# The long-run impacts of adolescent drinking: Evidence from Zero Tolerance Laws

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### ABSTRACT

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.

## 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 and 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 2019. 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 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

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[1] Excessive alcohol consumption or "binge drinking" is typically defined as five or more alcoholic drinks for males and 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.

find that individuals exposed to ZT Laws during adolescence were 3 percent less likely to report a physical or cognitive limitation by ages 40–49. In contrast, we find no significant effects among the 35–39 age group, suggesting that the health impacts only materialized after an extended lag, 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 physical limitations and less likely to suffer visual/auditory limitations in middle age. We also find some evidence of reduced incidence of cognitive limitations among affected cohorts. 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 $18 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 from $27 to $50 billion annually (Bonnie and O'Connell, 2004; Bouchery et al., 2006).

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 graduation rates, but were no more likely to marry. Nevertheless, the effect sizes for education 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).

Finally, 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 highlights the importance of conditions at initiation for long-run consumption of addictive substances (Becker and Murphy, 1988; DeCicca et al., 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 et al., 2011), risky sexual behavior (Dee, 2001b; Fertig and Watson, 2009), crime (Carpenter, 2005; Carpenter and Dobkin, 2015), and mortality (Dee and Evans, 2001; Carpenter and Dobkin, 2009, 2011; Carpenter et al., 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 requirements for persons under age 21.[4] Subsequent legislation under the National Highway Systems Design Act in

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[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.

[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.

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 developments (Spear, 2016; Miranda 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 et al., 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 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 2019 (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 cohorts and birth states for individuals born between 1969 and 1984, although we also include older cohorts to better control for state-specific trends in outcomes (see Table A.2).[10] Finally, 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

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[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 Oleson, 1994). In our empirical analysis, we address for potential regional clustering in the timing of policy adoption.

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

[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.

[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'Haultfoeuille (2020), since the estimates rely more heavily on comparisons across treatment 'switchers' to untreated cohorts.

[11] Physical limitations include conditions that substantially limit one or more basic physical activities 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.

2019. We identify exposure to ZT Laws based on respondent's birth year and current state of residence. We construct several measures of alcohol consumption during the previous month including: average number of drinks consumed per episode of drinking, whether the individual was a heavy drinker, an indicator for at least one binge drinking episode in the previous 30 days, and whether the individual consumed any alcohol in the past 30 days.[12]

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.[13] 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.

Figure A.2 reports the mean disability rates by years of exposure to ZT Laws for age groups 35–39, 40–44, and 45–49. Each outcome is reported relative to cohorts aged 21 at the time of enactment (who were too old to be affected). For individuals over age 40, the figures show a sharp reduction in mean disability rates among the first cohorts exposed to the policy. Meanwhile, we find that relative disability rates were stable across cohorts that were slightly too old to be affected by the policy. These means motivate the difference-in-difference framework described in the following section.

## 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:

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

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 individual age dummies, gender, and a dummy for white. State-level covariates include the current unemployment rate to control for contemporaneous economic conditions and contemporaneous state beer excise taxes, as well as a series of 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, and vertical identification card laws.[14]

Eq. (1) also includes a series of fixed effects, $\lambda_c$, $\delta_s$, and $\eta_t$, that represent indicators 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.[15]

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 specific age groups, $Age_{icst}$ to allow the effects of early exposure to ZT Laws to vary with age. In our main specification, $Age_{icst}$ is an indicator for whether an individual is over age 40. We also interact ZT Laws with a set of 5-year age group dummy variables (35–39, 40–44, and 45–49), to allow the effects to differ across each 5-year age group. The estimates $\beta_{Age}$ capture how exposure to ZT Laws during adolescence affects later-life outcomes for individuals within each age group. We report estimated impacts for all individuals over age 40, $\beta_{40+}$, as well as the estimated impacts by 5-year age group, $\beta_{35-39}$, $\beta_{40-44}$, and $\beta_{45-49}$.[16] Given the extended lag between adolescent treatment and observed outcomes towards middle age, the estimates of $\beta_{Age}$ are weighted more heavily based on policy changes that occurred among the earlier adopting states (see Table A.2). This is particularly true for the coefficient associated with the oldest age group, $\beta_{45-49}$, which is identified solely based on policy changes in states that enacted ZT Laws by 1995. In contrast, because there is a shorter lag between adolescent treatment and observed outcomes, the estimates for $\beta_{40+}$, $\beta_{35-39}$, and $\beta_{40-44}$ are identified based on all state ZT Laws adopted from 1990 to 1998. Given potential concerns regarding the disproportionate influence of early adopting states on the estimates, we explore the sensitivity of the results to sequentially dropping early adopting states from the analysis.

