Journal of Public Economics 72 (1999) 289–315

State alcohol policies, teen drinking and traffic fatalities

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Received 17 January 1998; received in revised form 19 June 1998; accepted 2 July 1998

Abstract

This empirical study evaluates the policy responsiveness of teen drinking in models that can condition on the unobserved state-specific attributes that may have biased conventional evaluations. The results demonstrate that cross-state heterogeneity can be important and that beer taxes have relatively small and statistically insignificant effects on teen drinking. Models of youth traffic fatalities also indicate that the conventional beer tax elasticities are not robust to additional controls for omitted variables. The importance of these omitted variables is illustrated by a counterfactual which compares models of nighttime fatalities to those that occur in the daytime when the rate of alcohol involvement is substantially lower.

Keywords: Alcohol use; Traffic fatalities; Alcohol taxes; Minimum legal drinking age; Young adults

JEL classification: H2; H7; I1

1. Introduction

Despite the considerable efforts of policy-makers and public-interest groups, teens in the United States continue to engage in the illegal use and abuse of alcohol at persistently high rates (Grant et al., 1991). The policy relevance of this abusive drinking is motivated by the varied evidence that links it to a broad range of negative outcomes that are realized both contemporaneously and over the life

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0047-2727/99/$ – see front matter
PII: S0047-2727(98)00093-0

cycle.[1] Most notably, the abuse of alcohol is strongly implicated in traffic fatalities, the leading cause of mortality among young adults (Rosenberg et al., 1996). In 1996, over 21% of the 15 to 20 year old drivers killed in fatal crashes were intoxicated.[2] Beginning in the late 70's, a growing awareness of this link led several state governments and, ultimately, the Federal government to seek reductions in teen access to alcohol through increases in the states' minimum legal drinking ages (MLDA).[3] In 1977, 29 states and the District of Columbia had a MLDA of 18. However, by the late 80's, every state had raised their MLDA to 21. The National Highway Traffic Safety Administration (NHTSA) estimates that the movement to higher MLDA has saved over 16 500 lives (U.S. Department of Transportation, 1997).

The uniform adoption of a minimum legal drinking age of 21 suggests that, in the United States, the scope for similarly straightforward, dramatic and relatively costless reductions in teen alcohol availability may now be rather limited. However, a widely cited body of empirical research has argued that this is not so (Grossman et al., 1987, 1994; Coate and Grossman, 1988; Kenkel, 1993; Cook and Moore, 1994). More specifically, this research has suggested that price policies (in particular, excise taxes on beer) represent an unexploited lever for reducing the prevalence and consequences of abusive drinking among teens. However, the empirical evidence in support of these assertions has been based on somewhat limited econometric specifications. In particular, the direct evidence that abusive teen drinking is responsive to alcohol taxes has been based on individual-level demand equations that are identified solely by the cross-sectional variation in alcohol prices or taxes.[4] Unfortunately, the finding that teens in states with relatively high beer taxes are less likely to abuse alcohol is observationally equivalent with very different hypotheses. The prior research in this field has argued that this correlation reflects the responsiveness of teen drinkers to the pecuniary costs of acquiring beer in states with high taxes. However, it is equally reasonable to suppose that this cross-state relationship merely reflects the presence of unobserved state-specific attributes that influence both the level of beer taxation and the prevalence of teen drinking. For example, it may be that states with

[1] Alcohol abuse among teens has been linked to risky sexual activity, violence, reduced educational achievement as well as the development of an adult habit (see Grossman et al., 1993, 1994). However, it is not always clear that these links represent policy-relevant causal relationships (e.g. Dee and Evans, 1997).

[2] A driver is defined as intoxicated if his or her blood alcohol concentration (BAC) is at least 0.10 g/dl, the legal limit in most states (U.S. Department of Transportation, 1997).

[3] The Federal government compelled many states to raise their MLDA to 21 by withholding some highway funds for non-compliance.

[4] Most of the cross-sectional variation in alcohol prices is driven by the cross-state variation in alcohol taxes. Though the alcohol industry is not perfectly competitive, there is evidence that higher taxes are fully passed on to consumers in the form of higher prices (Cook and Moore, 1993). Like most prior research, this study focuses on beer taxes because prices are endogenously determined, because beer is the drink of choice among teens and because beer taxes are observed without error.

effective cultural prohibitions against alcohol use also choose to enact higher alcohol taxes.[5] Distinguishing between these hypotheses is critical for evaluating the widely held view that alcohol price policies represent an important and unexploited health policy lever.

This empirical study addresses these concerns by presenting new evidence on the efficacy of state alcohol policies (i.e. excise taxes on beer and minimum legal drinking ages) in reducing the prevalence of teen drinking and its key related outcome, youth traffic fatalities. First, in Section 2, the conventional empirical models directly linking state alcohol policies and teen drinking are reconsidered. The evaluations presented here are based on an unusual data set that consists of pooled cross-sections from the 1977–92 Monitoring the Future (MTF) surveys of high school seniors. Because these survey data contain both cross-state and time-series variation, these evaluations can condition unambiguously on the unobserved state attributes that may have biased most previous estimates of the policy responsiveness of abusive teen drinking. The results of these estimations indicate that the movement to higher minimum legal drinking ages substantially reduced teen drinking. For example, these estimations indicate that the movement away from an MLDA of 18 reduced heavy teen drinking among MTF respondents by 8.4%. In contrast, these models also demonstrate that the conventional links between higher beer taxes and lower levels of teen drinking are not robust. In particular, estimates from specifications that include state fixed effects indicate that the within-state variation in beer taxes has a small and statistically insignificant relationship with the prevalence of abusive teen drinking.[6]

The other key evidence linking state alcohol policies and teen drinking has been based on reduced-form models of youth traffic fatalities. In particular such models suggest that, even conditional on state fixed effects, higher levels of beer taxes reduce youth traffic fatalities. Section 3 presents new evidence on these relationships by evaluating empirical models of youth traffic fatalities based on a relatively long panel (1977–92) of annual state-level data. The results of these estimations uniformly indicate that the movement to higher minimum legal drinking ages led to large and statistically significant reductions in youth traffic fatalities. In contrast, these results also demonstrate that the conventional links between beer taxes and youth traffic fatalities are not robust to the inclusion of demonstrably important regressors (i.e. state-specific trend variables). However, because these additional trend variables remove much of the sample variation in beer taxes, the implications of this sensitivity are somewhat ambiguous. An ad-hoc

[5]In fact, Coate and Grossman (1988) find that the price responsiveness of teen drinking is not robust in specifications that include proxies for ''drinking sentiment'' (i.e., the population concentration of religious denominations). Similarly, DiNardo and Lemieux (1996) report that the tax elasticity of teen drinking participation is sensitive to the inclusion of state fixed effects.

[6]Furthermore, the loss of precision associated with introducing state fixed effects is not severe. For example, for models of heavy teen drinking, the largest tax elasticity implied by the 95% confidence interval is still less than one-sixth of the conventionally estimated elasticity.

counterfactual based on comparing models of daytime and nighttime fatalities provides a more direct and compelling commentary. The rate of alcohol involvement in daytime traffic fatalities is substantially lower than in the nighttime. This well-known pattern suggests that, if the conventional link between traffic fatalities and beer taxes were accurate, it should appear sharply attenuated in models of daytime fatalities. The evidence presented in this study demonstrates that, in conventional specifications, beer taxes actually appear to have a large and statistically significant effect on daytime traffic fatalities. This implausibility of this relationship suggests that conventionally specified models have confounded the variation in beer taxes with important and unobserved determinants of the within-state variation in youth traffic fatalities. The key findings of this study are summarized and discussed in Section 4.

