Minimum Wages and Alcohol-Related Traffic Fatalities among Teens

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Abstract

Using cross-state variation in minimum wages, we observe a positive relationship between the minimum wage and the number of alcohol-related accidents involving teen drivers. A similar effect is not observed when examining accidents among adults. The results are consistent with a high income elasticity for alcoholic beverages among young people, in particular for consumption out of discretionary income accorded by higher minimum wages. Evidence of a sizeable impact of beer taxes on alcohol-related accidents among youths suggests that beer taxes are one avenue for policy makers to consider in counteracting the unintended consequences of minimum wages for drunk-driving fatalities.

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“Giving money and power to government is like giving whiskey and car keys to teenage boys.”
P.J. O’Rourke, *Parliament of Whores*

1. Introduction

Motor vehicle crashes are the leading cause of death for 16-20 year olds in the U.S.; nearly one-third of these crashes are alcohol related (National Highway Traffic Safety Administration, 2008). Although all states (and D.C.) have laws prohibiting the purchase and public possession of alcoholic beverages by individuals under the age of 21, survey evidence reveals that more than 20 percent of young people (16-20 year olds) have driven under the influence of alcohol (Substance Abuse and Mental Health Services Administration, 2004). This is a particularly troublesome statistic, given the relative inexperience of young drivers that leaves them more prone to traffic accidents. In fact, the fraction of fatal motor vehicle accidents that involve alcohol (30 percent) is essentially identical between teenage drivers and drivers over the age of 25. Minimum drinking-age laws at best only partially deter the additional risks posed by inebriated drivers under the age of 21, so that alcohol-related fatal accidents caused by young people remain a serious social problem.

Understanding the various ways in which public policy might affect the incidence of underage drinking and driving is an important undertaking. Research should address the efficacy of policies that intended to lessen teenage drinking and driving, but it should also consider unintended consequences of policies that may affect the prevalence of this activity. In this paper, we present evidence on one such policy that may have potential unanticipated consequences on teen drinking -- increasing the minimum wage. By increasing the disposable income of teenagers, minimum wage increases appear to raise the consumption of alcohol by teenage drivers and thus lead to a further increase in observed traffic fatalities for this group. Using
information on cross-state variation in minimum wages during the 1998-2006 period, our estimates suggest that a 10 percent increase in the minimum wage will on average increase the incidence of fatal accidents involving drivers ages 16-20 by between 5 and 10 percent. We also show that beer taxes have an important impact on youth drinking and driving behavior.

2. Background and Theoretical Considerations

The basic theoretical premise behind our empirical modeling of alcohol-related traffic fatalities is that higher pay for teenage workers increases their consumer choice possibilities in a way that makes higher expenditures on both driving and drinking considerably more likely and more common. We do not expect older individuals to exhibit a similar response to increases in their pay. Teenagers are much more likely to be financially dependent on others (primarily their parents) for payment on the usual necessities (housing and food). A substantial increase in earnings that a teenager might enjoy as a result of a minimum wage increase, then, can often be committed to discretionary non-necessities that parents may not fund, such as music, games, cigarettes, gasoline, and alcohol. A survey conducted in 2003 on young teenagers (those 12-17) found that indeed a relatively small increase in a teenager’s income (by $25 per week) leads to roughly doubling the incidence of drinking, and of getting drunk, among individuals in this age group (National Center on Addiction and Substance Abuse, 2003). Hashimoto (1987) pointed to statistically significant evidence that increases in the minimum wage raised the probability of arrests for drug abuse among teens, while no such evidence was available among 20-24 year-olds. Addison, Blackburn, and Cotti (2009) report evidence consistent with higher minimum wages increasing the demand for alcoholic beverages, as employment in alcoholic beverage

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1 A similar pattern of results was found in examining minimum wage effects on the probability of arrests for driving under the influence, and for drunkenness, but these results were not statistically significant.
stores appears to be positively correlated with minimum wages. For the most part, however, the connection between teenagers’ income and their drinking choices has gone largely unexplored in the economics literature.  

A more commonly-examined policy impact is the effect of beer taxes on teen drinking and driving. Several previous studies have examined the impact of the beer tax in a state on traffic fatalities among teens, generally finding a very large effect lowering fatalities when the tax is raised (Ruhm, 1996; Chaloupka et al., 2002). This finding is corroborated by evidence that suggests that raising beer taxes substantially deters self-reported drinking among teens (Cook and Moore, 2001).  

Also of relevance are the several studies that have found that teenagers have a much higher price elasticity of demand than the typical adult population for cigarettes (see Gallet and List, 2003; Farrelly et al., 2001). As beer and cigarettes are likely to be comparable products in the choice set for youths, it would seem likely that younger people will also respond more strongly to changes in alcohol taxes than older individuals.  

Standard theory predicts that a minimum wage increase should lead to earnings gains for those who remain employed following the increase, but generally has no clear predictions on how these effects will be distributed across the population. One thing that is certain, however, is teenagers make up a large percentage of the minimum wage workforce and therefore stand much to gain as a group from increases in the minimum wage. As an illustration of this fact, we 

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2 Cook and Moore (2001) do include wage and salary income as a control variable in some of their models of drinking behavior among young individuals in the 1979 National Longitudinal Survey Youth cohort, and find weak evidence of an effect where income increase the probability of drinking. However, this measure is not robust to estimation of their models separately by gender. Also, their analysis includes both teens and individuals in their 20s, and some measures are reported at a time when minimum drinking ages were 18 in many states. Other studies have included measure of per capita income in the state as a control in models of teen drinking, but have not attempted to isolate the impact of changes in incomes for teens.  

3 Dee (1999) has criticized the evidence for both connections as lacking robustness.
constructed measures of the prevalence of low-wage employment using data from the Current Population Survey Outgoing Rotation Group samples for two years (1998 and 2006) that bookend the years of study in this paper. Table 1 presents these measures by age-group. In 1998, 16 percent of 16-20 year olds worked at or below the effective minimum wage in their state of residence, while 64 percent were no more than $2 above their minimum (a reasonable range when considering those potentially impacted by minimum wage increases). For adults over the age of 25, these same measures were only 2 percent and 10 percent, respectively. A similar pattern in statistics was observed for workers in 2006, although every age group had lower percentages of earners at or near the minimum wage in 2006 compared with 1998.

Although several states had increased their minimum wages over this period, the constancy in the nominal value of the federal minimum wage over this period is likely behind these declines, as the average value of the minimum wage declined both in real terms and relative to the average wage in the economy as a whole.

