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 positive income elasticity for alcoholic beverages and driving activities among young people, in particular for consumption out of discretionary income accorded by higher minimum wages. Evidence of a sizable impact of beer taxes on alcohol-related accidents among youths suggests that beer taxes are one avenue for policymakers to consider in counteracting this unintended consequence of minimum wages.

Giving money and power to government is like giving whiskey and car keys to teenage boys.

—P. J. O'Rourke

I. Introduction

MOTOR vehicle crashes are the leading cause of death for 16 to 20 year olds in the United States; nearly one-third of these crashes are alcohol related (National Highway Traffic Safety Administration, 2008). Although all states (and Washington, DC) 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% of young people 16 to 20 years old 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. 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.

In this paper, we consider evidence on a government policy that may have potential unanticipated consequences on teen drinking: raising the minimum wage. By adding to the disposable income of teenagers, minimum wage increases may raise the expenditures of teenagers on alcoholic beverages and driving activities. Using information on cross-state variation in minimum wages during the 1998–2006 period, we estimate difference-in-difference-in-difference models that examine the impact of a state's minimum wage level on the incidence of alcohol-related fatal accidents among teens. Our estimates suggest that a 10% increase in the minimum wage will on average increase the incidence of fatal accidents involving drivers ages 16 to 20 by between 5% and 10%. We also show that beer taxes have an important impact on youth drinking and driving behavior.

II. Background and Prior Research

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 driving and drinking both 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 nonnecessities that parents may not fund, such as music, games, cigarettes, gasoline, and alcohol.[1] Given that a minimum wage will have a much more substantial impact on the typical teenager's discretionary income than on that of adults, we can anticipate that expenditures on both drinking and driving by teenagers could be materially affected by an increase in the minimum wage.

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 that 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 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 book-end the years of study in this paper. Table 1 presents these measures by age group. In 1998, 16% of 16 to 20 year olds worked at or below the effective minimum wage in their state of residence, while 64% were no more than $2 above their minimum (a reasonable range when considering those potentially affected by minimum wage increases). For adults over the age of 25, these same measures were only 2% and 10%, 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

Received for publication August 5, 2009. Revision accepted for publication January 7, 2011.

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We thank seminar participants at American University, Iowa State University, University of Louisville, University of Wisconsin–Milwaukee, and Winona State University for helpful comments.

[1] Meeks (1998) reports evidence that among teens, 87% spend at last half of their earned income on discretionary items, while 50% spend all of their earnings on such items.

TABLE 1.—PERCENTAGE OF WORKERS EARNING LOW HOURLY WAGES, BY AGE GROUP, 1998 AND 2006

Age Group	Minimum Wage or Less	No More than $1 above the Minimum	No More than $2 above the Minimum
1998			
16–20	16.4%	49.2%	64.1%
21–25	5.6	17.1	28.1
26 and over	1.8	5.8	9.9
2006			
16–20	15.6	36.2	54.8
21–25	5.7	12.5	21.3
26 and over	1.6	3.8	7.0

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 paid by the hour at a rate in the stated range.

value of the federal minimum wage is likely behind these declines, as the average value of the minimum wage declined in both 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 comprehensive review, see Neumark & Wascher, 2008). The strand of literature that appeals to cross-state variation in teenage employment has tended to find effects that are on the negative side, although the magnitudes do vary. For example, Zavodny (2000) presents effects on the employment of teenagers that are small and negative, while Sabia's (2009) results suggest that a 10% increase in the minimum wage reduces employment among teenagers by 2% to 3%. In Sabia's estimates, some of the largest in the literature, a reduction in teenage employment from a minimum wage increase would essentially be offset by the higher income of teenagers who keep their jobs, leaving overall income in the hands of teenagers unchanged. On the other hand, if employment effects of minimum wages are essentially zero (as is often found in the literature), the impact on overall earnings would be substantial. A back-of-the envelope calculation suggests that a $1.00 increase in the minimum wage would increase the aggregate income of teenagers by $2.62 million per week in an average-sized state.

Even in the case where disemployment effects are more important, minimum wages should still affect the distribution of income among teenagers in a way that could increase the likelihood of increased expenditures on alcohol. A substantial number of teenagers would still receive an increase in pay on their jobs even if there is a loss of employment by a small minority. If the marginal propensity to consume alcohol is particularly high for teenagers, the teenagers who stay employed may be the ones most affected in their consumption patterns. 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.[2]

There is some prior evidence that increased income among teenagers leads to an increase in their alcoholic beverage consumption. A survey conducted in 2003 on young teenagers (those ages 12 to 17) found that a relatively small increase in a teenager's income (by $25 per week) is associated with roughly a doubling of 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 observed among those 20 to 24 years old.[3] Using data from the 1997 National Longitudinal Survey of Youth Cohort, Markowitz and Tauras (2009) report evidence that higher individual income due to either allowances or earnings is associated with increased use of alcohol among teens.[4] Addison, Blackburn, and Cotti (2009) find that employment in alcoholic beverage stores is positively correlated with minimum wages, which is consistent with higher minimum wages increasing the demand for alcoholic beverages.