2. The policy determinants of teen drinking

For most of the time that has elapsed since the repeal of Prohibition, U.S. policy-makers have paid little attention to the possibility that the prevalence of abusive drinking could be influenced through supply regulations.[7] During the 1960's and 70's, the widely held notion that such regulations were ineffective was instrumental in encouraging several states to lower their MLDA.[8] However, the subsequently observed link between the lowered MLDA and increased teen traffic fatalities demonstrated that state policies which influenced alcohol availability could also change the prevalence and consequences of abusive drinking among young adults (e.g., Cook and Tauchen, 1984). More direct evidence on the effect of state policies on teen drinking has come from a recent series of econometric studies based on individual-level survey data from young adults (Grossman et al., 1987, 1994; Coate and Grossman, 1988; Kenkel, 1993; Cook and Moore, 1994).

These studies uniformly report that the cross-sectional variation in beer taxes or prices is negatively related to teen alcohol use and conclude that tax increases would generate substantial reductions in teen alcohol use. However, the plausibility of all these results and their dramatic policy implications hinge critically on the quality of their shared identification strategy. More specifically, a key feature in all of these research designs is the assumption that the policy responsiveness of teen drinking can be effectively identified by comparing teens who reside in states with different alcohol policies and prices. But to the extent that states' beer taxes and

[7]Cook and Tauchen (1982) cite a survey of Alcohol Beverage Control administrators (Medicine in the Public Interest, 1979) which suggested that these officials defined their roles in terms of revenue collections and maintaining orderly markets but not public health concerns.

[8]The movement to lower MLDA over this period was largely motivated by the sentiment that those old enough for military service should be allowed access to other adult activities like voting and alcohol use.

minimum legal drinking ages were correlated with unobserved attributes that also influence teen drinking (e.g. shared cultural attributes like drinking sentiment), the cross-state variation in such policies may not provide a valid ''natural experiment'' (Besley and Case, 1994; Meyer, 1995). Coate and Grossman's (1988) finding that the price responsiveness of teen drinking was not robust to empirical models that included proxies for drinking sentiment suggests that this key specification concern deserves closer inspection. Similarly, other more recent studies have noted the possible importance of cross-state heterogeneity in such policy evaluations (e.g. Dee and Evans, 1997; Kaestner, 1997; DiNardo and Lemieux, 1996).

More ad-hoc evidence on the possible biases that cross-state heterogeneity can introduce into evaluations of teen drinking can be constructed with straightforward counterfactuals that parallel the conventional use of the cross-state variation in alcohol policies. For example, I matched a state policy that is plausibly unrelated to teen drinking (i.e. whether the state has a death penalty) to the individual-level data from the high school seniors who participated in the 1989 Monitoring the Future (MTF) survey.[9] Using this match, I estimated the ''effect'' of a death penalty law on teen participation in heavy drinking. A naive interpretation of the results would lead to the conclusion that the introduction of a death penalty can reduce heavy teen drinking by a statistically significant 16%.[10] The ambiguities associated with the strong cross-sectional correlation between this state policy and teen drinking provide an ad-hoc but illustrative cautionary tale on the difficulties that unobserved state heterogeneity can introduce into policy evaluations.

2.1. Monitoring the future (MTF) surveys

Less ambiguous evidence on whether state alcohol control policies influence the prevalence of abusive teen drinking could be constructed with an empirical design that controlled for the cross-state heterogeneity that might influence both alcohol use and state alcohol policies. However, the possible sources of this cross-state heterogeneity (e.g. culture attributes like drinking sentiment) are undoubtedly difficult to specify and measure. In light of this, an ideal approach for addressing the specification concerns raised here would be to have an improved data set that pooled cross-sections of survey data on teen drinking from different points in time. Because such a data set would contain both cross-sectional and time-series variation, the empirical model could include state fixed effects that would

[9]Grossman et al. (1994) also use data from this 1989 survey in cross-sectional evaluations. Dee and Evans (1997) present counterfactuals similar to these in the context of educational attainment and alcohol policies. The data on state death penalties are from the *Book of the States*. The MTF data and empirical specifications used here are described in detail in the next section.

[10]The absolute value of this marginal effect is roughly 3.9 times larger than its standard error.

unambiguously control for whatever the state-specific and time-invariant determinants of teen drinking are.

Unfortunately, no suitable and nationally representative data sets of this kind are publicly available. For example, the public-use MTF surveys, which are sponsored by the National Institute on Drug Abuse (NIDA), have annually gathered data since 1975 from samples of high school seniors on attitudes, drug and alcohol use.[11] However, for confidentiality reasons, the public-use MTF data do not include the state identifiers that could match teen behaviors to state policies. But through a special contractual arrangement with the Institute for Social Research at the University of Michigan, an extract of cross-sectional data from the 1977–92 Monitoring the Future surveys which included state identifiers was purchased. As a condition of this agreement, only a limited set of variables from the surveys could be made available. This special extract contains information on three kinds of drinking behavior, state codes, survey year and three demographic variables. More specifically, within ''cells'' defined by state, survey year, age (i.e. under 18 or not), race/ethnicity (white non-Hispanic or not) and gender, the number of respondents and the proportion who satisfy each drinking definition are known. This data set consists of 3941 cells representing the responses of 255 560 high school seniors in 44 states over the 1977–92 period.[12] The three drinking definitions contained in this extract are frequently employed in this literature and capture some of the heterogeneity in the frequency and quantity of teen alcohol use. These three definitions refer to ''drinkers'' (any drink of alcohol in the last month), ''moderate drinkers'' (10 or more drinks of alcohol in the past month) and ''heavy drinkers'' (5 or more drinks in a row sometime in the last 2 weeks). Nearly 66% of the high school seniors in these pooled surveys are drinkers; 13.8% are moderate drinkers and 36.7% are heavy drinkers (Table 2).

2.2. Difference-in-differences results

A basic ''difference-in-differences'' model illustrates how the policy responsiveness of teen alcohol use can be identified with these unique MTF data. For example, between 1989 and 1992, five states (California, Delaware, New Jersey,

[11] Each yearly MTF survey has included a nationally representative sample of over 15 000 respondents from roughly 130 schools.

[12] Respondents whose race was not black or white were excluded. For confidentiality reasons, cells with fewer than 5 respondents were also deleted from this data set. However, the unrestricted public-use MTF surveys over this period had only 271 012 respondents, so this editing was not substantial. Furthermore, the empirical results generated by this data set closely replicate the prior results based on unrestricted data.

New York and Rhode Island) increased their excise tax on beer.[13] Because these increases were typically large in percentage terms, the conventionally estimated tax elasticities would suggest that these increases had an observable impact on teen alcohol use and its associated consequences. Using the grouped data from the 1989 and the 1992 MTF surveys as they were received, a sparse model for evaluating the actual effects of the tax hike could take the following form:

$$D_{ist} = u_s + v_t + T_{st}\gamma + \epsilon_{ist} \tag{1}$$

where D_{ist} represents the proportion of respondents who satisfy a given drinking definition in cell i and ϵ_{ist} is a mean-zero random error. The terms, v_t, and u_s, respectively represent binary indicators for the 1992 survey cohort and for whether the respondents were in a state that changed its beer tax over this period. The term, T_{st}, is a binary indicator equal to 1 for 1992 respondents from states that raised their beer taxes (i.e. the interaction of v_t and u_s). The evaluation parameter, γ, identifies the effect of the tax increases on teen drinking. It is straightforward to show that the value of γ estimated in Eq. (1) equals the difference between the change in the average value of D_{ist} in the ''treatment'' states (i.e. those that raised their beer tax) and the change in the ''control'' states (i.e. those that did not raise their beer tax). The logic of this so-called difference-in-differences estimator is straightforward. The first difference reflects the observed changes in states that raised their beer taxes. However, since several time-varying determinants, including possibly beer taxes, were likely to influence youth drinking over this period, this difference could generate an inaccurate impression of the tax response. The second difference (i.e. the change in youth drinking in states without tax changes) provides a measure of the shared changes in youth drinking unrelated to the tax increases. Under the key assumption that $E(\epsilon_{ist}|T_{st})=0$, the difference in these differences is an unbiased estimate of the change in youth drinking attributable only to the tax increases. This difference-in-differences estimator clearly illustrates the unique identification strategy made possible by these MTF data. Instead of relying on possibly confounded cross-sectional comparisons, this approach differences out the time-invariant cross-sectional heterogeneity (in this context, u_s) and relies instead on comparing the changes within the groups that did or did not face beer tax increases over this period.

