Alcohol Control Policies and Youth Alcohol Consumption: Evidence from 28 Years of Monitoring the Future

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Alcohol Control Policies and Youth Alcohol Consumption: Evidence from 28 Years of Monitoring the Future*

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Abstract

We provide the first historical comparative analysis of the effects of Minimum Legal Drinking Ages (MLDA), beer taxes, and “Zero Tolerance” (ZT) underage drunk driving laws on the drinking behaviors of high school seniors using confidential area-identified data from the 1976-2003 waves of the Monitoring the Future (MTF) Surveys. We estimate reduced form models of drinking participation and heavy episodic drinking that account for state and year fixed effects. Our findings confirm that nationwide increases in the MLDA in the late 1970s and 1980s and adoption of ZT laws in the 1990s both significantly reduced alcohol consumption by high school seniors, with larger effects for the MLDA than for ZT laws. Higher beer taxes are also estimated to reduce youth drinking participation. Overall, the results confirm that a variety of types of government intervention can have important effects on youth alcohol consumption.

KEYWORDS: alcohol, youth, zero tolerance, minimum drinking age, beer tax

*We are grateful to the editor and two anonymous referees for very useful comments.
1. Introduction
Rates of youth drinking participation and heavy episodic alcohol consumption have declined substantially over the past three decades (see Johnston, O’Malley, Bachman, & Schulenberg, 2005 and Figure 1).

In addition to nationwide trends in sentiment toward youth drinking, there have been at least three major types of policy interventions that researchers have asserted have contributed to the decline: nationwide increases in the Minimum Legal Drinking Age (MLDA) in the 1970s and 1980s, adoption of tough drunk driving laws in the 1980s and 1990s, and a handful of direct price increases – including a doubling of the federal excise tax on beer in 1991. Indeed, independent literatures have documented the effects of each of these policy regimes in alcohol consumption by youths. The general consensus is that the nationwide movement to an MLDA of 21 in the 1980s was associated with significant reductions in drinking participation and drinking intensity among youths (see O’Malley and Wagenaar 1991, Dee 1999, and others). Similarly, recent research has documented that adoption of tougher age-targeted “Zero Tolerance” drunk driving laws in the 1990s also reduced heavy drinking by young adults (Carpenter 2004). The evidence on beer taxes is mixed, with several studies showing a negative relationship between the level of a state’s excise tax on beer and its youth drinking rate (Coate and Grossman 1988 and others) and others showing little or no relationship between changes in beer taxes and changes in youth drinking or traffic fatalities in the 1970s and 1980s (Dee 1999 and others).
In this paper we provide a unifying contribution that documents the relative contributions of MLDA increases, beer taxes, and adoption of Zero Tolerance (ZT) laws toward reducing alcohol consumption by youths.\footnote{It is worth noting that a recent study by Grossman (2005) also takes a long-range view of the effects of prices on youth substance use. A key difference is that he performs his analysis as a national time series rather than focusing on individual state policies. His paper also does not address age-targeted drunk driving laws adopted in the 1990s.} For policymakers choosing among alternative instruments, this information is crucial. Previous research focuses on one or at most two of these sets of policies in isolation. This feature of the literature is largely driven by the fact that the relevant variation in each of the policies occurs in different decades: the majority of the changes in state Minimum Legal Drinking Ages took place in the late 1970s and early 1980s, while adoption of Zero Tolerance laws occurred mainly in the 1990s. There are simply very few sources of data on youth drinking spanning the historical periods when the relevant variations in each of the three sets of alcohol control policies occurred to provide credible information on their comparative effectiveness.\footnote{We recognize that these were not the only types of alcohol regulation during this time period, although they were arguably the most direct (compared to, say, server liability laws or dram shop laws).}

A notable exception, however, is the University of Michigan’s Monitoring the Future (MTF) study, which has surveyed high school seniors in a variety of states in every year since 1975 (Johnston et al., 2005). In this paper we provide the first comprehensive analysis of the relative effects of these three major sets of alcohol control policies on youth drinking behaviors over the past quarter century.\footnote{To preview, we find strong evidence that exposure to a Minimum Legal Drinking Age of 18 was associated with large and statistically significant increases in drinking participation and heavy episodic drinking by high school seniors, on the order of 2-3 percentage points. Put differently, we estimate that nationwide increases in the MLDA (i.e. movements away from the most permissive age of 18) reduced youth drinking by about four percent relative to pre-existing levels. We find comparable percentage reductions associated with adoption of Zero Tolerance drunk driving laws: ZT laws reduced drinking and...} In doing so, we confirm previous results on MLDA's, present the first detailed analysis of the effects of Zero Tolerance drunk driving laws on high school seniors, and extend previous research on the effects of beer taxes on youth alcohol consumption. All of our models include controls for individual demographics and state and year fixed effects; as such, the policy effects are identified from within-state changes in drinking behaviors coincident with changes in prices and policies. The resulting “difference-in-differences” (DD) type estimates are purged from time-invariant differences across states.

To preview, we find strong evidence that exposure to a Minimum Legal Drinking Age of 18 was associated with large and statistically significant increases in drinking participation and heavy episodic drinking by high school seniors, on the order of 2-3 percentage points. Put differently, we estimate that nationwide increases in the MLDA (i.e. movements away from the most permissive age of 18) reduced youth drinking by about four percent relative to pre-existing levels. We find comparable percentage reductions associated with adoption of Zero Tolerance drunk driving laws: ZT laws reduced drinking and...
heavy episodic drinking by about 1.5-2 percentage points. Finally, unlike previous research that has considered shorter intervals of the MTF, we find a negative and statistically significant relationship between state beer taxes and youth drinking participation over the 1976-2003 period. Overall, our results confirm that a wide variety of policy interventions can have meaningful effects on youth alcohol consumption.