Our analysis requires the identifying assumption 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 in response to changes in local public sentiment regarding youth drunk driving.[17] 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). Similarly, we find no relationship between the underlying political environment in 1990 and the timing of subsequent ZT Law enactment at the state-level (Table A.10). We also report results from event-study specifications, which support the common trends assumption (see Section 5.1).

Our research design is analogous to the staggered difference-in-differences framework, and is also subject to concerns raised by recent econometric studies on the topic (see de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021, for example). Although our sample period falls entirely *after* all states had enacted ZT Laws, the short-lived impact of the policy – which targets individuals only below age 21 – allows us to observe both treated and untreated cohorts in the post-2000 period.[18] The

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[12] Heavy drinkers are defined as women (men) who consumed more than two (three) drinks per drinking episode.

[13] 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.

[14] These alcohol control policies are assigned based on cohort and state of birth. Specifically, we construct a series of indicators for whether an individual was subject to each law prior to age 21. 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, 2001a). 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).

[15] In some specifications, we also include quadratic and cubic cohort trends interacted with state of birth.

[16] We suppress the main effect of $ZT_{cs}$ from Eq. (1), so there is no reference treatment age. Instead, each estimate of $\beta_{Age}$ captures the age-specific treatment effect of the policy. In estimating the 40+ treatment effect, $\beta_{40+}$, we use the full 35–54 age group, and control for the ZT Law effect among the 35–39 age group, $\beta_{35-39}$.

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

[18] In our setting, treatment status is assigned based on year of birth and state of birth, as opposed to calendar year and state of residence as in the standard difference-in-difference setup.

staggered design stems from cross-cohort variation in ZT Law exposure, depending on the year of state implementation.

Each estimate of $\beta_{Age}$ is constructed as a weighted average across three types of comparisons. First are comparisons between "treated vs. never treated" cohorts. For example, if state A passed a law in 1993 and state B passed a law in 1997, one of the "treated vs. never treated" comparisons contributing to the $\beta_{45-49}$ coefficient would be based on cohorts born in 1970 and 1974: $(\bar{y}_{1974}^{A,45-49} - \bar{y}_{1970}^{A,45-49}) - (\bar{y}_{1974}^{B,45-49} - \bar{y}_{1970}^{B,45-49})$. In state A, the difference in outcomes is between a treated cohort (1974) versus untreated cohort (1970), whereas in state B the difference in outcomes is between two untreated cohorts.[19]

Second are comparisons between "early vs. late treated" cohorts. In our setting, "late treated" cohorts denote untreated cohorts who belong to a state in which subsequent birth cohorts were treated by a ZT Law.[20] Continuing with the same example, the $\beta_{35-39}$ estimate also depends on the comparison between cohorts born in 1970 and 1974, $(\bar{y}_{1974}^{A,35-39} - \bar{y}_{1970}^{A,35-39}) - (\bar{y}_{1974}^{B,35-39} - \bar{y}_{1970}^{B,35-39})$, but in this case the comparison is classified as an "early vs. late treated" comparison. The difference in classification stems solely from the fact that for the 45–49 age group, we never observe treated cohorts in state B (individuals born after 1977 do not reach age 45 in any sample year), whereas for the 35–39 age group, we do observe subsequent cohorts who were treated (individuals born after 1977 reach age 35 beginning in the year 2012). In practice, the distinction between "never treated" and "late treated" comparison units has no empirical relevance for the analysis. In both cases, they reflect control cohorts that were not exposed to a ZT Law in adolescence.