The key results from a difference-in-differences analysis of the effects of the

[13] It is appropriate to focus on this period since all states had a minimum legal drinking age of 21. Within this period, California raised its nominal tax from $0.04 to $0.20 per gallon; Delaware from $0.06 to $0.16; New Jersey from $0.03 to $0.10; New York from $0.11 to $0.21 and Rhode Island from $0.06 to $0.10 (DISCUS, 1996a). Only three of these five states are represented in the MTF data. New Hampshire raised and lowered its tax over this period and is also excluded from this analysis. The District of Columbia is also excluded.

Table 1
The effects of beer tax increases on youth drinking and traffic fatalities, difference-in-differences estimates[a]

Dependent variable	Dependent mean 1989	Dependent mean 1992	Difference in states with beer tax increases	Difference in states without beer tax increases	Difference in differences
Drinker	0.562	0.482	−0.1056	−0.0772	−0.0284
	(0.011)	(0.011)			(0.0496)
Moderate drinker	0.109	0.086	−0.0094	−0.0247	0.0153
	(0.006)	(0.005)			(0.0253)
Heavy drinker	0.310	0.252	−0.0525	−0.0592	0.0067
	(0.011)	(0.010)			(0.0462)
Traffic fatality rate, ages 18–20	44.2	36.4	−5.2	−8.1	2.9
	(1.8)	(1.7)			(7.7)

[a]The difference-in-difference estimates are based on OLS estimations of models that include as covariates fixed effects for the 1992 observations and for whether the observation was in a state that raised its beer tax over this period. Standard errors are reported in parentheses. The traffic fatality rates are defined per 100 000 in the age-specific population.

beer tax increases that occurred between 1989 and 1992 are reported in Table 1.[14] Over this 4-year period, the levels of teen drinking and traffic fatalities declined dramatically. For example, the results in Table 1 indicated that the overall amount of heavy teen drinking fell by 18.7% ([0.252 − 0.310]/0.310). However, the results in Table 1 provide little evidence that these reductions were due at all to beer tax increases. With the exception of the model for drinking participation, all of these results indicate that the reductions in teen drinking and traffic fatalities were actually larger in states that did not raise their beer taxes than in states that did. Therefore, the implied difference-in-differences estimates suggest that raising beer taxes increased heavy and moderate teen drinking as well as traffic fatality rates. However, given the sparseness of the specification and the limited use of the available data, it is not surprising that none of these estimates are very precise statistically. Nonetheless, if the large, conventionally reported tax elasticities were unbiased, one might at least expect to observe the impact of tax increases in the conditional means of these ostensibly related outcomes.

2.3. Pooled cross-sectional results

The standard extensions to the basic difference-in-differences model facilitate several important robustness checks and provide the scope for utilizing all the available data for efficient evaluations of both the tax and drinking age policies.

[14]For completeness, the results using youth traffic fatality rates are also presented here. The data are described in the next section. In order to preserve the confidentiality of the MTF data, only the changes and not the levels of teen drinking are reported for the state groups.

However, the basic empirical specification for the drinking equations reported here continues to utilize the data much as they were received. More specifically, the equation of interest is:

$$D_{ist} = W_{ist}\Pi + M_{st}\gamma + u_s + v_t + \epsilon_{ist} \tag{2}$$

where D_{ist} now refers to the proportion of respondents from cell i in state s and year t that satisfy a given drinking definition. The vector, W_{ist}, includes binary indicators for the demographic indicators available in the MTF extract. The terms, u_s and v_t, represent respectively the unobserved state-specific and year-specific determinants of teen alcohol use and ϵ_{ist} is a mean-zero random error. The term, M_{st}, represents both of the key state alcohol policies in a given year: the state and Federal excise taxes on a gallon of beer in 1982–84 dollars and the state's drinking age policy.[15] Because the grouped nature of these data may have introduced heteroscedasticity, efficient estimates of the parameters in Eq. (2) are given by the weighted least-squares (WLS) estimator where the weights are the number of respondents per cell. However, the subsequent results prove robust to other possible specifications.[16]

Like the difference-in-differences model in Eq. (1), the basic identification strategy implicit in Eq. (2) purges the unobserved and potentially confounded cross-sectional heterogeneity by relying on the within-state variation in teen drinking and alcohol policies over time and by using respondents who did not face changed policies as a control for unrelated time-series variation. However, unlike the sparse difference-in-differences model, the two-way fixed effects model in Eq. (2) provides a richer framework for evaluating the precision and robustness of the evaluations. For example, these models clearly omit information on attributes that may influence teen alcohol use (e.g. socioeconomic variables like family income, family structure and parental education). These unfortunate omissions are a result of the limitations placed on this unique data set by the confidentiality restrictions required for obtaining state identifiers. Fortunately, it is not clear that such omissions would prove problematic in evaluating the responsiveness of teen drinking to state alcohol policies (i.e. generating unbiased estimates of γ) since the determinants of the within-state variation in alcohol policies should be uncorrelated with the state-specific time profiles of such omitted variables. The available

[15] Data on state alcohol policies are from DISCUS (1996a,b). For states with mid-year policy changes, the policy is defined as the one in effect for the longest portion of the calendar year.

[16] For example, it is straightforward to convert these data back to a collection of individual-level observations of binary choices. Using these individual-level data for ordinary least-squares (OLS) estimates of a linear probability model or for maximum-likelihood estimates of a probit or logistic model generates results similar to those reported here. Another option is to transform the dependent variable for weighted least-squares estimation of a log-odds model. However, since a high number of cells contain no drinkers, many observations would have a weight of zero and would implicitly be omitted from the estimations (Cox and Snell, 1989).

evidence does suggest that the within-state variation in these alcohol policies is independently given. For example, the anecdotal evidence mentioned earlier implies that state changes in beer taxes have largely been driven by revenue, not public health, concerns. Similarly, strong Federal compulsion and a growing awareness of the link between alcohol use and traffic fatalities were the key factors that motivated the movement to higher MLDA. Nonetheless, including several additional regressors in Eq. (2) provides some checks for omitted variable bias. These additional variables include state-year measures for family income, parental education and family structure.[17] Other variables included in versions of Eq. (2) control for the unobserved and possibly unique time-series variation in gender- and race-specific drinking behavior by interacting the race and gender indicators with the year fixed effects.

Given that the novel results in these drinking equations are associated with the introduction of state fixed effects, the implications of this specification change deserve careful scrutiny. In particular, a major concern is that the introduction of state fixed effects may remove too much of the sample variation in alcohol policies to allow accurate and meaningful inferences. This is not particularly relevant for the minimum legal drinking age changes which exhibit considerable within-state variation.[18] However, in contrast, a substantial portion of the sample variation in real beer taxes is effectively removed by state fixed effects.[19] A set of basic auxiliary regressions provides an illustrative decomposition. More specifically, using data on the 48 contiguous states over the 1977–92 period, the R^2 from a regression of real state and Federal beer taxes on state fixed effects demonstrates that 63.9% of the sample variation in beer taxes is explained by these controls alone. Furthermore, introducing year fixed effects into this model raises the R^2 to 0.936.[20] This implies that only 6.4% of the sample variation in beer taxes remains unexplained in a basic two-way fixed effects model like Eq. (2). The relatively small amount of unexplained sample variation in beer taxes that remains after the

[17]Six variables were created using pooled cross-sections from the 1977–92 October Current Population Survey (CPS). More specifically, using households with enrolled children between 14 and 17 years-old, state-year measures were constructed for real median family income, the proportions of households where the highest adult education is a high school dropout, high school graduate and has some college and the proportions of households where the head of household or ‘‘reference’’ person is unmarried and widowed, separated or divorced.