Although minimum wage workers who remain employed after a minimum wage increase will get a potentially sizable increase in earnings, these earnings increases may be offset by employment losses, leaving the aggregate impact on teenagers uncertain. The literature on the employment effects of minimum wages is substantial, but has hardly reached a consensus over the size or even the existence of potential disemployment effects (for a recent comprehensive review, see Neumark and Wascher, 2008). Studies that use an industry-specific focus in identifying minimum wage effects (most notably, studies of the fast food restaurant sector) have often found no evidence of a disemployment effect, although these studies have not gone unchallenged (for example, see Card and Krueger, 1994, and the comment of Neumark and Wascher, 2000). An alternative strand of the minimum-wage/employment literature appeals to
cross-state variation in employment for specific demographic groups, and has tended to find effects that are on the negative side (although the magnitude does vary). For example, Zavodny (2000) finds effects on the employment of teenagers that are small and negative, and that are not robust to the empirical approach for identifying minimum-wage effects. On the other hand, Sabia (2009) analyzes data on the employment of teenagers from 1979-2004 and finds that a 10 percent increase in the minimum wage reduces employment among teenagers by 2 to 3 percent.

For our purposes, we want to understand to what extent a minimum-wage increase puts more money into the hands of teenagers. Of course, if the disemployment effect from minimum wages is larger in percentage terms than the impact on average earnings of teenagers, there will be no net increase in income at the disposal of teenagers. The range of employment elasticities from the minimum wage literature, however, does not suggest that a negative employment effect will dominate the extra earnings effect. That said, the literature is far short of a consensus and provides a wide range of earnings and employments estimates to consider. Sabia (2009) provides some of the largest employment estimates in the literature, with an estimated minimum wage elasticity for teen employment of -0.33. However, his estimated elasticity of the impact on teen wages is 0.32, suggesting no addition to earnings would occur for the teen population as a whole. Of course, this is an extreme possibility, given the large assumed employment elasticity. If estimated employment elasticities instead are closer to zero (as is often found in the literature), the impact of minimum wages on teen earnings could be substantial. We illustrate this in Table 2 through a back-of-the-envelope calculation. Assuming a $1.00 increase in a starting minimum wage of $5.50 (an 18 percent increase) for a state with 400,000 teenagers (the average from our sample), $131,000 extra per hour (across all teenagers working) would accrue to the states’ teenage population. Assuming a 20-hour work week, this would create an aggregate increase of
$2.62 million per week to teenagers in a typical state following a $1.00 increase in the minimum wage.

Table 2 demonstrates that average earnings for teenagers could grow considerably following minimum wage increases, in particular if employment losses are minor. Even in the case where disemployment effects are more important, however, minimum wages should still affect the distribution of income among teenagers in a way that will increase the likelihood of increased expenditures on goods like alcohol. A substantial number of teenagers will receive an 18 percent increase in pay, as the pay on their jobs increases by $1 per hour (or $20 per week). If the marginal propensity to consume alcohol is particularly high for teenagers, this group may be the ones most likely affected in their consumption patterns. And in any of the calculations, this group is considerably larger than the group who lose employment and income. The size of the group that might increase its risky behavior as a result of the minimum wage increase should be substantially larger than the group that might lessen such behavior in the face of a loss in income. As a result, we find it quite plausible that increases in minimum wages lead to significant increases in alcohol-related driving fatalities among teens.

As noted earlier, the consensus of research has shown that teenagers are more price sensitive in their consumption of cigarettes, and it would seem reasonable to suggest that a similar difference in price sensitivity in alcoholic beverages (and in particular beer) would be observed between teenagers and adults. These heightened elasticities may at least in part be due to the fact that a consumer’s degree of price sensitivity tends to be a function of the cost share of the product in the consumer’s total budget. The income effect from higher prices on beer is likely to be considerably more important for teenagers, to the extent that alcoholic beverages make up a substantial share of teenagers’ expenditures out of their disposable income. The
relationship between beer taxes and automobile accidents among teenagers has been studied in a number of previous studies, though we are unaware of any attempt to focus on alcohol-related accidents in particular, or to draw the contrast between teenagers and adults in their reaction to higher taxes on beer.

Our paper estimates how minimum wages and beer taxes affect driving fatalities involving underage drivers. These two policies differ across states, and often change over time, allowing us to use this variation across states to identify the effects of these policies. By putting more disposable income in the hands of teenagers, a higher minimum wage in a state should increase that state’s rate of accidents among teenagers. On the other hand, higher beer taxes will increase the price of beer and should lead to a lower rate of drunk driving fatalities among younger people. We also anticipate that both of these policies will have small, if any, effects among adults, and so we also estimate the impact of these policies on alcohol-related driving fatalities for an older population.

3. Data and Methods

3.1. Data sources

Our empirical analysis uses data from 1998-2006 to examine the impact of government policies (and other factors) on driving fatalities at the state level. As such, our policy instruments are measured by state for each year. From 1998 to 2006, there were no legislated changes in the federal minimum wage (which was $5.15), though there was an abundance of changes in minimum wages for individual states. As is common in the literature on minimum wages, our measure of the minimum wage is the enforced minimum wage in the relevant state – the higher
of the state minimum wage (if one exists) and the federal minimum wage.\textsuperscript{4} During our sample time period, 21 states had a minimum wage that was above the federal standard for at least some period of time, with the state of Washington having the highest nominal minimum wage level of $7.63 in 2006. We merge this information with data on the level of excise taxes for beer, which range from as low as $0.02 to as much as $1.07 per gallon.\textsuperscript{5} The focus on beer taxes is common in the literature, based on the observation that beer makes up a much larger share of teenagers’ alcoholic consumption than wine and liquor.

Data on fatal vehicle crashes are obtained through the Fatality Analysis Reporting System (FARS) of the National Highway Traffic Safety Administration (NHTSA). Our primary variable of interest is the annual number of fatal accidents in a state for which a driver’s blood alcohol content (BAC) was greater than zero and at least one of the drivers was 16-20 years old.\textsuperscript{6} We also estimate effects of minimum wages and beer taxes on fatal accidents involving drivers over 26 years of age. This will allow for comparisons across age groups and reveal the underlying trends in accident data. The 16-20 year old age group (which we often refer to as teenage or

\textsuperscript{4} Information on state minimum wage changes are reported in the January editions of the \textit{Monthly Labor Review}. In the event the minimum wage is passed in the middle of a year, the new and old minimum wage levels are an average based on the number of months of the year each was in effect.

\textsuperscript{5} This information is maintained on the Federation of Tax Administrators website.