Our consideration of minimum wages as a policy potentially important to teen driving fatalities is new. A more commonly examined policy is the effect of beer taxes on teen drinking and driving. Previous studies that have examined this impact generally find a very large effect in which fatalities fall as the tax is raised (Ruhm, 1996; Chaloupka, Grossman, & Saffer, 2002). This finding is corroborated by evidence that suggests that raising beer taxes substantially deters self-reported drinking among teens (Cook & Moore, 2001).[5] 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 & List, 2003; Farrely 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

[2] A number of studies have examined whether alcohol consumption is countercyclical, with mixed evidence. Arkes (2007) focuses the question specifically on teen drinking, finding evidence that increases in the overall unemployment rate tend to increase the days per month in which alcohol is consumed. Minimum-wage increases are not likely to substantially affect the overall unemployment rate, though they could increase the unemployment rate for teens. If unemployment increases among teens do increase their drinking (either because of depression over being unemployed or worries about unemployment among those still employed), this could be another avenue through which minimum wages affect alcohol-related fatalities.

[3] 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.

[4] 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 of income on 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.

[5] Dee (1999) has criticized the beer tax evidence for lacking robustness.

also respond more strongly to changes in alcohol taxes than older individuals would.

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. If there are effects of minimum wages and beer taxes on fatalities, we anticipate these effects to be isolated to teens, so we also estimate the impact of these policies on alcohol-related driving fatalities for an older population to test this expectation.

III. Data and Methods

A. Data Sources

Our empirical analysis uses data from 1998 to 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.[6] Nominal values for our minimum wages variables for each state are reported in appendix A. During our sample time period, 23 states (including Washington, DC) 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 in 2006: $7.63. 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.[7] 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 16 to 20 year old had a blood alcohol concentration (BAC) that was greater than 0.[8] We also estimate effects of minimum wages and beer taxes on fatal accidents involving alcohol-impaired drivers age 26 and over. This will allow comparisons across age groups and reveal the underlying trends in accident data. The age group 16 to 20 years old (which we often refer to as teenage or underage drivers) was chosen to focus on individuals who are above driving age (in most states) but not legally able to purchase alcohol.

Table 2.—State-Level Descriptive Statistics (Means and Standard Deviations)

	Ages 16 to 20	Ages 26 and Over
Age-specific variables		
Alcohol-related fatal accidents per year		
Number	33	175
Percentage of at-risk individuals	0.000096	0.000056
	(0.000045)	(0.000025)
Non-alcohol-related fatal accidents per year		
Number	77	334
Percentage of at-risk individuals	0.000209	0.000103
	(0.000080)	(0.000040)
Unemployment rate (as a percent)	13.95 (3.82)	3.57 (0.97)
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 (number of states)	459 (51)	

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 to 20-year-old age group by state-year cells. We link our annual fatal accident totals to other data available by state annually. Most important, we use age-specific information on population from the U.S. Census Bureau to form accident rates for teenagers and older adults, measured for both 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 50 states and Washington, DC, for each of the nine years in our sample period. Table 2 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.

Our research design requires that the states not changing their minimum wage serve as a valid control group to states that increase their minimum wages. Therefore, we would like the trends in the alcohol-related accident rate before the passage of minimum wage increases to be similar in the treatment and control states. Of the 23 states that increased their minimum wage during our sample time frame, 12 did not have an increase in the minimum wage until after 2003. This six-year period (where approximately half of the treatment group had no change) provides us with an opportunity to investigate if a difference in general trends exists between the two groups. Specifically, we compare the time trend in the alcohol-related fatal accident rate before 2004 among the 12 states that did not increase their minimum wage until after 2003, to the trend for the 28 states without a minimum wage increase over the 1998–2006 period. Figure 1 shows

[6] Information on state minimum wage changes is reported in the January edition of *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.

[7] This information is maintained on the Federation of Tax Administrators web site.

[8] For many accidents the BAC level is imputed based on other characteristics of the accident. Appendix B discusses issues associated with this imputation in more detail.

FIGURE 1.—ALCOHOL-RELATED FATAL ACCIDENT RATES FOR 16 TO 20 YEAR OLDS: STATES WITH MINIMUM WAGE INCREASES AFTER 2003 VERSUS CONTROL STATES