Although independent literatures have investigated each of these alcohol control policies in isolation, there are several reasons that our more comprehensive approach provides a useful contribution. First, previous research that has failed to reach consensus on the effects of state beer taxes on youth alcohol consumption has explicitly acknowledged that that lack of within state variation in beer taxes limits the ability of state fixed effects models to either confirm or rule out an important role for tax policy in affecting youth alcohol consumption. Our research extends the longest previous MTF series used in the literature by over ten years, thus allowing us to incorporate more tax changes into our analyses. Indeed, our preferred models produce tax estimates that are largely similar to those estimated previously but that have sufficient precision to reject that they are equal to zero at standard confidence levels. Second, estimating all of the policy effects in a unified model using the same data allows a direct comparison of efficacy. Previous estimates for the policy variables come from different data sources with different sampling structures, age groups, and variable definitions. Our “apples to apples” comparisons (i.e. we are comparing the identical outcomes over time within a data source that has had a consistent methodology since 1976) provide more confidence in the estimated relative effectiveness of the policies on youth drinking behaviors.

The paper proceeds as follows. Section 2 briefly outlines the relevant literature on the effects of Minimum Legal Drinking Ages, beer taxes, and Zero Tolerance laws on youth drinking. Section 3 describes the Monitoring the Future data and our empirical approaches. Section 4 presents the main results, and Section 5 concludes.

2. Previous Literature
The literatures on the effects of Minimum Legal Drinking Ages and beer taxes on youth alcohol consumption are well established, and as such we provide only a brief review here. With respect to the MLDA, 28 states experimented with lowering the legal drinking age in the early 1970s, only to experience increased

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3 For example, a regression of our beer tax measure on state and year dummies in the longest MTF series used by previous research (through 1992) returns an R squared of .94. Adding observations through 2003 and performing this same exercise returns an R squared of .90.

4 For a more detailed literature review of the MLDA, see Wagenaar and Toomey (2001). For a review of the effects of price on youth consumption, see Grossman et al. (1994).
youth traffic fatalities. In response to a variety of forces, but especially federal threat of loss of highway funds, these and other states began increasing their drinking age in the 1980s, and by 1989 every state had a Minimum Legal Drinking Age of 21.

There is strong evidence that youths exposed to lower Minimum Legal Drinking Ages were more likely to drink and drink heavily than otherwise similar youths exposed to more restrictive (higher) MLDAs. This result has been shown in cross-sectional comparisons of youths in states with different MLDAs, as well as in multi-state policy evaluations that consider changes in the drinking age over time coincident with changes in youth drinking behaviors (see, for example, O’Malley and Wagenaar 1991, Cook and Tauchen 1984, and others). Several of these studies have also shown that Minimum Legal Drinking Age increases reduced youth traffic fatalities likely to have involved alcohol (i.e. single vehicle crashes at night or on the weekends), which is further support for a link between the MLDA and youth consumption (see, for example, Dee 1999; O’Malley and Wagenaar 1991).

In contrast, the literature on the effects of direct price increases on youth alcohol consumption is more mixed. Several studies have shown a strong cross-sectional relationship between the monetary price of beer and observable measures of youth drinking (Grossman et al. 1987, 1994 and others). However, these studies have come under recent scrutiny by economists who argue that the inverse relationship between a state’s beer tax and its rate of youth drinking at any point in time may reflect other omitted variables such as state sentiment toward drinking. One solution to this problem would be to examine whether changes in price policy were associated with predictable changes in youth consumption; unfortunately, however, state beer tax changes occur infrequently and typically are small in magnitude, both of which make it difficult to test this hypothesis. In an empirical context, this means that beer tax evaluations can be sensitive to inclusion of state fixed effects, which has been demonstrated for both youth alcohol consumption using the MTF study (e.g. DiNardo and Lemieux 1999) and traffic fatalities using data from the Fatal Accident Reporting System (e.g. Mast et al. 1999, and others). Despite this, others have found that the negative relationship between state beer taxes and youth alcohol consumption survives the inclusion of state fixed effects (Cook and Moore 2001). Grossman (2005) provides a complementary look at this question by comparing national trends in consumption and taxes in the MTF. He finds a negative price/consumption relationship but writes that “more research on this important issue is necessary.”

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5 This is in stark contrast to state tobacco taxation, which has been characterized by several very large price increases over the past two decades. There was one particularly large increase in the beer tax in 1991 at the federal level; however, empirical models that employ year fixed effects render this variation unusable since the variation is fully absorbed by the 1991 indicator variable.
In addition to the Minimum Legal Drinking Age experiments of the 1970’s and 1980’s and the various beer tax changes over this time period, there have more recently been a set of policy experiments related to drunk driving among youths. Specifically, in the early 1990’s states began enacting “Zero Tolerance” laws that set a very low legal blood alcohol content (BAC) limit for drivers under age 21. The 1995 National Highway Systems Designation Act mandated reduced federal funds (as with the MLDA) for a state if it did not adopt such a law. In addition to setting a BAC at .02 or lower, the federal mandate required states to temporarily revoke the driver’s license of any youth found to be in violation of Zero Tolerance. These uniform sanctions were much harsher than some state’s previous laws, which required only fines and/or community service. By 1998, all states had adopted a ZT law. Because the states adopted these laws at different times over the 1990’s, however, there is substantial variation to identify youth drinking effects even after allowing for flexible state controls.