\textsuperscript{6} Despite Federal mandates, BAC levels are not always taken at crash scenes, yielding inaccurate data on the role of alcohol in accidents. Because of this, the NHTSA has developed a procedure that allows for the imputation of the BAC for all drivers who were not tested in a crash. This is done via a multiple imputation procedure yielding ten different simulated BAC measures for each driver in every accident. These values are obtained using factors such as time of day, day of week, contents of the police report, position of car in the road, etc. (NHTSA, 2002). This follows suggestions from Rubin et al. (1998), improving on the former procedure based on discriminant analysis (Klein, 1986; NHTSA, 2002), and is the one used by NHTSA in their official statistics. While previous studies using counts generated from older FARS data used imputed values based on discriminant analysis, or relied on counts generated from accidents that were more likely to be alcohol-related (for example, crashes on weekend evenings), more recent studies have used data generated by the new procedure (for example, Hingson et al., 2005; Cummings et al., 2006; Adams and Cotti, 2008). One must still be aware that estimated effects of policies may be biased if missing BAC information is systematically related to the policy in question, such as changes in the BAC requirement. There is little concern that this is the case here, as minimum-wage level or beer-tax rates should not affect how officers investigate a crash scene.
underage drivers) was chosen to focus on individuals who are both above driving age (in most states) but who are not legally able to purchase alcohol.

Following NHTSA procedures that are used to generate their official statistics, we calculate the number of fatal accidents involving a driver with a positive BAC for the 16-20 year old age group by state-year cells. We link our annual fatal accident totals to other data available by state annually. Most importantly, we use age-specific information on population from the U.S. Census Bureau to form accident rates for teenagers and older adults, measured both for accidents that are alcohol-related and accidents that are not. This procedure provides a sufficient number of accidents for each state such that we can estimate all of our models using all 51 states (including D.C.) for each of the nine years in our sample period. Table 3 reports averages for the annual counts and population percentages for both alcohol-related and non-alcohol-related accidents for both teenagers and adults over the age of 25.

3.2 Empirical Models and Estimation Issues

Our estimation strategy is based on the following specification for accident rates:

$$Y_n = \Phi(X_n' \beta + c_i)$$  \hspace{1cm} (1)

where $Y$ is the fraction of the population at risk that has a fatal alcohol-related accident in state $i$ in year $t$, $X$ represents state-level, time-varying characteristics that might affect $Y$, $c$ is a state-specific effect, and $\Phi(.)$ is the standard normal cumulative distribution function. This is a probit functional form, which would conventionally be estimated by maximum likelihood procedures if data on accident rates for individuals were being modeled. When “grouped” data (such as state level data) are used, models of this type have been estimated by taking the inverse normal function of both sides to obtain a “transformed” model, that is:

$$\Phi^{-1}(Y_n) = X_n' \beta + c_i + u_{it}$$  \hspace{1cm} (2)
providing a straightforward linear regression that can be estimated by fixed effects. This is analogous to the more common procedure that uses the log-odds of a fractional variable as the dependent variable to obtain logit models estimates by least squares, the only difference being the probit rather than logit functional form.\footnote{These two functional forms perform similarly in practice, such that the conventional approach is just to use whichever is considered more convenient.}

Papke and Wooldridge (1996, 2007) have noted that the usual least-squares approach to estimating fractional response models suffers from a retransformation problem in using the estimated parameters to infer the magnitude of responses. The implication is that the usual marginal effects should be considered biased when calculated from estimates of the transformed model.\footnote{Estimating the transformed model also suffers from the fact that observations that have a value for $Y$ of either 0 or 1 must either be excluded from the data, or changed in some arbitrary fashion to be included. This problem does not arise in our data, but has been a problem in the literature on alcohol-related fatalities.}

They instead suggest estimating equation (1) directly by quasi-likelihood. In Papke and Wooldridge (1996), this method is straightforward, as their paper doesn’t assume a panel-data setting. This extension is addressed in Papke and Wooldridge (2007), where they offer alternatives for estimations allowing $c$ to be potentially correlated with $X$. Specifically, assuming $c$ is normal and follows a distribution with a mean equal to $\overline{X_i} \delta$, estimates that control for fixed effects can be obtained by directly estimating the equation:

$$Y_u = \Phi(X_u' \beta + \overline{X_i} \delta) + \epsilon_u$$

which includes state-specific means along with the actual variables. Note that if one were estimating a linear model, this approach would provide exactly the same estimates as a standard fixed effects estimator.\footnote{Mundlak (1978) showed that the conventional FE estimator in a linear model can be derived by assuming that random effects have expected values that are linear functions of the individual specific means.} Several quasi-likelihoods could be used to estimate equation (3); we use
a normal likelihood for $\varepsilon$, which is equivalent to estimating equation (3) by non-linear least squares.\textsuperscript{10} Standard errors are calculated so as to be robust to functional form misspecification, as well as to any remaining correlation in $\varepsilon$ over time within a state through clustering. The incidental parameters problem is less worrisome in estimating year effects than state effects (given there are 51 observations to use in estimating each year effect), so we simply include year dummies as part of $X$ in equation (3).

The typical accident rate model in the literature is often estimated using a weighted least-squares estimator, based on the fact that the precision of the accident rate estimates will vary with the underlying group size (for example, both Ruhm, 1996, and Dee, 1999, use this estimation approach). In estimating equation (3), for example, the weighting scheme would be based on

$$\text{V}(\varepsilon_{it}) = \frac{\Phi(X'_{it} \beta + \bar{X}'_{it} \delta)(1 - \Phi(X'_{it} \beta + \bar{X}'_{it} \delta))}{n_{it}}$$ (4)

where $n$ is the population size for the group from which the dependent variable ($Y$) is formed (see Greene, 2003, pp. 686-689). One limitation of this weighting approach is that it implicitly assumes that the only reason why the sample proportion $Y$ differs from its predicted value $\Phi(X'_{it} \beta + \bar{X}'_{it} \delta)$ is because of sampling error in estimating $Y$ (as $Y$ is an estimated probability of a fatal accident, not the actual probability).\textsuperscript{11} We find it more plausible to assume that, even if we have the actual probabilities for each group, there would still be differences between the actual probabilities and the predicted values. In other words, there would still be an error term $\varepsilon$ in equation (3) even if the underlying proportions had no sampling error. Assuming this

\textsuperscript{10} The general pattern of our results is similar when the Bernoulli likelihood is used.

