate Control	Light Control
Maine	Alabama	Iowa*
Pennsylvania	Idaho	Michigan
Montana	Iowa*	Mississippi
Utah	New Hampshire	West Virginia*
	North Carolina	Wyoming
	Ohio	
	Oregon	
	Vermont	
	Virginia	
	Washington	
	West Virginia*	

* Iowa was Moderate Control prior to 1987 and Light Control after. West Virginia was Moderate Control prior to 1990 and Light Control after.

Table 7. Alcohol Related Traffic Fatalities as a Function of Alcohol Policies

	Legal Age Traffic Fatality Rates		Underage Traffic Fatality Rates	
Factor	**Alcohol Involved**	**Alcohol Impaired**	**Alcohol Involved**	**Alcohol Impaired**
Alcohol Market Controls				
Heavy Control	0.007	0.007	0.005 *	0.004
	(0.006)	(0.005)	(0.003)	(0.002)
Moderate Control	-0.006	-0.008 **	0.001	-0.001
	(0.005)	(0.003)	(0.002)	(0.002)
Light Control	0.037 ***	0.034 ***	0.015 ***	0.013 ***
	(0.005)	(0.005)	(0.002)	(0.002)
Alcohol Sales and Use Controls				
Minimum Drinking Age	-0.001	-0.001	-0.0003	-0.0010
	(0.001)	(0.001)	(0.0007)	(0.0006)
Mandatory Seat Belt	-0.002	-0.001	-0.0008	-0.0010
	(0.002)	(0.001)	(0.0015)	(0.0012)
BAC Limit	-0.003	-0.003	-0.0025	-0.0019
	(0.002)	(0.002)	(0.0016)	(0.0013)
Zero Tolerance	-0.003	-0.001	-0.0033 **	-0.0026 **
	(0.002)	(0.002)	(0.0016)	(0.0014)
Keg Registration	-0.002	-0.001	0.0041 **	0.0044 ***
	(0.003)	(0.002)	(0.0020)	(0.0017)
Preliminary Breath Test	-0.010 ***	-0.010 ***	-0.0016	-0.0011
	(0.002)	(0.002)	(0.0013)	(0.0011)
Open Container	-0.008 ***	-0.006 ***	-0.0023	-0.0007
	(0.002)	(0.002)	(0.0015)	(0.0012)
Dram Shop	-0.003	-0.003	-0.0036 **	-0.0017
	(0.002)	(0.002)	(0.0015)	(0.0015)
R^2	0.817	0.805	0.735	0.694

Panel data fixed effects OLS, 1029 observations (49 states, annual data 1982 to 2002). Standard errors corrected for cross-section specific AR(1) and heteroskedasticity. Dependent variables: Alcohol-involved/impaired traffic fatalities and underage alcohol-involved/impaired traffic fatalities per 1,000 people. Significant levels: *** at 1%, ** at 5%, and * at 10%. Standard errors are reported in parentheses. Alcohol market control coefficients are deviations from the "no control" case.