Carpenter (2004) evaluated the effects of Zero Tolerance laws on the drinking and drunk driving behaviors of 18 to 20 year olds, controlling for the associated behaviors of 22 to 24 year olds. Using self reports from the Centers for Disease Control’s Behavioral Risk Factor Surveillance System (BRFSS), the study found that ZT laws reduced binge drinking among 18-20 year olds. This is consistent with several published studies indicating that ZT laws significantly reduced youth traffic fatalities (see Voas et al. 2003 and reviews by Shults et al. 2001 and Zwerling and Jones 1999). No previous study has used surveys of high school students – who should have also been affected by age-targeted drunk driving laws – to estimate the consumption effects of ZT laws.6

In this paper we use confidential area-identified data from the 1976-2003 waves of the Monitoring the Future (MTF) surveys (Johnston et al. 2005). The MTF is a large, school-based survey of secondary school youth in grades 12, 10, and 8. The grade 12 survey – which we consider here – began in 1975, and alcohol consumption has been included in the core questionnaire every year.

The MTF samples high school seniors at schools in the US in three stages. The first stage selects geographic areas as primary sampling units. These areas are chosen as part of nationwide interview studies by the University of Michigan’s Survey Research Center. The second stage samples schools within PSUs with a probability proportionate to the size of the senior class. Finally, in the third stage up to 350 seniors within each selected school are sampled. In large high schools a random sample of classrooms is surveyed, while in small high schools all seniors are surveyed. Sample weights are assigned to the selected

6 The BRFSS is a household survey, not a school-based survey.
samples to correct for any variations in probability of selection at all stages Schools generally agree to participate in the MTF for two consecutive years; the resulting sampling structure results in about 130 schools participating in each year (65 “new” schools and 65 second year participants). For each school that declines to participate, a similar school (in terms of size, geographic area, urbanicity, etc.) is recruited as a replacement. Replacements for declining schools are obtained in the vast majority (over 95%) of cases. Twelfth grade response rates over the history of the MTF have averaged 84%. About 16,000 high school seniors complete the questionnaires each year; the resulting sample for the period 1976-2003 consists of almost 400,000 observations of high school seniors.7

Students in the MTF are asked if they consumed any alcoholic beverages over the past month. They are also asked about episodes of heavy episodic drinking (defined as 5 or more drinks in a row) within the past two weeks. We consider two main outcome variables based on responses to these questions. First, we create an indicator variable called Drinker equal to one if the person reports any past month alcohol consumption. We then create an indicator variable called Heavy Episodic Drinker equal to one if the respondent indicates he or she consumed five or more drinks in a row at least once within the past two weeks. These variables are standard in the literature.

We estimate the effects of ZT laws, Minimum Legal Drinking Ages, and beer taxes in reduced form models for each outcome variable. These models account for unobserved secular year effects, as well as state specific determinants of alcohol use through inclusion of state fixed effects. The method is akin to the standard difference-in-differences (DD) type approach to identify the effects of the various policies in that the models are identified using within state variation in timing of policy adoption (i.e. MLDA increase, beer tax change or adoption of a ZT law), controlling for differences across states that were not treated over the same time period.8

Most of our analyses use ordinary least squares to estimate linear probability models.9 This model is given by:

\[
Y_{ist} = \alpha + \beta_1 X_{ist} + \beta_2 (\text{Zero Tolerance})_{st} + \beta_3 (\text{MLDA of 18})_{st} + \beta_4 (\text{Beer Tax})_{st} + w_s + v_t + \epsilon
\]

where \(Y_{ist}\) refers to the drinking behaviors listed above for youth \(i\) in state \(s\) in year \(t\). \(X\) is a vector of demographic information, including the individual’s age, a

7 We exclude a small fraction of individuals who failed to answer the question about any past month alcohol consumption.
8 This type of empirical approach has a long history in the class of policy evaluation and economics research related to alcohol control. See, for example, Cook and Tauchen (1984).
9 Below in Table 3 we show that our qualitative results persist when we instead perform maximum likelihood estimation of a logit model. The similarity across estimation methods is not surprising since the outcomes we consider are common among high school seniors.
male indicator, an African American indicator, an “other race” indicator, and a Hispanic ethnicity indicator. Zero Tolerance equals one for those covered by a ZT law of 0.02 BAC or lower. MLDA of 18 is an indicator variable for youths in states where the MLDA is 18 for any alcoholic beverage. Beer Tax is the real value of the state and federal excise tax on a gallon of beer (in 1982-84 dollars).\footnote{Our choices with respect to how to model the key variables of interest are largely driven by a desire to be comparable to previous research. This is why, for example, we control for beer taxes instead of taxes on all alcoholic beverages. Existing studies focus on beer taxes because beer is the “drink of choice” among youths in our age range (Coate and Grossman 1988, and others). We choose to model the MLDA as “exposure to an MLDA of 18” – the most permissible drinking age adopted in the US – to be consistent with existing research (Dee 1999 and others). There are, however, several alternative ways to control for Minimum Legal Drinking Ages, such as including an indicator for whether the respondent is strictly below her state’s MLDA. Not surprisingly, this method returned very similar results (available upon request). Finally, we only define a policy to be a “Zero Tolerance” law if it sets the legal limit at 0.02 or lower for those under age 21. Individuals are coded as being treated by the policy if they were surveyed at least one month after the implementation date of the state’s policy. This accounts for the 30 day window for the drinking participation question. See Carpenter (2004) for a description of these issues.} The coefficients of interest, $\beta_2 - \beta_4$, indicate the independent effects of each of the three sets of policies on youth drinking.

The state and year fixed effects are captured by $w_s$ and $v_t$, respectively. These variables purge the estimates of potentially confounding influences that are time invariant within a state or are common to each survey year across states, respectively. In some models we also incorporate state-specific linear time trends by interacting an indicator variable for each state with a variable Trend that equals one in 1976, two in 1977, and so forth. Inclusion of the state trends is a flexible way to control for a variety of other variables that might determine alcohol related behaviors (such as religious sentiment) but which trend linearly within states over time (Friedberg, 1998).\footnote{Allowing for quadratic state trends produced very similar results to the models with linear state trends.} Heteroskedasticity-robust standard errors are clustered throughout at the state level, and all regressions are weighted by the MTF sampling weight (Bertrand et al. 2004).