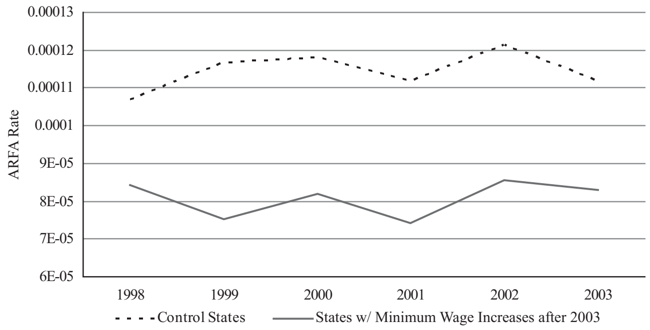
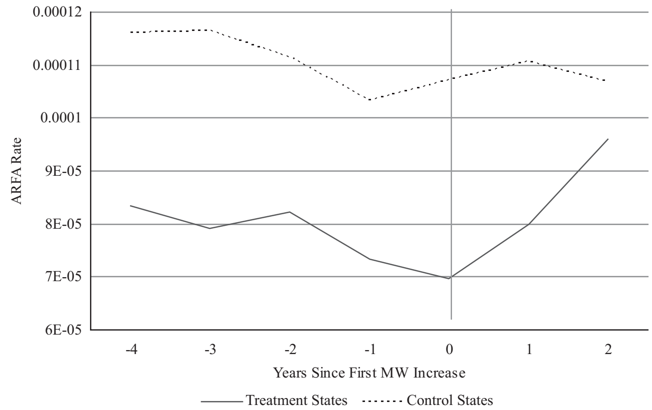


FIGURE 2.—ALCOHOL-RELATED FATAL ACCIDENT RATES FOR 16 TO 20 YEAR OLDS: BEFORE AND AFTER MINIMUM WAGE INCREASE



that there is no discernable difference in this trend between these two groups prior to 2004, alleviating concern that our research design may be undermined by a difference in underlining trends between the treatment and control states.

Figure 2 summarizes our research approach using accident rates over time by presenting an event-history representation of the patterns in average teenage alcohol-related fatal accident rates in states that increase their minimum wage above the federally mandated level.[9] Acknowledging that changes in minimum wages occur at different times in different states, the horizontal axis represents the number of years before or after the first minimum wage increase above the federal level. In the years leading up to the minimum wage change, there was, if anything, a decline in accidents. In the years following the first minimum wage increase, there was a steep upswing in accidents. We also constructed a synthetic control group using a weighted average of the alcohol-related fatal accident rates in the control states.[10] We find no suggestion of an upswing in the control states in the post-increase years. While figure 2 is suggestive of an impact of minimum wages, it has no controls for other potentially important influences on accident rates and does not account for differences in the relative size of minimum wage changes in treatment states. To consider this relationship more carefully, we develop a more complete modeling of the alcohol-related accident rate.

B. *Empirical Models and Estimation Issues*

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

$$Y_{it} = \Phi(X'_{it}\beta + \eta_i + \delta_t), \tag{1}$$

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, η is a state-specific effect, δ is a year-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 accidents 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,

$$\Phi^{-1}(Y_{it}) = X'_{it}\beta + \eta_i + \delta_t + u_{it}, \tag{2}$$

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 model estimates by least squares, the only difference being the probit rather than logit functional form.[11]

Papke and Wooldridge (1996, 2008) 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.[12] They instead suggest estimating equation (1) directly by quasi-likelihood. In Papke and Wooldridge (1996), this method is straightforward, as their paper is not for a panel data setting. This extension is addressed in Papke and Wooldridge (2008), where they offer alternatives for estimations allowing η to be potentially correlated with X. Specifically, assuming η is

[9] For clarity and simplicity, states that increased their minimum wage during our sample period but were above the federal minimum-wage level throughout the sample (such as Oregon) were excluded from figure 2. That said, these states are included in the empirical approach discussed in sections IIIB and IIIC, as well as the results presented in section IV.

[10] In this synthetic control group, the calendar years for the control group are weighted to match the calendar years represented in the corresponding lead or lag period for the treated group.

[11] These two functional forms perform similarly in practice, such that the conventional approach is just to use whichever is considered more convenient.

[12] 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.

normal and follows a distribution with a mean equal to $\overline{X}_i'\gamma$, estimates that control for fixed effects can be obtained by directly estimating

$$Y_{it} = \Phi(X_{it}'\beta + \overline{X}_i'\gamma + \delta_t) + \varepsilon_{it}, \tag{3}$$

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.[13] Several quasi-likelihoods could be used to estimate equation (3); we use a normal likelihood for ε, which is equivalent to estimating equation (3) by nonlinear least squares.[14] Standard errors are calculated so as to be robust to functional form misspecification, as well as to any remaining correlation in ε 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

$$V(\varepsilon_{it}) = \frac{\Phi(X_{it}'\beta + \overline{X}_i'\gamma + \delta_t)(1 - \Phi(X_{it}'\beta + \overline{X}_i'\gamma + \delta_t))}{n_{it}}, \tag{4}$$

where n is the population size for the group from which the dependent variable (Y) is formed (see Greene, 2003). One limitation of this weighting approach is that it implicitly assumes that the only reason that the sample proportion Y differs from its predicted value $\Phi(X_{it}'\beta + \overline{X}_i'\gamma + \delta_t)$ is sampling error in estimating Y (as Y is an estimated probability of a fatal accident, not the actual probability).[15] 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 ε in equation (3) even if the underlying proportions had no sampling error. Assuming this component of the error term is homoskedastic (and uncorrelated with the sampling error), it follows that

$$V(\varepsilon_{it}) = \alpha + \lambda \frac{\Phi(X_{it}'\beta + \overline{X}_i'\gamma + \delta_t)(1 - \Phi(X_{it}'\beta + \overline{X}_i'\gamma + \delta_t))}{n_{it}} \tag{5}$$

is the appropriate equation for the variance. The parameters α and λ 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(ε) 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).[16]