\textsuperscript{11} In our case, we could actually argue that this sampling problem is not an issue, as we have a complete count of the underlying number of accidents and population. However, we still want to allow for the possibility that the error variance may vary between small and large states, so we estimate our suggested generalization of the usual procedure.
component of the error term is homoskedastic (and uncorrelated with the sampling error), it follows that

\[ V(\varepsilon_{it}) = \alpha + \gamma \frac{\Phi(X_n' \beta + \bar{X}_i' \delta)(1 - \Phi(X_n' \beta + \bar{X}_i' \delta))}{n_{it}} \]  

(5)

is the appropriate equation for the variance. The parameters \( \alpha \) and \( \gamma \) in equation (5) can be consistently estimated by OLS, using the squared residuals \( \hat{\varepsilon}_{it}^2 \) calculated from unweighted estimates in place of \( V(\varepsilon) \) (and with predicted values from the unweighted estimates used to form the right-hand-side variable in equation 5). Our estimated version of equation (5) is then used to generate the predicted variances, the inverses of which are used to perform weighted nonlinear least squares in estimating equations (2) and (3). \[12\]

3.3 Specification of Independent Variables

Although equation (3) captures any fixed factors that may cause alcohol-related driving accidents to vary across states, there may be changes over time within states that need to be captured through additional controls added to \( X \). These variables are listed in Table 3, along with their means and standard deviations. Our specifications are similar to those in several recent studies that estimate the determinants of drunk driving (for example, Dee, 1999; and Eisenberg, 2003). State population (in logarithmic form) is included as an independent variable in our equations in order to capture something analogous to congestion effects. State level per capita personal income from the Bureau of Economic Analysis (BEA) controls for the potential impacts of variations in a state’s wealth on drunk driving.

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\[12\] A similar approach is used in estimating the transformed probit model of equation (2), although the particular formula for the variance in equation (4) becomes

\[ V(u_{it}) = \frac{\Phi(X_n' \beta + \bar{X}_i' \delta)(1 - \Phi(X_n' \beta + \bar{X}_i' \delta))}{n_{it} \phi(X_n' \beta + \bar{X}_i' \delta)} \]
There may also be concerns that the changes in beer taxes or minimum wages are correlated with government policies specifically designed to deter drunk driving. As our sample is from 1998-2006, we do not see this as a major concern because most of the important policy changes – such as setting the minimum age level for alcoholic purchases – did not vary over our sample. Nevertheless, during our sample period, there was one additional state-level variable that changed enough to think that its potential effects might confound the interpretation of the policy estimates in our paper. Specifically, a number of states lowered the minimum blood alcohol content used to determine whether a driver was legally intoxicated from 0.10 to 0.08. Although BAC thresholds do not apply specifically to underage drivers, who typically face a zero tolerance policy in all states during our sample, changes in these laws from 0.10 to 0.08 may signal a change in a states’ general disposition toward drinking and driving. Both Dee (2001) and Eisenberg (2003) find results supporting the argument that stricter BAC requirements reduce drunk driving accidents among teens. For this reason, we include a control for whether a state had a 0.08 statute in a given year (the remainder of the states had 0.10 BAC laws during this time period). As reported in Table 3, 68 percent of our state-year observations have a BAC requirement at the 0.08 level.

Other regulations have also been shown to reduce drunk driving, most notably provisions that hold those who sell alcohol legally responsible for potential harm caused by customers. The extent of this liability, whether the regulations are codified, and whether the liability extends beyond serving minors, varies by locality, but all such regulations have been collected under the moniker of dram shop laws. According to many studies, dram shop laws have a strong effect on

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13 According to the National Highway Systems Designation Act of 1995, states must apply zero tolerance laws to all persons under the age of 21.
decreasing drunk driving (Eisenberg 2003). We uncovered insufficient variability in such laws to identify an effect in our sample, given our fixed effects panel data approach.\(^\text{14}\)

Another important concern is that there may be an underlying propensity for all traffic accidents to change in a state over time because of differences in speed limits, gas prices, general economic activity, highway construction, weather patterns, insurance rates, or other factors that might confound the interpretation of our estimates of alcohol-related accidents. To capture these, we employ an approach utilized by Adams and Cotti (2008) that attempts to control for these influences by including as an independent variable the log of the state-level age-specific accident rate for accidents that were not alcohol related (also measured with the FARS). This control isolates the effect of the independent variables (including the policy variables of interest) apart from the many potentially omitted factors that make it more dangerous to drive in a particular locality.

The identification strategy outlined to this point is predicated on the assumption that after the inclusion of fixed effects and time-varying controls, the states that are increasing beer tax rates or minimum wage levels (the treatment group) are comparable to states that do not (the control group). Even though we control for changes in non-alcohol accident patterns, there is always the concern that changes in the laws are correlated with some unobserved trend in alcohol-related accidents within a state. Although we view this to be unlikely for minimum wages, it may be more of a concern with beer taxes that may be raised in response to increases in drunk-driving accidents. We handle the potential presence of such trends by using a difference-

\(^{14}\) There are obviously dozens of other state, national, and local laws and regulations aimed at deterring drunk driving, many of which might be effective in certain areas. As controls, however, these likely will have little effect on our results. As we will show, adding controls for BAC laws does not substantially change our estimates of the effect of beer taxes or minimum wages. We also consider the inclusion of controls for graduated driver’s license laws, finding our results robust to this policy control. If these much more visible and effective policies are not correlated with changes in these laws, it is unlikely our results are affected by less visible policies.
in-difference-in-difference type analysis, comparing the estimated policy effects for the group we expect to be most affected (teenagers) to similar estimates obtained from the sample of accidents involving drivers that are age 26 or older. This analysis explicitly tests our hypothesis that young drivers respond more strongly to minimum wages increases because of their much greater likelihood of having wages increased by the legislation. We can also examine the potential for larger effects of beer taxes on younger people compared with those over age 25, reflecting the difference in price elasticity we expect for these two groups.

4. Empirical Results

4.1. Basic estimates for teens

We begin by estimating equation (2) using fatal alcohol-related accident rates for the 16-20 year old population using the standard OLS estimator with state and year fixed effects. Parameter estimates are provided in the first column of results in Table 4, with marginal effects for the primary policy variables at the bottom of the column. Given that fatal accidents are a relatively rare event, we calculate the marginal effects as elasticities representing the percentage change in the predicted fatal accident rate divided by the percentage change in the numerical value of the policy variable. Consistent with earlier work, our estimates suggest that higher beer tax levels have a strong negative impact on accident rates. Specifically, a 10 percent increase in the beer tax is estimated to decrease the alcohol-related accident rate among 16-20 year olds by roughly 4 percent. Our estimates also support a statistically significant impact from minimum wages on the alcohol-related accident rate, with a 10 percent increase in the minimum wage increasing accident rates by roughly 11 percent. Both the beer tax and minimum wage effects are quite sizeable, and though the minimum-wage elasticity appears to be larger, the beer tax is
also important in explaining cross-state variation in fatal accident rates. A one standard-deviation increase in the minimum wage is predicted to increase accident rates by 11 percent, while a one standard-deviation increase in the beer tax decrease these rates by 32 percent.