4. Main Results
We present descriptive statistics of key variables in Table 1. Over our long sample period, 60 percent of youths report that they consumed alcohol in the previous month, while over one third report heavy episodic drinking. Only about 14 percent of our sample was ever exposed to an MLDA of 18, while over a quarter of the sample faced a strict ZT drunk driving law. The average state and federal combined excise tax on a gallon of beer was about 51 cents in 1982-84 US dollars. Slightly less than half of our sample is male, the average age of the high school senior sample is 17.5, and the sample is largely white.
Table 1:
Descriptive Statistics
Sample is High School Seniors, 1976-2003 MTF

<table>
<thead>
<tr>
<th>Variable Description</th>
<th>Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Outcome Variables</strong></td>
<td></td>
</tr>
<tr>
<td>Drinking Participation (drank any alcohol in past 30 days)</td>
<td>.60</td>
</tr>
<tr>
<td>Heavy Episodic Drinking (drank 5 or more drinks in a row at least once in past 2 weeks)</td>
<td>.34</td>
</tr>
<tr>
<td><strong>Alcohol Policy Variables</strong></td>
<td></td>
</tr>
<tr>
<td>Minimum Legal Drinking Age = 18</td>
<td>.14</td>
</tr>
<tr>
<td>Real beer tax (state + federal) in 1982-84 dollars</td>
<td>.51</td>
</tr>
<tr>
<td>Zero Tolerance Law (.02 BAC or less for youths under 21)</td>
<td>.26</td>
</tr>
<tr>
<td><strong>Student Demographics</strong></td>
<td></td>
</tr>
<tr>
<td>Male</td>
<td>.48</td>
</tr>
<tr>
<td>Age</td>
<td>17.5</td>
</tr>
<tr>
<td>Black</td>
<td>.11</td>
</tr>
<tr>
<td>Other non-White race</td>
<td>.07</td>
</tr>
<tr>
<td>Hispanic</td>
<td>.07</td>
</tr>
</tbody>
</table>

Means are weighted by the MTF sampling weight.

Table 2 presents the baseline results for the effects of the policy variables on drinking participation (Column 1) and heavy episodic drinking (Column 2) in models that include controls for individual demographic characteristics and state and year fixed effects. Like previous research, we find strong evidence in Table 2 that exposure to an MLDA of 18 is associated with a statistically significant increase in teen drinking participation on the order of 3 percentage points. We also find that exposure to an MLDA of 18 increased the likelihood of heavy episodic drinking by about 1.7 percentage points among high school seniors in the MTF, significant at the ten percent level. We also find a role for Zero Tolerance laws at affecting youth alcohol use. Specifically, we estimate that a ZT law reduced the likelihood of drinking participation among high school seniors in the MTF by about 2.1 percentage points and reduced heavy episodic drinking by about 1.6 percentage points, though the latter estimate is not statistically significant. Finally, we also find a significant effect of state beer taxes on youth drinking participation over the period 1976-2003. The implied price elasticity associated with the estimate in Column 1, for example, is -.97; i.e., a price increase of 10% would reduce teen drinking participation by about an equal
The beer tax estimate for heavy episodic drinking is negative in sign but not statistically significant. The demographic covariates in Table 2 enter as predicted by previous research: high school senior males are estimated to be more likely to drink and to drink heavily, while minority youths drink less than similarly situated white youths.

Table 2:
Minimum Legal Drinking Ages, Zero Tolerance Laws, and Beer Taxes
Effects on Drinking Participation and Heavy Episodic Drinking
Sample is High School Seniors, 1976-2003 MTF
Models Include State and Year Fixed Effects

<table>
<thead>
<tr>
<th>Variable</th>
<th>(1) Drunking Participation</th>
<th>(2) Heavy Episodic Drinking</th>
</tr>
</thead>
<tbody>
<tr>
<td>MLDA = 18</td>
<td>.029** (.010)</td>
<td>.017‡ (.010)</td>
</tr>
<tr>
<td>Beer Tax</td>
<td>-.114* (.050)</td>
<td>-.054 (.058)</td>
</tr>
<tr>
<td>ZT Law</td>
<td>-.021‡ (.011)</td>
<td>-.016 (.010)</td>
</tr>
<tr>
<td>Male</td>
<td>.085** (.005)</td>
<td>.162** (.003)</td>
</tr>
<tr>
<td>Age</td>
<td>.001 (.002)</td>
<td>.016** (.002)</td>
</tr>
<tr>
<td>Black</td>
<td>-.241** (.012)</td>
<td>-.214** (.008)</td>
</tr>
<tr>
<td>Other non-White race</td>
<td>-.147** (.022)</td>
<td>-.107** (.017)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>-.092** (.016)</td>
<td>-.062** (.017)</td>
</tr>
<tr>
<td>R-squared</td>
<td>.085</td>
<td>.074</td>
</tr>
<tr>
<td>N</td>
<td>394547</td>
<td>394547</td>
</tr>
</tbody>
</table>

Sample is high school seniors in 1976-2003 Monitoring the Future (MTF) surveys. Models also include controls for demographic covariates, including: age, a male indicator, an indicator for Hispanic ethnicity, an indicator for African American race, and an indicator for “other race”. Standard errors, corrected for state clustering, are presented below in parentheses. ‡ significant at 10%; * significant at 5%; ** significant at 1%.

12 This is calculated in the following way: given a mean real beer tax of .51 and a mean drinking participation rate of .60, this suggests a tax elasticity of drinking participation of –.097 (–.114 * [.51/ .60]). Using the conventional estimate that beer taxes constitute about 10% of the price of beer, this figure implies a price elasticity of teen drinking participation of -.97.
Table 3 investigates the robustness of our baseline estimates from Table 2. We restrict attention to the policy variables and present results for drinking participation in the top panel and for heavy drinking in the bottom panel.

