REVIEW OF STUDIES ON LIQUOR CONTROL AND CONSUMPTION

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Policy Variables 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 summarize research on the effects of alcohol policy variables on alcohol policy targets, with a primary focus on privatizations. The goal is to determine whether there are trends in the research findings that can inform future policy makers.

Outlet Density

Controlling alcohol markets 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.

Table 1. McCarthy (2003) results (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

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).

Table 2. Stockwell et al. (2009) results (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

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.

Table 3. 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

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.

Privatization

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.

Table 4. MacDonald (1986) results.

	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) 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).

Table 5. Holder and Wagenaar (1990) results.

	Beer Sales	Wine Sales	Spirits Sales
Privatization of retail spirits stores (Iowa)	No change	Decrease	Increase

Table 6. Wagenaar and Holder (1991) results.

	Beer Sales	Wine Sales	Spirits Sales	Total Sales
Privatization of retail wine stores (Iowa, West Virginia)	No change	Increase	No change	Increase

Table 7. Wagenaar and Holder (1995) results.

	Beer Sales	Wine Sales	Spirits Sales
Privatization of retail wine stores (Alabama, Idaho, Maine, Montana, New Hampshire)	No change	Increase	No change

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.

Table 8. Mulford, Ledolter, Fitzgerald (1992) results.

	Beer Sales	Wine Sales	Spirits Sales
Privatization of retail wine stores (Iowa)	No change	Temporary increase; No change after two years	No change

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.

Table 9. Trolldal (2005a) results.

	Beer Sales	Wine Sales	Spirits Sales	Total Sales	Alcohol-Related Vehicle Fatalities
Privatization of retail spirits stores (Alberta)	No change	No change	Increase	No change	No change

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.

Table 10. Trolldal (2005b) results.

	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

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.

Table 11. Stockwell et al. (2009) results.

	Per-Capita Alcohol Consumption
Increase in proportion of privately owned retail stores	Increase

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.

Table 12. Miller et al. (2006) results.

	Underage Drinking	**Underage Binge Drinking**	**DUI Fatalities**
Privatization of retail wine or spirits stores (50 U.S. states)	Increase	Increase	Increase

Like Miller et al., Pulito and Davies (2009) perform difference of means tests for differences in the incidences of underage drinking, underage binge drinking, per-capita alcohol consumption, and DUI fatalities among privatized versus non-privatized states. Where Miller et

al. look at the states in a single year, Pulito and Davies look at the states over a period of 16 years (for per-capita consumption and DUI fatalities) and classify states according to the *degree* of privatization. They categorize the possible degrees of privatization versus control as full control (sales of beer, wine, and spirits are controlled at the retail and wholesale levels), moderate control (sales beer, wine, and spirits are controlled at the wholesale level, and sales of only one type of alcohol are controlled at the retail level), light control (sales of beer, wine, and spirits are controlled at the wholesale level, and sales of all three types are privatized at the retail level), and no control (sales of beer, wine, and spirits are privatized at the wholesale and retail levels). They compare the degree of control to per-capita alcohol consumption, the incidence of underage drinking, the incidence of underage binge drinking, and alcohol-related traffic fatalities. They find that alcohol consumption is significantly greater under no control than under light control, but statistically identical among full, moderate, and light control. They find no significant difference in the incidence of underage drinking or the incidence of underage binge drinking among the four control classifications. While they find no difference in the rate of DUI arrests among the four control classifications, they do find that the number of alcohol-related traffic fatalities per DUI arrest are the same under full control and no control, and significantly lower under moderate and light control.

Table 13. Pulito and Davies (2009) results (changes are as compared with no privatization).

	Underage Drinking	Underage Binge Drinking	Total Sales	DUI Fatalities
Partial privatization of retail stores (48 U.S. states)	No change	No change	No change	Decrease
Full privatization of retail stores (48 U.S. states)	No change	No change	Decrease	Decrease
Full privatization of retail and wholesale stores (48 U.S. states)	No change	No change	Increase	Decrease

Conclusion

These and other studies suggest that there is no clear evidence that privatization of alcohol markets leads to decreased social measures – whether consumption, underage drinking, or DUI fatalities. Studies that show relationships are counterbalanced by other studies, of the same data, that show no relationship. Some studies that show relationships may suffer from unaddressed statistical anomalies that bias results in favor of finding relationships where none exist. Studies that show relationships also suffer from unaddressed causality, making the results useless for guiding policy makers. Future studies can correct some of these shortcomings by employing more rigorous statistical techniques, though the issue of causality may never be adequately addressed. Nonetheless, even if causality were left unaddressed, a preponderance of statistically defensible results in one direction or the other would go a long way to informing policy.

Currently Pennsylvania, one of the few remaining states that have privatized neither wine nor spirits stores, is considering privatizing these markets. A bill introduced by Rep. Mike Turzai (House Bill 2350) would allow Pennsylvania to auction off 750 retail and 100 wholesale licenses. Segal and Underwood (2007) estimate that Pennsylvania could raise almost $2 billion from auctioning off its wholesale and retail liquor stores plus an additional $350 million annually from alcohol sales taxes. In the last fiscal year, Pennsylvania incurred its largest ever budget deficit – the bulk of which was closed by Federal monies. The money raised from privatizing the state liquor stores would, by itself, have closed the budget deficit.

Similarly, Robert McDonnell, governor of Virginia, is pushing for that state to privatize its liquor stores. The governor's staff is considering four approaches to privatization: selling the state's outlets to a single private firm, offering spirits licenses to the 3,000 private firms that currently sell beer and wine, selling existing state liquor stores to private firms, and auctioning

off a number of licenses. To proceed cautiously, and reflecting on studies sighted in this paper, Virginia might be best served by auctioning off licenses and, perhaps, coupling the licenses with options to buy existing stores to which the licenses would apply. This would allow the state to control outlet density, pending further and more rigorous studies on the effect of outlet density on alcohol sales, plus unload physical plant that it no longer needs.

Appendix: Non-Stationarity and Time Series Estimates

Time series data frequently exhibits what is known as *non-stationarity*. Measurements on things such as population, income, production, and trade will naturally grow (on average) over time. The result is that the time series will exhibit trends over time. Statistically, the trends cause the time series to have undefined population means and infinite population variances. For example, because per-capita income rises (on average) each year, the mean and variance of per-capita income (from some fixed past point in the past to the present) increase with each passing year. When analysts attempt to measure the relationship between two non-stationary series, the natural trend dominates the relationship and drowns out any underlying relationship between the two series. For example, because both the world population increases over time and the S&P 500 has been increasing over time, one can expect to find a strong correlation between world population and the S&P 500. The strong correlation results from the fact that the two trends dominate the analysis. To remove non-stationarity, the analyst can look at the first difference or the growth rates in the two series. If there is no strong correlation between the historical growth rate in the S&P 500 and the historical population growth rate, one can conclude that one likely does not cause the other.

There are formal statistical tests for non-stationarity, but typically the problem manifests itself in regression correlations close to 1.0, and test statistics that are remarkably high. For example, the probability of a large asteroid hitting the earth on any given day is about one in 40 billion. That corresponds to a test statistic of about 6.5. An analysis that reports test statistics above 6.5 is claiming probabilities that are possibly unbelievably small. At that size, it becomes more likely that the data suffers from non-stationarity than that the analyst has truly found an immensely improbable relationship.

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