The estimates for other independent-variable effects tend to be as anticipated. The non-alcohol-related accident rate is included as a control for any time-varying characteristics related to traffic safety in the state, and as anticipated states with high non-alcohol-related accident rates have higher alcohol-related accident rates. Population was included as a measure of congestion, and indeed there appears to be evidence of a risk of higher fatal accident rates in states whose populations are growing over time. The BAC level measure, however, has a very small coefficient that is statistically insignificant – given the small variation of the BAC limits in our time period, and the fact that they do not apply to underage drivers, this result is perhaps not surprising. There is also little evidence that higher average incomes in general lead to more fatal accidents, perhaps because the minimum wage variable better captures income variation for teens, or because there is limited variation across states in the change in average income over time.

The Papke-Wooldridge approach to estimating the fractional response model for the probit was used in obtaining the estimates reported in column (2) of Table 4. The magnitude of the estimates in column (2) is directly comparable to those of column (1), in the sense that if the retransformation problem did not bias the usual procedure the two set of estimates should be similar. Although the magnitude of the coefficient estimates for the policy variables of interest are somewhat smaller with the Papke-Wooldridge estimator, this choice of estimator does not
seem to be an important concern. On the other hand, weighting the observations (using the procedure discussed in section 3.2) does have a somewhat larger impact on the coefficient estimates. Weighted estimates using the conventional estimator are reported in column (3) of Table 4, and the weighted Papke-Wooldridge estimates are reported in column (4). In both cases, weighting does tend to reduce the standard errors of the coefficient estimates (as we should expect if weighting improves efficiency). Compared to column (1), the final column (which is our preferred estimator) has estimated policy elasticities that are reduced by roughly 25 percent. However, the magnitude of the estimates is still quite significant. The minimum wage elasticity of 0.76 continues to be larger than the beer tax elasticity of -0.31, though slightly more of the variation in accident rates across states in our data is due to the variance in the beer tax than to variance in state minimum wages.

While we more fully address the robustness of our estimates in section 4.3, it is perhaps useful to note two extensions we pursued. First, we also estimated our models using a weighted grouped-logit estimator. These results (which we report later in the paper) provide quite similar elasticities to our weighted grouped-probit estimates. Second, we also examined the relationship between our primary policy variables and the rate of fatal accidents among teenagers in which alcohol was not involved. In particular, we used weighted NLS to estimate an equation in which this non-alcohol related accident rate was the dependent variable, and the independent variables were the same controls as used in Table 4 (minus the log of the non-alcohol-related rate). Both

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15 On the other hand, the inclusion of state fixed effects is quite important to the estimation results, as none of the policy variables are statistically significant when there are no controls for state fixed effects.

16 In general, both $\hat{\alpha}$ and $\hat{\gamma}$ tend to be statistically significant in the estimation of equation (5).
the minimum-wage and beer-tax coefficient estimates were small and statistically insignificant.\footnote{The estimated minimum-wage elasticity (and standard error) was -0.110 (0.189), and the estimated beer-tax elasticity was 0.023 (0.059).} Our estimation strategy allowed for the possibility of a nonzero correlation between these variables and non-alcohol-related accidents – the inclusion of this rate as a control in Table 4 was an attempt to capture time-varying state-specific factors that might affect general traffic safety but also be correlated with the policy variables. However, we find no evidence for such a correlation between non-alcohol related accident rates and our policy variables of interest.

4.2 Comparison to estimates for older individuals.

Results in Table 4 indicate that increases in a state’s minimum wage leads to large and statistically significant increases in alcohol-related fatal accidents among 16-20 year olds. This finding is striking, and one may worry that the empirical connection is somehow driven by uncaptured state-specific trends in traffic accidents. Although we include controls for accidents in the state that are not alcohol-related, there is still the chance that there may be changes within states that affect alcohol-related accidents differently than non-alcohol-related ones – for example, a reduced use of roadblocks to catch drunk-driving violators – and that these changes might be correlated with adoption of higher minimum wages. To consider this possibility, we also estimated our specification in Table 4 for an older population of drivers (those over age 25). Evidence from older individuals is potentially useful in two different manners. Results suggesting that the effect of minimum wages is near zero for those age 26 leaves us less worried about bias from omitted state trends affecting our estimates for teens. But even if the estimated effects for the older sample are nonzero, the difference in estimated effects across age groups (young minus old) can be used as a difference-in-difference-in-difference estimator of the effect
of minimum wages on teenagers (removing any bias from state-specific trends). On the other hand, if estimates from the older population are similar to those found for the 16-20 year-olds, and no significant difference is detected, it would cast doubt on the validity of the earlier results in Table 4.

Table 5 presents the results of this analysis, with the results for 16-20 year-olds duplicated from columns (1), (2), and (4) from Table 4 to provide easy comparison. For all three estimators, the estimated coefficients for the older population yield much lower and statistically insignificant estimates of minimum wage effects on alcohol-related fatal accident rates than observed in the younger sample. A test that the minimum wage coefficients are the same for the two groups easily rejects the null, supporting that the teenage coefficient estimate is larger than for the older group (p-values range from less than 0.001 to 0.004, depending on the specification). This reveals that indeed the effects of minimum wages on alcohol-related accidents fall squarely on the groups theory suggests it should.

We also suspect that the effect of beer taxes should be stronger among young people, though, given the literature, we still anticipate that older individuals are influenced at least somewhat by beer taxes. The results in Table 5 support the inference that beer taxes are much more effective at reducing drunk driving for young people than older people. Unlike the minimum-wage effect, there is evidence that older individuals respond to the beer tax, though the

---

18 Interpreting the result in this way relies on the assumption that the true effect of minimum wages is zero for the older population. If this is not true, the estimated difference in effects still allows us to test whether or not the response among the younger group is larger than the older population in a manner that is arguably free of bias from changes in state-specific factors.
19 Estimates related to column (3) of Table 4 were excluded for brevity. These estimates were quite similar to the others reported in Table 5.
20 Estimates were also obtained using data on the 21-25 year old age group. Results were largely similar to those estimated for the older population.
elasticity of this response is considerably smaller for older individuals compared to the young. The difference in these responses is also statistically significant.

