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Abstract

With the goal of lowering the cost of age verification and curbing underage drinking and smoking, vertical identification laws (VILs) — which require state drivers' licenses issued to individuals under age 21 to be to be vertically oriented — have been adopted widely across the United States. In Volume 32, Issue 5 of the Journal of Health Economics, Bellou and Bhatt (2013) used data from the 1991-2009 national Youth Risk Behavior Survey (YRBS) and found that VIL adoption was associated with a reduction in teenage drinking and smoking. This study revisits these findings (1) using newly available data from the state YRBS, (2) exploiting additional policy variation and a decade of more recent data, and (3) employing novel difference-in-difference estimation strategies designed to expunge bias in estimated treatment effects caused by heterogeneous and dynamic policy impacts. In contrast to prior results, we find little support for the hypothesis that VILs are an effective policy tool to curb teenage alcohol or cigarette use.

Keywords: vertical identification requirements; vertical license laws; teenage drinking; alcohol consumption; smoking

JEL codes: I12; K42

1. Introduction

"Cigarette and alcohol purchases make up almost half of [average convenience stores'] transactions. When the stores are especially busy… a clerk is more prone to misread driver's licenses. If the card were vertical, no reading would be required; the card's appearance would allow the clerk quickly to accept or reject the purchase."

– Michigan Senate Analysis of Vertical Identification Law (S.B. 924 & 925), 2002

With the goal of lowering the cost of age verification to venders, vertical identification laws (VILs) have been widely adopted across the United States. VILs require state drivers' licenses and identification (ID) cards issued to those under the age of 21 be vertically oriented.[1] Upon reaching age 21, a young adult may obtain a horizontally oriented license or ID card. VILs are primarily designed to curb illegal sales of tobacco and alcohol to minors by reducing (1) the likelihood that a vendor makes an error in reading an ID card, and (2) the supply of passable "false IDs" that underage consumers could obtain from legally aged peers (i.e., siblings, friends, acquaintances) and present at the time of purchase.[2,3]

In Volume 32, Issue 2 of this journal, Bellou and Bhatt (2013) used repeated cross-sectional data from the 1991-2009 national Youth Risk Behavior Surveys (YRBS) and, employing a two-way fixed effects (TWFE) empirical approach, found that the enactment of a state VIL was associated with a 4 percentage-point (9 percent) decline in prior-month alcohol consumption and a 3 percentage-point (11 percent) decline in prior-month smoking among 16-year-old U.S. high school students.

[1] See, for example, *Michigan S.B. 924, 925* and *Tennessee Senate Bill 384 – 2017.*

[2] A false ID is more likely to be verified for purchase if the in-person purchaser's appearance sufficiently resembles the ID card presented. Thus, the age difference between the legitimate ID card holder and the underage consumer is one factor that will determine whether a false ID is accepted. For example, while a 16-year-old smoker might be able to successfully use the ID card of his 18-year-old same-sex sibling or peer to purchase cigarettes, it is it less likely that a 16-year-old would be able to physically approximate the appearance of a 21-year-old with a horizontal ID.

[3] There is evidence that in the absence of vertical ID laws, underage purchases are relatively common, including among vendors who check the purchaser's ID. For instance, in March 2002, the Michigan Liquor Control Commission hired 19-year-olds to enter restaurants or stores to purchase alcohol. The commission found that 29 percent of the time, vendors sold alcohol to these teenagers and in 81% of those cases, "the clerk asked for identification from the minor, looked at the card, and then sold the minor alcohol" (Michigan State Senate Analysis for S.B. 924 and S.B. 925)

Taken at face value, the findings of Bellou and Bhatt (2013) suggest that the enactment of VILs generated important health benefits. The health costs of underage drinking are estimated to be $28 billion per year (2019 dollars) (Sacks et al. 2015). Moreover, excessive (i.e., binge) drinking among adults, which has often been linked with early alcohol initiation (Hingson et al. 2006; Hingson and Zha 2009), generates an additional $249 billion per year in health costs (Sacks et al. 2015). The long-run costs of tobacco use, where the majority of initiation decisions are also made prior to age 18, (Everett et al. 1999; Gilliand 2006; Centers for Disease Control and Prevention 2020a) — are estimated to be nearly $325 billion per year (2019 dollars) (Office of the Surgeon General 2014).