[18]In 1977, nearly 60% of the MTF respondents were in states with an MLDA of only 18 but by the late 1980’s, every state had an MLDA of 21.

[19]During this time several states did not change their excise tax on beer at all. For example, of the 44 states represented in the MTF extract, 19 states had beer tax changes over the period that high school seniors from that state were included in the survey. The number of respondents in these 19 states is 133 854. Estimations with this sample will provide an important robustness check.

[20]The shared time-series variation in beer taxes is also important. Over most of the 1977–92 period, inflation led to reductions in the real burden of excise taxes on beer while a Federal increase in 1991 raised its value. It will be important to note for the traffic fatality models presented later that introducing linear state-specific time trends into these estimations raises the R^2 to 0.99.

introduction of state fixed effects suggests that the results of Eq. (2) may be highly imprecise or particularly sensitive to other specification errors. However, the subsequent results and robustness checks suggest that these concerns are not problematic. Nonetheless, the limited beer tax variation in models that include state and year fixed effects underscores an important caveat to the interpretation of the results presented here. In particular, it suggests that policy-makers may have to remain somewhat agnostic about the probable impact of a very large increase in beer taxes. There simply is not sufficiently dramatic within-state variation in beer taxes to allow for a reliable policy simulation of a very large tax increase.

The results of estimating models for teen drinking using all of the available MTF data are reported in Table 2. These models appear to fit the data well and explain from 49 to 74% of the variation in teen drinking behaviors. The coefficients on the demographic variables are not reported for any of these models. However, those estimates consistently demonstrate that older, male and white teens are substantially more likely to drink alcohol. For example, males are nearly 18 percentage points more likely than females to drink heavily. Similarly, non-Hispanic whites are nearly 17 percentage points more likely than black and Hispanic teens to participate in heavy drinking. Additionally, the models reported in Table 2 represent a state's minimum legal drinking age only with a simple binary indicator for whether it has an MLDA of 18. Specifications that include more general binary indicators for minimum legal drinking ages of 19 and 20 demonstrate that their effects are small and statistically indistinguishable from those of an MLDA of 21. In other words, among the high school seniors represented in the MTF data, it was the movement away from an MLDA of 18 that was particularly relevant.[21]

The upper panel in Table 2 reports the key results from estimations that exclude state fixed effects. Like the conventional evaluations in this literature, these empirical models are effectively identified by the cross-state variation in state alcohol policies. As in prior evaluations, these models uniformly suggest that both alcohol tax and drinking age policies have large and statistically significant effects on teen alcohol use. For example, Model (8) suggests that exposure to a minimum legal drinking age of 18 increased the prevalence of heavy drinking by 4.2 percentage points (i.e. 11.4%). Similarly, the tax elasticity of heavy teen drinking implied by the estimate from Model (8) is -0.158 ($-0.117\times[0.496/0.367]$). One immediate indication that this conventional tax elasticity may be inaccurate is its implausibly large magnitude. Since beer taxes constitute only about 10% of the price of beer, these conventional estimates imply that the price elasticity of heavy

[21]This suggests that it was an MLDA of 18 that reached into high schools and influenced alcohol availability among teens. These results raise some doubts about the frequent practice of entering a state's minimum legal drinking age as a simple linear regressor in such models (e.g. Grossman et al., 1987; Ruhm, 1996).

Table 2
Policy determinants of teen drinking: with and without state fixed effects[a]

Independent variables	Dependent variables								
	Drinker			Moderate drinker			Heavy drinker		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Without state fixed effects									
Beer tax	−0.135	−0.170	–	0.024	−0.042	–	−0.092	−0.117	–
	(13.6)	(16.7)		(4.0)	(6.9)		(10.3)	(12.6)	
MLDA of 18	–	0.059	0.034	–	0.031	0.025	–	0.042	0.025
		(11.7)	(6.9)		(10.2)	(8.5)		(9.2)	(5.7)
R^2	0.601	0.615	0.588	0.487	0.500	0.494	0.659	0.667	0.653
With state fixed effects									
Beer tax	0.035	−0.001	–	0.032	0.009	–	0.067	0.036	–
	(1.2)	(0.1)		(1.7)	(0.5)		(2.4)	(1.3)	
MLDA of 18	–	0.036	0.035	–	0.022	0.022	–	0.030	0.031
		(6.5)	(6.6)		(6.2)	(6.4)		(5.8)	(6.2)
R^2	0.714	0.717	0.717	0.575	0.579	0.579	0.737	0.739	0.739
Dependent mean	0.657				0.138			0.367	
(Standard deviation)	(0.160)				(0.084)			(0.157)	

[a]These estimations are based on the responses of 255 560 high school seniors grouped into 3941 observations by state, year, age, race and gender and are weighted by the number of students grouped into each observation. Absolute values of t-statistics are reported in parentheses. All models include binary indicators for race/ethnicity, gender, age and year fixed effects. The mean value of the beer tax variable is 0.497.

drinking participation is -1.58.[22] This means that an increase of only 6% in the price of beer would reduce heavy teen drinking by more than 9%. This sizable marginal effect is larger than the preferred estimate for the effect of the nationwide movement to higher minimum legal drinking ages. Another indication that the typically ignored cross-state heterogeneity is important is that, in all three drinking models, the unobserved state attributes are jointly significant determinants of teen alcohol use.[23]

A more direct commentary on the accuracy of these conventional results comes from the models which include state fixed effects and are reported in the lower panel of Table 2. These results also confirm the conventional wisdom that a state's MLDA had a large and statistically significant influence on all levels of teen drinking. For example, Model (9) indicates that exposure to a minimum legal drinking age of 18 increased the prevalence of heavy teen drinking by 3.1 percentage points (i.e. 8.4%) relative to higher drinking ages. However, like the difference-in-differences results in Table 1, these estimates uniformly suggest that the effect of beer taxes on teen drinking is relatively small and statistically insignificant or has the wrong sign. Nonetheless, as noted earlier, the relatively high degree of collinearity between the state fixed effects and beer taxes raises the possibility that these models may generate imprecise and inaccurate estimates. There are several kinds of evidence to suggest that this concern is not very empirically relevant. First and perhaps most importantly, the loss of precision associated with introducing state fixed effects is in fact relatively small. For example, consider again the heavy drinking equation (Model (8) in Table 2) that includes state fixed effects. Despite the inclusion of state fixed effects, the standard error on the beer tax's coefficient is still sufficiently small to reject the hypothesis that it is equal to the point estimate from the conventional specification that does not include state fixed effects.[24] Similarly, using the 95% confidence interval around this point estimate implies a tax elasticity of no larger than -0.026. This conservatively large elasticity is still less than one-sixth of the conventionally estimated elasticity (-0.158). Second, the inclusion of state fixed effects did more than reduce the precision of the estimated tax responsiveness. It also dramatically

[22] According to the Bureau of Labor Statistics, the 1997 price of an alcoholic malt beverage was roughly \$6.75 per gallon. These prices are 1.48 times their 1982–84 value. This implies that the nominal value of a gallon of beer in the 1982–84 period was around \$4.56. The nominal value of state and Federal excise taxes over that period was \$.46 for the MTF respondents. This implies that excise taxes constituted around 10% of the total price. Therefore, the price elasticity implied by the conventional tax elasticity is -1.58 (0.158/0.10).

[23] For example, using the R^2 from heavy drinking equations (Model (8)), the F-statistic for the hypothesis that the 43 state fixed effects are jointly zero is 25 $(([0.739-0.667]/43)/([1-0.739]/3879))$. Therefore, that hypothesis is rejected. The results for the drinking and moderate drinking models generate similar conclusions.

[24] The t-statistic for this test is 5.5. This hypothesis can also be rejected in the drinking and moderate drinking models.

reduced the magnitudes of these estimates. In fact, in the moderate and heavy drinking models, the sign on these coefficients became positive. The sharp change in the magnitude of these coefficients is not necessarily consistent with the presence of collinearity. However, it is entirely consistent with the hypothesis that there are important and unobserved state-specific attributes that influence both teen drinking and beer taxes.