C. *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 2, along with their means and standard deviations. Our specifications are similar to those in several 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. Age-specific unemployment rates are calculated from the outgoing rotation group samples of the Current Population Survey for each state. Ruhm (1996) has argued that it is important to control for both unemployment and personal income in capturing macroeconomic influences on drunk driving. His expectations (and findings) are that higher unemployment rates lead to fewer drunk-driving fatalities because drinking (especially in bars and restaurants) falls in economic downturns.[17]

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 to 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

[13] 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.

[14] The general pattern of our results is similar when the Bernoulli likelihood is used.

[15] 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 the possibility that the error variance may vary between small and large states, so we estimate our suggested generalization of the usual procedure.

[16] 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

$$V(u_{it}) = \frac{\Phi(X_{it}'\beta + \overline{X}_i'\gamma + \delta_t)(1 - \Phi(X_{it}'\beta + \overline{X}_i'\gamma + \delta_t))}{n_{it}\phi(X_{it}'\beta + \overline{X}_i'\gamma + \delta_t)}.$$

[17] As mentioned earlier, the evidence on this relationship is mixed. Higher unemployment rates may lead to increased alcohol-related fatalities, as the state of unemployment (or worry of potential unemployment) may increase feelings of depression, which may itself lead to more binge drinking.

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 concentration used to determine whether a driver was legally intoxicated from 0.10 to 0.08. Dee (2001) reports results supporting the argument that stricter BAC requirements reduce drunk-driving fatal accidents among teens. However, the results do not support that the particular change from 0.10 to 0.08 has an important impact, a result corroborated in Eisenberg (2003). This may not be surprising given that BAC thresholds do not apply specifically to underage drivers, who face a zero tolerance policy in all states during our sample.[18] Nonetheless, the impact of BAC law changes over the time period we analyze was not considered in these previous studies, so 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). As reported in table 2, 68% 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. Previous research has suggested that these laws have a strong effect in decreasing drunk driving (Eisenberg 2003). However, we uncovered insufficient variability in such laws to identify an effect in our sample, given our fixed-effects panel data approach.

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 such effects, we employ an approach used 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.[19]

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). Although we control for changes in nonalcohol 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 given the similarity in trends across states in figure 1, we handle the potential presence of such trends by using a difference-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 who are age 26 or older.[20] This analysis explicitly tests our hypothesis that young drivers respond more strongly to minimum wage 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.

IV. Empirical Results

A. *Basic Estimates for Teens*

We begin by estimating equation (2) using fatal alcohol-related accident rates for the 16 to 20-year-old population using the standard OLS grouped-probit estimator with state and year fixed effects. Parameter estimates are provided in the first column of results in table 3 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% increase in the beer tax is estimated to decrease the alcohol-related accident rate among 16 to 20 year olds by roughly 4%. Our estimates also support a statistically significant impact from minimum wages on the alcohol-related accident rate, with a 10% increase in the minimum wage increasing accident rates by roughly 11%. Both the beer tax and minimum wage effects are quite sizable, and though the minimum-wage elasticity appears to be larger, the beer tax would seem to be more important in explaining cross-state variation in fatal accident rates. A 1 standard deviation increase in the minimum wage is predicted to increase accident rates by approximately 11%, while a

[18] According to the National Highway Systems Designation Act of 1995, states must apply zero tolerance laws to all persons under the age of 21.

[19] One limitation of this control is that it will suffer a measurement error problem (due to the imputation of BAC levels), and this error will lead to a spurious negative correlation between the alcohol- and non-alcohol-related accident rates. This negative correlation leads to a negative bias in the coefficient estimate for the non-alcohol-related accident rate. In practice, the results for our prime policy variables are essentially unaffected by the inclusion of this control, so this potential bias does not appear to represent a problem for our analysis.

[20] A graph similar to figure 1 was constructed for the 26 and over age group and, similarly, no difference in pretreatment trends was observable.