The third comparison is based on "late vs. early" cohorts. For example, the $\beta_{35-39}$ estimate also depends on comparisons between cohorts born 1974 and 1980 in states A and B: $(\bar{y}_{1980}^{B,35-39} - \bar{y}_{1974}^{B,35-39}) - (\bar{y}_{1980}^{A,35-39} - \bar{y}_{1974}^{A,35-39})$. In state B, the difference in outcomes is between a treated cohort (1980) versus untreated cohort (1974), whereas in state A, the difference in outcomes is between two previously treated cohorts.

In our setting, comparisons across "treated vs. never treated" cohorts and "early vs. late treated" cohorts both reflect 'good' comparisons that do not require us to impose any assumptions regarding treatment heterogeneity. Both types of comparisons correspond to $2 \times 2$ differences in outcomes across treated versus untreated cohorts in a given state relative to the difference in outcomes across two corresponding untreated cohorts in a different state. In contrast, "late vs. early" cohort comparisons may reflect 'bad' comparisons, since the trends in outcomes across previously treated cohorts may serve as invalid counterfactual trend for newly treated cohorts.

Given our staggered difference-in-differences research design, a second identifying assumption is the absence of treatment heterogeneity. In particular, dynamic treatments effects may bias our main estimates if the overall difference-in-difference estimator is constructed largely on "late vs. early" comparisons.[21] To assess this issue, we report diagnostic tests based on Goodman-Bacon (2021) decompositions, that allow us to calculate the weights from each comparison in the construction of our difference-in-differences estimators. We also report results based on a version of the Callaway and Sant'Anna (2021) estimator, that are robust to treatment effect heterogeneity. The results from both of these analyses support the identifying assumption.

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 et al., 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.[22] 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 Eq. (1), in which the main coefficient, $\beta_{Age}$, is allowed to vary with event time $\tau \in \{-5, 3\}$.[23][24] The dependent variable is an indicator for any self-assessed limitation (physical, cognitive, or visual/auditory).

In the top panel, Fig. 1(a) reports estimates for the full 40+ age group, while the bottom panel (Fig. 1(b)–(d)) reports the estimate separately by age groups 35–39, 40–44, and 45–49. 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 – $\beta_{40+}^{-\tau}$, $\beta_{35-39}^{-\tau}$, $\beta_{40-44}^{-\tau}$, $\beta_{45-49}^{-\tau}$ – are small and statistically insignificant.[25] 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 effects are largest among the 45–49 age group.[26] In contrast, the effects for $\beta_{35-39}^{+\tau}$ 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.[27]

Table 1 reports the average ZT Law effects from the difference-in-differences version of Eq. (1). We report the results separately based on different versions of Eq. (1). Column (1) includes year, birth state,

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[19] Note that these outcome differences could be calculated only in the last sample year, 2019, when all cohorts were at least 45 years old.

[20] This situation contrasts with calendar year treatment assignment, in which untreated individuals may be treated at a future date (late treated) or remain never treated.

[21] Treatment effect heterogeneity might arise if the duration of exposure to ZT Laws during adolescence impacted long-run outcomes. For example, an individual's long-run drinking habits might differ if she was exposed to a ZT Law throughout adolescence versus if she was exposed to a law for just one year at age 20.

[22] Despite reporting error, both self-assessed health and self-reported alcohol consumption have been shown to correlate strongly with more objective measures (Baker et al., 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.

[23] Event time captures the duration of exposure to a ZT Law, with $\tau = 0$ identifying individuals who were exposed to a law for one year or less. $\tau$ is defined on the basis of a cohort's year and quarter of birth and the timing of state law enactment. Each coefficient, $\beta_{Age}^{\tau}$, captures the long-run impact of a ZT Law on a cohort with $\tau$ years of exposure. For example, $\beta_{Age}^{+2}$, captures the effects of a law on individuals who were 18 years old at the time of enactment, while $\beta_{Age}^{-2}$ identifies the effects on individuals who were 22 at the time of enactment (and thus too old to have been affected).