Another approach for evaluating the impact of the collinearity between the state fixed effects and beer taxes is to replicate the results in Table 2 using only respondents from states that do have within-state sample variation in their nominal beer taxes. Of the 44 states represented in the MTF data, 19 states had changes in their nominal beer tax over periods for which there were also MTF respondents from those states. The key results from estimations with this sample are reported in Table 3. The first row in Table 3 uses this smaller sample to replicate the key results from the upper panel of Table 2. The results of these replications are quite similar to those reported in Table 2. More specifically, in the absence of state fixed effects, both beer tax and drinking age policies appear to have large and statistically significant effects on all levels of teen drinking. The second row in Table 3 reports the results from specifications that add state fixed effects. As in Table 2, the results indicate that minimum legal drinking ages have large and statistically significant effects on all levels of teen drinking.[25] However, also as in

Table 3
Policy determinants of teen drinking: alternative specifications[a]

Model	Dependent variable					
	Drinker		Moderate drinker		Heavy drinker	
	Beer tax	MLDA of 18	Beer tax	MLDA of 18	Beer tax	MLDA of 18
Demographic variables and year fixed effects	−0.181 (9.59)	0.093 (12.10)	−0.035 (3.36)	0.052 (12.02)	−0.122 (7.43)	0.061 (9.24)
Previous model and state fixed effects	−0.023 (0.45)	0.044 (5.71)	0.027 (0.87)	0.027 (5.61)	0.016 (0.35)	0.035 (4.86)
Previous model and state-year covariates	−0.025 (0.48)	0.039 (4.67)	0.005 (0.16)	0.021 (4.00)	−0.012 (0.24)	0.028 (3.56)
Previous model and gender-specific year fixed effects	−0.025 (0.48)	0.039 (4.66)	0.006 (0.19)	0.021 (4.00)	−0.011 (0.22)	0.028 (3.58)
Previous model and race-specific year fixed effects	−0.189 (0.36)	0.040 (4.71)	0.008 (0.26)	0.023 (4.36)	−0.008 (0.16)	0.029 (3.70)

[a]These estimations are based on the responses of 133 854 high school seniors grouped into 1852 observations by state, year, age, race and gender and are weighted by the number of students grouped into each observation. The 19 states represented in this sample have observations both before and after increases in the state excise tax on beer. Absolute values of *t*-statistics are reported in parentheses.

[25]However, the results in both Tables 2 and 3 indicate that specifications that exclude state fixed effects but include beer taxes overstate the impact of minimum legal drinking ages.

Table 2, the results in Table 3 demonstrate that the beer tax elasticities of teen drinking are relatively small and statistically insignificant in specifications that include state fixed effects. Furthermore, despite the sharp drop in sample size, there is still considerable precision in the tax coefficient from models that include these fixed effects. For example, the hypothesis that the tax coefficient from the heavy drinking model in the second row (0.016) equals the tax coefficient from the model in the first row (−0.122) can be rejected.[26] These results suggest that the empirical sensitivity of the conventional beer tax elasticities is not simply due to its collinearity with state fixed effects.

A final indication that the collinearity between the state fixed effects and the beer tax is not problematic is that the results generated by the empirical models that include state and year fixed effects are relatively robust to incremental additions to the set of included regressors. For example, the estimates reported in the third row of Table 3 are based on specifications that include state-year measures of family income, parental education and family structure. Similarly, the results reported in the fourth and fifth rows of Table 3 are based on specifications that respectively include gender-specific and race-specific year fixed effects. In all three of these richer specifications, the estimated tax responsiveness of teen drinking remains relatively small and statistically insignificant. Similarly, these models uniformly indicate that minimum legal drinking ages have a large and statistically significant effect on all levels of teen drinking. The relative stability of these results also provides some evidence that omitted variables that influence the alcohol policies and teen drinking are not a problematic feature of these evaluations.[27]

3. The policy determinants of youth traffic fatalities

The evaluations presented in the preceding section provided direct evidence that the tax responsiveness of teen alcohol use is relatively small and statistically insignificant. However, there has also been considerable indirect evidence that links higher beer taxes with reductions in teen alcohol use. The most notable indirect evidence has taken the form of reduced-form estimations that model teen traffic fatalities as a function of state policies that influence alcohol availability. This literature has uniformly reported that increases in beer taxes are associated with a reduction in youth traffic fatalities (e.g. Ruhm, 1996; Chaloupka et al., 1993; Saffer and Grossman, 1987). For example, using data from the 1975–81 period, Saffer and Grossman (1987) conclude that the elasticity of the motor

[26]The absolute value of the *t*-statistic is roughly 3.0.

[27]Furthermore, given the key finding that the links between beer taxes and teen drinking are small and statistically insignificant, the negative bias imparted by omitted variables like state trends in drinking sentiment would not be very empirically relevant for these tax results.

vehicle death rate with respect to the real beer tax is −0.09 for 15–17 year olds and −0.17 for 18–20 year olds. Using 1982–88 data on traffic fatalities among 18–20 year olds, Ruhm (1996) reports similar results. In particular, in models that include state and year fixed effects, Ruhm (1996) finds that the elasticity of the motor vehicle fatality rate with respect to the real beer tax is −0.17 for this age group.

This evidence that higher beer taxes are associated with fewer youth traffic fatalities even in models that include state and year fixed effects is inconsistent with the direct evidence from the similarly specified drinking equations presented in the previous section. However, there is reason to believe that the empirical models presented by Ruhm (1996) and others overstate the true impact of beer taxes on youth traffic fatalities. As in the drinking equations, one immediate reason to question these results is the implausibly large magnitude of the tax elasticities. For example, the beer tax elasticity of traffic fatality rates reported in these studies for 18–20 year olds (−0.17) implies a price elasticity of roughly −1.7 since beer taxes constitute only about 10% of price. However, only about half of traffic fatalities involve a driver or non-occupant (e.g. pedestrian) with a blood alcohol concentration of 0.01 g/dl or greater. Therefore, the implied price elasticity of youth traffic fatalities with any alcohol involvement is at least −3.4.[28] Another important way to put the implied price elasticity into perspective is to compare its magnitude to that of the widely recognized effect that MLDA changes had on youth traffic fatalities. For example, the evaluations presented here indicate raising the MLDA to 21 reduced traffic fatalities among 18–20 year olds by at least 9%. However, if the conventional traffic fatality models are correct, this implies that a mere 6% increase in the price of beer would reduce youth traffic fatalities by more than the nationwide move to an MLDA of 21 had.

The magnitude of the beer tax elasticities of traffic fatalities reported in previous studies suggests that these empirical models may be misspecified and that the probable source of this misspecification is omitted variables. In characterizing the likely impact of omitted variables on the link between beer taxes and traffic fatalities, it is important to recognize that, even in states that changed their nominal excise taxes, a large portion of the within-state variation in most state's real beer taxes is defined by the shared effects of inflation. In other words, even though 27 of the 48 contiguous states raised their beer taxes over the 1977–92 period, the within-state variation in beer taxes over time is still largely characterized by a declining time-series profile. This implies that traditional fixed-effects specifications could conceivably confound the true effect of beer taxes with any

[28]The implied price elasticity is even higher if we consider fatal accidents involving intoxicated drivers. For the 16–20 age group in 1986, the proportion of intoxicated drivers in fatal crashes was 0.237 (U.S. Department of Transportation, 1997). Therefore, the conventional tax elasticities reported by prior studies imply that the price elasticity of traffic fatalities involving intoxicated drivers is roughly −7.2.

unobserved state-specific trends in traffic fatalities. There are a variety of plausible sources for such unobserved state-specific trends. The 1977–92 period was characterized by important safety innovations as well as by growing concerns regarding various dimensions of traffic safety. For example, indignation over the link between alcohol use and traffic fatalities paralleled a marked increase in grass-roots activity and state and local law making directed at reducing drunk driving Ross (1991). Furthermore, youth traffic fatalities might also have been influenced by other state-specific changes over time in the quality or congestion of roads, driving patterns and by changes in the safety features of predominantly used automobiles.

