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DISCUSSION PAPER SERIES

IZA DP No. 12833

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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 binge drinking 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 substantially 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, binge drinking, disabilities, substance abuse

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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 almost nothing about the long-run consequences of this behavior and whether the costs extend into later-life.

This paper provides the first long-run assessment of adolescent drinking 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.

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.[2] We link this policy variation to later-life health and socioeconomic outcomes for the period 2000 to 2017. Specifically, we use annual individual-level data from the American Community Survey (ACS), which 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.

The results show clear evidence that increased regulation of adolescent drinking led

[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.

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 9 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.

We estimate significant effects on a range of health outcomes. Individuals exposed to ZT Laws during adolescence were less likely to suffer from physical, cognitive, and visual/auditory limitation by middle age. 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).

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. Moreover, we find no effects of ZT Laws on outcomes among slightly older cohorts who turned 21 before the laws were implemented. Taken together, this evidence provides strong support for the research strategy and our identification assumption that outcomes among affected cohorts would have trended similarly absent the adoption of ZT Laws.

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 $7 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 and were slightly 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).

This paper 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).[3] 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.

More broadly, our analysis contributes to the literature demonstrating how temporary policies that target critical ages can have long-lasting effects (e.g., Almond and Currie, 2011; Aizer et al., 2016). Our findings also complement 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).

The paper precedes as follows: Section 2 discusses the ZT Laws and the potential link between adolescent binge drinking and later-life outcomes, Section 3 describes the data, Section 4 presents the empirical framework, Section 5 presents the results, and Section 6 concludes.

2 Background

2.1 Zero Tolerance Laws

Zero Tolerance Laws emerged as a response to a growing awareness of the high rates of alcohol-related crashes among young drivers.[4] These laws set a legal blood alcohol content (BAC) limit of 0.02 for individuals under age 21, with violators facing penalties

[3]In contrast, researchers have found mixed evidence on the effects of state beer excise taxes on youth drinking (Dee, 1999; Cook and Moore, 2001).

[4]The alcohol-related crash rate among drivers under age 21 is nearly twice the rate of of older drivers (NHTSA, 2000).

of license suspension or revocation. A number of states voluntarily implemented ZT Laws in the early 1990s. Following the passage of the National Highway Systems Design Act in 1995, the federal government mandated that states enact ZT Laws, with non-compliant states facing the possible withholding of federal highway funding. In response, a number of states quickly enacted ZT Laws, and by 1998, these laws were enforced nationwide.

ZT Laws led to large decreases in alcohol-related fatalities, primarily through decreased heavy drinking among adolescents. 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.[5] A priori, it is unclear whether the laws reduced youth binge drinking: the decreased alcohol consumption among young drivers might be offset by other behavioral responses, such as increased drinking at home. Nevertheless, 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 Adolescent Binge Drinking and Later-Life Health and Labor Market Outcomes

There are several plausible channels through which heavy drinking during adolescence may influence health and labor market outcomes in later-life. First, initiation into heavy drinking during adolescence may increase the likelihood of this behavior in adulthood, with potentially harmful long-run consequences. Researchers have identified a number of adolescent-specific alcohol sensitivities, due to both biological and contextual factors, that contribute to heavy drinking at this age. For example, neural

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

developments during adolescence temporarily increase individual sensitivity to alcohol's rewarding and stimulating effects (Spear, 2016; Miranda Jr et al., 2014). Meanwhile, increased reliance on peer groups can contribute to risk-taking behavior such as binge drinking (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.[6]

Finally, 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. Animal studies have found that heavy alcohol exposure during adolescence disrupts hippocampus development and can lead to persistent loss of neurogenesis (White and Swartzwelder, 2004; Taffe et al., 2010). There is also evidence that exposure to high levels of alcohol during adolescence may have long-term effects on cognition and behavior through epigenetic mechanisms (Guerri and Pascual, 2010; Pandey et al., 2015). Epidemiological studies

[6] See Oreopoulos (2007), Cutler and Lleras-Muney (2010), and Clark and Royer (2013) for evidence on the effects of education on later-life health.

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.

