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ISSN: 2365-9793

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ABSTRACT

The Long-Run Impacts of Adolescent Drinking: Evidence from Zero Tolerance Laws

This paper provides the first long-run assessment of adolescent alcohol control policies on later-life health and labor market outcomes. Our analysis exploits cross-state variation in the rollout of "Zero Tolerance" (ZT) Laws, which set strict alcohol limits for drivers under age 21 and led to sharp reductions in youth binge drinking. We adopt a difference-in-differences approach that combines information on state and year of birth to identify individuals exposed to the laws during adolescence and tracks the evolving impacts into middle age. We find that ZT Laws led to significant improvements in later-life health. Individuals exposed to the laws during adolescence were substantially less likely to suffer from cognitive and physical limitations in their 40s. The health effects are mirrored by improved labor market outcomes. These patterns cannot be attributed to changes in educational attainment or marriage. Instead, we find that affected cohorts were significantly less likely to drink heavily by middle age, suggesting an important role for adolescent initiation and habit-formation in affecting long-term substance use.

JEL Classification: I18, I12, J20

Keywords: Zero Tolerance laws, disability, alcohol consumption, labor market

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1 Introduction

In 2015, more than one quarter of 18-20 year olds reported excessive alcohol consumption in the past 30 days (NSDUH, 2015).[1] Binge drinking has been linked to a range of negative outcomes among adolescents including poor academic performance, risky sexual behavior, crime, drunk driving, and mortality. The prevalence of excessive adolescent drinking and the associated harms have received considerable attention from policymakers and the media. Nevertheless, we know very little about the longer term consequences of this behavior and whether the costs extend into later-life.

This paper provides the first long-run assessment of adolescent alcohol control policies on later-life outcomes. Our analysis relies on cross-state variation in the rollout of "Zero Tolerance" (ZT) Laws during the 1990s. These laws established strict blood alcohol content requirements for drivers under age 21, and previous research has documented that they led to sharp reductions in adolescent binge drinking (Carpenter, 2004). We link individual exposure to these laws during adolescence to a rich set of later-life outcomes to track the evolving impacts into middle age. Specifically, we use annual individual-level data from the American Community Survey (ACS) for the period 2000 to 2017. The ACS provides measures of self-assessed health status along with a range of labor market outcomes (Ruggles et al., 2019). We supplement this analysis with microdata from the Behavioral Risk Factor Surveillance System (BRFSS), which provides direct measures of alcohol use in later-life.

Our research design is based on a synthetic-cohort approach, in which adolescent exposure to ZT Laws is identified based on an individual's state and year of birth and then linked to later-life outcomes.[2] Our identifying assumption, that within-state changes in outcomes across cohorts were not systematically related to the timing of ZT Law implementation, is supported by at least three pieces of evidence. First, given the legal history of ZT Laws, which arose primarily in response to congressional legislation that incentivized states to pass tough youth drunk driving laws, they are plausibly exogenous to changing local attitudes towards youth drunk driving. Second, the timing of ZT Law adoption across states is unrelated to both a host of underlying socioeconomic conditions

[1] Excessive alcohol consumption or "binge drinking" is typically defined as five or more alcoholic drinks for males or four or more alcoholic drinks for females on the same occasion.

[2] Bailey (2006) uses a similar approach to study the long-run impact of early access to the birth control pill on women's lifecycle labor market outcomes.

and pre-existing alcohol-related policies. Third, estimates from ‘event study’ regressions show no evidence of divergent pre-trends. In particular, exploiting the sharp change in exposure to ZT Laws across cohorts, we find no effects on outcomes for individuals who turned 21 immediately prior to law passage.

The results show clear evidence that increased regulation of adolescent drinking led to long-run improvements in adult health. We find that individuals exposed to ZT Laws during adolescence were 3 percent less likely to report a physical or cognitive limitation by ages 40-44, and 8 percent less likely by ages 45-48. In contrast, we find no significant effects among younger age groups, suggesting that the health impacts only materialized as individuals approached middle age. The broad patterns for long-run population health are stable across a range of different specifications, and are robust to various covariates including state-specific linear trends and controls for other alcohol-related policies. Taken together, this evidence provides further support for the research strategy that outcomes among affected cohorts would have trended similarly absent the adoption of ZT Laws.

We find that adolescent ZT Law exposure had significant effects on a range of later-life health outcomes. Individuals exposed to these laws were less likely to report *both* physical limitations and cognitive limitations by middle age. Treated cohorts were also less likely to suffer visual/auditory limitations. These findings are consistent with the established link between heavy alcohol consumption and vision/hearing difficulties (Chong et al., 2008), major depressive disorders and impaired cognitive function (Rehm et al., 2017), along with a range of other chronic health problems (WHO, 2018).

Next, we explore the effects of ZT Laws on long-run labor market outcomes. We find that ZT Laws led to increases in labor market attachment that mirror the patterns for health. Individuals exposed to these policies during adolescence worked more weeks per year and more hours per week, and had higher employment rates by middle age. Our estimates imply that the nationwide adoption of ZT Laws, and the associated decrease in adolescent drinking, averted large long-run economic costs. The coefficient estimates imply that the laws generated annual gains of more than $6 billion due to increased labor market attachment among middle aged workers. This value does not account for the potential economic gains as affected cohorts enter older age or the non-pecuniary benefits from improved health or greater longevity. Nevertheless, it is comparable to previous calculations of the short-run harms from youth alcohol consumption, which are estimated to cost $50 billion annually (Bonnie and O'Connell, 2004).

What explains the relationship between ZT Laws and later-life outcomes? These laws represented only a temporary barrier to drinking, so it is unclear why they had persistent effects on outcomes decades in the future. One explanation is that the laws were in operation at a critical age juncture when individuals made human capital investment decisions that ultimately impacted long-run outcomes. To assess this possibility, we estimated the effects of ZT Laws on educational attainment and marriage entry. We find that exposed cohorts experienced modest increases in high school and college graduation rates, but were no more likely to marry. Nevertheless, the effect sizes are too small to account for the long-run changes in health or labor market outcomes.

Second, the results may reflect a permanent change in adult drinking behavior resulting from temporary exposure to the policy. To assess this possibility, we use BRFSS data to estimate the impact of ZT Laws on drinking patterns in later-life. We find that the laws led to large reductions in heavy episodic drinking by middle age, but had little impact on moderate alcohol consumption. These findings are consistent with previous research that documents a number of adolescent-specific sensitivities that may influence initiation into binge drinking and the strong dependency of this behavior into adulthood (Spear, 2016; Degenhardt et al., 2013).

In addition to the long-run changes in drinking behavior, the effects may also capture the direct impact of heavy adolescent drinking on later-life health. In fact, evidence from animal studies shows that adolescence is a particularly harmful period for heavy alcohol consumption, and that exposure to high concentrations of alcohol during adolescence can have permanent developmental effects (e.g., Taffe et al., 2010).

Our analysis contributes to the literature demonstrating how policies that target critical ages can have long-lasting effects (e.g., Almond and Currie, 2011; Aizer et al., 2016). Although individuals were exposed to ZT Laws for a brief period during adolescence, our findings indicate that these policies had persistent effects on behavior through middle-age, with substantial economic and health consequences. Our results are consistent with recent work by Kueng and Yakovlev (2020), who show how a temporary change in the relative supply of alcohol drinks in Russia during the mid-1980s had lasting effects on consumers' preferences for hard versus light alcoholic drinks, and led to substantial decreases in male mortality. It is notable that despite the widely differing contexts, policy changes, and outcomes variables, both ours and their study demonstrate the important role of early habit formation for long-run health.[3] More broadly, our analysis complements both theoretical and empirical research that highlight the importance of conditions at initiation for long-run consumption of addictive substances (Becker and Murphy, 1988; DeCicca, Kenkel and Mathios, 2002).

This paper also contributes to the literature on the consequences of policies that restrict adolescent drinking. Much of the literature has focused on either the effects of ZT Laws or minimum legal drinking age laws on youth outcomes. Previous work has documented significant effects of these policies on youth binge drinking (Dee, 1999; Carpenter, 2004), academic performance (Carrell, Hoekstra and West, 2011), risky sexual behavior (Dee, 2001*b*; Fertig and Watson, 2009), crime (Carpenter, 2005; Carpenter and Dobkin, 2015), and mortality (Dee and Evans, 2001; Carpenter and Dobkin, 2009, 2011; Carpenter, Dobkin and Warman, 2016). Our results imply that there may be substantial long-run economic costs associated with excessive adolescent drinking that are not accounted for by short-run evaluations.

2 Background

2.1 Zero Tolerance Laws

The public campaign to reduce alcohol-related fatalities began in earnest in the 1980s. Following the founding of MADD in 1980 and increased media attention, many states enacted laws restricting blood alcohol content (BAC) for drivers. Despite these legislative changes, by the end of the decade just four states had imposed separate BAC limits for minors (Table A.1), even though the alcohol-related fatality rate among younger drivers was nearly twice that of older drivers (NHTSA, 2000).