### Table 3:
**Robustness Exercises, All Models Include State and Year Fixed Effects**
*Sample is High School Seniors, 1976-2003 MTF*

<table>
<thead>
<tr>
<th></th>
<th>(1) With linear state trends</th>
<th>(2) Only state/year combinations with at least 300 observations</th>
<th>(3) Only observations from the 23 states in every MTF wave</th>
<th>(4) Logit coefficients, implied derivatives in brackets</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Drinking participation</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>MLDA = 18</td>
<td>.039** (.010)</td>
<td>.029* (.012)</td>
<td>.029* (.011)</td>
<td>.159** (.053) [-.038]</td>
</tr>
<tr>
<td>Real Beer Tax</td>
<td>.098 (.074)</td>
<td>-.105‡ (.062)</td>
<td>-.093 (.063)</td>
<td>-611** (.204) [-.147]</td>
</tr>
<tr>
<td>ZT Law</td>
<td>-.023* (.010)</td>
<td>-.024* (.010)</td>
<td>-.027* (.011)</td>
<td>-.078 (.050) [-.019]</td>
</tr>
<tr>
<td>R squared</td>
<td>.086</td>
<td>.087</td>
<td>.085</td>
<td>.065</td>
</tr>
<tr>
<td><strong>Heavy episodic drinking</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>MLDA = 18</td>
<td>.032** (.008)</td>
<td>.008 (.011)</td>
<td>.015 (.012)</td>
<td>.077‡ (.044) [-.017]</td>
</tr>
<tr>
<td>Real Beer Tax</td>
<td>.129‡ (.072)</td>
<td>.001 (.069)</td>
<td>-.007 (.080)</td>
<td>-.207 (.291) [-.046]</td>
</tr>
<tr>
<td>ZT Law</td>
<td>-.017‡ (.010)</td>
<td>-.017* (.008)</td>
<td>-.020 (.013)</td>
<td>-.091‡ (.051) [-.020]</td>
</tr>
<tr>
<td>R squared</td>
<td>.075</td>
<td>.075</td>
<td>.073</td>
<td>.061</td>
</tr>
<tr>
<td>N</td>
<td>394547</td>
<td>316312</td>
<td>315795</td>
<td>394547</td>
</tr>
</tbody>
</table>

See notes to Table 2.

In Column 1 we present results from a model that, in addition to year and state fixed effects, also includes linear state trends. Results from these models with smooth state trends continue to provide evidence that the MLDA and ZT laws had significant effects on both measures of youth drinking: all of the relevant MLDA and ZT estimates from models with state trends return estimates that are...
the same sign as the baseline and are statistically significant at the ten percent level or better. The beer tax estimates, in contrast, are very sensitive to inclusion of state trends: both estimates in Column 1 are wrong signed (positive) and sizable in magnitude. Notably, beer taxes – unlike MLDA and ZT laws – exhibit extremely little independent variation net of the augmented controls in Column 1; as such, the associated tax estimates should be interpreted with caution.

In Columns 2 and 3 we provide estimates from samples that address concerns about some limitations of the MTF data. Specifically, the sampling structure of the MTF suffers from at least two key limitations: 1) the MTF was not designed to produce state representative estimates; and 2) not all states are sampled in each year (we return to these issues below in the discussion section). In Column 2 we present estimates for the three variables of interest from a model that restricts attention to state/year combinations with at least 300 senior respondents, which preserves the vast majority (80%) of the sample. Estimates from this sample are largely similar to the baseline estimates for the drinking participation outcome and continue to suggest a strong role for alcohol control policies in affecting youth drinking. For heavy episodic drinking we find that restricting attention to the “large” state/year observations confirms a significant role for ZT in reducing heavy episodic drinking, though the support for the MLDA and beer tax effects is weaker.

In Column 3 we perform a related exercise by restricting attention to the 23 states represented in each survey year of the MTF. This approach also preserves the majority of the sample size and – like the estimates in Column 2 – continues to provide evidence supporting an important role for alcohol control policies in contributing to the decline in youth drinking participation over the past two decades. Both the MLDA and ZT estimates in the top panel of Column 3 are statistically significant at the five percent level, and the beer tax coefficient estimate is very similar to the baseline. For heavy episodic drinking we find sizable coefficient estimates for the MLDA and ZT variables in the predicted direction, though evidence for the beer tax is again weaker than the baseline. Overall, Columns 2 and 3 of Table 3 show that the main policy effects we have identified are largely robust to directly addressing concerns about the validity of the MTF data, particularly in the case of drinking participation.

In Column 4 of Table 3 we show that the main results are largely robust to logit estimation (we have used linear probability models throughout). We present the logit coefficient estimates and standard errors for each policy variable, as well as the implied derivative below each estimate in brackets (using the mean value of the relevant drinking outcome to compute this value). The logit estimates confirm that youths who were exposed to an MLDA of 18 were significantly more likely to consume alcohol than otherwise similar youths who were not exposed to the permissive drinking age. As in the baseline model, we find a statistically
significant role for beer taxes in reducing youth drinking participation, and for both outcomes we find ZT estimates that are very similar to the baseline effects.