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.[7] We restrict attention to individuals born between 1963 and 1985 who were aged 35 to 54 at the time of observation for a total sample of 5,799,964. We link these individuals to the relevant ZT Laws during adolescence based on state and year of birth.[8] Specifically, we construct an indicator for whether a ZT Law was enforced in an individual's state of birth prior to age 21.[9] Since the ACS reports quarter of birth but not the exact birthdate, 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 an indicator for reported physical limitation, measured as a condition that substantially limits one or more basic physical activity such as walking, climbing stairs, reaching, lifting, or carrying. We also construct an indicator for cognitive limitations, which captures difficulties learning, remembering, concentrating, or making decisions due to either physical, mental, or emotional conditions. We construct a dummy variable for any reported vision or hearing difficulties. Finally, we construct a dummy variable

[7]From 2001 to 2004, the ACS sampled 1-in-250; since 2005, the ACS has sampled is 1-in-100 of the population.

[8]We follow Carpenter (2004) in the assignment of state ZT Laws. Exposure to ZT Laws varied across cohort and state of birth for individuals born between 1969 and 1976. We include cohorts born from 1963 to 1968 and 1977 to 1985 in order to better control for state-specific trends in outcomes. The results are not sensitive to either the age or cohort sample restrictions.

[9]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.

equal to one if an individual reports any physical or cognitive limitation. 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 nationally representative survey which reports detailed individual-level information on alcohol consumption. The BRFSS allows us to directly assess whether adolescent exposure to ZT Laws had long-term impacts on substance use in later-life. Our main sample is a repeated cross section of individuals aged 35 to 54 for the period 2001 to 2017. We construct several measures of alcohol consumption during the previous month including: an indicator for binge drinking, average number of drinks consumed per episode of drinking, number of binge drinking episodes among drinkers, and whether the individual consumed any alcohol.[10]

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 state of residence.[11] Second, the public-use files do not identify exact age but instead classify individuals into five-year age categories, limiting our ability to precisely identify cohorts exposed to the laws. Given this limitation, we assign treatment based on the fraction of each age group that was exposed to a ZT Law prior to age 21.[12] This approach introduces random error in the assignment of treatment status which will tend to bias the coefficient estimates towards zero.

[10]We exclude individuals who could not recall the answer to each specific drinking-related question in the survey.

[11]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. Since the likelihood that individuals have moved across states increases with age, the attenuation bias will be larger among older age groups.

[12]Consider, a state ZT Law that was enacted in 1992. For residents aged 40-44, we assign a treatment status of zero for the years 2001 to 2011 (since none were below age 21 at the time of passage) and a treatment status of one for the years 2016 and 2017 (since all were below age 21 at the time of passage), and we assign a treatment status of 0.2, 0.4, 0.6, and 0.8 for the years 2012 through 2015, respectively.

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:

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

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 characteristics including 5-year age group dummies, gender, and a dummy for white. The fixed effects, λ_c, δ_s, and η_t represent indicators for birth cohort, state of birth, and year of observation, respectively. We include a linear state of birth time trend, $\delta_s \cdot c$, to allow for differential trends in outcomes across cohorts born in different states. Finally, we include a vector of age group - birth state fixed effects, $\delta_s \cdot Age_{icst}$, to control for different lifecycle patterns in outcomes across states.[13]

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 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.

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,

[13] For example, differences in the underlying occupational structure may lead to differences in average disability rates across states as well as differences in the trajectory of disability over the lifecycle.

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

including a linear state of birth trend. There are two main threats to identification. First, the timing of ZT Law adoption across states may have been systematically related to the adoption of other relevant state policies. To address this concern, we add controls for other alcohol-related policies relevant in adolescence and early adulthood including the state's minimum legal drinking age (MLDA), drunk driving laws, and vertical identification card laws, and contemporaneous state beer excise taxes.[15] We also include controls for the current unemployment rate in the state of birth to control for contemporaneous economic conditions. Second, despite these controls there is still the possibility that preexisting time trends may confound policy inference. To address this final concern, we conduct a series of placebo exercises to assess whether ZT Laws had effects on cohorts that reached age 21 in the years before leading up to the law's implementation.