From a social welfare perspective, many of these health costs may not be fully internalized by youths when making consumption decisions. This may be due to (1) irrational decision-making by teenagers caused by an underdeveloped prefrontal cortex (Banks et al., 2007; Gongora et al., 2019; Rees et al. 2022), (2) time-inconsistent preferences, which result in teenagers giving insufficient weight to the future costs of addiction when making current consumption decisions (Gruber and Köszegi 2001; 2004; Friedson and Rees 2020), and (3) negative externalities associated with alcohol and cigarette consumption, such as traffic fatalities to third parties (Carpenter 2004, Carpenter and Dobkin 2009), crime (Carpenter 2005; Carpenter and Dobkin 2015; Anderson et al. 2018), and disease caused by exposure to secondhand (or thirdhand) smoke (Max, Sung, and Shi 2012, Centers for Disease Control and Prevention 2021a). If the costs of implementing VILs are small relative to the external benefits of curbing underage drinking and smoking[4], Bellou and Bhatt's findings would suggest that the adoption of a VIL is welfare enhancing

However, we believe that there are several reasons why the findings of Bellou and Bhatt (2013) may be worthy of reconsideration. First, their *state* policy analysis was conducted using the *national* YRBS. An important drawback of using this dataset (in isolation) to study the effects of a state policy shock is that it not designed to be representative of state-level trends in youth risky

[4] These cost of implementing a VIL include the value of utility losses to underage smokers and drinkers from lost consumption, the costs of evasion (i.e., costs of obtaining a false ID, soliciting older third-party purchasers, cross-state travel, and theft), and the costs to states of implementing a VIL (i.e., creating new ID cards, monitoring residents' 21st birthday to issue a new ID card, increasing the overall supply of ID cards). A number of states have provided fiscal estimates of the coasts of VIL adoption. For instance, the fiscal cost of implementing a state VIL was estimated to be approximately $1.4 million in Michigan (in 2019$) and $105,000 in Tennessee (in 2019$) (Michigan State Senate Analysis for S.B. 924 and S.B. 925 2002; Tennessee General Assembly Fiscal Review Committee 2017). Such reports focused largely on the shorter-run costs of creating and implementing the technology needed for verification.

behaviors. Over the 1991-2009 period, the median state in the national YRBS included 38 16-year-olds per year with information on their drinking and smoking behaviors. A total of 14 states contributed fewer than 20 observations on 16-year-olds per year. If measurement error in state-level adolescent drinking and smoking caused by the sampling design was (unluckily) correlated with VIL enactment, then the TWFE estimate reported in earlier work could be biased.

A second limitation of the national YRBS is that it does not sample individuals in all states in each wave of data collection. Between 1991 and 2009, 43 U.S. states adopted VILs, but 10 of these states did not contribute to identification of their estimated treatment effects due to a lack of national YRBS data before and after the policy change. Among treatment states that did contribute to their identification, the average number of post-treatment waves for which there were data available was about 2.5 waves.

Finally, the estimated treatment effects (and event-studies) generated by Bellou and Bhatt (2013) were largely based on TWFE estimates.[5] Recent developments in the difference-in-differences literature suggest that in the presence of heterogeneous and dynamic treatment effects, TWFE estimates of treatment effects may be biased (Goodman-Bacon 2021; Sun and Abraham 2021; Callaway and Sant'Anna 2021). Given that there are relatively few never adopters by 2009 (eight states) and all states had adopted a VIL by 2019, exploring the robustness of the estimated treatment effect to an alternate difference-in-difference estimator that excises "contaminated" controls may be important.[6]