TABLE 3.—PROBIT MODEL ESTIMATES FOR THE DETERMINANTS OF FATAL ALCOHOL-RELATED ACCIDENT RATES AMONG DRIVERS AGED 16 TO 20

	(1)	(2)	(3)	(4)
Estimation Method	OLS with Transformed Dependent Variable	NLS	WLS with Transformed Dependent Variable	Weighted NLS
Minimum wage in 2006 dollars	0.046**	0.040**	0.030**	0.032**
	(0.012)	(0.012)	(0.007)	(0.009)
Beer tax in 2006 dollars	−0.378**	−0.303**	−0.338**	−0.290**
	(0.033)	(0.052)	(0.066)	(0.032)
Log of non-alcohol-related accident rate (16–20 year olds)	0.053**	0.048**	0.040**	0.034*
	(0.023)	(0.024)	(0.019)	(0.019)
Log of population	0.279*	0.270**	0.233**	0.247**
	(0.147)	(0.089)	(0.108)	(0.091)
BAC law of 0.08	−0.003	0.000	−0.002	−0.005
	(0.013)	(0.013)	(0.008)	(0.008)
Log of per capita personal income	−0.008	0.063	0.139	0.134
	(0.187)	(0.187)	(0.151)	(0.154)
Unemployment rate (16–20 year olds)	−0.001	−0.001	−0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Elasticities				
Minimum wage	1.11	0.96	0.72	0.78
Beer tax	−0.41	−0.33	−0.37	−0.31

All regressions include both state and year fixed effects. The sample size is 459 observations (from 50 states plus Washington, DC). 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.

1 standard deviation increase in the beer tax lowers these rates by 33%.

The estimates for other independent-variable effects tend to be as anticipated. States with higher non-alcohol-related accident rates also appear to have higher alcohol-related accident rates. Population was included as a measure of congestion, and indeed there appears to be a positive relationship between population changes and fatal accident rates.[21] The BAC level measure has a very small coefficient that is statistically insignificant, consistent with previous research on the particular BAC changes observed in our data. 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 unemployment rate for teenagers is also statistically insignificant in these regressions.[22]

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 3. 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 sets of estimates should be similar. Although the magnitude of the coefficient estimates for the policy variables of interest is somewhat smaller with the Papke-Wooldridge estimator, this choice of estimator does not seem to be an important concern.[23] 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 3, 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).[24] Compared to column 1, the final column (which is our preferred estimator) has estimated policy elasticities that are reduced by roughly 30%. However, the magnitude of the estimates is still quite significant. The minimum wage elasticity of 0.78 continues to be larger in magnitude 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 the variance in state minimum wages.

While we more fully address the robustness of our estimates in section 4.3, it is perhaps useful here to note two extensions we pursued. First, we 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 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

[21] The inclusion of the log of population is equivalent to the inclusion of the log of population per square mile, given that state fixed effects are included and that the area size of states does not vary over time.

[22] The lack of evidence for a macroeconomic effect is not due to collinearity between income per capita and unemployment rates, as each is individually insignificant when included in the regression without the other.

[23] The inclusion of state fixed effects is quite important to the estimation results, however, as none of the policy variables are statistically significant when there are no controls for state fixed effects.

[24] In general, both $\hat{\alpha}$ and $\hat{\gamma}$ tend to be statistically significant in the estimation of equation (5).

TABLE 4.—COMPARISON OF PROBIT MODEL ESTIMATES FOR DRIVERS AGED 16 TO 20 WITH DRIVERS 26 AND OVER

	(1)		(2)		(3)	
Estimation Method:	OLS with Transformed Dependent Variable		NLS		Weighted NLS	
Age Group:	Ages 16–20	Ages 26 and Over	Ages 16–20	Ages 26 and Over	Ages 16–20	Ages 26 and Over
Minimum wage in 2006 dollars	0.046**	0.005	0.040**	0.005	0.032**	0.005
	(0.012)	(0.006)	(0.012)	(0.007)	(0.009)	(0.007)
Beer tax in 2006 dollars	–0.378**	–0.098**	–0.303**	–0.086**	–0.290**	–0.098**
	(0.033)	(0.021)	(0.052)	(0.021)	(0.032)	(0.028)
Log of non-alcohol-related accident rate among same age group	0.053**	0.057**	0.048**	0.018	0.034*	0.014
	(0.023)	(0.026)	(0.024)	(0.028)	(0.019)	(0.030)
Log of population	0.279*	−0.098	0.270**	−0.113*	0.247**	−0.093*
	(0.147)	(0.073)	(0.089)	(0.057)	(0.091)	(0.054)
BAC law of 0.08	−0.003	0.003	0.000	−0.002	−0.005	0.002
	(0.013)	(0.006)	(0.013)	(0.006)	(0.008)	(0.006)
Log of per capita personal income	−0.008	0.051	0.063	0.011	0.134	0.073
	(0.187)	(0.127)	(0.187)	(0.077)	(0.154)	(0.078)
Age-specific unemployment rate	−0.001	−0.005	−0.001	−0.005	−0.001	−0.004
	(0.002)	(0.004)	(0.002)	(0.004)	(0.001)	(0.003)
Elasticities						
Minimum wage	1.11	0.13	0.96	0.14	0.78	0.12
Beer tax	−0.41	−0.10	−0.33	−0.10	−0.31	−0.11
p-value for test that the MW coefficients are equal across age groups	0.003		< 0.001		0.002	
p-value for test that the beer tax coefficients are equal across age groups	0.001		< 0.001		0.001	

See notes to table 3. **,* denote statistical significance at the 0.05 and 0.10 levels, respectively.

used weighted NLS to estimate an equation in which the non-alcohol-related accident rate was the dependent variable, and the independent variables were the same controls as used in table 3 (minus the log of the non-alcohol-related rate). Both the minimum-wage and beer tax coefficient estimates were small and statistically insignificant.[25] 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 3 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.