In 1991, Congress passed legislation that provided grants to states to establish strict BAC

[3] Whereas our focus is on cohorts of U.S. adolescents that reached adulthood during a period of relative economic prosperity and social stability, their study is based on cohorts of Russian adolescents who reached adulthood immediately following the collapse of the Soviet regime. The two sets of policies differed as well. In the Russian setting, the policies *temporarily* restricted alcohol among *all* age groups, although the long-run effects were concentrated most heavily among younger cohorts. In contrast, the U.S. policies were *permanent* and *targeted* specifically to adolescents, allowing us to identify sharp differences in exposure across birth cohorts. Another difference is that the Russian policies altered the relative availability of alcohol *types* (vodka versus beer) but had little impact on extensive margin consumption, whereas the U.S. policies had large effects on alcohol consumption and binge drinking in particular. Finally, their analysis focuses on an extreme health outcome – mortality – which was substantially affected by the high rates of alcohol abuse in the Russian context. We focus on measures of disability and other socioeconomic variables; outcomes that are more relevant in the U.S. context where rates of alcohol abuse are substantially lower.

requirements for persons under age 21.[4] Subsequent legislation under the National Highway Systems Design Act in 1995 mandated that states enact Zero Tolerance (ZT) Laws, with non-compliant states facing the possible withholding of federal highway funding.

The federal legislative changes led to rapid adoption of ZT Laws. Between 1990 and 1998, ZT Laws were enacted in all 50 states and the District of Columbia. Figure A.1 shows no clear geographic patterns in the timing of enactment across regions. This is unsurprising, since both the 1991 and 1995 federal programs created strong financial incentives for states to enact ZT Laws, weakening the link between state adoption and local policy preferences.[5]

ZT Laws made it illegal for individuals under age 21 to drive with measurable traces of alcohol regardless of impairment. Violators faced penalties of license suspension or revocation. In practice, there were minor differences in the stringency of these laws across states, although all states were required to enforce BAC limits of 0.02 percent or less.

A number of studies show that ZT laws had large impacts on alcohol-related fatalities that were driven by decreased rates of youth drunk driving.[6] Carpenter (2004) finds that the laws led to large decreases in excessive alcohol consumption among adolescents, particularly among males. Consistent with these patterns of decreased adolescent drinking, Carpenter (2005) finds a negative relationship between ZT Laws and arrests for nuisance crimes. Whether the effects of these policies on exposed cohorts extended into later adulthood remains an open question.

2.2 ZT Laws, Adolescent Binge Drinking, and Later-Life Outcomes

There are several plausible channels through which ZT Laws may influence health and labor market outcomes in later-life. First, by limiting initiation into heavy drinking during adolescence may reduce the likelihood of this behavior in adulthood, offsetting the potentially harmful long-run consequences. Researchers have identified a number of adolescent-specific alcohol sensitivities that contribute to heavy drinking at this age. These include both biological factors, such as neural

[4]These grants were established as part of the Intermodal Surface Transportation Efficiency Act (ISTEA), which authorized $150 million to establish a new 6-year incentive program, during which states could receive federal grants if they enacted and implemented strict BAC requirement for individuals under age 21.

[5]There may be some evidence of earlier adoption in Western states, perhaps as a result of the initial concentration of MADD chapters in this region (Marshall and Aleson, 1994). In our empirical analysis, we address for potential regional clustering in the timing of policy adoption.

[6]See Hingson (1994); Zwerling and Jones (1999); Dee and Evans (2001); Eisenberg (2003). In contrast, Grant (2010) finds little impact on traffic fatalities.

developments (Spear, 2016; Miranda Jr et al., 2014), and social/contextual factors that contribute to risk-taking (Schriber and Guyer, 2016; Steinberg, 2008). Given the strong dependency of this behavior, individuals who initiate binge drinking during adolescence may be more likely to continue into adulthood (Waters and Sloan, 1995; Esser et al., 2014).

Long-term heavy drinking has been linked to a range of negative health outcomes, including chronic conditions such as cardiovascular diseases, liver diseases, diabetes, and digestive problems (WHO, 2018), vision and hearing difficulties (Chong et al., 2008; Gong et al., 2015; Curhan et al., 2015), and increased risk of certain cancers (IARC, 2007). Epidemiological studies have also shown a consistent link between heavy alcohol consumption, major depressive disorders, and impaired cognitive function (Rehm et al., 2017; WHO, 2018).

Long-term heavy drinking has also been linked to divorce and poor employment outcomes (Leonard and Rothbard, 1999; Feng et al., 2001). Heavy drinking during adolescence may also influence adult outcomes through changes in human capital formation. Researchers have identified the negative consequences of heavy drinking on school performance (Carrell, Hoekstra and West, 2011), which may have long-lasting effects on later-life health and labor market outcomes.

In addition, because adolescence is a period of rapid brain maturation and cognitive development, exposure to high concentrations of alcohol at this age can have long-lasting health consequences through neurocognitive alternations and epigenetic mechanisms (White and Swartzwelder, 2004; Taffe et al., 2010; Guerri and Pascual, 2010; Pandey et al., 2015). Epidemiological studies also show an association between heavy adolescent drinking and neuropsychological deficits (Jacobus and Tapert, 2013; Lisdahl et al., 2013), although it is unclear whether these patterns are causal, and whether they reflect temporary versus persistent deficits.

Finally, ZT Laws may affect later life outcomes by increasing the rates of DUI arrests among exposed cohorts (e.g. Carpenter, 2007). A priori, it is unclear how this increase in DUI arrests might influence later-life outcomes. On the one hand, a DUI arrest could harm long-run outcomes by making it more difficult to obtain college admissions or employment opportunities.[7] On the other hand, following a DUI arrest, individuals were typically mandated to attend alcohol education programs, which may have led to decreases in future alcohol consumption and improved long-run

[7] Adolescent DUIs were not always expunged, and those arrested over the age of 18 could be treated as an adult. In rare cases (usually as a result of a fatality), a DUI arrest could result in jail time, with potentially long-lasting consequences.

outcomes (e.g. Wells-Parker et al. 1995).

3 Data

We draw on annual individual-level data from the American Community Survey (ACS) for the period 2000 to 2017 (Ruggles et al., 2019). The ACS is a large-scale nationally representative cross-sectional survey of the U.S. population. We restrict attention to individuals aged 35 to 54 at the time of observation.

We link individuals to the relevant ZT Law during adolescence based on state and year of birth. Specifically, we construct a dummy variable for whether a ZT Law was in place in an individual's state of birth prior to age 21.[8] We define ZT Laws as BAC restrictions of 0.02 or less that applies to all individuals below age 21.[9] Exposure to ZT Laws varied across cohort and state of birth for individuals born between 1969 and 1977. We include cohorts born from 1946 to 1968 and 1978 to 1982 in order to better control for state-specific trends in outcomes.[10] We exclude all individuals who turned age 21 during the same quarter of state implementation.

Respondents were asked a series of questions on physical and mental health. We construct separate indicator variables for reported physical limitations, cognitive limitations or vision/hearing difficulties.[11] In addition to these self-assessed health outcomes, we construct a number of socioeconomic outcomes including: weeks worked last year, usual hours worked per week, current employment status, wage earnings, marital status, and educational attainment.

We supplement these data with outcomes from the BRFSS, a representative survey at the state-level which reports detailed individual-level information on alcohol consumption. Our main sample is a repeated cross section of individuals aged 35 to 54 for the period 1990 to 2017. We identify exposure to ZT Laws based on respondent's birth year and current state of residence. We construct

[8] This is the same approach used by Bailey (2006) to explore the impact of early legal access to the birth control pill on women's lifecycle labor force participation.

[9] Our definition follows the assignment of ZT Laws in Carpenter (2004). While several states enacted separate BAC requirements for minors in the 1980s, these were typically less stringent and covered only a subset of minors. Our results are not sensitive to excluding states that enacted these prior restrictions.

[10] The extended sample of pre-treatment cohorts also helps address concerns regarding negative weights in difference-in-differences estimators raised by De Chaisemartin and D'Haultfoeille (2020), since the estimates rely more heavily on comparisons across treatment 'switchers' to untreated cohorts. The results are not sensitive to either the age or cohort sample restrictions.

[11] Physical limitations include conditions that substantially limit one or more basic physical activity such as walking, climbing stairs, reaching, lifting, or carrying. Cognitive limitations include difficulties learning, remembering, concentrating, or making decisions due to either physical, mental, or emotional conditions.

several measures of alcohol consumption during the previous month including: an indicator for binge drinking, average number of drinks consumed per episode of drinking, and whether the individual consumed any alcohol.

While the BRFSS allows us to directly identify long-run behavioral effects, there are several drawbacks to the survey. First, it does not provide information on state of birth, so we must assign ZT Laws on the basis of current state of residence.[12] Second, information on alcohol consumption – typically asked over the previous 30-day reference period – is self-reported and may suffer from respondent's errors in recall. To the extent that attitudes towards drinking were shaped during adolescence, respondent's willingness to accurately report drinking behavior may be systematically related to ZT Law exposure.