Finally, in Table 4 we take advantage of the longer historical period available in the MTF to effectively trace out the time path of MLDA and ZT effects. This is a common exercise in policy evaluations such as ours and can provide useful information about issues such as policy endogeneity as well as long run versus short run policy effects. To implement this approach, we replace each of the MLDA and ZT indicators with a set of variables that captures how far away (in the past or future) the relevant policy adoption is relative to the current year. For ZT adoption, this is straightforward: we identify the ZT adoption year and define variables relative to this adoption year. We then define mutually exclusive variables that reflect whether the state-specific observation is: 7 or more years before ZT adoption, 5-6 years before ZT adoption, 3-4 years before ZT adoption, 1-2 years before ZT adoption, 1-2 years after ZT adoption, 3-4 years after ZT adoption, 5-6 years after ZT adoption, and 7 or more years after ZT adoption. We then replace the ZT indicator in equation (1) with this series of indicator variables. We operationalize the MLDA variable in a similar way, though because this variation was not simply “on/off” as in ZT, we modify the relevant base year to reflect the last year a state experienced a relatively permissive MLDA of 18. A “classic” difference in differences story would result in zero estimated coefficients for the policy leads and negative coefficients for the policy lags. Moreover, deviations from this pattern could shed important light on the policy dynamics. If laws are systematically adopted in response to surges in youth drinking problems, for example, then we might expect that the “1-2 years before policy adoption” variable would be large and positive.

The results from this exercise are presented in Table 4. We present the relevant coefficient estimates relative to the policy “base year”, as well as the beer tax estimate. Notably, the beer tax estimates in both cases are largely unchanged from the baseline and continue to suggest an important role for taxes in reducing youth drinking participation. Turning to the lead/lag coefficients, we note that for the case of Zero Tolerance laws there is little evidence of policy endogeneity: in the years leading up to each state’s ZT adoption, there does not appear to have been a systematic spike in youth drinking participation or heavy episodic

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13 This approach is far from perfect for several reasons. First, there are very few observations that reflect 7+ years before the last year of an MLDA of 18 (only 1.1 percent of the sample) because this policy variation was generally at the beginning of our sample period. Put differently, there was a large amount of policy variation prior to the start of our sample period, and we miss these potentially important dynamics. Another problem is that 7+ years before the last year of an MLDA of 18 will mean different things for different states, depending on how long they experienced an MLDA of 18. For some states, the drinking age 7 years prior to the last age 18 year would be 21, while for others it will be 18. We note that the main lead/lag patterns for the ZT variable were not sensitive to how we entered the MLDA variation.

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drinking. In fact, all of the ZT policy leads are very small in magnitude, with none larger than 0.9 percentage points. In contrast, the ZT policy lags are consistently negative and larger than 1 percentage point in magnitude. Notably, the magnitude generally increases over time for both outcomes, suggesting that ZT policies take some time to exhibit their full effect on youth drinking.

In contrast, the lead and lag coefficients for the MLDA variation provide less compelling evidence that the assumptions of a true “natural experiment” are justified in this case. Although the years immediately following (1-2 and 3-4 years) the last year of a permissive drinking age saw sharp reductions in alcohol consumption, the estimated coefficient estimates for each outcome for the “1-2 years before” variable are large and positive – at least as large in magnitude as the “1-2 years after” indicator. This suggests that drinking among high school seniors may have spiked just prior to the removal of the permissive drinking age. This is not entirely consistent with movements away from an MLDA of 18 being truly “exogenous”, despite the federal threat of losing highway funds. Of course, we note that standard errors throughout the exercise in Table 4 are sufficiently large that we cannot rule out alternative policy timing stories, including the “ideal” difference in differences setting.

Table 4:
Tracing Out the Time Path of the MLDA and ZT Effects
Models Include State and Year Fixed Effects

<table>
<thead>
<tr>
<th>Variable</th>
<th>Drinking Participation</th>
<th>Heavy Episodic Drinking</th>
</tr>
</thead>
<tbody>
<tr>
<td>7+ years before the last year</td>
<td>-0.010</td>
<td>0.031</td>
</tr>
<tr>
<td>of an MLDA 18</td>
<td>(.026)</td>
<td>(.023)</td>
</tr>
<tr>
<td>5-6 years before the last year</td>
<td>0.010</td>
<td>0.010</td>
</tr>
<tr>
<td>of an 18 MLDA</td>
<td>(.018)</td>
<td>(.013)</td>
</tr>
<tr>
<td>3-4 years before the last year</td>
<td>0.011</td>
<td>0.018</td>
</tr>
<tr>
<td>of an 18 MLDA</td>
<td>(.016)</td>
<td>(.014)</td>
</tr>
<tr>
<td>1-2 years before the last year</td>
<td>0.022</td>
<td>0.019</td>
</tr>
<tr>
<td>of an 18 MLDA</td>
<td>(.015)</td>
<td>(.013)</td>
</tr>
<tr>
<td>The last year of an 18 MLDA</td>
<td>--</td>
<td>--</td>
</tr>
<tr>
<td>1-2 years after the last year</td>
<td>-0.022</td>
<td>-0.013</td>
</tr>
<tr>
<td>of an 18 MLDA</td>
<td>(.015)</td>
<td>(.013)</td>
</tr>
<tr>
<td>3-4 years after the last year</td>
<td>-0.018</td>
<td>-0.011</td>
</tr>
<tr>
<td>of an 18 MLDA</td>
<td>(.017)</td>
<td>(.013)</td>
</tr>
<tr>
<td>5-6 years after the last year</td>
<td>-0.012</td>
<td>0.010</td>
</tr>
<tr>
<td>of an 18 MLDA</td>
<td>(.016)</td>
<td>(.020)</td>
</tr>
<tr>
<td>7+ years after the last year</td>
<td>-0.007</td>
<td>0.007</td>
</tr>
<tr>
<td>of an 18 MLDA</td>
<td>(.018)</td>
<td>(.017)</td>
</tr>
<tr>
<td>Real Beer Tax</td>
<td>-0.004</td>
<td>-0.047</td>
</tr>
<tr>
<td></td>
<td>(.049)</td>
<td>(.063)</td>
</tr>
<tr>
<td>7+ years before ZT</td>
<td>0.002</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(.030)</td>
<td>(.026)</td>
</tr>
</tbody>
</table>
5-6 years before ZT  .009  .001
   (.028)   (.026)
3-4 years before ZT  .005  -.001
   (.021)   (.018)
1-2 years before ZT  .009  .003
   (.014)   (.013)
Year of ZT adoption  --  --
1-2 years after ZT  -.010  -.010
   (.014)   (.012)
3-4 years after ZT  -.012  -.021
   (.020)   (.017)
5-6 years after ZT  -.025  -.022
   (.026)   (.022)
7+ years after ZT  -.023  -.026
   (.028)   (.027)
R-squared   .085  .074
N  394547  394547

See notes to Table 2.