This section addresses the specification concerns raised here by presenting new empirical models of youth traffic fatalities. These models, which are based on longer panels of state-level data (1977–1992), compare the traditional empirical specifications with some that introduce controls for the unobserved state-specific trends in youth traffic fatalities.[29] Consistent with the drinking equations presented in the previous section, these models uniformly suggest that the move to higher MLDA reduced youth traffic fatalities. However, these models also demonstrate that the relationship between beer taxes and youth traffic fatalities is not a robust one. In particular, the conventional links between beer taxes and youth traffic fatalities prove sensitive to the introduction of controls for state-specific time trends. However, since the introduction of these trend variables removes most of the remaining sample variation in beer taxes, the implications of this sensitivity are somewhat uncertain.[30] Therefore, perhaps the most intriguing commentary on the prior youth traffic fatality models and on the importance of the state-specific time trends in these models comes from a straightforward counterfactual based on comparing models of daytime and nighttime traffic fatalities. The evidence from this counterfactual underscores the importance of controlling for state-specific trends and raises considerable skepticism regarding the conventional links between youth traffic fatalities and beer taxes.

3.1. Fatal Accident Reporting System (FARS)

The National Highway Traffic Safety Administration (NHTSA) maintains a census of all motor vehicle traffic accidents that involve a fatality. The data from this Fatal Accident Reporting System (FARS) include detailed information on the characteristics of the involved vehicles, drivers, occupants and non-occupants. The

[29] A virtue of introducing state-specific trend variables into the traffic fatality models is that they can detect the presence of important, unobserved and state-specific time-series variation even though the source of this variation may be difficult to identify or measure. Introducing such trend variables into the drinking equations of the previous section does not substantively alter the results. In particular, the effect of an MLDA of 18 in those equations is robust to including state-specific trends.

[30] Nonetheless, similar results are obtained in estimations that are limited to states that changed their beer taxes.

prior research on youth traffic fatalities has used specific mortality counts from FARS along with age-specific population estimates from the Census Bureau to construct a variety of vehicle fatality rates by state and year. This study is based on similarly constructed panel data on several vehicle fatality rates in the 48 contiguous states over the 16-year period from 1977 to 1992. In particular, total and driver vehicle fatality rates were constructed for the 18–20 year old group. These data indicate that, on average over this period, there were roughly 45 traffic fatalities per year (28 of them drivers) among every 100 000 18–20 year old youths.[31]

The remaining fatality rates presented in this study were based on the hour of the accident. It is well established that the rate of alcohol involvement in fatal accidents is substantially higher during the nighttime. For example, data from the 1982–92 FARS indicate that, between midnight and 4:59 AM, the rate of alcohol involvement is between 68 and 75% among 18–20 year olds' driver fatalities. In contrast, between 7 AM and 2:59 PM, this rate is only between 10 and 20%.[32] Because of this striking pattern, several studies have also focused on nighttime fatality rates (e.g. Chaloupka et al., 1993). This study also includes a nighttime fatality rate based on the traffic fatalities among 18–20 year olds that occur between midnight and 4:59 PM. On average, 15.9 of every 100 000 18–20 year olds die annually in a traffic accident during this period. A fatality rate for the daytime accidents of 18–20 year olds (i.e. those that occur between 7 AM and 2:59 PM) is also considered here. On average, 6.7 of every 100 000 18–20 year olds die in a traffic accident during this period. The motivation for considering this more novel daytime measure is that it will provide an important and compelling counterfactual for evaluating the plausibility of the traditional empirical links between alcohol policies and traffic fatalities.

3.2. Youth motor vehicle fatalities

The specification employed for the traffic fatality models presented here parallels those used in prior studies. More specifically, the prior research has

[31]These means are population-weighted. I would like to thank Frank Chaloupka for confirming that there is an inconsistency in the youth fatality rates reported in a prior article. Chaloupka et al.'s (1993) *Journal of Legal Studies* paper (page 185) states that the population-weighted *driver* fatality rate for the 18–20 age group is 54.5 per 100 000 in that age-specific population. Using data from the same period, Ruhm (1996) reports a population-weighted fatality rate of 42.9 for *all* 18–20 year olds. The author's calculations suggest that Ruhm's mean value is accurate and that Chaloupka et al.'s is overstated by a factor of roughly 2. In a private correspondence, Frank Chaloupka confirmed that the driver fatality rate in that article was actually a total fatality rate and that remaining differences with other published numbers may simply be due to revisions in the Census' Bureau's population estimates and to whether non-occupants (i.e. pedestrians) are included in other counts.

[32]Prior to 1982, the definition of alcohol involvement in FARS differed significantly. This rate of alcohol involvement is police-reported and excludes drivers for whom alcohol involvement was not determined. However, this variable is consistent with alcohol tests given and subsequent charges made.

emphasized that the state-year traffic fatality rates are grouped data generated by a binary process (i.e. whether an individual perishes in a traffic accident in a given year). A functional form that explicitly recognizes this is based on a logistic transformation of the traffic fatality rates (Berkson, 1953). This transformation converts the fatality rates, P_{st}, into the natural logarithm of the odds ratio, $F_{st} = \ln(P_{st}/(1-P_{st}))$, which is then used as the dependent variable in a least-squares estimation. More specifically, this procedure calls for estimating an equation of the following form:

$$F_{st} = W_{st}\Pi + M_{st}\gamma + u_s + v_t + \epsilon_{st} \tag{3}$$

where W_{st} is a vector of state-year attributes that influence traffic fatalities, M_{st} represents the key alcohol policies in a given year, u_s is a state-specific effect, v_t is a year-specific effect and ϵ_{st} is a mean-zero random error. Because Eq. (3) has an explicit heteroscedasticity, efficient estimates are generated by weighted least squares where the weight is the inverse of the estimated asymptotic variance of ϵ_{st}. More specifically, the weight is $n_{st}(P_{st})(1-P_{st})$ where n_{st} is the age-specific population for the fatality rate in question (Cox and Snell, 1989; Maddala, 1983). The point elasticities implied by the parameter estimates in Eq. (3) can be calculated as the product of the parameter estimate, $(1-P_{st})$ and a value for the independent variable under consideration.[33]

Unfortunately, the logistic transform in Eq. (3) is not defined when P_{st} has a value of 0 or 1. Some minor modifications of this transform have been recommended (Cox and Snell, 1989). However, even after such modifications, observations with a P_{st} of 0 or 1 would still make no contribution to a weighted least-squares estimation since their weights would be zero. This is not a concern for the total and driver fatality rates presented here since their values are between 0 and 1 for all 768 state-year cells. However, in some low-population states, the nighttime and daytime fatality rates have a small number of state-year cells with a value of zero. Rather than implicitly exclude only those state-year cells with a value of zero, the subsequent estimations of a particular fatality model exclude any state that has zero fatalities in any year over the 16-year period. For example, the nighttime fatality models reported here exclude Delaware, North Dakota, Rhode Island and Vermont ($n = 704$). The daytime fatality models reported here exclude those four states as well as South Dakota and Maine ($n = 672$). Linear probability models that are based on all 768 state-year observations returns results similar to those based on these smaller panels of states and the specification in Eq. (3). Therefore, these minor adjustments to the data set appear to have no consequences

[33] As in prior studies, the tax elasticity discussed here are defined for the mean value of the tax variable. Since the probability of dying in a traffic accident in a given year is rather low and since the mean value of the real beer tax is 50 cents in this sample, the relevant tax elasticities are roughly equal to one-half of the reported parameter estimates. Similarly, the coefficients on the binary MLDA indicators are roughly equal to the percentage reduction in fatalities associated with that variable.

for the novel results presented here but do provide continuity with the prior research by facilitating the use of the same functional form.