4 Empirical Strategy

Our empirical approach is based on standard difference-in-differences regressions that exploit cross-state differences in the timing of ZT Law implementation to identify within-cohort effects of adolescent exposure on later-life outcomes. We estimate the following regression equation:

$$Y_{icst} = \alpha + \beta_{Age}\left(ZT_{cs} \times Age_{icst}\right) + \gamma X_{icst} + \lambda_c + \delta_s + \eta_t + \delta_s \cdot c + \delta_s \cdot Age_{icst} + \epsilon_{icst}, \qquad (1)$$

where Y denotes the outcome of interest for individual i, from cohort c, born in state s, observed in year t. The term X_{icst} denotes a vector of individual and state-level controls. Individual controls include 5-year age group dummies, gender, and a dummy for white. State-level covariates include the current unemployment rate to control for contemporaneous economic conditions, and a series controls for alcohol-related policies relevant in adolescence and early adulthood. These controls include the state's minimum legal drinking age (MLDA), drunk driving laws, vertical identification card laws, and contemporaneous state beer excise taxes.[13]

Equation (1) also includes a series of fixed effects, λ_c, δ_s, and η_t, that represent indicators

[12]Measurement error due to differences in the current state of residence and the state of residence during adolescence will tend to bias the estimated effects of ZT Laws towards zero.

[13]Variation in the MLDA laws occurs only to pre-treatment cohorts, since all states set a 21 age limit by 1988. The drunk driving laws include the presence of 0.08 and 0.10 BAC Laws, which have been found to significantly decrease drunk driving among adolescents (Dee, 2001*a*). Meanwhile vertical ID laws, which were adopted between 1994 and 2009, made it easier to establish a person's age, and have been associated with significant, albeit short-term, decreases in drinking among 16 year olds (Bellou and Bhatt, 2013).

for birth cohort, state of birth, and year of observation, respectively. We include a vector of interactions between the state of birth and a linear cohort trend, $\delta_s \cdot c$, to allow for differential trends in outcomes across cohorts born in different states. Finally, we include interactions between state of birth and age group, $\delta_s \cdot Age_{icst}$, to allow for invariant differences in the lifecycle trajectory of outcomes across states. These controls allow for the possibility, for example, that differences in the underlying occupational structure across states may lead to differences in the age profile of disability over the lifecycle.

The variable of interest, ZT_{cs}, is an indicator for whether the individual was exposed to a ZT Law prior to age 21. We interact this variable with a set of 5-year age group dummy variables, Age_{icst}, to allow the effects of early exposure to ZT Laws to vary with age (35-39, 40-44, and 45-48).[14] The term β_{Age} is the vector of coefficients of interest, each element capturing the average, age-group specific, within-cohort, within-state of birth impact of adolescent exposure to ZT Laws.[15]

Given the extended lag between adolescent treatment and observed outcomes towards middle age, the estimates for β_{45-48} are identified based on policy changes among the earliest adopting states (Arizona and Maryland in 1990, Oregon in 1991, New Jersey and Utah in 1992). In contrast, because there is a shorter lag between adolescent treatment and observed outcomes, the estimates for β_{40-44} and β_{35-39} are identified based on all state ZT Laws adopted from 1990 to 1998.[16] Given potential concerns regarding the small number of states identifying the estimates for β_{45-48}, we also estimate versions of equation (1) in which we collapse treatment exposure to all individuals aged 40 years and older, β_{40-48}.[17]

The identifying assumption for the empirical analysis is that trends in outcomes across states were not systematically related to the timing of ZT Law implementation. In practice, this assumption must only hold after conditioning on other covariates, including a linear state of birth trend. This assumption is supported by the legislative history of zero tolerance policies. State adoption of ZT Laws in the 1990s arose in large part in response to federal legislation that incentivized the enactment of these policies. As a result, there is less concern that these policies arose endogenously

[14]Since the first ZT Laws were enacted in 1990, the oldest treated individuals were aged 48 in 2017.

[15]We suppress the main effect of ZT_{cs} from equation (1), so there is no reference treatment age. Instead, each estimate of β_{Age} captures the age-specific treatment effect of the policy.

[16]For each estimate of β_{Age} early adopting states are weighted more heavily among the treated cohorts. That is because we observe later-life outcomes across more waves of these cohorts in our sample.

[17]We also explore the sensitivity of the results to dropping early adopting states from the analysis.

in response to changes in local public sentiment regarding youth drunk driving.[18] Consistent with this narrative, we find no significant relationship between various state socioeconomic conditions or pre-existing alcohol policies and the timing of ZT Law adoption (Table A.2).

Two final estimation details are worth noting. First, the analysis relies on several subjective self-assessed outcomes, including reported activity limitations due to physical/mental health issues and self-reported alcohol consumption. These measures may be subject to considerable reporting error (see Baker, Stabile, and Deri, 2004). Nevertheless, it is unlikely that these reporting errors made in middle-age will be systematically related to alcohol-control policies during adolescence, so they should not bias the main estimates.[19] Second, for statistical inference, standard errors are clustered by state of birth to adjust for heteroskedasticity and within-state correlation over time.

5 Results

5.1 Adolescent Exposure to ZT Laws and Later-Life Health

To motivate the regression analysis and assess the validity of our common trends assumption, we first present 'event study' graphs based on the timing of ZT Law adoption across states. These graphs are based on a generalized version of equation (1), in which the main coefficient, β_{Age}, is allowed to vary with event time $\tau \in \{-5, 3\}$. The dependent variable is an indicator for any self-assessed limitation (physical, cognitive, or visual/auditory).

Figures 1a and b report the results for age groups 35-39 and 40-48, while the bottom panel (Figures 1c and 1d) report the estimates separately for the ages 40-44 and 45-48. Across the four figures, we find no evidence of pre-trends among cohorts in the years leading up to ZT Law passage. The point estimates on all the pre-treatment coefficients – $\beta^{-\tau}_{35-39}$, $\beta^{-\tau}_{40-48}$, $\beta^{-\tau}_{40-44}$, $\beta^{-\tau}_{45-48}$ – are small and statistically insignificant.[20] Among the 40+ age groups, we observe a sharp drop in disability rates among the first cohorts exposed to a ZT Law. Decomposing this overall effect, we see that the

[18]In some specifications, we exclude states that enacted partial youth BAC restrictions during the 1980s, given potential concerns regarding endogenous policy adoption.

[19]Despite reporting error, both self-assessed health and self-reported alcohol consumption have been shown to correlate strongly with more objective measures (Baker, Stabile, and Deri, 2004; Kenkel, 1993). That said, we cannot rule out that adolescent exposure to ZT Laws permanently altered how individuals responded to questions regarding alcohol consumption, independent from actual behavior. This caveat should be kept in mind when interpreting these findings.

[20]F-tests for the joint significance of these pre-treatment effects fail to reject that they are jointly equal to zero.

effects are largest among the oldest age group, age 45-48.[21] In contrast, the effects for $\beta^{+\tau}_{35-39}$ are smaller in magnitude. Taken together, these figures suggest that ZT Laws led to a sharp reduction in disability rates in later life among the first exposed cohorts, and that these relative reductions were not preceded by a gradual longer run trend in improved health.

Table 1 reports the average ZT Law effects from the difference-in-differences version of equation (1). We report the results separately based on different versions of equation (1). Column (1) includes year, birth state, and cohort fixed effects along with a linear birth state-cohort trend. In column (2) we add individual demographic controls for age group, gender, and race. In column (3) we include other state alcohol-related policies and the current unemployment rate. In column (4), we add the vector of birth state - age group dummies.

We find that adolescent exposure to ZT Laws is associated with significant decreases in reported health limitations. Consistent with Figure 1, we also find larger negative effects among older age groups. Among individuals over age 40, the point estimates are consistently large, negative, and statistically significant. These broad patterns are stable across the different specifications and are generally unaffected by the inclusion of individual- or state-level covariates or controls for age-specific state fixed effects. The preferred estimates (col. 4), imply that ZT Laws led to decreases in reported limitation of 3%(= 0.32/9.8) for 40-44 year olds and 8%(= 0.98/12.0) for 45-48 year olds.

In Table 2, we explore the sources of health improvements. We estimate versions of equation (1) separately for three outcome variables: indicators for any physical limitation, any cognitive limitation, or any vision/auditory difficulties. We find no evidence that ZT Laws affected any of these outcomes among the 35-39 year old age groups. For 40-44 year olds, ZT Laws led to significant decreases in physical limitations, with effect sizes ranging from 5 to 8 percent. In contrast, the effects on cognitive limitations are more modest and generally insignificant, and we find no significant effects on vision or hearing difficulties at this age. Meanwhile, we estimate large and statistically significant effects across all three outcomes for the 45-48 year old age group. Together, these results suggest that ZT Laws exposure during adolescence led to broad improvements in both physical and cognitive health, although it appears that the timing of the benefits varied with the underlying limitation, with cognitive and visual/auditory effects emerging at slightly older ages.