B. *Comparison with Estimates for Older Individuals*

Results in table 3 indicate that increases in a state's minimum wage lead to large and statistically significant increases in alcohol-related fatal accidents among 16 to 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, especially ones that have differential impacts on drunk- and non-drunk-driving accidents. As an example, a reduced use of roadblocks to catch drunk-driving violators might be correlated with the adoption of higher minimum wages if budget considerations due to higher minimums lead to state governments cutting back on other expenses. To allow for this possibility, we also estimated our specification in table 3 for an older population of drivers (those over age 25). Evidence from older individuals is potentially useful in two different manners. First, 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. Second, 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).[26] On the other hand, if estimates from the older population are similar to those found for the 16 to 20 year olds and no significant difference is detected, it would cast doubt on the validity of the earlier results in table 3.

Table 4 presents the results of this analysis, with the results for 16 to 20 year olds duplicated from columns 1, 2, and 4 from table 3 to provide easy comparison.[27] For all three estimators, the estimated coefficients for the older

[25] The estimated minimum-wage elasticity (and standard error) was −0.120 (0.181), and the estimated beer tax elasticity was 0.026 (0.056). This lack of a relationship between the prime policy variable and the non-alcohol-related rates alleviates concerns about the negative bias in the coefficient estimate noted earlier.

[26] 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 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.

[27] Estimates related to column 3 of table 3 were excluded for brevity. These estimates were quite similar to the others reported in table 4.

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 0.003, depending on the specification). This reveals that indeed the effects of minimum wages on alcohol-related accidents fall squarely on teenagers.[28]

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 4 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 elasticity of this response is considerably smaller for older individuals compared to the young. The difference in these responses is also statistically significant. One surprising result is the lack of evidence for an impact of strengthening the BAC limit, as earlier research (in particular, Eisenberg, 2003) had suggested such an impact existed for adults. The explanation likely lies in the fact that we are studying different time periods. The states that lowered their BAC limit after 2000 largely did so under pressure from the federal government (which in 2000 tied highway funds to lowering the limit), and these states may have been less motivated to enforce this requirement than states that had lowered their limits before 2000.[29]

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 policymakers 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 that does not appear to be due to some omitted factor changing within states over time.

C. *Robustness Checks*

Several choices were necessarily made in the specification and estimation of our models in tables 3 and 4, 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 to 20 and over-25 year olds in table 5.[30] For reference, row 1 of table 5 repeats our preferred weighted NLS estimates from table 4.

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 IVA, we 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 5). 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,

$$E(w_{it}|x_{it}, d_i) = e^{X'_{it}\lambda + d_i}. \tag{6}$$

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 negative binomial estimator, which we report in row 4.[31] These estimates are closer to our estimates in row 1, although the estimated minimum-wage elasticity for teenagers is somewhat smaller than before.

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

[28] Estimates were also obtained using data on the 21- to 25-year-old age group. Results fell between the two groups, as expected, although they were not significant.

[29] Another surprising result is the lack of statistical significance for the non-alcohol-related accident rate for older individuals in the last two specifications of table 4 (though given the high standard error, we also do not reject that the coefficient is the same for teenagers and older individuals). On the other hand, it is again the case that the omission of this variable from the specification has essentially no effect on the policy coefficient estimates of interest.

[30] Detailed results for any estimated model discussed but not fully reported in this paper are available from the authors.

[31] The Poisson estimator for count-data models is actually more robust than the negative binomial estimator (see Wooldridge, 1999). Despite this fact, the negative binomial estimator has seen more use in the recent literature. These two estimators provide estimates that are largely similar in this case.

TABLE 5.—ROBUSTNESS OF ESTIMATED ELASTICITIES TO MODEL AND ESTIMATION ALTERNATIVES

	Minimum Wage Elasticity		Beer Tax Elasticity	
Estimated Alternative	Ages 16–20	Ages 26 and Over	Ages 16–20	Ages 26 and Over
(1) Elasticities from weighted NLS probit estimates (specification 3 of table 4)	0.78**	0.12	−0.31**	−0.11**
	(0.22)	(0.17)	(0.03)	(0.03)
Alternative functional forms				
(2) Weighted logit model estimates	0.73**	0.18	−0.38**	−0.14**
	(0.17)	(0.19)	(0.07)	(0.06)
(3) Least squares with log of number of accidents as the dependent variable	1.02**	0.14	−0.41**	−0.11**
	(0.30)	(0.16)	(0.04)	(0.03)
(4) Negative binomial for number of accidents	0.73**	0.19	−0.39**	−0.11**
	(0.23)	(0.14)	(0.03)	(0.02)
(5) Weighted NLS probit with state dummies	0.53**	0.06	−0.25**	−0.13**
	(0.14)	(0.17)	(0.09)	(0.06)
Alternative specifications				
(6) WLS grouped-probit estimates with state-level trends	0.68*	−0.02	−0.28**	−0.01
	(0.36)	(0.20)	(0.14)	(0.06)
(7) Graduated drivers license laws added as additional independent variable	0.79**	0.05	−0.35**	−0.12**
	(0.19)	(0.16)	(0.04)	(0.03)
Alternative sample				
(8) Only states with minimum wages above the federal minimum at some point	0.91**	−0.14	−0.25**	−0.10**
	(0.32)	(0.19)	(0.04)	(0.02)