5. Discussion, Limitations, and Conclusion
In this paper we have leveraged the unique nature of the MTF data to provide a comparative analysis of the three major sets of policy interventions related to youth alcohol behaviors over the past 30 years. These historical results are particularly important for guiding future decisions concerning the most effective ways to reduce alcohol-related problems among youth. Previous research has focused on at most two of these sets of policies in isolation, largely because of insufficient data. Our results suggest important roles for a variety of public policies at reducing youth alcohol consumption, including: the direct command and control mechanism of regulating minimum legal drinking ages, manipulating prices through state beer tax increases, and indirectly affecting alcohol use by toughening drunk driving laws for youths.

Our results are subject to some important caveats and limitations, most of which have to do with the nature of the data. First, although our study follows a line of previous research estimating the effects of state policies on youth behaviors using these data (Dee 1999, Gruber 2001, O’Malley & Wagenaar 1991, and others), the MTF sampling frame was not explicitly designed to produce state representative estimates. Second, because the survey is administered at school, it excludes youths absent on the day of the survey as well as high school dropouts. Although omission of these two groups is unlikely to seriously affect our estimates of the trends in youth drinking, it is possible that absent students or dropouts are systematically more or less responsive to alcohol control policies.

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than students in the sample.\textsuperscript{14} Suppose that high school dropouts were systematically less sensitive to alcohol control policies than other youths (this would be true, for example, if dropouts were less responsive to a variety of types of legal interventions more generally). If so, their exclusion could make the policies appear artificially effective due to the selection and composition biases.\textsuperscript{15} In this sense, it is important to think of the MTF as representative of enrolled high school seniors, not of a particular age group per se.

Another potentially serious limitation of our data is that we use self-reported alcohol consumption as our main outcome of interest. One may be concerned that self-reported alcohol consumption responds endogenously to alcohol control policy; that is, reported youth drinking rates may appear artificially responsive to public policy even though actual drinking rates are less affected.\textsuperscript{16} This could arise if, for example, tougher alcohol control changes social norms such that there is more stigma associated with alcohol consumption. In fact, this phenomenon may be especially pronounced for, say, the minimum legal drinking age since underage youths who drank alcohol may fear legal or administrative reprisals from school officials by honestly stating so, even though the MTF survey guarantees strict confidentiality.\textsuperscript{17}

\textsuperscript{14} Omission of absentees and dropouts is unlikely to alter the trends estimates for a couple of key reasons. First, both groups are fairly rare. About 10-15 percent of seniors are absent on the day of the survey for reasons usually due to extracurricular activities (recall that the surveys are administered in the spring of the senior year). Questions in the MTF about recent student absences actually allow for the possibility of directly adjusting for absenteeism; previous research has shown that the adjusted rates of substance use are very similar to the unadjusted rates but suffer from less precision (Johnston et al. 2005). Independent data on high school dropouts suggests that no more than 15-20 percent of youths in this age range are high school dropouts, a figure that has varied little over the past 25 years. These youths have slightly higher alcohol use rates, though an independent technical review of the MTF data found that omission of dropouts and absent students did not substantially alter the estimated rates of substance use (Clayton and Voss 1982). Annual alcohol prevalence is likely underestimated by about two percent due to the omission of dropouts. Again, however, their exclusion only imparts bias to our policy estimates if they are differentially responsive to alcohol control.

\textsuperscript{15} Note that if alcohol use has an independent causal effect on absenteeism or high school dropout, this would cause our samples to have systematically fewer dropouts when alcohol control policies are toughened (since the policies reduce alcohol consumption). These types of composition effects are unlikely for MLDA and ZT given the modest effects on alcohol consumption they appear to have had. Moreover, recent research suggests that there is no casual effect of alcohol use on completed education (Dee and Evans 2003).

\textsuperscript{16} Alcohol sales data would be useful in this regard, though the obvious problem with these types of data is that they are not age-disaggregated.

\textsuperscript{17} Note that if this underreporting were a time invariant feature about certain states we would not be concerned because it would be accounted for in our use of state fixed effects; instead, the worry is that such under-reporting could be systematically associated with adoption of tougher alcohol policies.
There are several considerations to keep in mind with respect to this underreporting problem. First, previous research on MLDA changes and youth alcohol consumption using the MTF has shown that the policy changes were predictably related to individual reports of their (unnamed) friends’ alcohol consumption in addition to their own self reported consumption. This research also showed that the MLDA changes were predictably related to changes in perceived harms associated with alcohol consumption and disapproval of alcohol use. It is unlikely that this wide array of outcomes would be predictably affected by MLDA changes in the absence of true drinking changes (O’Malley and Wagenaar 1991). Second, we were able to directly evaluate one possible route through which endogenous underreporting could have affected our estimates. Specifically, we estimated the independent association between our policy variables and the likelihood that a youth did not respond to the alcohol consumption questions (despite responding to other questions on the survey). Since one way that stigma-induced underreporting could manifest itself is through failing to respond at all to the alcohol questions, we would expect that tougher alcohol control policies would be systematically associated with nonresponse if this type of underreporting were severe. Yet we found no substantively or statistically significant relationships between nonresponse probability and our policy variables, suggesting that endogenous underreporting of drinking behaviors may not be too problematic in our context. Moreover, recoding the nonresponders as “drinkers” did not change our main policy estimates. Of course, there are other possible ways that youths could have underreported consumption besides failing to respond to the alcohol questions.