[21] The effects for $\beta^{+\tau}_{45-48}$ are much larger but less precise, given the small number of states on which each estimate is identified. For this age group, the evolution of the post-treatment effects should be interpreted with caution, since they are identified off an unbalanced sample of states (see section 4).

5.2 Robustness Checks

Table A.3 reports the results from several alternative specifications and sample restrictions.[22] To begin, we assess whether geographic clustering in the timing of policy enactment can account for the observed effects. We estimate versions of equation (1) that control for cohort-by-region and cohort-by-division fixed effects. These models rely solely on within-region policy variation for identification, so will not be biased by differential trends in health across regions. The results are similar in sign, significance, and magnitude to the baseline findings (cols. 2-3). In column (4), we exclude pre-2008 observations, given a slight change in wording of disabilities in the questionnaire.[23] The results are unaffected by this sample restriction. In column (5), we restrict the sample to white individuals. This restriction addresses concerns that contemporaneous anti-drug policies or changes in police enforcement practices may have differentially impacted long-run outcomes among minorities. The broad patterns are similar. In column (6), we report results from regressions that exclude states that had previously enacted partial BAC restrictions for minors.[24] The results are not sensitive to this sample restriction.

In Tables A.5 and A.6, we assess the sensitivity of the main findings to idiosyncratic trends in any particular early adopting state. Table A.5 reports results from a modified version of equation (1) in which treatment exposure is collapsed to a common effect for all individuals aged 40 years and older, β_{40-48}. This version addresses concerns that the estimates for β_{45-48} are identified based on a small number of early adopting states. Across the various specifications, the results are negative and statistically significant. In Table A.6, we report results from regressions in which we sequentially drop states that enacted ZT Laws by 1994. The estimates are remarkably stable across these models, suggesting that main effects are not driven by any particular outlier early adopting state.[25]

In Table A.8, we explore concerns related to the fact that we measure of ZT Law exposure based on state of birth, which may not reflect state of residence during adolescence. Columns (3)

[22] Table A.4 reports the corresponding estimates separately by type of limitation.

[23] Prior to 2008, the ACS asked respondents whether they had a limitation that lasted at least six months. Beginning in 2008, the ACS no longer inquired about the duration of limitation.

[24] These laws typically covered a subset of minors (see Table A.1). The one exception is Maine, which first enacted a 0.02 BAC restriction on all minors in 1983 and subsequently lowered the limit to 0.00 BAC in 1995.

[25] In Table A.7., we also test for the consistency of our inferences, by estimating Seemingly Unrelated Regressions (SUR) that allows for potential correlation across multiple outcomes. Although the estimates for the 45+ age group are less precise, the broad conclusions remain unchanged.

and (4) report estimates based on the restricted sample of individuals who reside in their state of birth. The point estimates are similar to the baseline findings, albeit marginally less significant given the reduction in sample size. We also estimate versions of equation (1) where the dependent variable is an indicator for whether the individual no longer resides in the state of birth. We find no evidence of endogenous cross-state migration in response to ZT Law enactment (cols. 5, 6). These findings suggest that measurement error caused by unobserved state of residence during adolescence is largely random and not due to, for instance, differential migration of heavy drinking teenagers in an effort to avoid enforcement under ZT Laws.

Finally, in Table A.9 we assess the sources of identifying variation for ZT Law exposure, given the staggered policy timing. Recent econometric research has highlighted concerns regarding the weighting of DD estimators when treatment timing is staggered (Goodman-Bacon, 2021; de Chaisemartin and d'Haultfoeuille, 2020). To address these issues, Panel A reports Goodman-Bacon (2021) decompositions that allow us to assess the sources of identifying variation for each of the age group specific ZT Law effects.[26] We find that a small fraction of the identifying variation is based on 'late' versus 'early' ZT Law adopters, and that for older age groups most of the identifying variation is based on 'never treated' cohorts. Following de Chaisemartin and d'Haultfoeuille (2020), we also report details on the average treatment on the treated (ATT) cells used to construct the DD estimator. For the 40+ age groups, almost all ATTs are positive, and the sum of the negative ATT weights is close to zero. Together these findings suggest that the staggered nature of our treatment is unlikely to cause substantial bias in the estimates.[27]

5.3 Adolescent Exposure to ZT Laws and Later-life Labor Market Outcomes

Table 3 reports the effects of adolescent exposure to ZT Laws on a range of later-life labor market outcomes. Given the distinctive patterns of lifecycle employment outcomes by gender, we estimate the regressions separately for males (Panel A) and females (Panel B). For males, we find large effects on weeks worked, usual hours, and employment status that are concentrated among

[26] We estimate Goodman-Bacon decompositions across each sample year, and report the average of these decompositions across all sample years for each of the three main estimates: β_{35-39}, β_{40-44}, β_{45-48}. We follow this approach since our synthetic-cohort analysis includes repeated observations of treatment units (cohort*state of birth) across multiple sample years, in contrast to the standard two dimensional DD framework.

[27] These findings are unsurprising, given the extended pre-treatment time horizon and the short post-treatment time period in which older age groups are observed. As a result, the majority of comparisons are based on switchers to never-treated cohorts.

the oldest age group. For females, these effects are generally more modest and less significant. In contrast, we find some evidence of positive employment effects for women at younger ages. These patterns mirror the gender-specific effects on health (cols. 1 and 2). Among both groups, we find no evidence of positive effects on earnings. Indeed, the estimates for the 45+ age groups are consistently negative, albeit small in magnitude and largely insignificant. The absence of positive earnings effects could reflect offsetting forces. The increase in average worker productivity due to reduced drinking may have been counteracted by increased participation among lower productivity workers through a selection effect.

The effects of ZT Laws on later-life labor market outcomes are quantitatively important. Multiplying the preferred point estimates for men aged 45-48 (Table 3, col. 4), by median weekly earnings among this age group, we calculate that increases in annual work weeks raised this group's annual earnings by \$736 ($= 0.7 \times \$1,051$) (BLS, 2017). Multiply these estimates by the size of the male population aged 45-48, we estimate that the nationwide rollout of ZT Laws during the 1990s led to long-run annual economic gains of \$6.0 billion dollars by 2017.

To compare the short-run and long-run costs of youth binge drinking, we can rescale the ZT Law impact by the 'first stage' impact of the laws on adolescent binge drinking. Combining previous estimates of the short-run effects of ZT Laws on youth binge drinking with their long-run impact on annual earnings, we calculate an implied long-run economic cost of youth drinking of \$10.7 billion per year.[28] This estimate should be interpreted with caution, given that ZT Laws may influence later-life outcomes through a number of channels other than youth binge drinking, which would lead us to overstate the long-run costs.

Our long-run cost estimates are comparable to previous estimates of the short-run economic costs associated with adolescent binge drinking, which are on the order of \$50 billion per year (Bonnie and O'Connell, 2004). Moreover, they do not account for the potential for improved labor market outcomes as the affected cohorts continue to age. Projecting forward to age 64, assuming a constant marginal impact on labor market outcomes, we calculate that the implied long-run costs associated with youth binge drinking would be \$48 billion per year. These calculations do not account for any non-pecuniary benefits associated with improved adult health or increased

[28]Carpenter (2004) finds that ZT Laws reduced youth binge drinking by 17 percent. Our calculation is obtained by dividing the long-run economic gains (\$6.0 billion) by this estimate and discounting over a 30-year time horizon at a 4 percent interest rate.

longevity which are also likely to be large.

5.4 Mechanisms

What explains the relationship between exposure to ZT Laws in adolescence and later-life health and labor market outcomes? In this section, we explore the mechanisms underlying these long-run effects.

First, we explore the extent to which changes in educational attainment and marriage entry can account for the later-life outcomes. Table 4, cols. 4-5 report the effects of ZT Laws on high school completion and college completion. The point estimates are small in magnitude and generally not statistically significant, suggesting that increased educational attainment cannot account for the improved health and labor market outcomes among older age groups. Nevertheless, the estimates for education are too small to account for the improvements in later-life health.[29] [30] Similarly, we find no evidence that cohorts exposed to ZT laws were more likely to marry (col. 6), suggesting that the estimated decline in reported disabilities cannot be attributed a marriage-health premium (Ross, Mirowsky and Goldsteen, 1990; Wood, Goesling and Avellar, 2007).

Second, the results may capture the benefits of ZT Laws through reduced teenage motor vehicle accidents, given the potentially long-lasting medical consequences for occupants injured in car crashes (Gustafsson et al., 2015; Stigson et al., 2015; Beck and Coffey, 2006). In practice, however, the underlying risks of serious motor vehicle accident were simply too low to account for the observed improvements in later-life health. Indeed, we calculate that less than 3 percent of our observed effects can plausibly be attributable to this channel.[31] Similarly, the results cannot be attributed to sample selection due to reduced rates of vehicle fatalities in states that were early adopters of ZT Laws.[32]

[29] Applying Oreopoulos's (2007) estimates of the impact of schooling on self-assessed health, and assuming that individuals who graduated high school and college as a result of ZT Laws obtained an additional two years of schooling than they otherwise would have, we calculate that increases in education can account for less than 15 percent of the decline in reported health limitations among women.