Two final estimation details are worth noting. First, all regressions are weighted by survey sampling weights. 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

Table 1 reports the estimated effects of ZT Laws on later-life health. The dependent variable is an indicator for any self-assessed limitation (physical, cognitive, visual/auditory). 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

[15] 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 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.

Adolescent exposure to ZT Laws is associated with significant decreases in reported health limitations. The effects are consistently *more* negative for 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 $3\%(= 0.27/9.4)$ for 40-44 year olds and $9\%(= 0.99/10.9)$ for 45-48 year olds.[16]

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. The estimated coefficients are consistently small and insignificant. 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

[16]We also find similar age-patterns for both men and women, although the effects are generally larger among males (Table A.1). These findings should be interpreted with some caution, given that they are based on self-assessed health, rather than objective health measures. As a result, the coefficient estimates could also reflect gender differences in the willingness to identify physical or cognitive limitations.

slightly older ages.

Table 3 reports the results from several robustness tests. To begin, we assess the validity of the common trends assumption underlying the research design. We estimate 'event study' regressions, in which the ZT Law coefficients are allowed to vary across pre-treatment cohorts, who were too old to have been influenced by the policies. We allow for separate ZT Law effects for individuals aged 22, 23, and 24 at the time of enactment, each of which captures the difference in adult health outcomes relative to the reference cohort (aged 21 at the time of enactment).[17] The results, reported in column (2), show no evidence of differential trends in outcomes among these slightly older cohorts. As a further robustness test, we replace the measure of adolescent exposure to ZT Laws with a placebo treatment: an indicator for individuals aged 21 to 24 at the time of enactment. We then interact this variable with the three age group categories (35-39, 40-44, and 45-48). The point estimates from this placebo are all small and statistically insignificant (column 3). Taken together, these placebo exercises provide strong support for the identification assumption that outcomes among affected cohorts would have trended similarly absent the policy change.

Table 3, col. 4-5 report the results from two further sample restrictions. In column (4), we exclude pre-2008 observations, given a slight change in wording of disabilities in the questionnaire.[18] The results remain similar in both sign, size, and statistical significance. In column (5), we restrict the sample to white individuals. The broad patterns are similar and slightly larger in magnitude.[19]

[17] Following Kline (2012), we include a separate indicator for individuals aged 25 or older at the time of enactment but suppress this endpoint coefficient which places unequal weight across the various cohorts aged at least 25 at the time of enactment.

[18] 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. There was also a slight difference in the examples of limitations provided to participants. In principle, any aggregate changes in self-assessed disabilities due to these change should be captured by the survey year fixed effects.

[19] Tables A.2-A.4 report the robustness tests for each health outcome individually: physical, cognitive, or visual/auditory. The results all support the main findings.

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

Table 4 reports the effects of adolescent exposure to ZT Laws on a range of later-life labor market outcomes. Columns 1-2 report the results for the full sample. We find that ZT Laws led to significant increases in labor market attachment among older age groups. The point estimates for weeks worked last year and usual weekly hours are positive statistically significant for individuals aged 45 to 48. The preferred estimates imply increases in attachment of 2 percent. We also find some evidence that exposure to the laws increased employment: the point estimates increase with age but are not significant for the 45-48 year old group in the preferred specification. We find mixed effects for annual earnings of full time workers: the point estimates are positive and significant for 40-44 year olds, but insignificant for 45-48 year olds. This latter coefficient could reflect a positive selection effect, in which less healthy individuals are induced to remain in full time work as a result of the laws, lowering average earnings.

Given the distinctive patterns of lifecycle employment outcomes by gender, we estimate the regressions separately for males (cols. 3-4) and females (cols. 5-6). For males, we find large effects on weeks worked, usual hours, and employment status that are concentrated among the oldest age group. In contrast, the effects for women are generally more modest and less significant. Meanwhile, we find some evidence of positive employment effects for women at younger ages. These patterns mirror the gender-specific effects on health reported in Table A.1: the effects are generally larger for males although the benefits for women appear to emerge at slightly earlier ages.