[24] For each age group, we estimate a separate event-study regression that controls for the effect of ZT Laws among other ages. For example, for the 35–39 age group, the model is given by: $Y_{icst} = \alpha + \sum_{\tau \in \{-5,3\}} (\beta_{35-39}^{\tau} ZT_{cs\tau} \times I(Age_{icst} \in 35 - 39)) + \beta_{40-44} ZT_{icst} \times I(Age_{icst} \in 40 - 44) + \beta_{45-49} ZT_{icst} \times I(Age_{icst} \in 45 - 49) + \gamma X_{icst} + \lambda_c + \delta_s + \eta_t + \delta_s \cdot c + \epsilon_{icst}$. In this model, the coefficients of interest are $\beta_{35-39}^{\tau=-5}, \ldots, \beta_{35-39}^{\tau=3}$, which capture the relative disability rates at ages 35–39 across cohorts with $\tau$ years of exposure to the policy. We estimate analogous event-study regressions for age groups 40–44, 45–49, and 40+.

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

[26] The effects for $\beta_{45-49}^{+\tau}$ are larger but less precise, given the small number of states on which each estimate is identified. For this age group, the evolution
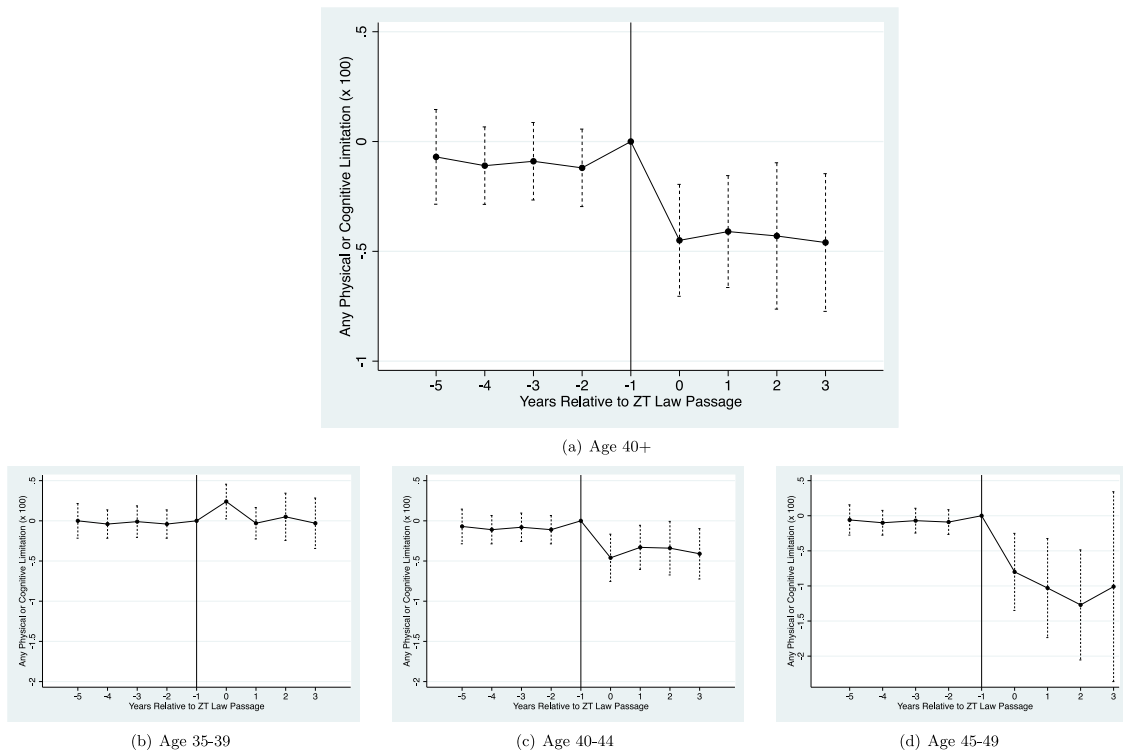
(a) Age 40+



(b) Age 35–39



(c) Age 40–44



(d) Age 45–49

**Fig. 1.** Event study estimates. *Notes:* These figures report the 'event study' estimates based on a generalized version of Eq. (1). Panel (a) reports the effects for ages 40+; panels (b)–(d) report the effects separately for age groups 35–39, 40–44, 45–49. The dependent variable is an indicator for any physical or cognitive limitation ($\times$ 100). The coefficients plot the time path for $\beta$ 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 0.76 for the 40+ age group, 0.93 for the 35–39 age group, 0.66 for the 40–44 age group, and 0.69 for the 45–49 age group.