[30] When we control for both education and ever married status in the health regressions (Table A.10), the point estimates for ZT Laws remain largely unchanged.

[31] Among individuals aged 15 to 20, the rate of motor vehicle traffic injuries related to alcohol was 235 per 100,000 (CDC, 1993; NTSA, 1996), of which we assume 12 percent were associated with long-term medical impairment (Stigson et al., 2015). Even if the adoption of ZT Laws fully eliminated alcohol-related teenage motor vehicle accidents, the implied reduction in the probability of long-term disability is 0.028% = $((235/100,000) \times 0.12)$, less than 3 percent of the main effect reported in Table 1.

[32] A similar calculation based on the underlying rates of alcohol-related fatalities among teenagers indicates that this channel can account for less than 0.8% of the estimated effects.

Third, it is possible that ZT Laws reduced initiation to binge drinking at a critical age period, and given the importance of habit-formation for heavy drinking, ultimately led to decreases in heavy consumption in later adulthood. To explore this possibility, we use data from the BRFSS to estimate regressions that link exposure to ZT Laws during adolescence to alcohol consumption in later-life.

Table 4 (cols. 1-3) reports the results. We find clear evidence that exposure to ZT Laws during adolescence reduced heavy alcohol consumption during later-life. We estimate significant effects on both the average number of drinks per sitting and frequency of heavy episodic drinking, particularly among older age groups.[33] In contrast, we find no systematic evidence that the laws reduced moderate drinking in later-life (col. 3). For individuals aged 40 to 44, the point estimates for any alcohol consumption are negative and significant, but small in magnitude.[34] Meanwhile, the coefficient estimates for any alcohol consumption in the previous month are positive but not significant for the oldest age group.

The results suggest that exposure to ZT Laws during adolescence led to persistent decreases in heavy episodic drinking, and in fact, may have fostered more responsible drinking among older individuals. Given the harmful effects of long-term heavy drinking on physical and cognitive health (WHO, 2018), these changes in adult alcohol consumption may account for the persistent impacts of ZT Laws on later-life health. Nevertheless, these results do not rule out the possibility that heavy adolescent drinking has negative effects on long-term health, independently from later-life drinking patterns (White and Swartzwelder, 2004; Taffe et al., 2010).

A final possibility is that ZT Laws affected later-life alcohol consumption and health directly, by increasing the rates of DUI arrests among adolescents (see Carpenter, 2007). While a DUI arrest could harm long-run outcomes by making it more difficult to obtain college admissions or employment opportunities, it may also have led to a decrease in future alcohol consumption given that individuals were typically mandated to attend alcohol education programs.[35] Nevertheless,

[33] The differential effects could reflect true treatment heterogeneity by age. Alternatively, the patterns may reflect evolution in the effectiveness of ZT Laws over time. To the extent that youths gradually adapted to the policies, they may have found alternative ways to continue drinking despite the restrictions (Bellou and Bhatt, 2013). Since younger age groups are disproportionately comprised of individuals exposed to laws that had already been in operation for a number of years, the estimated effects on their long-run drinking would be expected to be smaller.

[34] Since ZT Laws may have induced some binge drinkers to abstain entirely, the -0.021 coefficient likely overestimates the negative impact of the laws on participation in moderate drinking.

[35] A large literature suggests that exposure to these alcohol treatment programs is associated with decreases in the probability of subsequent DUI arrest (Wells-Parker et al., 1995)

the rates of DUI arrests under the new ZT Laws were simply too low to account for the overall improvement in later-life health.[36] Instead, the indirect effects of these laws, by reducing alcohol consumption among the broader population of adolescents, appears to be the main driver of the long-run health improvements.

6 Conclusion

The rollout of ZT Laws during the 1990s led to sharp reduction in adolescent binge drinking among affected cohorts. Despite the fact that individuals were subject to these laws for a brief period during late adolescence, we document significant improvements in later-life health and labor-market outcomes. The health and labor market impacts were concentrated among the oldest age groups, suggesting that the harms from youth drinking may intensify with age.

The results suggest substantial long-run costs from heavy adolescent drinking. Simple calculations, based on the forgone earnings of middle aged workers, indicate that the long-run economic costs may exceed the typical short-run cost estimates from adolescent binge drinking. Future work might explore the extent to which these costs extend through middle age, and whether the deterioration in self-reported health status translated into increased risk of long-run disability, morbidity, or mortality.

The persistent improvements in health and labor market outcomes, following temporary exposure to ZT Laws, highlights the critical role of habit-formation for long-run substance use. Indeed, we find that individuals exposed to these policies were substantially less likely to drink heavily in later-life. Our findings are consistent with theoretical models of addictive goods that highlight the importance of conditions at initiation for later-life consumption (Becker and Murphy, 1988). The findings also illustrate the potential scope for policy to influence these initiation decisions and ultimately shape outcomes over the lifecycle.

[36] A simple calculation that compares the change in the probability of adolescent DUI after a ZT Law passed to later-life health outcomes across cohorts indicates that less than 10 percent of the overall decline in disability can be attributed to this mechanism.

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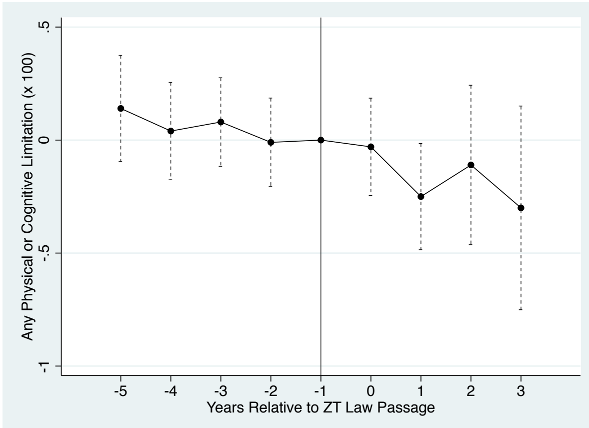
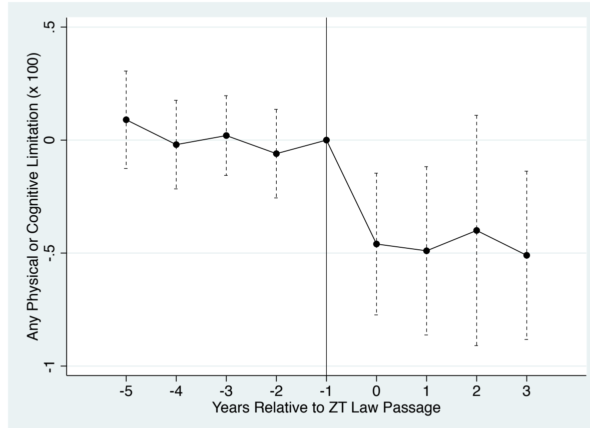
7 Figures and Tables

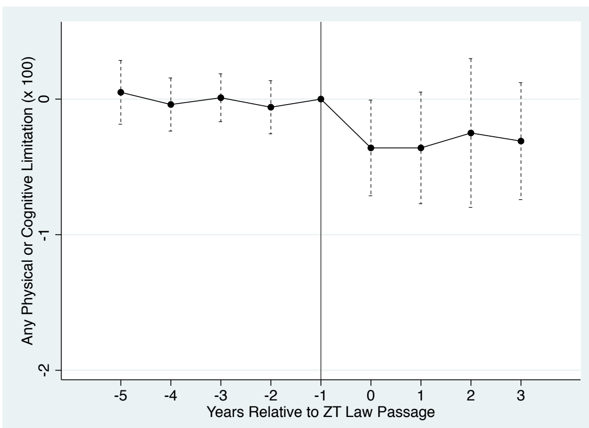
Figure 1: Event Study Estimates



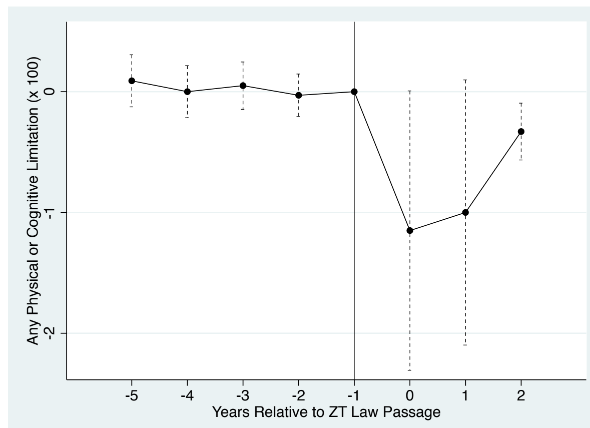
(a) Age 35-39



(b) Age 40-48



(c) Age 40-44



(d) Age 45-58

Notes: These figures report the 'event study' estimates based on a generalized version of equation (1), separately for age groups 35-39, 40-48, 40-44, 45-48. The dependent variable is an indicator for any physical or cognitive limitation ($\times$ 100). The coefficients plot the time path for β in event-time from $\tau \in \{-5, 3\}$ for cohorts treated by ZT Laws relative to control cohorts. Vertical dotted lines denote the 95% confidence interval based on standard errors clustered by state of birth. P-values from tests of joint significance for the pre-treatment coefficients are 0.56 for the 35-39 age group, 0.45 for the 40-48 age group, 0.68 for the 40-44 age group, and 0.69 for the 45-48 age group.