Unless otherwise noted, specifications of independent variables are the same as in tables 3 and 4. Estimates for alternatives 7 and 8 are obtained using the weighted NLS probit estimation procedure with state fixed effects (discussed in the text). Estimates for alternatives 2 to 6 explicitly incorporate state dummies to control for state effects. Specifications 3 and 4 add the log of 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.

bias our estimates due to incidental parameters problems in nonlinear models using a small number of 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 5. 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.[32] 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.[33] Reported in row 6 of table 5, 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% increase in the minimum wage is estimated to raise alcohol-related traffic fatalities among youths by roughly 6% or 7%. Interestingly, state trends cause the estimated impact of beer taxes for the over-25 age group to be statistically insignificant, though they substantially decrease the precision of this estimate. 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 polices 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 incor-

[32] Dee is skeptical of the large size of the beer tax elasiticities reported in the literature (generally around −0.3 to −0.4), 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% of all fatal accidents on average (see table 3), our estimate of the beer-tax elasticity for all fatal accidents is only −0.10.

[33] 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).

porating 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 its law (see Morrisey & Grabowski, 2008, for more detail).[34] We reestimated equation (3) to include GDL dummies, and the minimum-wage and beer tax elasticities are reported in row 7 of table 5. 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.

From the perspective of cultural or regional norms, the states that pass minimum wage increases may be fundamentally different from those that do not. To consider this possibility, we reestimated our models, restricting the sample to 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 5. Despite the smaller sample size, the evidence of increased traffic fatalities among youths as a result of minimum wage increases is perhaps even stronger with the more similar control group.

As a final extension, we considered whether the effects of our primary policy variables differed for 18 to 20 year olds compared to 16 to 17 year olds. This could be the case, for example, if minimum wages affected income more for one group than for the other or if 18 to 20 year olds found it easier to obtain alcohol. Estimating our model for 18 to 20 year olds using the weighted NLS probit estimator, we obtain an estimated elasticity (robust standard error) of 0.66 (0.24) for minimum wages and −0.32 (0.03) for beer taxes. For 16 to 17 year olds, we obtain an estimated elasticity of 0.70 (0.50) for the minimum wage and −0.25 (0.08) for the beer tax. While the estimates for the 16 and 17 year olds are very imprecise (such that the minimum wage elasticity is no longer statistically significant), the estimated values are quite similar for the two groups, with neither the minimum-wage nor beer tax elasticities significantly different across these age groups.

V. 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 are. Our results are robust to the inclusion of controls for area and time fixed effects, changes in population, changes in other policies that may affect 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.

Our estimated elasticities for minimum-wage effects are not insubstantial. Using our preferred specification, a 10% increase in a state's minimum wage (say from \$6.00 to \$6.60) would increase the number of teen-related drunk-driving fatalities by around 7.8%. If this increase were uniform across all states (say, because of a federal minimum increase), the number of deaths associated with accidents involving drunk drivers who are ages 16 to 20 would be predicted to increase by roughly 127 individuals per year. Even our lower-bound estimate predicts an additional 77 deaths per year from this small change in the minimum wage.

Policymakers 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 to offset the traffic fatality consequences of an increase in minimum wages.

[34] 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.