Overall, the results of the difference-in-difference-in-difference analysis are highly supportive of our initial conclusions. They suggest that the minimum-wage effects on alcohol-related accidents observed among young people are not likely the result of some omitted variable correlated with the minimum wage. The implication is that minimum wages place more earnings in the hands of young people, who use this increase in earnings to engage in behavior that increases their propensity to cause drunk-driving accidents. This is a previously unidentified source of drunk driving among young drivers, and should be considered by policy makers in places where minimum wage increases are enacted. The results also support the literature in suggesting that higher beer prices can substantially lower accident rates for young individuals, a finding which does not appear to be due to some omitted factor changing within states over time.

4.3 Robustness checks

Several choices were necessarily made in the specification and estimation of our models in Tables 4 and 5, and we recognize that there were several alternative definitions of the control group, the dependent variable, and estimation methods that could have been employed. In order to consider the sensitivity of our results to these choices, we estimated several additional models that varied some of these choices. The minimum-wage and beer-tax elasticity estimates obtained from these alternative estimations are reported for 16-20 and over-25 year-olds in Table 6.\textsuperscript{21} For reference, row 1 of Table 6 repeats our preferred weighted NLS estimates from Table 5.

The primary empirical model estimated in the literature on alcohol-related traffic fatalities assumes the accident rate follows a logistic specification. As noted in section 4.1, we

\textsuperscript{21} Detailed results for any estimated model discussed but not fully reported in this paper are available from the authors.
also estimated our models by weighted least squares using the conventional logit estimator (using the log odds of the alcohol-related accident rate as the dependent variables) and obtained very similar estimated elasticities to those from our preferred probit estimation (see row 2 of Table 6). Another functional form that has also been used in this literature models the number of fatal accidents, rather than the rate. In particular, the expected number of accidents in state $i$ and year $t$ ($w_{it}$) is usually assumed to follow an exponential specification, that is:

$$E(w_{it} | x_{it}, d_{it}) = e^{x_{it}d_{it}}.$$  

(6)

This type of model can be transformed into a linear model where the log of the number of accidents becomes a linear function of $X$, and then estimated by fixed effects (including the log of the population at risk as an additional factor in $X$). These results are reported in row (3), and provide somewhat larger estimates for most of the policy effects, compared to our preferred estimates. Alternatively, the same functional form can be estimated by a fixed-effects Poisson estimator, which we report in row (4).\(^{22}\) These estimates are closer to our estimates in row (1), although the estimated minimum-wage elasticity for teenagers is somewhat smaller than before.

The Poisson estimator avoids the retransformation problem that can bias the row (3) estimates, so the relative closeness of these estimates to our probit estimates suggest the general findings are robust to this change in functional form assumption.

The Papke-Wooldridge estimator controlled for state effects in the grouped-probit models by including state-specific means for all of the independent variables as additional independent variables. Directly estimating state effects by including dummy variables for each state can bias our estimates due to incidental-parameters problem in nonlinear models using a small number of

\(^{22}\) One advantage of the Poisson estimator is that the fixed effects can be conditioned out of the model in a manner similar to linear models, so no additional assumption is needed about the distribution of the count variable or state effects in the model past the conditional mean assumption in equation (6) (see Wooldridge, 1999).
observations for identification (in our case, nine observations for each state dummy). If we ignore this concern and directly include state dummies (rather than state-specific means) in our weighted NLS estimation, we obtain the results reported in row (5) of Table 6. While the estimated magnitudes are somewhat smaller than in our row (1) estimates, the general conclusions of the analysis are not affected.

Dee (1999) has criticized much of the early literature on beer taxes and traffic fatalities among teens for not considering the possibility of state-level trends in fatalities that may be correlated with changes in the beer tax.23 Dee supported his criticism by explicitly including state trends in his estimated model, finding that inclusion of these trends removed any evidence of a beer tax effect. As we have argued above, we believe any such trends are largely handled by our controls, as the inclusion of the non-alcohol-related accident rate should capture any general trends in traffic safety in the state, and the comparison with older individuals should capture any general trends associated with drunk driving. However, we also attempted to handle trends in the same manner as Dee, and so estimated models that directly incorporated state-level trends in the error term.24 Reported in row 6 of Table 6, these results continue to provide a statistically-significant minimum-wage elasticity for teens, and an insignificant elasticity for older adults. The magnitude of the estimates for youths is similar to our previous estimations, as a 10 percent increase in the minimum wage is estimated to raise alcohol-related traffic fatalities among youths.

23 Dee is skeptical of the size of the beer-tax elasticities reported in the literature (generally around -0.3 to -0.4) as being unbelievably large, and this characterization is generally acknowledged among researchers in the area. While our preferred elasticity estimate (-0.31) is in this range, it should not be compared to these earlier estimates. The elasticities in the previous literature are for percentage effects on all fatal accidents, while our elasticities are calculated as percentage effects on alcohol-related accidents only. Given that alcohol-related accidents are only 33 percent of all fatal accidents on average (see Table 3), our estimate of the beer-tax elasticity for all fatal accidents is only -0.10.

24 Given the shortness of our panel, directly estimating such trends with the NLS probit estimator is likely to run into a substantial incidental parameters problem, so we estimated state trends only for the WLS estimator applied to equation (2).
by roughly 6 percent. Interestingly, state trends remove any evidence of an impact of beer taxes for the over-25 age group. While our estimates support the contention of Dee that omission of trends causes the impact of beer taxes to be overstated (at least for the older population), we continue to find significant impacts from this policy control for the younger population no matter which estimator is used.

We recognize that many readers may point to alternative drunk-driving state policies whose effects might be captured by minimum wages. We estimated several additional models that incorporated changes in state policies available at the Alcohol Policy Information Systems of the National Institutes of Health (see http://alcoholpolicy.niaaa.nih.gov). None of these additional policy changes obtained statistically significant effects, and had only minor effects on the other coefficient estimates. This is perhaps due to a general lack of variation in these policy parameters over time within states. As an example of the unimportance of incorporating these additional policy controls, we included measures of graduated drivers license (GDL) laws that place greater restrictions on the legal driving abilities of young drivers. Using the definition of GDL-law severity assigned by the Insurance Institute of Highway Safety, each state is placed into one of four categories depending on the strength of their law (see Morrisey and Grabowski, 2009, for more detail).\textsuperscript{25} We re-estimated equation (3) to include GDL dummies, and the minimum-wage and beer-tax elasticities are reported in row 7 of Table 6. Although the GDL coefficient estimates were not statistically significant, they did follow a pattern for the younger population that is consistent with expectations. However, their inclusion does not materially affect the significance or size of the coefficient estimates for our policy variables of interest.