Table 1: Effects of Early ZT Law Exposure on Later-Life Health

Dependent Variable	Mean Dep. Var.	Any Physical or Cognitive Limitation (× 100) (1)	(2)	(3)	(4)
Early ZT Law Exposure					
× Age 35-39	7.8	0.03	-0.09	-0.09	-0.12
		(0.11)	(0.11)	(0.11)	(0.12)
× Age 40-44	9.8	-0.60	-0.42	-0.42	-0.32
		(0.13)***	(0.13)**	(0.13)**	(0.15)**
× Age 45-48	12.0	-1.12	-0.83	-0.81	-0.98
		(0.27)***	(0.26)**	(0.28)**	(0.28)***
Year, birth state, & cohort FEs		Y	Y	Y	Y
Birth state-cohort trend		Y	Y	Y	Y
Demographic controls			Y	Y	Y
State controls				Y	Y
Birth state × Age group FEs					Y
		Observations = 9,914,094			

Notes: Each column reports the point estimate from a different regression. Demographic controls include 5-year age group dummies, sex, and race. State controls include the current unemployment rate and beer excise tax, and state minimum legal drinking age, drunk driving laws, and vertical identification card laws in adolescence. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table 2: Effects of Early ZT Law Exposure on Physical, Cognitive, and Visual/Auditory Limitations

	Mean Dep. Var.	(1)	(2)	(3)	(4)
		Any Physical Limitation (× 100)			
Early ZT Law Exposure					
× Age 35-39	4.1	0.04	-0.03	-0.03	-0.03
		(0.09)	(0.10)	(0.10)	(0.10)
× Age 40-44	5.8	-0.47	-0.36	-0.36	-0.32
		(0.10)***	(0.10)***	(0.10)***	(0.10)**
× Age 45-48	7.6	-0.78	-0.52	-0.49	-0.43
		(0.12)***	(0.12)***	(0.14)**	(0.10)***
		Any Cognitive Limitation (× 100)			
Early ZT Law Exposure					
× Age 35-39	3.8	-0.07	-0.06	-0.06	-0.08
		(0.08)	(0.08)	(0.08)	(0.09)
× Age 40-44	4.3	-0.15	-0.16	-0.15	-0.10
		(0.08)*	(0.08)*	(0.08)*	(0.08)
× Age 45-48	5.0	-0.64	-0.66	-0.63	-0.73
		(0.11)***	(0.11)***	(0.11)***	(0.27)**
		Any Visual/Auditory Limitation (× 100)			
Early ZT Law Exposure					
× Age 35-39	2.2	0.05	-0.04	-0.05	-0.08
		(0.06)	(0.06)	(0.06)	(0.06)
× Age 40-44	2.8	-0.19	-0.04	-0.05	-0.01
		(0.07)**	(0.08)	(0.08)	(0.09)
× Age 45-48	3.7	-0.35	-0.27	-0.28	-0.35
		(0.12)**	(0.11)**	(0.11)**	(0.10)**
Year, birth state, & cohort FEs		Y	Y	Y	Y
Birth state-cohort trend		Y	Y	Y	Y
Demographic controls			Y	Y	Y
State controls				Y	Y
Birth state × Age group FEs					Y
		Observations = 9,914,094			

Notes: Each column reports the point estimate from a different regression. Demographic controls include 5-year age group dummies, sex, and race. State controls include the current unemployment rate and beer excise tax, and state minimum legal drinking age, drunk driving laws, and vertical identification card laws in adolescence. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table 3: Effects of Early ZT Law Exposure on Labor Market Outcomes

	Dependent variable:									
	Any Disability		Weeks Worked Last Year		Usual Hours per Week		Currently Employed (× 100)		Log Earnings, Full-time Workers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Males										
Mean Dep. Var.	11.1		42.6		39.7		83.7		5.53	
Early ZT Law Exposure (< age 21)										
× Age 35-39	0.09	0.08	0.10	0.10	0.03	0.03	0.17	0.17	0.004	0.005
	(0.17)	(0.17)	(0.11)	(0.11)	(0.10)	(0.10)	(0.24)	(0.23)	(0.005)	(0.004)
× Age 40-44	-0.43	-0.36	0.14	0.15	-0.06	-0.04	0.25	0.27	-0.001	-0.001
	(0.18)**	(0.19)*	(0.10)	(0.10)	(0.11)	(0.12)	(0.19)	(0.22)	(0.005)	(0.005)
× Age 45-48	-1.27	-1.13	0.72	0.70	1.02	1.08	1.21	1.07	-0.044	-0.035
	(0.38)**	(0.47)**	(0.18)***	(0.28)**	(0.40)**	(0.44)**	(0.51)**	(0.54)*	(0.021)**	(0.021)*
	Observations = 4,770,985								Obs = 3,217,321	
Females										
Mean Dep. Var.	11.3		36.6		30.2		73.0		5.21	
Early ZT Law Exposure (< age 21)										
× Age 35-39	-0.27	-0.32	0.28	0.30	0.32	0.33	0.70	0.76	0.006	0.007
	(0.14)*	(0.15)*	(0.14)*	(0.15)**	(0.13)**	(0.13)**	(0.30)**	(0.31)**	(0.005)	(0.005)
× Age 40-44	-0.42	-0.28	0.09	0.03	0.07	-0.00	0.39	0.22	0.006	0.005
	(0.16)**	(0.17)	(0.15)	(0.14)	(0.12)	(0.11)	(0.32)	(0.30)	(0.006)	(0.006)
× Age 45-48	-0.37	-0.84	-0.3	0.16	-0.30	-0.01	-0.04	0.89	-0.023	-0.036
	(0.72)	(0.81)	(0.27)	(0.46)	(0.23)	(0.36)	(1.12)	(1.31)	(0.017)	(0.022)
	Observations = 5,143,109								Obs = 2,502,079	
Baseline controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Birth state × Age group FEs	N	Y	N	Y	N	Y	N	Y	N	Y

Notes: Each column reports the point estimate from a different regression. Odd columns report estimates with the full set of controls from Table 1, column (3). Even columns report estimates with the full set of controls from Table 1, column (4). Full-time workers are individuals who were employed for at least 50 weeks during the previous year and report usually working at least 35 hours per week. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

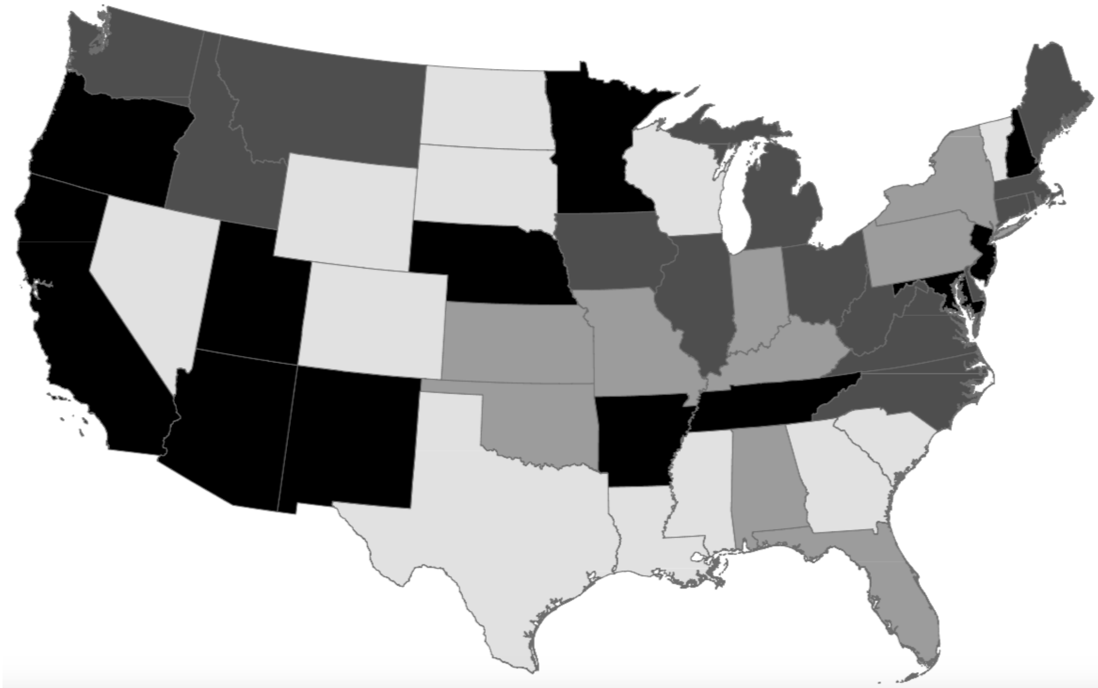
Table 4: Mechanisms: Long-term Alcohol Consumption, Education, and Marriage