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 4, 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 \$891 ($= 0.848 \times \$1,051$) (BLS, 2017). Together, these estimates imply that the nationwide rollout of ZT Laws during the 1990s led to long-run annual economic gains of $7.3 billion dollars by 2017.[20]

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 $13 billion per year.[21] This cost 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.[22]

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 $58 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.

5.3 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.

[20]This statistic is obtained by multiplying the previous estimate by the 8.21 million for the U.S. male population aged 45 to 48 (U.S. Census Bureau, 2017)

[21]Carpenter (2004) finds that ZT Laws reduced youth binge drinking by 17 percent. Our calculation is obtained by dividing the long-run economic gains ($7.3 billion) by this estimate and discounting over a 30-year time horizon at a 4 percent interest rate.

[22]For example, the presence of a large peer group who drank heavily may eventually influence later-life alcohol consumption even among individuals who did not drink in adolescence, a violation of the exclusion restriction.

First, we explore the extent to which changes in educational attainment and marriage entry can account for the later-life outcomes. Table 5, cols. 5-6 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. We find some evidence that cohorts exposed to the laws were more likely to marry (col. 7). Given the link between marriage and better physical and mental health (Ross, Mirowsky and Goldsteen, 1990; Wood, Goesling and Avellar, 2007), these findings may partly account for the decline in reported disabilities. Interestingly, when we decompose the effects by gender, it appears that males were primarily affected through changes in marriage entry while females were affected through educational attainment (Table A.5, cols. 5-7). Nevertheless, the estimates for education are too small to account for the improvements in later-life health.[23] [24]

A second possibility is 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 5 (cols. 1-4) 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 and frequency of heavy episodic drinking, particularly among older age groups.[25] In contrast, we find

[23]Applying Oreopoulos's (2007) estimates of the impact of schooling on self-assessed health, and assuming that women 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.

[24]When we control for both education and ever married status in the health regressions (Table A.6), the point estimates for ZT Laws remain largely unchanged, suggesting that the observed health effects were not mediated through either of these channels. These results should be interpreted with caution, however, given the potential endogeneity of the explanatory variables.

[25]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

no systematic evidence that the laws reduced moderate drinking in later-life (col. 4). For individuals aged 40 to 44, the point estimates for any alcohol consumption are negative and significant, but small in magnitude.[26] Meanwhile, the coefficient estimates for any alcohol consumption in the previous month are positive and 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).

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

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.

[26]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.

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.

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7 Tables

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	0.09	-0.03	-0.03	-0.09
		(0.09)	(0.09)	(0.09)	(0.09)
× Age 40-44	9.4	-0.59	-0.38	-0.38	-0.27
		(0.10)***	(0.10)***	(0.10)***	(0.13)**
× Age 45-48	10.9	-1.13	-0.89	-0.88	-0.99
		(0.27)***	(0.27)***	(0.27)***	(0.16)***
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 = 5,799,964			

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	4.0	0.13	0.05	0.06	0.03
		(0.08)	(0.08)	(0.08)	(0.08)
× Age 40-44	5.2	-0.41	-0.29	-0.28	-0.24
		(0.07)***	(0.07)***	(0.07)***	(0.08)***
× Age 45-48	6.0	-0.79	-0.56	-0.55	-0.63
		(0.11)***	(0.11)***	(0.11)***	(0.07)***
		Any Cognitive Limitation (× 100)			
Early ZT Law Exposure					
× Age 35-39	3.8	-0.04	-0.04	-0.04	-0.08
		(0.09)	(0.09)	(0.09)	(0.09)
× Age 40-44	4.4	-0.14	-0.13	-0.13	-0.05
		(0.07)**	(0.08)*	(0.08)*	(0.09)
× Age 45-48	4.9	-0.66	-0.61	-0.63	-0.76
		(0.10)***	(0.11)***	(0.12)***	(0.26)***
		Any Visual/Auditory Limitation (× 100)			
Early ZT Law Exposure					
× Age 35-39	2.2	0.05	-0.03	-0.03	-0.04
		(0.05)	(0.05)	(0.05)	(0.05)
× Age 40-44	2.9	-0.24	-0.11	-0.11	-0.08
		(0.07)***	(0.07)	(0.07)	(0.08)
× Age 45-48	3.9	-0.37	-0.37	-0.37	-0.49
		(0.12)***	(0.11)***	(0.11)***	(0.14)***
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 = 5,799,964			