This study uses a new data source and novel econometric strategies to revisit the conclusion that VILs are effective at curbing teenage drinking and smoking. In doing so, we make four contributions. First, we augment Bellou and Bhatt's analysis of the national YRBS with data from a new source: the *state* Youth Risk Behavior Surveys. These data include the same measures of drinking and smoking that are available in the national YRBS but include annual samples that are on average about six (6) times larger than those of the national YRBS. Importantly, while the state YRBS survey can be made nationally representative (akin to the national YRBS), the state YRBS surveys are, when properly weighted, also designed to produce estimates of health behaviors that are representative at the state level. This is an important measurement advantage given that we study a

[5] They also included specifications that included state-specific linear and quadratic time trends, which we discuss below.
[6] A Goodman-Bacon decomposition shows that approximately half (47 percent) of the weight in a TWFE estimate is obtained from a comparison of "later versus earlier" adopting VIL states (during the 1991-2019 period).

state policy and employ a research design (DD) that relies on state-level trends in youth drinking and smoking behaviors to identify VIL effects.

Second, we exploit more policy variation than was available to Bellou and Bhatt (2013) by (1) adding a decade of additional data to the analysis sample (the period between 2009 and 2019), and (2) supplementing analyses of the national YRBS with the state YRBS, a data source that has information on teenage drinking in states that are not covered by the national YRBS. Together, these data enhancements allow an additional 16 states and the District of Columbia to contribute to identification.[7] Moreover, our data also permit longer-run estimates of the effects of VILs through the inclusion of additional post-treatment data. For instance, nearly all (47) of the treatment states in our augmented analysis sample have at least three waves (six years) of post-treatment data and the vast majority have post-treatment data for four to five ways (eight to 10 years).

Third, we supplement the TWFE estimates relied upon by Bellou and Bhatt (2013) with the newly developed difference-in-differences (DD) estimator developed by Callaway and Sant'Anna (2021). This approach allows us to expunge potential bias caused by dynamic treatment effects across adoption time by flexibly relying on not-yet-adopters to serve as counterfactuals for VIL-enacting states.

Finally, we explore the potential mechanisms through which VILs might operate. We examine whether VILs differentially impact underage drinking and smoking by the strictness of the alcohol and tobacco control environments. This includes the presence of other policies that limit youth access to alcohol and tobacco, such as scanner ID laws, increased fines to vendors for sales to underage consumers, increases in the minimum legal purchasing ages for tobacco products, increases in beer, alcohol, and e-cigarette taxes, social host laws and zero tolerance drunk driving statutes. Additionally, we exploit novel data on youths' usual sources of alcohol and cigarettes (i.e., direct purchase, third-party purchase, and "social sources" such as friends) to explore whether VILs have changed how youths access these products. This may shed some light on the mechanisms

[7] Between 2009 and 2019, eight (8) additional states adopted VILs: California (2010), Maine (2011), Minnesota (2018), Missouri (2012), New York (2013), Oregon (2019), South Carolina (2011), and Tennessee (2018). Adding national YRBS data between 2009 and 2019 allows California, Maine, Missouri, New York, Oregon, and Tennessee to contribute to identifying variation; it also provides the necessary post-treatment data to allow the District of Columbia (2004), New Hampshire (2008), North Carolina (2008), and Rhode Island (2002) to contribute to identification when using the 1991-2019 national YRBS. Over the period 1991-2009, the state YRBS uniquely contributes an additional 6 states to identifying variation: Alaska (2004), Montana (2008), Nebraska (2003), North Dakota (2006), South Dakota (2009), and Wyoming (2005). Adding state YRBS data for 2011-2019 allows South Carolina to contribute to identifying variation, bringing the total number of states identifying the treatment effect in the augmented YRBS sample to 50. Minnesota is the only state for which we do not have pre- and post-VIL data in either the state or national YRBS.

through which VILs operate given that they are most directly expected to reduce direct own purchases of alcohol and tobacco from vendors.