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 a quadratic birth state-cohort trend.

We find that adolescent exposure to ZT Laws is associated with significant decreases in reported health limitations by middle age. Among the 40+ age group (Panel A), the estimates are large, negative, and statistically significant. The preferred estimates (col. 3), imply that ZT Laws led to decreases in reported limitation of 3% ($= -0.35/12.2$) among the 40+ population.

Consistent with Fig. 1, we also find larger negative effects on outcomes among older individuals. Panel B shows that the estimates are insignificant and close to zero for the 35–39 age group. Meanwhile the estimates are negative and significant for both the 40–44 and 45–49 age groups, with the largest effects found among the oldest age group. These broad patterns are stable across the different specifications and are generally unaffected by the inclusion of individual- or state-level covariates.

In Table 2, we explore the sources of health improvements. We estimate versions of Eq. (1) separately for three outcome variables: indicators for any physical limitation, any cognitive limitation, or any vision/auditory difficulties. Among the 40+ age group, we find

significant decreases in physical and visual/auditory limitations (Panel A). In contrast, the effects on cognitive limitations are more modest and not statistically significant. Consistent with the patterns in Table 1, we find that effects sizes for physical disability and visual/auditory limitations increase with age (Panel B), although there is some evidence of decreased cognitive limitations at earlier ages. Together, these results suggest that ZT Law exposure during adolescence led to broad improvements in both physical and cognitive health, although it appears that the timing of the benefits varied somewhat with the underlying limitation.

*5.2. Robustness checks*

Table A.4 reports the results from several alternative specifications and sample restrictions. For reference, column (1) reports the baseline estimates.[28] In columns (2) and (3), we assess whether geographic clustering in the timing of policy enactment can account for the observed effects. We estimate versions of Eq. (1) that control for cohort-by-region and cohort-by-division fixed effects. These models rely solely on within-region (division) 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. In column (4), we exclude pre-2008 observations, given a slight change in wording of disabilities in the questionnaire.[29] The results are unaffected

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of the post-treatment effects should be interpreted with caution, since they are identified off an unbalanced sample of states (see Section 4).

[27] Figure A.2 shows similar patterns based in event-study estimates based on raw disability means by age group. For the 45–49 age groups, the means are somewhat positive at $\tau = -4, -5$, which suggests that the assumption of common pre-trends holds only conditionally. Nevertheless, caution needs to be taken in interpreting the values of the raw means that fall far away from $\tau = 0$, since the are calculated based on unbalanced panels in event-time.

[28] We report both the original clustered standard errors (in parentheses), and standard errors from Seemingly Unrelated Regressions (SUR) that allow for potential correlation across multiple outcomes (in square brackets). Inference is similar across both specifications.

[29] 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.

**Table 1**
Effects of early ZT Law exposure on later-life health.

| | Mean Dep. Var. | Any Physical or Cognitive limitation (×100) | | | |
|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) |
| **Panel A:** *ZT Law effects, age 40+* | | | | | |
| Early ZT Law exposure | 12.2 | −0.33 | −0.34 | −0.35 | −0.32 |
| | | (0.10)** | (0.10)** | (0.10)** | (0.09)** |
| **Panel B:** *ZT Law effects, by age group* | | | | | |
| Early ZT Law exposure | | | | | |
| × Age 35–39 | 7.8 | 0.03 | 0.04 | 0.04 | 0.07 |
| | | (0.09) | (0.09) | (0.09) | (0.09) |
| × Age 40–44 | 9.7 | −0.29 | −0.30 | −0.32 | −0.29 |
| | | (0.10)** | (0.10)** | (0.10)** | (0.09)** |
| × Age 45–49 | 13.4 | −0.72 | −0.72 | −0.72 | −0.71 |
| | | (0.25)** | (0.25) | (0.26)** | (0.25)** |
| Year, birth state, & cohort FEs | | Y | Y | Y | Y |
| Birth state × linear cohort trend | | Y | Y | Y | Y |
| Demographic controls | | | Y | Y | Y |
| State controls | | | | Y | Y |
| Birth state × quadratic cohort trend | | | | | Y |
| | | Observations = 11,100,767 | | | |

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.