Despite these limitations, it is worthwhile to clarify that essentially all of the major data sources used in the published literature on policy determinants of youth drinking – the CDC’s national Youth Risk Behavior Survey (YRBS), SAMHSA’s National Household Surveys on Drug Abuse (NHSDA), the National Longitudinal Surveys of Youth (NLSY), and others – share these problems to varying degrees. None of these other data sources, for example, produced state representative estimates. And all researchers employing survey data on alcohol consumption must be concerned about endogenous underreporting. Importantly, the evidence on the effectiveness of these and other state policy efforts toward substance use comes from a variety of complementary sources such as alcohol-involved traffic fatalities or cirrhosis deaths (which are clearly not subject to the data concerns we have raised here); the value in using self reported drinking behavior such as that observed in the MTF is that it can provide important corroborating evidence on the underlying behavioral mechanisms through which these policies have resulting effects on outcomes.

How big are the policy effects we estimate? We estimate that exposure to an MLDA of 18 was associated with a statistically significant increase in drinking...
participation and heavy drinking of about 2.9 and 1.7 percentage points, respectively. In the late 1970’s mean rates of drinking participation and heavy drinking were 72 and 41 percent, respectively; we therefore estimate that movements away from this most permissive drinking age were responsible for declines of about 4 percent for both outcomes. Our estimates for adoption of Zero Tolerance laws in the middle of the 1990s suggest reductions in drinking participation and heavy drinking of about 2.1 and 1.6 percentage points, respectively. Right after the federal beer tax increase in 1991, the 1992 mean rates for these outcomes were 51 and 28 percent; we therefore estimate that adoption of tougher age-targeted drunk driving laws reduced youth alcohol consumption by about 4 and 6 percent, respectively. Interestingly, we estimate that both measures of alcohol consumption were either flat or slightly increasing in the aggregate over the mid-1990s (a phenomenon that has also been observed for youth smoking over this time period – see, for example, Gruber and Zinman 2001); our results suggest that ZT laws helped mitigate this harmful trend. Taken together, we estimate that movements away from the permissive drinking age of 18 and adoption of ZT laws can explain about 20 percent of the peak to trough reduction in youth drinking participation over this period.18

We find it notable that ZT laws achieved any significant reductions in teen drinking. Specifically, Zero Tolerance laws were adopted in a period when states had already set their MLDA at 21; indeed, a key motivation behind ZT policies was that since drinking under age 21 is illegal, it logically follows that drinking and driving under age 21 should also be illegal. This makes the reductions in teen drinking and heavy episodic consumption associated with ZT laws all the more remarkable, since underage youths who drank in the 1990s were already in violation of laws concerning the MLDA.19 The fact that additional policy-induced reductions in youth drinking are observed after explicit and independently successful attempts to limit alcohol availability to all underage youths suggests both that MLDA laws were not completely effective and that youths remain quite sensitive to other interventions related, for example, to their driving privileges or to the money price of alcohol.

It is also worth noting that underage drinking rates remain at or above 50 percent even after all states have made such activity illegal. While our estimates provide strong evidence that youth drinking behavior is responsive to a variety of

18 We arrive at this estimate by noting that youth drinking declined by 25 percentage points from 1978 to 2003. Our MLDA estimate combined with our ZT estimate suggest that these two policies alone can explain 5 percentage points of this overall decline, or about 20 percent. The associated figure for heavy episodic drinking is 23 percent. Note that the real value of the state and federal excise tax on beer has actually fallen over this long time period. Youth drinking is therefore higher now than what it would have been had the real value of the beer tax not declined over the past quarter century.

19 Note that the same is true for beer tax increases for youths who were under their state MLDA.
types of government interventions, they also tell a cautionary tale concerning the limits of regulation. Further research on the competing roles of family, peer, and state influences in youth substance use would be particularly useful in this regard. Indeed, our results are limited by the small set of covariates we have controlled for. We have chosen to estimate fairly sparse models of youth drinking participation and heavy drinking, largely to follow existing research. An alternative approach taken by other researchers has been to use rich panel data sources on youths that contain detailed information on relevant environmental factors.

Overall, our results confirm that a variety of types of government intervention have been and can be effective at reducing youth alcohol use. Of course, given that all states have now adopted an MLDA of 21, as well as strict Zero Tolerance drunk driving laws, what do our results imply about future policy efforts to reduce youth drinking? Because nearly all high school seniors are below the age of 21, further increases in the legal drinking age are unlikely to directly affect consumption, though indirect effects through changes in social norms are certainly plausible. In contrast, there is somewhat more latitude to further toughen state drunk driving laws targeted at youths, such as imposing harsher penalties, longer license revocations, and/or requiring all states to set the ZT limit at .00 BAC (i.e. “not a drop”) instead of .01 or .02, as in several states currently. Finally, our estimates suggest that the least utilized policy interventions over the past 25 years – direct increases in state beer taxes – have very meaningful effects (both in absolute terms and relative to the other policy levers) at reducing youth alcohol consumption and, by implication, alcohol-related problems. Recently, states such as Alaska, Nebraska, Nevada, and Utah have adopted sizable beer tax increases. Future empirical work that takes advantage of this recent policy activity will be important in resolving debates regarding the tax responsiveness of youth drinking and in determining the most effective policy instruments to reduce alcohol-related harms among youths.

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20 It is also worthwhile to calculate the implied effect on youth drinking participation if minimum legal drinking ages were lowered back to their 1970’s levels, as some have advocated. If we were to assume that exposure to a permissive drinking age were to have the same proportional effect today as it did two decades ago, we estimate that the drinking participation rate would rise about 2 percentage points from its current base of about 50 percent.
References

The Beer Institute (various years). Brewer’s Almanac, Washington D.C.