\textsuperscript{25} We thank Michael Morrisey and David Grabowski for providing the GDL data. Requests for the use of these data may be made to Michael Morrisey.
From the perspective of cultural or regional norms, the states that pass minimum wages increases may be fundamentally different from those that do not. As a final robustness check, we considered the impact on our estimates of restricting the sample to include only those states that had a state minimum wage above the federal mandate at some point during the time period under examination. The results using this restricted sample are reported in the final row of Table 6. Despite the smaller sample size, the evidence of increased traffic fatalities among youths as a result of minimum wages increases is perhaps even stronger with the more similar control group.

5. Conclusion

This paper provides statistically significant and robust evidence that higher minimum wages are associated with an increase in the rate of fatal traffic accidents among drivers under the legal drinking age. Our estimates also support a connection between higher beer taxes and lower fatality rates, with strong evidence that young drivers are much more price sensitive to beer prices than older adults. Our results are robust to the inclusion of controls for area and time fixed effects, changes in population, changes in other policies that may impact drunk driving behavior (for example, BAC laws), as well as changes in factors that may influence overall driving risk separate from drinking behavior. While strong evidence of a minimum-wage effect for youths is suggested, we find no similar evidence in estimates obtained for older populations. This difference-in-difference-in-difference comparison further supports the interpretation of our estimated minimum-wage effects as real effects on traffic fatalities. Overall, our results are consistent with evidence on the percentage of the age-specific population impacted by minimum wages, and with our predictions about how teens and adults should differ in their response to changes in discretionary income and prices.
Our estimated elasticities for minimum-wage effects are large. Using our preferred specification, a 10 percent increase in a state’s minimum wage (say from $6 to $6.60) would increase the number of alcohol-related traffic fatalities among youths by almost 8 percent. If this increase were uniform across all states (say, because of a federal minimum increase), the number of alcohol-related deaths among 16-20 year-olds would be predicted to increase by roughly 125 individuals per year. Even our lower-bound estimate predicts an additional 88 deaths per year from this small change in the minimum wage.

Policy makers are likely aware of the expected connection between beer taxes and the alcohol-related fatality rate, particularly for the younger demographic group. Our evidence provides additional support for the conclusion that higher beer taxes lower traffic fatalities, especially among youths, though our estimates suggest a somewhat smaller impact than those generally seen in the literature. Governmental bodies (city, state, or federal) should also be aware of the unexpected consequences of minimum-wage increases for traffic fatalities. That said, our results should not be interpreted as a condemnation of minimum wages, but rather as a quantitative analysis of how individuals respond to changes in income and price of a particular good with externality effects. At the least, local officials should recognize the potential need for increased deterrence measures in cases where minimum wage hikes may put more disposable income in the hands of young drivers. Our estimates suggest a beer-tax increase as a useful policy option that could be used to offset the traffic-fatality consequences of an increase in minimum wages.
References


### Table 1
Percentage of Workers Earning Low Hourly Wages, by Age Group

<table>
<thead>
<tr>
<th>Age Group</th>
<th>1998</th>
<th>2006</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Minimum Wage or Less</td>
<td>No More than $1 Above the Minimum</td>
</tr>
<tr>
<td>16-20</td>
<td>16.4%</td>
<td>49.2%</td>
</tr>
<tr>
<td>21-25</td>
<td>5.6</td>
<td>17.1</td>
</tr>
<tr>
<td>Over 26</td>
<td>1.8</td>
<td>5.8</td>
</tr>
<tr>
<td>16-20</td>
<td>15.6</td>
<td>36.2</td>
</tr>
<tr>
<td>21-25</td>
<td>5.7</td>
<td>12.5</td>
</tr>
<tr>
<td>Over 26</td>
<td>1.6</td>
<td>3.8</td>
</tr>
</tbody>
</table>

**Note:** Calculations are based on the 1998 and 2006 Outgoing Rotation Groups of the Current Population Survey. Minimum wage comparisons are made on the basis of the higher minimum wage (federal or state) effective in that year, based on state of residence. The reported statistics are the percentage of all workers that are paid by the hour at a rate in the stated range.
Table 2
Simulated Effects of a $1 Minimum Wage Increase in a State of 400,000 Teenagers
(Assuming No Disemployment Effects)

<table>
<thead>
<tr>
<th></th>
<th>Before minimum</th>
<th>After minimum</th>
</tr>
</thead>
<tbody>
<tr>
<td>Minimum wage</td>
<td>$5.50</td>
<td>$6.50</td>
</tr>
<tr>
<td>Number of employees</td>
<td>400</td>
<td>400</td>
</tr>
<tr>
<td>At or below MW</td>
<td>66</td>
<td>66</td>
</tr>
<tr>
<td>Within $1 of minimum</td>
<td>196</td>
<td>196</td>
</tr>
<tr>
<td>(assume average earnings is $6)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Hourly earnings gained by those keeping their job ($) for minimum wage workers and $0.50 for those previously within $1 of minimum</td>
<td>…</td>
<td>$131,000</td>
</tr>
<tr>
<td>Net gain per hour</td>
<td></td>
<td>$131,000</td>
</tr>
<tr>
<td>Net gain in a 20 hour work week</td>
<td></td>
<td>$2,620,000</td>
</tr>
</tbody>
</table>

Note: Counts of employees are in thousands.
| **Table 3**  
| State-Level Descriptive Statistics |
| Mean (Standard Deviations) |
| Age-specific variables | Ages 16 - 20 | Ages 26 & Up |
| Alcohol-related fatal accidents per year |  |
| Number | 33 | 175 |
| Percentage of at-risk individuals | 0.00096 | 0.00056 |
| Non-alcohol-related fatal accidents per year |  |
| Number | 77 | 334 |
| Percentage of at-risk individuals | 0.000209 | 0.000103 |
| Other variables |  |
| Minimum wage (2006 dollars) | 6.04 (0.63) |
| Prevailing beer tax (2006 dollars per gallon) | 0.27 (0.22) |
| Dummy variable for BAC law specifying minimum of 0.08 | 0.68 |
| Log of per capita income (2006 dollars) | 10.30 (0.18) |
| Log of state population | 15.04 (1.03) |
| Number of observations | 459 |
| (Number of states) | (51) |
Table 4
Probit-Model Estimates for the Determinants of Fatal Alcohol-Related Accidents Rates among Drivers Aged 16-20