Dependent Variable	Alcohol consumption in previous 30-days			Educational attainment & marriage		
	Ave. Drinks per Drinking Episode	=1, if Binge Drank	Any Alcohol	High School Graduate	College Graduate	Ever Married
	(1)	(2)	(3)	(4)	(5)	(6)
Mean Dep. Var.	2.3	0.27	0.53	0.93	0.56	0.84
Early ZT Law Exposure (< age 21)						
× Age 35-39	-0.073	-0.003	-0.001	0.003	0.004	-0.001
	(0.015)***	(0.003)	(0.004)	(0.001)**	(0.002)*	(0.002)
× Age 40-44	-0.08	-0.007	-0.013	0.004	0.000	0.002
	(0.022)***	(0.004)*	(0.005)**	(0.002)**	(0.002)	(0.002)
× Age 45-48	-0.193	-0.021	0.007	0.003	-0.004	0.014
	(0.027)***	(0.008)**	(0.015)	(0.002)	(0.013)	(0.012)
Full controls	Y	Y	Y	Y	Y	Y
Observations	1,324,156	1,327,767	2,704,197	9,914,094	9,914,094	9,914,094

Notes: Each column reports the point estimate from a different regression. All models include the full controls described in column (4) of Table 1. All drinking variables are constructed based on reported alcohol consumption in the previous 30-days. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

A Appendix (For Online Publication)

Figure A.1: Timing of Zero Tolerance (ZT) Law Adoption Across States



Notes: This map identifies states according to the timing of ZT Law adoption. We classify states by four adoption periods – prior to January, 1994; January 1994 to December 1995; January 1996 to December 1996; post January 1997 – where darker shades denote earlier periods of adoption. ZT Laws are defined as BAC restrictions of 0.02 or less that apply to all individuals below age 21 (see Table A.1).

Table A.1: Dates of Zero Tolerance (ZT) Law Adoption

State	Date Law Effective	State	Date Law Effective
Alabama	1996-05	Montana	1995-10
Alaska	1996-11	Nebraska	1994-01
Arizona	1990-06	Nevada	1997-07
Arkansas	1993-08	New Hampshire	1993-01
California	1994-01	New Jersey	1992-12
Colorado	1997-07	New Mexico	1994-01
Connecticut	1995-10	New York	1996-11
Delaware	1995-07	North Carolina*	1995-09
DC	1994-05	North Dakota	1997-07
Florida	1997-01	Ohio*	1994-05
Georgia	1997-07	Oklahoma*	1996-11
Hawaii	1997-12	Oregon*	1991-07
Idaho	1994-04	Pennsylvania	1996-08
Illinois	1995-01	Rhode Island	1995-06
Indiana	1997-01	South Carolina	1998-06
Iowa	1995-07	South Dakota	1998-07
Kansas	1997-01	Tennessee	1993-07
Kentucky	1996-01	Texas	1997-09
Louisiana	1997-07	Utah	1992-07
Maine*	1995-09	Vermont*	1997-09
Maryland	1990-05	Virginia	1994-07
Massachusetts	1994-06	Washington	1994-07
Michigan	1994'11	West Virginia	1994-06
Minnesota	1993-06	Wisconsin*	1997-10
Mississippi	1998-08	Wyoming	1998-07
Missouri	1996-08		

Notes: Zero Tolerance (ZT) Laws are BAC restriction of 0.2 percent or less that cover all individuals under age 21. Asterices denote states that had previously implemented partial BAC restrictions for minors: BAC restriction for individuals under age 18 (North Carolina 1983-09, Ohio 1990-07, Oklahoma 1995-07, Oregon 1989-10; Vermont 1991-07); BAC restriction for individuals under age 19 (Wisconsin 1984-07); BAC restriction for individuals under age 21 (Maine 1983-07).

Table A.2: 1990 State-level Predictors of Zero Tolerance (ZT) Laws

Dependent Variable	Year of ZT Law Adoption - 1990			
	(1)	(2)	(3)	(4)
Demographic characteristics				
% urban	3.82	3.42	1.22	1.21
	(2.31)	(2.54)	(3.33)	(3.33)
% college	-3.69	-2.69	-1.00	-1.07
	(5.19)	(5.89)	(5.59)	(5.65)
% white	-3.01	-2.95	-0.29	-0.29
	(1.90)	(2.09)	(2.46)	(2.51)
Labor market characteristics				
Log(med. hh. income)		-1.26	-2.02	-2.04
		(2.47)	(2.49)	(2.56)
Unempl. rate		-0.056	-0.01	-0.001
		(0.24)	(0.25)	(0.29)
Alcohol and traffic safety related regulations				
Earlier adoption of BAC law			-0.23	-0.23
			(1.18)	(1.23)
BAC 0.08 law			-2.44	-2.44
			(1.60)	(1.62)
License revocation law			0.39	0.39
			(0.62)	(0.61)
Social host law			0.18	0.17
			(0.55)	(0.57)
Beer tax			2.46	2.48
			(1.50)	(1.66)
Seat belt primary occupant			0.22	0.23
			(0.64)	(0.68)
Seat belt secondary occupant			0.62	0.62
			(0.80)	(0.82)
Traffic fatality rate per				-0.03
100 million vehicle miles				(0.93)
	Observations = 50			

Notes: Each column reports the point estimate from a different regression. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.3: Alternate Controls and Samples

		Additional controls		Alternate samples		
	Baseline estimates	Add region × year fixed effects	Add district × year fixed effects	Drop pre-2008 observations	Whites	Drop states with prior BAC law for minors
	(1)	(2)	(3)	(4)	(5)	(6)
	Any Physical or Cognitive Limitation (× 100)					
Early ZT Law Exposure (< age 21)						
× Age 35-39	-0.12	-0.15	-0.11	-0.17	-0.16	-0.10
	(0.12)	(0.12)	(0.11)	(0.09)*	(0.11)	(0.12)
× Age 40-44	-0.32	-0.36	-0.32	-0.28	-0.33	-0.34
	(0.15)**	(0.15)**	(0.16)*	(0.13)**	(0.15)**	(0.17)*
× Age 45-48	-0.98	-0.95	-0.92	-0.86	-0.85	-0.99
	(0.28)***	(0.26)***	(0.29)**	(0.12)***	(0.44)*	(0.30)**
Full controls	Y	Y	Y	Y	Y	Y
Observations	9,914,094	9,914,094	9,914,094	6,421,389	8,438,982	8,554,304

Notes: Each column reports the point estimate from a different regression. All models include the full controls described in column (4) of Table 1. See Table A.1 for states with prior partial BAC restrictions for minors. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.4: Effects on Physical, Cognitive, and Visual/Auditory Limitations: Alternate Controls and Samples

		Additional controls		Alternate samples		
	Baseline estimates	Add region × year fixed effects	Add district × year fixed effects	Drop pre-2008 observations	Whites	Drop states with prior BAC law for minors
	(1)	(2)	(3)	(4)	(5)	(6)
	Any Physical Limitation (× 100)					
Early ZT Law Exposure (< age 21)						
× Age 35-39	-0.03	-0.05	-0.03	-0.02	-0.07	-0.04
	(0.10)	(0.09)	(0.09)	(0.08)	(0.09)	(0.11)
× Age 40-44	-0.32	-0.36	-0.33	-0.2	-0.35	-0.29
	(0.10)**	(0.10)***	(0.10)**	(0.08)**	(0.08)***	(0.11)**
× Age 45-48	-0.43	-0.4	-0.39	-0.65	-0.32	-0.41
	(0.10)***	(0.08)***	(0.09)***	(0.08)***	(0.39)	(0.11)***
	Any Cognitive Limitation (× 100)					
Early ZT Law Exposure (< age 21)						
× Age 35-39	-0.08	-0.08	-0.07	-0.11	-0.07	-0.06
	(0.09)	(0.09)	(0.09)	(0.08)	(0.09)	(0.10)
× Age 40-44	-0.1	-0.1	-0.09	-0.03	-0.12	-0.13
	(0.08)	(0.08)	(0.09)	(0.08)	(0.09)	(0.09)
× Age 45-48	-0.73	-0.71	-0.73	-0.62	-0.84	-0.87
	(0.27)**	(0.27)**	(0.30)**	(0.16)***	(0.13)***	(0.26)**
	Any Visual/Auditory Limitation (× 100)					
Early ZT Law Exposure (< age 21)						
× Age 35-39	-0.08	-0.09	-0.07	-0.04	-0.07	-0.06
	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.06)
× Age 40-44	-0.01	-0.02	-0.01	-0.15	-0.02	-0.02
	(0.09)	(0.08)	(0.09)	(0.08)*	(0.09)	(0.10)
× Age 45-48	-0.35	-0.36	-0.36	-0.38	-0.45	-0.44
	(0.10)**	(0.09)***	(0.10)***	(0.17)**	(0.08)***	(0.09)***
Full controls	Y	Y	Y	Y	Y	Y
Observations	9,914,094	9,914,094	9,914,094	6,421,389	8,438,982	8,554,304