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: Robustness Tests

		Placebo tests		Alternate samples	
	Baseline estimates	Event study	Effects of late ZT Law exposure by age	Drop pre-2008 observations	Whites
	(1)	(2)	(3)	(4)	(5)
	Any Physical or Cognitive Limitation (× 100)				
Early ZT Law Exposure (< age 21)					
× Age 35-39	-0.09	-0.11	-0.06	-0.12	-0.17
	(0.09)	(0.09)	(0.12)	(0.09)	(0.10)*
× Age 40-44	-0.27	-0.29	-0.27	-0.25	-0.29
	(0.13)**	(0.12)**	(0.12)**	(0.13)*	(0.14)**
× Age 45-48	-0.99	-0.99	-1.14	-0.88	-0.98
	(0.16)***	(0.16)***	(0.21)***	(0.12)***	(0.28)***
Late ZT Law Exposure					
(Age = 22)		-0.03			
		(0.09)			
(Age = 23)		0.04			
		(0.09)			
(Age = 24)		-0.02			
		(0.09)			
Late ZT Law Exposure (age 21-24)					
× Age 35-39			0.04		
			(0.08)		
× Age 40-44			0.03		
			(0.08)		
× Age 45-48			-0.19		
			(0.15)		
Full controls	Y	Y	Y	Y	Y
Observations	5,799,964			4,605,573	4,908,792

Notes: Each column reports the point estimate from a different regression. All models include the full controls described in column (4) of Table 1. Column (2) reports the event-study coefficients: exposure to ZT Laws at ages 22, 23, 24 relative to exposure at age 21. Column (3) reports the average effect of ZT Law exposure between ages 21 and 24, interacted with current age. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table 4: Effects of Early ZT Law Exposure on Labor Market Outcomes

	All		Males		Females	
	(1)	(2)	(3)	(4)	(5)	(6)
	Weeks Worked Last Year					
Mean Dep. Var.	39.9		43.2		36.7	
Early ZT Law Exposure						
× Age 35-39	-0.123	0.062	-0.112	0.013	-0.134	0.114
	(0.085)	(0.083)	(0.103)	(0.100)	(0.122)	(0.128)
× Age 40-44	0.421	0.137	0.300	0.146	0.535	0.133
	(0.089)***	(0.081)*	(0.108)***	(0.107)	(0.128)***	(0.122)
× Age 45-48	0.617	0.701	0.942	0.848	0.268	0.544
	(0.144)***	(0.335)**	(0.153)***	(0.368)**	(0.220)	(0.363)
	Usual Hours per Week					
Mean Dep. Var.	35.0		40.1		30.1	
Early ZT Law Exposure						
× Age 35-39	-0.049	0.083	-0.107	-0.006	0.010	0.178
	(0.098)	(0.085)	(0.117)	(0.097)	(0.116)	(0.115)
× Age 40-44	0.257	0.056	0.125	0.013	0.381	0.108
	(0.097)***	(0.084)	(0.130)	(0.132)	(0.112)***	(0.108)
× Age 45-48	0.736	0.839	1.233	1.249	0.217	0.439
	(0.120)***	(0.174)***	(0.404)***	(0.369)***	(0.214)	(0.201)**
	Currently Employed					
Mean Dep. Var.	0.79		0.85		0.73	
Early ZT Law Exposure						
× Age 35-39	-0.002	0.002	-0.002	0.000	-0.001	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)*
× Age 40-44	0.009	0.004	0.006	0.003	0.013	0.004
	(0.002)***	(0.002)**	(0.002)***	(0.002)	(0.003)***	(0.003)**
× Age 45-48	0.014	0.014	0.017	0.015	0.011	0.014
	(0.006)**	(0.009)	(0.004)***	(0.007)**	(0.009)	(0.011)
	Log Earnings (Full-time Workers)					
Mean Dep. Var.	5.64		5.71		5.49	
	(Obs = 3,193,326)		(Obs = 1,876,441)		(Obs = 1,316,885)	
Early ZT Law Exposure						
× Age 35-39	0.002	0.005	-0.000	0.004	0.007	0.008
	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)*
× Age 40-44	0.013	0.009	0.015	0.009	0.011	0.009
	(0.004)***	(0.004)**	(0.004)***	(0.005)*	(0.006)*	(0.006)
× Age 45-48	-0.020	-0.023	-0.022	-0.019	-0.019	-0.034
	(0.016)	(0.016)	(0.015)	(0.014)	(0.019)	(0.027)
Year, birth state, cohort FEs	Y	Y	Y	Y	Y	Y
Birth state-cohort trend	Y	Y	Y	Y	Y	Y
Full controls		Y		Y		Y
Observations	5,799,964		2,797,422		3,002,542	