2. Background

The social costs of tobacco and excessive alcohol consumption are substantial (Cawley and Ruhm 2011; French and Mclean 2006, Carpenter and Dobkin 2011). There is strong evidence that habit formation around tobacco use (Friedson and Rees 2020; Centers for Disease Control and Prevention 2020a) and problem drinking (Cook and Moore 2001; Williams 2005; Chaloupka and Xu 2011; Centers for Disease Control and Prevention 2021b) begin prior to age 18. Thus, policy interventions to curb teenage smoking and alcohol use can generate important social benefits. However, the effectiveness of recent policy strategies to achieve these ends is mixed.

2.1 Anti-Drinking Efforts and Youth Alcohol Use

According to the 2019 national Youth Risk Behavior Survey, 29 percent of U.S. high school students report any prior-month alcohol consumption and 14 percent reporting prior-30-day binge drinking. Among those students who engaged in binge drinking, 61 percent reported having done so on three or more occasions (Jones et al. 2020, Centers for Disease Control and Prevention 2021b).

Teenage alcohol use has been documented to generate substantial public health costs (Carpenter and Dobkin 2011). Some of these costs are privately borne, including known risks of future alcohol-related health problems such as liver disease (O'Shea et al. 2010) or harm to oneself from alcohol-related accidents (Chikritzhs and Livingston 2021). Privately borne costs also include longer-run health and labor market costs to oneself that arise from rational addiction to alcohol (Becker and Murphy 1988). However, other costs are external, including those generated from (1) "internalities" that arise due to consumption decisions of those with time-inconsistent preferences (Gruber and Kozgeki 2001), or (2) negative externalities such as drunk driving-related harm to non-drinkers (Carpenter 2004, Kenkel 1993). Driving after drinking is a particular problem for younger individuals, with teenagers accounting for 15 percent of alcohol-related fatal crashes (Insurance Information Institute 2022) and the traffic fatality rate for 15-to-19-year-olds among the highest of any age cohort (Anderson et al. 2013).

There have been a plethora of public policies that aim to reduce teenage drinking and their associated negative externalities over the last three decades, including increases in the minimum legal

purchasing age (MLPA) for alcohol (Cook and Moore 2001; Carpenter 2005; Carpenter et al. 2007; Carpenter and Dobkin 2011), hikes in beer taxes (Saffer and Grossman 2017), and enactment of social host laws (Dills 2010), keg registration laws (Yoruk and Xu 2021), zero tolerance drunk driving laws (Carpenter et al. 2007) and scanner ID laws (Yoruk 2014; 2018; Zheng 2018). Their effectiveness has been mixed.

The MLPA has been shown to be the most consistently effective policy strategy to reduce both heavy drinking and alcohol-related deaths among teenagers and young adults (Cook and Moore 2001; Carpenter 2005; Carpenter et al. 2007; Carpenter and Dobkin 2011).[8] Carpenter and Dobkin (2011) use a regression discontinuity design (RDD) and find that an MLPA of 21 for alcohol purchases is associated with a 20 percent reduction in past-month drinking days and a 12 percent reduction in traffic fatalities.

In addition, zero tolerance (ZT) drunk driving laws, which make it illegal for persons under age 21 to have detectable amounts of alcohol in their blood (BAC > 0) while operating a motor vehicle, also have been shown to reduce traffic fatalities among 18-to-20-year-olds (Chang et al. 2012), driven by a reduction in number of drinks consumed, heavy episodic drinking and binge drinking (Carpenter 2004).[9] For instance, Carpenter (2004) finds that ZT laws led to a 13 percent reduction in underage heavy episodic drinking for males ages 18-to-20.

Evidence on the effectiveness of raising alcohol taxes in curbing teen drinking is mixed, in part owing to limited within-state variation in tax rates (Cawley and Ruhm 2011). Still, in a study using a particularly long state panel with more extensive policy variation than is typically available, Carpenter et al. (2007) use data from the 1976 to 2003 Monitoring the Future Study and find that beer taxes are negatively related to drinking participation among underage teenagers. This suggests that teenagers in the 1970s, 1980s, and 1990s were sensitive to changes in the prices of alcohol caused by state hikes in beer taxes.