<table>
<thead>
<tr>
<th>Estimation Method:</th>
<th>(1) OLS with Transformed Dependent Variable</th>
<th>(2) NLS</th>
<th>(3) WLS with Transformed Dep. Variable</th>
<th>(4) Weighted NLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Minimum Wage in 2006 dollars</td>
<td>0.046** (0.013)</td>
<td>0.041** (0.011)</td>
<td>0.029** (0.007)</td>
<td>0.031** (0.009)</td>
</tr>
<tr>
<td>Beer tax in 2006 dollars</td>
<td>-0.378** (0.033)</td>
<td>-0.302** (0.051)</td>
<td>-0.336** (0.063)</td>
<td>-0.289** (0.033)</td>
</tr>
<tr>
<td>Log of non-alcohol-related accident rate (16-20 year olds)</td>
<td>0.053** (0.023)</td>
<td>0.049** (0.023)</td>
<td>0.041** (0.018)</td>
<td>0.035* (0.019)</td>
</tr>
<tr>
<td>Log of population</td>
<td>0.279* (0.146)</td>
<td>0.265** (0.086)</td>
<td>0.231** (0.109)</td>
<td>0.242** (0.092)</td>
</tr>
<tr>
<td>BAC law of 0.08</td>
<td>-0.003 (0.013)</td>
<td>-0.004 (0.013)</td>
<td>-0.003 (0.008)</td>
<td>-0.005 (0.008)</td>
</tr>
<tr>
<td>Log of per capita personal income</td>
<td>0.009 (0.205)</td>
<td>0.090 (0.168)</td>
<td>0.166 (0.138)</td>
<td>0.157 (0.144)</td>
</tr>
</tbody>
</table>

Elasticities:

- Minimum Wage: 1.11
- Beer Tax: -0.41

Note: All regressions include both state and year fixed effects. The sample size is 459 observations (from 51 states). Column (2) is estimated by OLS using the inverse normal of the accident rate as the dependent variable. Column (3) is estimated by WLS using the inverse normal of the accident rate as the dependent variable, where weights are formed as predicted values from a regression of the squared residuals from column (1). Column (2) estimates text equation (3) by nonlinear least squares, while specification (4) estimates this equation by weighted nonlinear least-squares with weights formed as predicted values from a regression of the squared residuals from column (2). All elasticities are evaluated at the sample means for the independent variables. Standard errors are in parentheses, and are clustered at the state level to allow for arbitrary patterns in heteroskedasticity and correlation in errors over time in a given state. **, * denote statistical significance at the 0.05 and 0.10 levels, respectively.
<table>
<thead>
<tr>
<th>Estimation Method:</th>
<th>(1) OLS with Transformed Dependent Variable</th>
<th>(2) NLS</th>
<th>(3) Weighted NLS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age Group:</td>
<td>Ages 16-20</td>
<td>Ages 26 &amp; Up</td>
<td>Ages 16-20</td>
</tr>
<tr>
<td>Minimum Wage in 2006 dollars</td>
<td>0.046** (0.013)</td>
<td>0.006 (0.006)</td>
<td>0.041** (0.011)</td>
</tr>
<tr>
<td>Beer tax in 2006 dollars</td>
<td>-0.378** (0.033)</td>
<td>-0.097** (0.021)</td>
<td>-0.302** (0.051)</td>
</tr>
<tr>
<td>Log of non-alcohol-related accident rate among same age group</td>
<td>0.053** (0.023)</td>
<td>0.060** (0.026)</td>
<td>0.049** (0.023)</td>
</tr>
<tr>
<td>Log of population</td>
<td>0.279* (0.146)</td>
<td>-0.077 (0.069)</td>
<td>0.265** (0.086)</td>
</tr>
<tr>
<td>BAC law of 0.08</td>
<td>-0.003 (0.013)</td>
<td>0.003 (0.006)</td>
<td>-0.0004 (0.0133)</td>
</tr>
<tr>
<td>Log of per capita personal income</td>
<td>0.009 (0.205)</td>
<td>0.107 (0.118)</td>
<td>0.090 (0.168)</td>
</tr>
</tbody>
</table>

**Elasticities:**

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Minimum Wage</td>
<td>1.11</td>
<td>0.14</td>
<td>0.99</td>
</tr>
<tr>
<td>Beer Tax</td>
<td>-0.41</td>
<td>-0.11</td>
<td>-0.33</td>
</tr>
</tbody>
</table>

P-Value for test that the MW coefficients are equal across age groups | 0.004 | <0.001 | 0.003 |

P-Value for test that the beer tax coefficients are equal across age groups | 0.001 | <0.001 | 0.001 |

Note: See notes to Table 4

**, * denote statistical significance at the 0.05 and 0.10 levels, respectively.
<table>
<thead>
<tr>
<th>Estimated Alternative</th>
<th>Minimum Wage Elasticity</th>
<th>Beer Tax Elasticity</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Ages 16 - 20</td>
<td>Ages 26 &amp; Up</td>
</tr>
<tr>
<td>(1) Elasticities from Weighted NLS Probit Estimates (specification 3 of Table 5)</td>
<td>0.76** (0.22)</td>
<td>0.12 (0.16)</td>
</tr>
<tr>
<td>Alternative forms</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(2) Weighted Logit Model Estimates</td>
<td>0.71** (0.16)</td>
<td>0.20 (0.20)</td>
</tr>
<tr>
<td>(3) Least Squares with log of number of accidents as the dependent variable</td>
<td>1.16** (0.34)</td>
<td>0.19 (0.16)</td>
</tr>
<tr>
<td>(4) Poisson for number of accidents</td>
<td>0.55** (0.17)</td>
<td>0.25 (0.20)</td>
</tr>
<tr>
<td>(5) Weighted NLS Probit with State Dummies</td>
<td>0.52** (0.14)</td>
<td>0.07 (0.17)</td>
</tr>
<tr>
<td>Alternative Specifications</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(6) WLS Grouped-Probit Estimates with State-Level Trends</td>
<td>0.67* (0.36)</td>
<td>-0.04 (0.21)</td>
</tr>
<tr>
<td>(7) Graduated Drivers License Laws added as additional independent variable</td>
<td>0.76** (0.18)</td>
<td>0.06 (0.16)</td>
</tr>
<tr>
<td>Alternative Sample</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(8) Only states that had MW larger than the Federal at some point</td>
<td>1.05** (0.42)</td>
<td>-0.27 (0.19)</td>
</tr>
</tbody>
</table>

**Note:** Unless otherwise noted, specifications of independent variables are the same as in Tables 4 and 5. Estimates for (7) and (8) are obtained using the weighted NLS probit estimation procedure with state fixed effects (discussed in the text). Estimates for (2)-(6) explicitly incorporate state dummies to control for state. Specifications (3) and (4) add the population size for the at-risk population as an independent variable. All specifications include year dummies, and standard errors (in parentheses) are calculated allowing clustering at the state level.

**,** denote statistical significance at the 0.05 and 0.10 levels, respectively.