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. Full-time workers are individuals who worked at least 50 weeks in the previous year and report usually working at least 40 hours per week. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table 5: Mechanisms: Long-term Alcohol Consumption, Education, and Marriage

	Alcohol consumption in previous month				Educational attainment & marriage		
Dependent Variable	=1, if Binge Drank	Ave. Drinks per Drinking Episode	Number of Binge Drinking Episode	Any Alcohol	High School Grad	College Grad	Ever Married
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mean Dep. Var.	0.17	2.4	1.2	0.35	0.94	0.11	0.80
Early ZT Law Exposure							
× Age 35-39	-0.003	-0.091	-0.008	-0.004	0.002	0.002	-0.001
	(0.002)	(0.022)***	(0.021)	(0.004)	(0.001)*	(0.002)	(0.002)
× Age 40-44	-0.014	-0.166	-0.176	-0.021	0.003	0.003	0.004
	(0.004)***	(0.028)***	(0.054)***	(0.005)***	(0.002)*	(0.001)*	(0.002)**
× Age 45-48	-0.016	-0.366	-0.355	0.032	0.001	0.004	0.016
	(0.006)***	(0.044)***	(0.091)***	(0.006)***	(0.002)	(0.004)	(0.011)
Full controls	Y	Y	Y	Y	Y	Y	
Observations	2,071,681	1,172,778	1,172,617	2,095,811	5,799,964	5,799,964	5,799,964

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 month. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

A Appendix (For Online Publication)

Table A.1: Effects of ZT Laws on Later-life Health, by Gender

Dependent Variable	Any Physical or Cognitive Limitation (× 100)					
	Males			Females		
	Mean Dep. Var.	(1)	(2)	Mean Dep. Var	(3)	(4)
Early ZT Law Exposure						
× Age 35-39	0.078	0.17	0.09	0.077	-0.21	-0.28
		(0.14)	(0.14)		(0.13)*	(0.13)**
× Age 40-44	0.093	-0.33	-0.21	0.095	-0.43	-0.33
		(0.15)**	(0.16)		(0.15)***	(0.16)**
× Age 45-48	0.108	-1.33	-1.02	0.111	-0.45	-0.97
		(0.38)***	(0.55)*		(0.73)	(0.72)
Year, birth state, cohort FEs		Y	Y		Y	Y
Birth state-cohort trend		Y	Y		Y	Y
Demographic & state controls		Y	Y		Y	Y
Birth state × Age group FEs			Y			Y
	Observations =2,797,422			Observations = 3,002,542		

Notes: Each column reports the point estimate from a different regression. Demographic controls include 5-year age group dummies 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 A.2: Robustness Tests: Any Physical Limitation