More recently, attention has turned to other laws that target underage drinking, including scanner ID laws (SIDLs), which require restaurants, bars, and retailers to use electronic scanners,

[8] Carpenter and Dobkin (2011), based on the predicted increase in drinking incidence for individuals aged 18-20 if the minimum legal purchasing age were lowered to 18, calculate an additional private cost of at least $15 per underage drink to the drinker, and an additional public cost of nearly $3 per underage drink to society. Coupled with their estimated increase of 4.56 million drinks per 100,000 person-years if the drinking age were lowered, this suggests that the annual private cost of underage drinking is about $70 million per 100,000 individuals, and the annual public cost of underage drinking is about $12 million per 100,000 individuals.

[9] ZT law violators are generally met with license suspension and fines (Carpenter et al. 2007).

which read birth date information stored in bar codes on the ID cards to confirm that their customers are of the legal drinking age.[10] Evidence on the effectiveness of SIDLs is mixed. Yörük (2014) uses the data from the National Longitudinal Survey of Youth 1997 and finds that the enactment of a SIDL reduce the average number of drinks for minors by 0.22 drinks per day. Adding credence to this result, Nesson and Shrestha (2021) use data from the National Highway Traffic Safety Administration's Fatality Analysis Reporting Systems (FARS) and find that the enactment of a state SIDL reduces the number of alcohol related traffic fatalities for individuals ages 16-to-18 by approximately 13 percent. On the other hand, Zheng (2018) raises concerns about whether SIDLs actually reduce heavy drinking among youths. In her replication of Yoruk (2018), Zheng (2018) generates an event-study analysis and concludes that the trends assumption underlying Yörük's empirical approach is violated. Additionally, Zheng (2018) documents the absence of any SID law effects on drinking using national YRBS data.[11]

Other studies have examined policies related to social events involving drinking among underage participants (i.e., high school or college parties at which alcohol is served). Social host laws hold adults legally liable for hosting underage drinking parties, on alcohol-related traffic fatalities. Dills (2010) finds that the enactment of a social host law is associated with a 9 percent reduction in alcohol-related traffic fatalities.[12] A policy with similar goals, a keg registration law, requires alcoholic beverage retailers to record personal information of customers purchasing beer kegs and attach registration labels to kegs. Yörük and Xu (2020) find that binge drinking (respondents reporting having drunk five or more drinks in at least 1 day in the past 30 days) falls by 8 percent for underage individuals following the enactment of keg registration statutes.

2.2 Youth Tobacco Use and Tobacco Control Policies

Consistent with evidence on drinking, the vast majority of adult smokers initiate tobacco use as teenagers, often younger teenagers. Nearly 9 out of 10 adult smokers had their first cigarette by age 18 (Centers for Disease Control and Prevention 2020a), and approximately 1 in 3 high school students reported current use of some form of tobacco product (Creamer et al. 2020). Early

[10] Vendors who can document use of ID scanners as a means of age verification can legally exonerates vendors who inadvertently sell alcohol to minors (Yorük 2014).
[11] See Yörük (2018) for a response.
[12] Then, using data from the BRFSS to explore the mechanism(s) to explain this finding, Dills (2010) finds that the reduction is likely driven by a reduction in driving after drinking rather than solely a drinking effect.

initiation of cigarette smoking is correlated with increased probability of adult smoking (Everett et al 1999) and recent evidence suggests that if youths are exposed to stricter tobacco control policies (i.e., higher cigarette taxes) during their teenage years, the probability of adult tobacco use is substantially reduced (Friedson and Rees 2020).

The health benefits of deterring youth use may, therefore, be quite large. Tobacco use is the leading cause of preventable death in the United States and is responsible for nearly 500,000 lives lost per year (Centers for Disease Control and Prevention 2021c), 20 percent of all fatalities to men (GBD 2019 Collaborators 2021), and over $240 billion in direct public health costs annually (2019$) (Xu et al. 2021). While some of these costs may be internalized by youths making "rational" decisions over addictive good consumption, the social welfare arguments for curbing youth smoking center around the likelihood that teenagers have time-inconsistent preferences that cause them to give insufficient weight to future costs of addiction (Crettez et al. 2020), fail to account for the external costs of smoking when choosing current consumption (O'Donoghue and Rabin 2001), and may lack the neurological capacity to make rational choices over current consumption decisions that have important long-run addiction-related costs (Bryan et al. 2021).