Notes: Each column reports the point estimate from a different regression. All models include the full controls described in column (4) of Table 1. See Table A.1 for states with prior partial BAC restrictions for minors. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.5: Collapse to Common Age 40+ Treatment

	Full Sample		Males		Females	
	(1)	(2)	(3)	(4)	(5)	(6)
	Any Physical or Cognitive Limitation (× 100)					
Early ZT Law Exposure (< age 21)						
× Age 35-39	-0.10	-0.12	0.08	0.08	-0.27	-0.32
	(0.11)	(0.11)	(0.17)	(0.17)	(0.14)*	(0.15)**
× Age 40-48	-0.44	-0.34	-0.46	-0.39	-0.42	-0.3
	(0.13)**	(0.14)**	(0.18)**	(0.19)**	(0.15)**	(0.16)*
Baseline controls	Y	Y	Y	Y	Y	Y
Birth state × Age group FEs	N	Y	N	Y	N	Y
Observations	9,914,094		4,770985		5,143,109	

Notes: Each column reports the point estimate from a different regression. Odd columns report estimates with the full set of controls from Table 1, column (3). Even columns report estimates with the full set of controls from Table 1, column (4). Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.6: Sequentially Drop States that Implemented Early ZT Laws

	Dependent Variable: Any Physical or Cognitive Limitation (× 100)									
		Drop States that Passed ZT Law before 1993					Drop States that Passed ZT Law in 1993			
	Baseline Estimates	AZ	MD	OR	UT	NJ	AR	MN	NH	TN
Early ZT Law Exposure										
× Age 35-39	-0.12	-0.14	-0.12	-0.09	-0.11	-0.11	-0.14	-0.12	-0.13	-0.13
	(0.12)	(0.12)	(0.12)	(0.11)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
× Age 40-44	-0.32	-0.35	-0.31	-0.34	-0.35	-0.32	-0.28	-0.37	-0.3	-0.34
	(0.15)**	(0.15)**	(0.16)*	(0.15)**	(0.15)**	(0.16)*	(0.15)*	(0.14)**	(0.15)*	(0.15)**
× Age 45-48	-0.98	-0.73	-1.41	-0.97	-0.96	-0.98	-0.97	-1	-0.98	-0.99
	(0.28)***	(0.16)***	(0.11)***	(0.30)**	(0.28)**	(0.27)***	(0.28)***	(0.27)***	(0.28)***	(0.28)***
Observations	9,914,094	9,823,613	9,756,864	9,821,099	9,835,618	9,622,635	9,818,842	9,698,061	9,877,634	9,721,108

	Drop States that Passed ZT Law in 1994										
	CA	DC	ID	MA	MI	NE	NM	OH	VA	WA	WV
Early ZT Law Exposure											
× Age 35-39	-0.14	-0.12	-0.12	-0.13	-0.09	-0.11	-0.12	-0.1	-0.12	-0.12	-0.12
	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
× Age 40-44	-0.25	-0.34	-0.32	-0.31	-0.3	-0.32	-0.33	-0.31	-0.34	-0.34	-0.34
	(0.16)	(0.15)**	(0.15)**	(0.15)**	(0.16)*	(0.15)**	(0.15)**	(0.16)**	(0.15)**	(0.15)**	(0.15)**
× Age 45-48	-1	-0.99	-0.98	-0.98	-0.97	-0.98	-0.98	-1	-1	-0.99	-0.99
	(0.27)***	(0.28)***	(0.28)***	(0.28)***	(0.28)**	(0.28)***	(0.28)***	(0.28)***	(0.28)***	(0.28)***	(0.28)***
Observations	9,066,392	9,849,770	9,868,602	9,649,421	9,462,847	9,825,303	9,849,442	9,386,378	9,703,317	9,755,885	9,814,266

Notes: Each column reports the point estimate from a different regression. Each regressions omits the specified early adopting state from the analysis. All models include the full set of controls described in column (4) of Table 1. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.7: Adjust for Multiple Hypothesis Testing

Dependent Variable	Any Physical or Cognitive Limitation (× 100)		
	Baseline estimates	P-value	P-value adjusted for outcome corr. with labour market, education, & marriage outcomes
	(1)	(2)	(3)
Early ZT Law Exposure			
× Age 35-39	-0.12	0.317	0.183
	(0.12)		
× Age 40-44	-0.32	0.033**	0.004***
	(0.15)		
× Age 45-48	-0.98	<0.001***	0.070*
	(0.28)		
Full controls	Y		
	Observations = 9,914,094		

Notes: Column (1) reports the baseline estimates from column (4) of Table 1. Column (2) reports the P-values that correspond to the baseline estimates. Column (3) reports the P-values from Seemingly Unrelated Regressions (SUR) that adjust for inference across multiple outcomes (weeks worked, usual hours, currently employed, high school graduate, college graduate, and ever married). ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.8: Effects of ZT Laws on Migration

	Dependent variable:					
	Any physical or cognitive limitation (× 100)				Does not live in state of birth (× 100)	
	Baseline estimates		Restricted sample: Lives in state of birth			
	(1)	(2)	(3)	(4)	(5)	(6)
Early ZT Law Exposure (< age 21)						
× Age 35-39	-0.09	-0.12	0.01	-0.01	-0.11	-0.19
	(0.11)	(0.12)	(0.16)	(0.16)	(0.33)	(0.35)
× Age 40-44	-0.42	-0.32	-0.37	-0.25	-0.09	0.06
	(0.13)**	(0.15)**	(0.15)**	(0.17)	(0.40)	(0.37)
× Age 45-48	-0.81	-0.98	-0.90	-1.25	1.57	1.00
	(0.28)**	(0.28)***	(0.34)**	(0.29)***	(1.16)	(1.78)
Full controls	N	Y	N	Y	N	Y
Observations	9,914,094		6,107,009		9,914,094	

Notes: Each column reports the point estimate from a different regression. Odd columns report estimates with the full set of controls from Table 1, column (3). Even columns report estimates with the full set of controls from Table 1, column (4). In columns (3) and (4) the sample is restricted to respondents who currently reside in their state of birth. In columns (5) and (6) the dependent variable is an indicator (× 100) for whether the respondent does not currently reside in their state of birth. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.9: Assess Weighting in Difference-in-Differences Estimates

	Effect of Early ZT Law Exposure DD Estimates by Age Group		
	Age 35-39	Age 40-44	Age 45-48
Panel A: Weights from Goodman-Bacon (2018) decompositions			
Early Treated vs. Later Control	0.389	0.198	0.016
Later Treated vs. Earlier Control	0.185	0.052	0.003
Treated vs. Never Treated	0.426	0.750	0.981
Panel B: Weights from de Chaisemartin & d'Haultefoeuille (2020)			
Number of positive ATTs	269	124	9
Number of negative ATTs	125	15	0
Sum of positive weights	1.141	1.013	1
Sum of negative weights	-0.141	-0.013	0

Notes: Panel A reports the weights based on Goodman-Bacon (2018) decompositions. We conduct the decompositions separately for each of the three age groups: 35-39, 40-44, and 45-48. The decomposition is calculated separately for each sample year, and the reported weights are calculated as the average across all sample years. Panel B reports the weights for each of the three age groups, based on the approach described in de Chaisemartin & d'Haultefoeuille (2020).

Table A.10: Mechanisms: Controlling for Education and Marriage

Dependent Variable	Any Physical or Cognitive Limitation (× 100)		
	Baseline estimates	Control for education	Control for education & ever married
	(1)	(2)	(3)
All			
Early ZT Law Exposure			
× Age 35-39	-0.12	-0.05	-0.06
	(0.12)	(0.12)	(0.12)
× Age 40-44	-0.32	-0.26	-0.25
	(0.15)**	(0.16)	(0.16)
× Age 45-48	-0.98	-0.96	-0.87
	(0.28)***	(0.34)**	(0.41)**
	Observations = 9,914,094		
Males			
Early ZT Law Exposure			
× Age 35-39	0.08	0.17	0.19
	(0.17)	(0.16)	(0.16)
× Age 40-44	-0.36	-0.27	-0.25
	(0.19)*	(0.19)	(0.19)
× Age 45-48	-1.13	-1.21	-1.07
	(0.47)**	(0.39)**	(0.34)**
	Observations = 4,770,985		
Females			
Early ZT Law Exposure			
× Age 35-39	-0.32	-0.26	-0.28
	(0.15)*	(0.15)*	(0.15)*
× Age 40-44	-0.28	-0.26	-0.26
	(0.17)	(0.18)	(0.18)
× Age 45-48	-0.84	-0.71	-0.66
	(0.81)	(0.89)	(0.99)
	Observations = 5,143,109		
Full controls	Y	Y	Y
Education controls		Y	Y
Ever married controls			Y

Notes: Each column reports the point estimate from a different regression. All models include the full controls described in column (4) of Table 1. Educational controls include separate indicators for high school and college graduates. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.