	Baseline estimates	Placebo tests: Event study	Placebo tests: Effects of late ZT Law exposure by age	Alternate samples: Drop pre-2008 observations	Alternate samples: Whites
	(1)	(2)	(3)	(4)	(5)
	Any Physical Limitation (× 100)				
Early ZT Law Exposure (< age 21)					
× Age 35-39	0.03	0.05	0.06	0.02	-0.06
	(0.08)	(0.08)	(0.06)	(0.08)	(0.08)
× Age 40-44	-0.24	-0.22	-0.24	-0.20	-0.31
	(0.08)***	(0.08)**	(0.09)**	(0.08)**	(0.07)***
× Age 45-48	-0.63	-0.61	-0.64	-0.68	-0.60
	(0.07)***	(0.09)***	(0.12)***	(0.06)***	(0.29)**
Late ZT Law Exposure					
(Age = 22)		0.03			
		(0.07)			
(Age = 23)		0.06			
		(0.08)			
(Age = 24)		-0.02			
		(0.08)			
Late ZT Law Exposure (age 21-24)					
× Age 35-39			0.06		
			(0.06)		
× Age 40-44			-0.02		
			(0.07)		
× Age 45-48			0.00		
			(0.12)		
Full controls	Y	Y	Y	Y	Y
Observations	5,799,964			4,605,573	4,908,792

Notes: Each column reports the point estimate from a different regression. All models include the full controls described in column (4) of Table 1. Column (2) reports the event-study coefficients: exposure to ZT Laws at ages 22, 23, 24 relative to exposure at age 21. Column (3) reports the average effect of ZT Law exposure between ages 21 and 24, interacted with current age. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.3: Robustness Tests: Any Cognitive Limitation

		Placebo tests		Alternate samples	
	Baseline estimates	Event study	Effects of late ZT Law exposure by age	Drop pre-2008 observations	Whites
	(1)	(2)	(3)	(4)	(5)
	Any Cognitive Limitation (× 100)				
Early ZT Law Exposure (< age 21)					
× Age 35-39	-0.08	-0.09	-0.07	-0.11	-0.07
	(0.09)	(0.09)	(0.09)	(0.08)	(0.09)
× Age 40-44	-0.05	-0.06	-0.05	-0.02	-0.07
	(0.09)	(0.08)	(0.09)	(0.09)	(0.09)
× Age 45-48	-0.76	-0.78	-0.69	-0.66	-0.89
	(0.26)***	(0.26)***	(0.29)**	(0.17)***	(0.18)***
Late ZT Law Exposure					
(Age = 22)		-0.07			
		(0.06)			
(Age = 23)		0.00			
		(0.07)			
(Age = 24)		-0.01			
		(0.07)			
Late ZT Law Exposure (age 21-24)					
× Age 35-39			0.02		
			(0.05)		
× Age 40-44			-0.03		
			(0.06)		
× Age 45-48			0.09		
			(0.12)		
Full controls	Y	Y	Y	Y	Y
Observations	5,799,964			4,605,573	4,908,792

Notes: Each column reports the point estimate from a different regression. All models include the full controls described in column (4) of Table 1. Column (2) reports the event-study coefficients: exposure to ZT Laws at ages 22, 23, 24 relative to exposure at age 21. Column (3) reports the average effect of ZT Law exposure between ages 21 and 24, interacted with current age. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.4: Robustness Tests: Any Visual/Auditory Limitation

	Baseline estimates	Placebo tests: Event study	Placebo tests: Effects of late ZT Law exposure by age	Alternate samples: Drop pre-2008 observations	Alternate samples: Whites
	(1)	(2)	(3)	(4)	(5)
	Any Visual/Auditory (× 100)				
Early ZT Law Exposure (< age 21)					
× Age 35-39	-0.04	-0.06	0.01	-0.03	-0.04
	(0.05)	(0.05)	(0.07)	(0.06)	(0.04)
× Age 40-44	-0.08	-0.09	-0.04	-0.11	-0.09
	(0.08)	(0.08)	(0.08)	(0.09)	(0.09)
× Age 45-48	-0.49	-0.50	-0.64	-0.33	-0.57
	(0.14)***	(0.15)***	(0.16)***	(0.17)*	(0.15)***
Late ZT Law Exposure					
(Age = 22)		-0.02			
		(0.03)			
(Age = 23		-0.02			
		(0.06)			
(Age = 24)		-0.01			
		(0.07)			
Late ZT Law Exposure (age 21-24)					
× Age 35-39			0.04		
			(0.05)		
× Age 40-44			0.08		
			(0.05)		
× Age 45-48			-0.22		
			(0.09)**		
Full controls	Y	Y	Y	Y	Y
Observations	5,799,964			4,605,573	4,908,792