| | Any physical limitation (×100) | | Any cognitive limitation (×100) | | Any visual/Auditory limitation (×100) | |
|---|---|---|---|---|---|---|
| | Mean Dep. Var. | Estimate | Mean Dep. Var. | Estimate | Mean Dep. Var. | Estimate |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A:** *ZT Law effects, age 40+* | | | | | | |
| Early ZT Law exposure | 7.7 | −0.37 | 5.0 | −0.03 | 3.8 | −0.11 |
| | | (0.08)*** | | (0.07) | | (0.04)** |
| **Panel B:** *ZT Law effects, by age group* | | | | | | |
| Early ZT Law exposure | | | | | | |
| × Age 35–39 | 3.9 | 0.07 | 3.9 | −0.11 | 2.2 | 0.04 |
| | | (0.07) | | (0.06)* | | (0.06) |
| × Age 40–44 | 5.6 | −0.34 | 4.4 | −0.01 | 2.8 | −0.11 |
| | | (0.08)*** | | (0.07) | | (0.04)** |
| × Age 45–49 | 8.7 | −0.69 | 5.3 | −0.21 | 4.2 | −0.12 |
| | | (0.18)*** | | (0.17) | | (0.12) |
| Full controls | | Y | | Y | | Y |
| | | Observations = 11,100,767 | | | | |

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.

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.[30] The results are not sensitive to this sample restriction. Finally, in column (7), we include individuals who turned age 21 in

the same quarter that their state enacted a ZT Law, classifying these individuals as untreated. The results are similar to the baseline findings.

In Table A.5, we assess the sensitivity of the main findings to idiosyncratic trends in any particular early adopting state. This table reports 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.

In Table A.6, we explore concerns related to the fact that we measure ZT Law exposure based on state of birth, which may not reflect state of residence during adolescence. Column (2) reports 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

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[30] 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.

also estimate versions of Eq. (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 net cross-state migration flows in response to ZT Law enactment (col. 3). 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.7 we assess the sensitivity of our results to dynamic treatment heterogeneity, given the potential for bias in the staggered difference-in-differences framework (de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021). In columns (1)–(4), we report (Goodman-Bacon, 2021) decompositions that assess the relative weight of various comparisons used to identify the overall ZT Law impact.[31] For all four estimates, only a small fraction of the identifying variation is based on 'late' versus 'early' comparisons, suggesting that dynamic treatment heterogeneity is unlikely to introduce substantial bias in the ATT estimates. To further address these issues, column (5) reports estimates based on a modified version of the Callaway and Sant'Anna (2021) that is robust to treatment heterogeneity.[32] The broad patterns are consistent with the baseline findings.

### 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. Early exposure to ZT Laws is associated with long-run increases in labor market attachment. Among the 40+ population, we estimate large and statistically significant effects on weeks worked, usual hours, employment status, and earnings (Panel A). These effects mirror the patterns for disability, and are largest among the oldest age group (Panel B). We also find positive effects on earnings among full-time workers, consistent with improved workplace productivity among the 40+ age group, although these earnings effects are not significant among the 45–49 sample. The absence of positive earnings effects for this group could reflect offsetting forces. The increase in average worker productivity 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 point estimates for weeks worked for the 40+ population (Table 3, col. 2) by median weekly earnings among this age group, we calculate that increases in annual work weeks raised annual earnings by $443 (= 0.37 × $1,197) (BLS, 2017). Multiplying these estimates by the size of the 40–49 population, we estimate that the nationwide rollout of ZT Laws during the 1990s led to long-run annual economic gains of $18.5 billion dollars by 2019.

Our estimates imply that there were significant long-run economic costs associated with adolescent binge drinking. How do these costs compare to previous estimates of the short-run economic costs associated with adolescent binge drinking? There are several steps required to make this comparison. First, because the effects of ZT Law exposure emerge only after a period of several decades (when individuals approach middle age), we discount their future economic impacts into a present value measure. Second, we convert this estimate into a measure that relates changes in adolescent binge drinking rates to changes in longer-run economic outcomes. To do so, we rescale the reduced form economic effects of ZT Laws by their "first stage" impact on adolescent binge drinking.[33] Finally, we calculate the economic costs of adolescent binge drinking, by calculating the counterfactual *improvement* in long-run economic outcomes if adolescent binge drinking rates had been reduced from their average levels during the 1990s to zero.