The most prominent tobacco control policies aimed at curbing youth tobacco use include cigarette taxes, e-cigarette taxes, minimum legal purchasing ages for tobacco products, and the enactment of clean indoor air laws (including at schools). With respect to cigarette taxes, while there is strong evidence that higher cigarette taxes were effective at curbing youth tobacco use in the 1990s and 2000s (Carpenter and Cook 2008), more recent results suggest that youth tobacco use is largely insensitive to changes in cigarette taxes (Anderson et al. 2020; Carpenter and Sansone 2021; Hansen et al. 2017). This finding may be explained by the marginal smoker having a more inelastic demand for tobacco that in the past.

The explosion of alternate tobacco markets in the 2010s, most notably for e-cigarettes and other vaping products (Creamer et al. 2020; Cullen et al. 2019; Centers for Disease Control and Prevention; 2020b) led policymakers to play "catch-up" in trying to deter the use of these new products among youths. Prominent efforts included the enactment of e-cigarette taxes (Pesko et al. 2021) and the imposition of minimum legal purchasing ages for e-cigarettes (Friedman 2015; Abouk and Adams 2017). While MLPAs for e-cigarettes and higher taxes on electronic nicotine delivery systems (ENDS) were effective at reducing youth vaping, there is mounting evidence that each also induced substitution toward cigarettes, which could generate worse public health outcomes (Pesko et al. 2021; Courtemanche et al 2020; Friedman 2015).

To avoid tobacco product substitution among youths in response to targeted anti-tobacco policies, a new set of studies have begun to explore Tobacco-21 laws, which impose a MLPA of age 21 for all tobacco products (i.e., cigars, cigarettes, e-cigarettes, snus, pipe tobacco). Friedman and Wu (2020) and Bryan et al. (2021) find that state and local Tobacco-21 laws were effective at deterring youth tobacco use, and that the health benefits may extend to minors ages 16-to-17 who often rely on informal social sources for tobacco products.

A final set of anti-tobacco efforts include implementation of clean indoor air laws, including smoking bans on school property (see Chaloupka and Weschler 1997, Wakefield et al. 2000) and tobacco advertising bans (Pierce et al. 1991, Slater et al. 2007). The pattern of findings from these studies suggest that such policies may be effective at curbing youth smoking.

2.3 Vertical Identification Laws

Two studies of which we are aware have examined the impact of VILs on youth risky behaviors. The first, Bellou and Bhatt (2013), finds that VILs significantly reduce smoking and drinking among 16-year-olds. For tobacco, they also explore the mechanism that could explain their finding; they find that VILs increase the likelihood that buyers are asked to present an ID prior to a cigarette purchase and reduce the likelihood of both direct purchase (from a store) as well as the probability of borrowing (or bumming) cigarettes from someone else. Bellou and Bhatt (2013) offered no comparable analysis of how VILs affected how youths typically obtain alcohol due to data limitations.[13]

Second, in the course of studying the effect of false ID laws with scanner provisions — which provide incentives for retailers to use scanner technology when verifying the age of would-be purchasers — on traffic fatalities, Nesson and Shrestha (2021) show that VIL enactment has no statistically significant or economically important effect on alcohol-related traffic fatalities among 16-to-20-year-olds. This finding suggests that if VILs impact teenage drinking, these effects do not generate downstream reductions in alcohol-related fatalities.

3. Data

3.1 Youth Risk Behavior Surveys

[13] The national YRBS included only two waves of data (2007 and 2009) on youths' usual sources of alcohol. Only five states enacted a VIL during the years 2008 and 2009, and of those states, only one (Georgia) appears in both the 2007 and 2009 waves of the national YRBS.