Notes: Each column reports the point estimate from a different regression. All models include the full controls described in column (4) of Table 1. Column (2) reports the event-study coefficients: exposure to ZT Laws at ages 22, 23, 24 relative to exposure at age 21. Column (3) reports the average effect of ZT Law exposure between ages 21 and 24, interacted with current age. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.5: Mechanisms: Long-term Alcohol Consumption, Education, and Marriage

	Alcohol consumption in previous month				Educational attainment & marriage		
Dependent Variable	=1, if Binge Drank	Ave. Drinks per Drinking Episode	Number of Binge Drinking Episode	Any Alcohol	High School Grad	College Grad	Ever Married
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Males							
Mean Dep. Var.	0.25	2.9	1.7	0.40	0.93	0.10	0.78
Early ZT Law Exposure							
× Age 35-39	-0.006	-0.114	0.068	-0.007	0.003	0.002	0.002
	(0.003)*	(0.035)***	(0.041)	(0.005)	(0.001)**	(0.002)	(0.003)
× Age 40-44	-0.017	-0.191	-0.219	-0.012	0.004	0.000	0.005
	(0.009)**	(0.057)***	(0.092)***	(0.006)**	(0.002)*	(0.002)	(0.003)*
× Age 45-48	-0.009	-0.212	-0.239	0.035	-0.002	-0.002	0.020
	(0.008)	(0.116)*	(0.163)***	(0.025)	(0.006)	(0.010)	(0.008)**
Observations	843,903	533,614	533,413	856,428	2,797,422	2,797,422	2,797,422
Females							
Mean Dep. Var.	0.11	1.9	0.70	0.31	0.94	0.12	0.83
× Age 35-39	-0.003	-0.101	-0.079	-0.005	0.001	0.001	-0.004
	(0.002)	(0.017)***	(0.023)***	(0.005)	(0.001)	(0.002)	(0.002)
× Age 40-44	-0.012	-0.174	-0.136	-0.027	0.002	0.005	0.002
	(0.003)***	(0.024)***	(0.047)***	(0.008)***	(0.002)	(0.002)**	(0.003)
× Age 45-48	-0.018	-0.387	-0.395	0.029	0.004	0.008	0.012
	(0.008)**	(0.067)***	(0.060)***	(0.020)	(0.002)**	(0.003)***	(0.021)
Observations	1,227,786	639,164	639,204	1,239,383	3,002,542	3,002,542	3,002,542
Full controls	Y	Y	Y	Y	Y	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. All drinking variables are constructed based on reported alcohol consumption in the previous month. Standard errors are clustered at the state-level. ***,**, * denote significance at the 1%, 5%, and 10% level, respectively.

Table A.6: 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	-0.09	-0.05	-0.06
	(0.09)	(0.10)	(0.10)
× Age 40-44	-0.27	-0.21	-0.19
	(0.13)**	(0.13)*	(0.13)
× Age 45-48	-0.99	-0.95	-0.85
	(0.16)***	(0.13)***	(0.19)***
	Observations = 5,799,964		
Males			
Early ZT Law Exposure			
× Age 35-39	0.09	0.16	0.17
	(0.14)	(0.14)	(0.14)
× Age 40-44	-0.21	-0.15	-0.11
	(0.16)	(0.17)	(0.16)
× Age 45-48	-1.02	-1.06	-0.91
	(0.55)*	(0.55)*	(0.53)*
	Observations = 2,797,422		
Females			
Early ZT Law Exposure			
× Age 35-39	-0.28	-0.25	-0.27
	(0.13)**	(0.13)*	(0.14)*
× Age 40-44	-0.33	-0.28	-0.26
	(0.16)**	(0.15)*	(0.16)*
× Age 45-48	-0.97	-0.85	-0.79
	(0.72)	(0.70)	(0.81)
	Observations = 3,002,542		
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.