Like the difference-in-differences estimates presented in Table 1, the parameters of interest in Eq. (3), γ, are identified by the within-state variation in traffic fatalities and alcohol policies over time. The term, W_{st}, in Eq. (2) introduces controls for the other determinants of youth traffic fatalities that also vary within state over time. For example, these specifications include two measures for the state level of macroeconomic activity: the state unemployment rate and real state personal income per capita (Evans and Graham, 1988). Additionally, a binary indicator is also included for whether a state had a mandatory seat belt law. A lap and shoulder seat belt can reduce the risk of dying in a crash by 50% (Evans, 1986). Over this period, nearly every state adopted a mandatory seat belt law and these laws led to a sharp increase in belt usage whose magnitude was only slightly more modest among the young and those who drink heavily (Dee, 1998). The unobserved determinants that may have influenced a state's youth traffic fatalities over time are represented in some models by state-specific trend variables.

However, the introduction of state-specific trend variables raises additional questions. In particular, the most pressing concern with the introduction of state-specific trend variables is they remove most of the already limited sample variation in beer taxes.[34] However, as in the drinking equations, the implications of this high degree of collinearity can be addressed through evaluating across specifications the magnitude and precision of the key parameter estimates. Furthermore, as in the drinking equations, limiting the traffic fatality models to the states that changed their beer taxes over this period provides additional evidence on the importance of this collinearity. Nonetheless, the limited unexplained variation in beer taxes is a particularly important caveat to the inferences from models that include state-specific trend variables. It is because of this uncertainty, that the counterfactual based on daytime traffic fatalities is especially important. It provides direct evidence on the possible importance of unobserved state-specific trends in traffic fatalities and the relationship of these omitted variables to the state-specific variation in beer taxes without removing so much of sample variation.

The results of estimating Eq. (3) for the total and driver traffic fatality rates of 18–20 year old youths are reported in Table 4. The specification in Model (1), which excludes state fixed effects, suggests that there are small but plausibly signed and statistically significant beer tax effects. However, the estimated effects of minimum legal drinking ages are either implausibly signed or statistically insignificant in both of these estimations. Model (2) adds state fixed effects to the specification in Model (1). The reported p-value indicates that the unobserved and

[34]The auxiliary regressions discussed earlier demonstrate that the introduction of state-specific time trends into models that already include state and year fixed effects leaves only 1% of the sample variation in beer taxes unexplained.

Table 4
WLS estimates of traffic fatality equations, 18–20 year-olds, 1977–92 FARS[a]

Independent variables	Model (1)	Model (2)	Model (3)
	Year fixed effects	Model (1) and state fixed effects	Model (2) and state-specific trends
Total Fatalities			
Beer tax	−0.136	−0.649	0.351
	(2.63)	(7.22)	(1.66)
MLDA of 19	0.085	−0.015	−0.022
	(3.27)	(0.82)	(1.06)
MLDA of 20	−0.054	0.008	−0.009
	(1.03)	(0.22)	(0.22)
MLDA of 21	0.035	−0.090	−0.110
	(1.41)	(4.39)	(3.98)
R^2	0.502	0.843	0.881
p-value	–	0.0001	0.0001
Driver fatalities			
Beer tax	−0.144	−0.762	0.378
	(2.65)	(7.31)	(1.51)
MLDA of 19	0.099	−0.001	−0.003
	(3.66)	(0.06)	(0.14)
MLDA of 20	−0.037	0.022	0.007
	(0.68)	(0.49)	(0.16)
MLDA of 21	0.059	−0.081	−0.111
	(2.29)	(3.43)	(3.37)
R^2	0.504	0.809	0.846
p-value	–	0.0001	0.0001

[a]The dependent variable is the natural logarithm of $P_{st}/(1-P_{st})$ where P_{st} is the fatality rate in question for state s at time t. These estimations are weighted by the $n(P_{st})(1-P_{st})$ where n_{st} is the relevant age-specific population in state s at time t. All models include the two macroeconomic variables and a binary indicator for a mandatory seat belt law. The reported p-values refer to F-tests for the joint significance of the controls added in Models (2) and (3). Absolute values of t-statistics are reported in parentheses. The average population-weighted total and driver fatality rates (standard deviations) per 100 000 in the age-specific population are 44.9 (13.3) and 27.1 (8.4) respectively.

time-invariant state characteristics represented by the fixed effects are jointly significant determinants of youth traffic fatalities. The estimated coefficients from Model (2) also indicate that the movement to a minimum legal drinking age of 21 generated large and statistically significant reductions in total and driver traffic fatalities among 18–20 year old youths. For example, the estimates from the upper panel of Table 4 suggest that the movement to an MLDA of 21 reduced total youth traffic fatalities by roughly 9%. These findings prove robust to the inclusion of state-specific trend variables. In fact, the results from Model (3) suggest that increasing the minimum legal drinking age to 21 had a somewhat larger effect (roughly 11%) than that suggested by Model (2).

Like prior evaluations in this literature, the estimated coefficients from Model

(2) also suggest that beer taxes have large and statistically significant effects on the total and driver traffic fatalities among 18–20 year old youths. For example, these estimates suggest that the elasticity of total youth traffic fatalities with respect to the real beer tax is approximately -0.32 (i.e. roughly one-half the reported coefficient). This estimated elasticity is larger than those based on shorter but similarly specified panels (e.g. Ruhm, 1996). However, the results from Model (3) demonstrate that such conventionally estimated tax coefficients are not robust to the inclusion of state-specific trend variables.[35] More specifically, the tax coefficients from Model (3) have a positive sign and are statistically insignificant. Furthermore, the reported *p*-values in Table 4 also indicate that state-specific trend variables included in Model (3) are jointly significant determinants of youth traffic fatalities. This same sensitivity is also evident in models of traffic fatalities among younger teens, which are not reported here. For example, in empirical models of total and driver traffic fatalities among 15–17 year olds, the specification in Model (2) suggests that beer taxes have large and statistically significant effects. However, models that condition on unobserved, state-specific trends suggest that beer taxes have relatively small and statistically insignificant effects.[36]

The results in Table 4 indicate that the movement to higher MLDA substantially reduced youth traffic fatalities. However, those results also demonstrate that the implausibly large, conventional empirical links between beer taxes and youth traffic fatalities are not robust to controls for important omitted variables. These results are consistent with the direct evidence from the drinking equations presented in the previous section, which found that beer taxes had relatively small and statistically insignificant effects on teen drinking. However, as already noted, the introduction of state-specific trends removes nearly all of the available sample variation in beer taxes. Therefore, despite the joint significance of these trend variables, it is by no means clear that the sensitivity of the beer tax estimates implies that the conventional elasticities are inaccurate. Nonetheless, despite the sharply limited sample variation, it is interesting to note that the estimated tax coefficients reported in Table 4 are sufficiently precise to reject the conventional estimates.[37] Furthermore, empirical models that are limited to the 27 states that changed their beer taxes over this period generate results similar to those reported in Table 4.[38]

[35] Young and Likens (1998) report similar results based on two-way fixed effects models that do not include state-specific trend variables but do include a larger set of state-year covariates.

[36] These results are available upon request. One surprising finding is that the estimated effects of minimum legal drinking ages are relatively weak or inconsistent among these younger teens. Cook and Tauchen (1984); Saffer and Grossman (1987) discuss similar results for such younger teens.

[37] For example, the hypothesis that the estimated tax coefficient in the total fatality equation (Model (3)) equals the estimate from Model Eq. (2) can be rejected. The absolute value of the *t*-statistic is roughly 4.7.

[38] But even in these models, only about 2% of the sample variation in beer taxes is unexplained by the introduction of state fixed effects, year fixed effects and state-specific trend variables.