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APPENDIX A

Nominal Minimum Wages by State and Year (in dollars), 1998–2006

State	1998	1999	2000	2001	2002	2003	2004	2005	2006
Alabama	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Alaska	5.65	5.65	5.65	5.65	5.65	7.15	7.15	7.15	7.15
Arizona	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Arkansas	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.43
California	5.65	5.75	5.75	6.25	6.75	6.75	6.75	6.75	6.75
Colorado	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Connecticut	5.18	5.65	6.15	6.40	6.70	6.90	7.40	7.40	7.40
Delaware	5.15	5.44	5.78	6.15	6.15	6.15	6.15	6.15	6.15
District of Columbia	6.15	6.15	6.15	6.15	6.15	6.15	6.15	6.60	7.00
Florida	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.82	6.40
Georgia	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Hawaii	5.25	5.25	5.25	5.25	5.75	6.25	6.25	6.25	6.75
Idaho	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Illinois	5.15	5.15	5.15	5.15	5.15	5.15	5.50	6.50	6.50
Indiana	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Iowa	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Kansas	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Kentucky	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Louisiana	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Maine	5.15	5.15	5.15	5.15	5.15	5.75	6.28	6.39	6.56
Maryland	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	6.03
Massachusetts	5.25	5.25	6.00	6.75	6.75	6.75	6.75	6.75	6.75
Michigan	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.60
Minnesota	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.40	6.15
Mississippi	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Missouri	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Montana	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Nebraska	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Nevada	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.23
New Hampshire	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
New Jersey	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.40	6.40
New Mexico	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
New York	5.15	5.15	5.15	5.15	5.15	5.15	5.15	6.00	6.75
North Carolina	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
North Dakota	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Ohio	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Oklahoma	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Oregon	6.00	6.50	6.50	6.50	6.50	6.90	7.05	7.25	7.50
Pennsylvania	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Rhode Island	5.15	5.40	5.82	6.15	6.15	6.15	6.75	6.75	7.04
South Carolina	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
South Dakota	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Tennessee	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Texas	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Utah	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Vermont	5.25	5.38	5.75	6.25	6.25	6.25	6.75	7.00	7.25
Virginia	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15
Washington	5.15	5.70	6.50	6.72	6.90	7.01	7.16	7.35	7.63
West Virginia	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.50
Wisconsin	5.15	5.15	5.15	5.15	5.15	5.15	5.15	5.47	6.17

Source: January Edition of the *Monthly Labor Review*. Minimum wage values reflect the monthly average nominal minimum wage for every state in every year. Minimum wage values used for estimation in the paper were adjusted to 2006 dollars to correct for inflation.

APPENDIX B

BAC Imputation in the FARS Data

Despite federal mandates, blood alcohol concentration (BAC) levels are not always taken at crash scenes. In fact, roughly half of all fatal accidents in the FARS do not have BAC reports. For many years, missing BAC levels were imputed using a linear discriminant analysis to predict whether the BAC level fell within three different intervals (Klein, 1986; NHTSA, 2002). Starting with the 2001 data, the method of imputation was changed to use a "general location model" to provide continuous

BAC imputations (Subramanian & Utter, 1998). One part of this modeling process is an estimated log-linear model for the probability that the BAC is positive, which is the primary part of the imputation of relevance to our analysis, given our definition of "alcohol related" as a positive BAC. The factors that are used in modeling this probability are age, gender, safety belt or helmet use, license expiration, prior traffic convictions, day of the week, time of day, the role of the vehicle in the accident, whether the car remains on the road, the type of vehicle driven, and whether police at the accident believed drinking was involved. (The final two factors are the most related to BAC levels when BAC is observed.) The NHTSA has provided new imputations for years prior to 2001, which we use. A validation test for the imputation of zero or positive BAC status performs extremely well across the years (see Subramanian & Utter, 1998). The new BAC imputation procedures use the same statistical model to provide multiple imputations of BAC levels for each accident with missing BAC. We followed the suggestions in NHTSA (2002) for combining these multiple imputations to form a single location-specific accident rate to use as the dependent variable in our regressions.

We see the use of imputed BAC levels in forming alcohol-related fatal accident rates as an improvement over much of the earlier literature that uses nighttime/daytime or weekend/weekday comparisons to assess the impact of drinking on accidents. In essence, these studies are using a single factor to impute the drinking behavior of drivers when they make these comparisons and actually focus on factors that are not the most highly correlated with drinking behavior. By using imputed BAC levels, we are using several additional factors (including actual BAC levels on roughly half of all accidents) to separate accidents that are more likely to be alcohol related from ones that are less likely to be so.

One concern that might arise in our regression analysis is that the increase in alcohol-related accident rates might be spurious if minimum wage increases have a tendency to increase non-alcohol-related accidents among teens that end up falsely imputed as alcohol related. This might occur, for example, if minimum wages lead to more nighttime or weekend driving among teens, even though this driving is not under the influence. We would consider this a particular concern for our analysis if the rate of imputation among teens had increased after minimum wages were raised, as then our observed increases in alcohol-related accidents after minimum wage increases could largely be due to increased false imputations. In fact, in states that raised their minimum wages, the percentage of teen accidents with imputed BAC levels fell slightly, from 44% before the state first increased their minimum wage to 42% afterward. In states that always stayed at the federal minimum, the rate of imputation was higher (50%). A similar pattern was observed for imputation levels for older individuals, as the percentage of adult accidents with imputed BAC fell from 50% before the state first increased its minimum wage to 48% afterward, while a higher imputation rate (55%) was again observed in states that always stayed at the federal minimum.

It is also worth noting that while the BAC level data are not always accurate, the reporting of a fatal accident is. Our finding that minimum-wage increases tend to increase alcohol-related fatalities among teens but have no impact on nonalcohol fatalities implies that the overall fatal accident rate for teens is increased by higher minimum wages. On the other hand, our finding of no impact on minimum wages for either type of accident for older individuals suggests that the increased teenage accident rate is not merely reflecting changing driving conditions in those states that raise the minimum wage.