We calculate an implied long-run economic cost of adolescent drinking of $16.8 billion per year.[34] This long-run cost estimate is comparable, but somewhat smaller than previous estimates of the short-run economic costs associated with adolescent binge drinking, which are on the order of $27 to $50 billion per year (Bonnie and O'Connell, 2004; Bouchery et al., 2006). Moreover, it does not account for the potential for improved labor market outcomes as the affected cohorts continue to age. Projecting forward to age 60, assuming a constant marginal impact on labor market outcomes, we calculate that the implied long-run costs associated with adolescent binge drinking would be $28.2 billion per year. These calculations do not account for any non-pecuniary benefits associated with improved adult health or increased 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 A.8 (Panel A) reports the effects of ZT Laws on dummy outcomes for high school completion, college attendance, and ever married. We find that ZT Laws led to a small and statistically significant increase in high school graduation rates, but had no impact on college attendance. Nevertheless, the positive effects for education are too small to account for the improvements in later-life health.[35] Similarly, we find no evidence that cohorts exposed to ZT Laws were more likely to marry (Panel A, col. 3), suggesting that the estimated decline in reported disabilities cannot be attributed to a marriage-health premium (Ross et al., 1990; Wood et al., 2007). In Panel B, we find that the inclusion of controls

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[31] We estimate Goodman-Bacon decompositions across each sample year, and report the average of these decompositions across all sample years for each of the four main estimates: $\beta_{40+}$, $\beta_{35-39}$, $\beta_{40-44}$, $\beta_{45-49}$. 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.

[32] A challenge to implementing this approach is that the CSDID command proposed by Callaway and Sant'Anna (2021) is based on the standard two dimensional difference-in-differences framework, whereas our analysis includes repeated observations of treatment units (cohort and state of birth) across multiple sample years that may also differ across multiple age groups. In order to implement the approach, we estimate the CSDID model separately by calendar year for each age group (40+, 35–39, 40–44, and 45–49). We then calculate the overall age-specific ATT estimate as a weighted average of these year-specific estimates. We use these same weights to calculate the age-specific standard errors based on the year-specific standard errors that are clustered at the state-level. While this approach is robust to treatment heterogeneity, the estimates are not directly comparable to the baseline analysis, since the two models implicitly rely on different sets of covariates.

[33] This approach is akin to a Wald Estimator in which the ZT Laws are used as an instrument for adolescent binge drinking, which in turn affects long-run economic outcomes. The results should be interpreted with caution, however, given that ZT Laws may influence later-life outcomes through a number of channels other than youth binge drinking.

[34] Discounting the long-run annual economic gains from ZT Laws ($18.5 billion) over a 30-year time horizon at a 4 percent interest rate, we obtain a present value estimate of $5.7 billion per year. We divide this estimate by the 17 percent reduction in adolescent binge drinking attributable to ZT Laws (Carpenter, 2004), and then multiply by the mean adolescent binge drinking between 1993 and 2000 (50.2 percent). Thus the annual economic cost of adolescent drinking is given by: ($5.7 billion/0.17) × 0.502 = $16.8 billion.

[35] Applying Oreopoulos's (2007) estimates of the impact of schooling on self-assessed health, and assuming that individuals who graduated high school as a result of ZT Laws obtained an additional year of schooling than they otherwise would have, we calculate that increases in education can account for less than 10 percent of the decline in reported health limitations.