3.3. A counterfactual: daytime traffic fatalities

The results presented in Table 4 provide suggestive but far from definitive evidence that the conventionally reported tax responsiveness of youth traffic fatalities is inaccurate. A straightforward counterfactual based on comparing empirical models of daytime and nighttime traffic fatalities can provide more convincing and direct evidence on the plausibility of the conventional estimates as well as on the importance of controlling for unobserved state-specific trends. As noted earlier, the rate of alcohol involvement in the nighttime fatality rate employed here is 3.4 to 7.5 times higher than the daytime rate. This divergence provides an intriguing opportunity to evaluate the traditional empirical links between alcohol policies and traffic fatalities. More specifically, if those links were credible, we would expect to find them sharply attenuated in empirical models of daytime fatalities. However, if the traditional links between alcohol policies and traffic fatalities were fairly robust across models of daytime and nighttime fatalities, it would underscore the presence of important omitted variables that covary with those policies.[39] In particular, to the extent that the beer taxes appear to have a relatively substantial influence in conventionally specified models of daytime youth fatalities, we can conclude that the traditional empirical approach is misspecified.

The key results of estimating models of daytime and nighttime fatalities are reported in Table 5. The estimated effects of minimum legal drinking ages in these models indicate that this counterfactual is functioning correctly. The models in the top panel of Table 5 demonstrate that the movement to a minimum legal drinking age of 21 had a robust and statistically significant impact on nighttime traffic fatalities among 18–20 year olds. For example, Model (2) suggests that an MLDA of 21 reduced these fatalities by roughly 12.4%. However, in models of daytime traffic fatalities, the estimated effect of an MLDA of 21 is smaller in magnitude and only weakly significant. Since alcohol involvement is substantially lower in such daytime fatalities, this pattern is what would be expected if the movement to an MLDA of 21 actually reduced teen alcohol use and traffic fatalities.

However, the estimated beer tax elasticities in models of daytime and nighttime traffic fatalities follow a different pattern. The beer tax elasticity of nighttime traffic fatalities implied by Model (2) is a statistically significant 38%, which is somewhat larger than the similarly estimated tax elasticities from the total and driver fatality models (Table 4). However, the veracity of all of the conventional tax elasticities based on this specification is called into question by the results of

[39] It is possible that there is heterogeneity in the tax-sensitivity of youths that are involved in accidents at different times of day. However, this heterogeneity is likely to add further force to this counterfactual. For example, we might expect those involved in nighttime accidents to be the tax-sensitive social drinkers and those involved in alcohol-related daytime accidents to be relatively tax-insensitive abusers of alcohol.

Table 5
WLS estimates of daytime and nighttime traffic fatality equations, 18–20 year-olds, 1977–92 FARS[a]

Independent variables	Model (1)	Model (2)	Model (3)
	Year fixed effects	Model (1) and state fixed effects	Model (2) and state-specific trends
Nighttime fatalities			
Beer tax	−0.363	−0.764	0.577
	(5.70)	(4.99)	(1.56)
MLDA of 19	0.035	−0.012	−0.039
	(1.15)	(0.42)	(1.15)
MLDA of 20	−0.036	−0.019	−0.073
	(0.58)	(0.30)	(1.13)
MLDA of 21	−0.153	−0.124	−0.172
	(5.21)	(3.70)	(3.75)
R^2	0.490	0.709	0.761
p-value	–	0.0001	0.0001
Daytime fatalities			
Beer tax	−0.104	−0.557	−0.241
	(1.12)	(2.86)	(0.51)
MLDA of 19	0.150	0.002	−0.010
	(3.09)	(0.05)	(0.20)
MLDA of 20	−0.134	0.011	0.044
	(1.10)	(0.11)	(0.42)
MLDA of 21	0.227	−0.054	−0.089
	(4.93)	(1.20)	(1.32)
R^2	0.288	0.659	0.706
p-value	–	0.0001	0.0001

[a]The dependent variable is the natural logarithm of $P_{st}/(1-P_{st})$ where P_{st} is the fatality rate in question for state s at time t. These estimations are weighted by the $n(P_{st})(1-P_{st})$ where n_{st} is the relevant age-specific population in state s at time t. All models include the two macroeconomic variables and a binary indicator for a mandatory seat belt law. The reported p-values refer to F-tests for the joint significance of the controls added in Models (2) and (3). Absolute values of t-statistics are reported in parentheses. Nighttime fatalities occur between 12:00 AM and 4:59 AM. Daytime fatalities occur between 7:00 AM and 2:59 PM. The average population-weighted nighttime and daytime fatality rates (standard deviations) per 100 000 in the age-specific population are 15.9 (5.5) and 6.7 (2.8) respectively.

the daytime traffic fatality model. The conventional specification in Model (2) implies that the tax elasticity of daytime traffic fatalities is a statistically significant 27.9%. Since the rate of alcohol involvement in daytime fatalities is substantially lower than in the nighttime, the relative robustness of this tax elasticity cannot be due to the effect that beer taxes might have on traffic fatalities. Instead, like the models presented in Table 4, this counterfactual suggests the existence of important omitted variables that bias the traditional estimates of the beer tax elasticities of youth traffic fatalities. As in Table 4, this interpretation is also supported by the sensitivity of the beer tax coefficients to the inclusion of state-specific trend variables (Model (3), Table 5). Empirical models that control for state-specific time trends suggest that beer taxes have a small and statistically insignificant effect on youth traffic fatalities.[40] However, the particular importance of this counterfactual is that it confirms, without introducing highly collinear controls for unobserved variables, that the conventional link between beer taxes and youth traffic fatalities should be viewed with skepticism.

4. Conclusions

The abuse of alcohol by young adults in the United States is justifiably a continued source of concern to parents and policy-makers. A recent body of research has suggested that there is an unexploited opportunity to generate dramatic reductions in the prevalence of teen drinking and its related consequences through increases in beer taxes. However, the direct evidence that teen drinking is highly tax-elastic has been based on empirical specifications that were identified solely by the cross-state variation in beer taxes. This study presented new estimates of the policy responsiveness of teen drinking that were based on the 1977–92 Monitoring the Future surveys. Unlike most prior studies, these estimates conditioned on unobserved state-specific attributes that influence a state's teens as well as its alcohol policies. Estimates based on these specifications demonstrate that beer taxes have a relatively small and statistically insignificant impact on teen drinking. The evidence linking alcohol policies and youth traffic fatalities was also reconsidered in this study. The empirical models of traffic fatalities presented here, which were based on a longer panel of state-level data, demonstrated that the conventional link between beer taxes and youth traffic fatalities is not a robust one. In particular, the implausibility of the traditionally estimated tax elasticity of youth traffic fatalities was underscored by the fact that such an elasticity also appeared in an empirical model of daytime traffic fatalities even though a substantially smaller proportion of fatal accidents that occur during the daytime involve any alcohol.

In sum, the direct evidence from teen drinking equations and the indirect evidence from youth traffic fatality models suggest we should view with skepticism the claim that increased beer taxes will substantially reduce teen drinking and its associated consequences. However, because the controls introduced in this study removed much of the sample variation in beer taxes, the probable impact of the kind of large tax increases typically modeled in policy simulations should instead be viewed as uncertain. In sharp contrast to these tax results, the evidence from both the drinking equations and the traffic fatality models consistently demonstrate that the movement to a minimum legal drinking age of 21 substantially reduced abusive teen drinking. More specifically, the

[40] Furthermore, the trend variables included in Model (3) are jointly significant determinants of both daytime and nighttime fatalities.

estimates presented here suggest that the movement to higher MLDA reduced heavy teen drinking by at least 8% and traffic fatalities by at least 9%. The success of higher minimum legal drinking ages in reducing the prevalence and social consequences of teen drinking suggests that current and future efforts to curb teen alcohol use should recognize the probable efficacy of policies that further increase the non-pecuniary cost to teens of acquiring alcohol.

Acknowledgements

I owe a particular debt of gratitude to William N. Evans for helpful discussions regarding this study. I would also like to thank Paul Harrison, James Poterba and an anonymous referee for insightful comments on earlier drafts and Patrick O'Malley for his assistance with the MTF data. The usual caveats apply.

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