**Table 3**
Effects of early ZT Law exposure on labor market outcomes.

| | Dependent variable: | | | | |
|---|---|---|---|---|---|
| | Any disability (×100) (1) | Weeks worked Last year (2) | Usual hours per week (3) | Currently employed (×100) (4) | Log earnings, Full-time workers (5) |
| Mean dep. Var. | 12.2 | 39.5 | 34.6 | 78.1 | 5.41 |
| **Panel A:** *ZT Law effects, age 40+* | | | | | |
| Early ZT Law exposure | −0.35 | 0.37 | 0.25 | 0.82 | 0.010 |
| | (0.10)** | (0.08)*** | (0.07)** | (0.16)*** | (0.004)** |
| **Panel B:** *ZT Law effects, by age group* | | | | | |
| × Age 35–39 | 0.04 | −0.14 | −0.08 | −0.24 | −0.003 |
| | (0.09) | (0.09) | (0.08) | (0.19) | (0.004) |
| × Age 40–44 | −0.32 | 0.34 | 0.21 | 0.72 | 0.010 |
| | (0.10)** | (0.08)*** | (0.08)** | (0.16)** | (0.003)** |
| × Age 45–49 | −0.72 | 0.77 | 0.68 | 1.83 | 0.002 |
| | (0.26)** | (0.18)*** | (0.15)*** | (0.26)*** | (0.010) |
| Full controls | Y | Y | Y | Y | Y |
| | Obs = 11,100,767 | | | Obs = 6,468,420 | |

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.

for both educational attainment and ever married status leads to modest decreases in the magnitude of the main point estimates.[36]

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 accidents 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.[37] 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.[38]

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 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, the probability of being a heavy drinker, and negative albeit insignificant effects on the frequency of heavy episodic drinking. The effects on alcohol consumption appear to emerge at earlier ages than those for disability, suggesting that the health effects of excessive

drinking may take several years to materialize.[39] We also find some evidence that the laws reduced extensive margin drinking in later-life (col. 4), although the effects are small and not individually significant for either the 35–39 or 45–49 age groups.

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.[40] Nevertheless, the rates of DUI arrests under the new ZT Laws were simply too low to account for the overall improvement in later-life health.[41] 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.

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[36] These results should be interpreted with caution, given that the strong assumptions required for this type of mediation analysis – notably the exogeneity of the mediating variables – is unlikely to hold in this context (see Mackinnon, 2008).

[37] Among individuals aged 15 to 20, the rate of motor vehicle traffic injuries related to alcohol was 235 per 100,000 (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 motor vehicle accidents, the implied reduction in the probability of long-term disability is $0.028\% = ((235/100,000) \times 0.12)$, less than 10 percent of the main effect reported in Table 1.

[38] 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.

[39] Interestingly, we find more modest effects on drinking among the 45+ population. This pattern could be due to the fact that the onset of disability may lead individuals to curb heavy drinking. Alternatively, the negative income effects associated with disability onset may, in turn, cause heavy drinkers to reduce alcohol consumption (Ruhm, 1995; Ruhm and Black, 2002; Cotti et al., 2015).

[40] 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).

[41] 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. Intuitively, because only a tiny fraction (0.4 percent) of adolescents were ever arrested under a ZT Law (Carpenter, 2007), too few individuals who have been exposed to mandated alcohol education programs to account for the magnitudes of the later-life health improvements.

**Table 4**
Effects of early ZT Law exposure on long-term alcohol consumption.

| | Dependent variable: | | | |
|---|---|---|---|---|
| | Ave. Drinks per drinking episode | Heavy drinker | = 1 if binge drank | Any alcohol |
| | (1) | (2) | (3) | (4) |
| Mean dep. Var. | 1.18 | 0.10 | 0.28 | 0.53 |
| **Panel A:** *ZT Law effects, age 40+* | | | | |
| Early ZT Law exposure | −0.066 | −0.015 | −0.004 | −0.008 |
| | (0.019)** | (0.003)*** | (0.003) | (0.003)** |
| **Panel B:** *ZT Law effects, by age group* | | | | |
| × Age 35–39 | −0.084 | −0.020 | −0.003 | 0.000 |
| | (0.016)*** | (0.004)*** | (0.002) | (0.004) |
| × Age 40–44 | −0.067 | −0.016 | −0.004 | −0.008 |
| | (0.021)** | (0.003)*** | (0.003) | (0.003)** |
| × Age 45–49 | −0.055 | −0.009 | −0.009 | −0.008 |
| | (0.030)* | (0.007) | (0.009) | (0.008) |
| Full controls | Y | Y | Y | Y |
| Observations | 1,436,824 | 1,436,824 | 1,441,108 | 2,904,367 |

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.

## 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 are in the same order of magnitude as 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.

### Declaration of competing interest

### Data availability

Data will be made available on request.

### Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jpubeco.2024.105066.

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