Our analysis begins with the identical dataset used by Bellou and Bhatt (2013), the national Youth Risk Behavior Surveys (YRBS). The national YRBS is a repeated, cross-sectional, biennial, school-based survey of U.S. high school students conducted by the Centers for Disease Control and Prevention. When weighted, the survey is designed to be representative of all U.S. high school students. This pencil-and-paper survey covers health topics related to health behaviors, including alcohol consumption and tobacco use. Our focus is on 16-to-18-year-olds, following Bellou and Bhatt (2013). Like the original authors, we begin by focusing on 16-year-olds who are treated immediately and then examine 17-to-18-year-olds, whose behavior may be affected with a lag.[14,15] We begin by using data over the period examined by Bellou and Bhatt (2013), 1991-2009, and later extend the analysis by a decade, analyzing the period 1991-2019.

We supplement our analysis of the national YRBS with data from the state YRBS. Like the national YRBS, the state YRBS is also a school-based survey of U.S. high school students. The surveys are coordinated by the CDC, but typically administered by individual state Departments of Education or Health and Human Services across public and private schools. Over the period from 1991-2019, 47 states appear in the state YRBS. We apply sample weights such that (1) each state's sample is representative of the state's population of high school students, and (2) the pooled sample of states is representative of the population of the U.S. population of 16-year-olds.[16]

Finally, to maximize states contributing to identification, we augment state YRBS data (which measure state-level changes in teenage risky behaviors with less error than the national YRBS) with the national YRBS when state-year data cells from the state YRBS are missing. This augmented state and national YRBS dataset has the important advantage of maximizing state policy variation available for identification (see discussion below). An approach similar to this has been used in a number of YRBS-based studies that analyze the impacts of state public policies on youth risky behaviors when each YRBS source contributes unique states to identifying the effects of a state

[14] In separate regressions, we also examine those ages 17 or 18, who would be affected in a treatment state one or two years following the enactment of the law For instance, if a VIL were enacted by a state in 2011, anyone age 16 in the state would be treated, while 17-year-olds would be coded as treated by the state statute in 2012, and 18-year-olds would be coded as treated by the state statute beginning in 2013.

[15] We note that there are a number of ways to identify a sample of those youth who are treated. For instance, one could examine birth cohorts of individuals who were affected when they were ages 16 (the minimum legal license age in most states) through 20 and not grandfathered by previous horizonal licensing (or ID) policies.

[16] These weights are generated as the product of each person's state YRBS-provided survey (renormalized) weight and the state-by-year population of 16-year-olds, as estimated by the National Cancer Institute's Surveillance, Epidemiology and End Results Program (SEER) (see Bryan et al. 2021 for an application).

policy (see, for example, Hansen et al. 2017; Anderson et al. 2020; Rees et al. 2022; Pesko et al. 2021).[17,18] From 1991-2019, we identify 53,454 16-year-old high school students in the national YRBS, 385,353 in the state YRBS, and 408,580 in the "augmented" state and national YRBS.[19]

3.2 Dependent Variables

Our primary focus is on the two main outcome variables identified by Bellou and Bhatt (2013), *Alcohol Use* and *Cigarette Use*. *Alcohol Use* is a dichotomous variable set equal to 1 if the respondent answered the question, "During the last 30 days, on how many days did you have at least one drink of alcohol?" with a response of one day or more; it is set equal to 0 otherwise. Over the period from 1991-2019, 43.0 percent of 16-year-olds in the national YRBS reported drinking alcohol in the prior 30 days. If we use the state YRBS, this mean is comparable (37.6 percent).

We measure *Cigarette Use* analogously. *Cigarette Use* is set equal to 1 if the respondent answered the question, "During the last 30 days, on how many days did you smoke cigarettes?" with a response indicating a positive number of days; it is set equal to 0 otherwise. During the 1991-2019 period, 22.8 percent of 16-year-olds in the national YRBS reported smoking in the last 30 days. This number was 17.5 percent in the state YRBS and 20.1 percent in the augmented YRBS.

In addition to these measures of alcohol and cigarette use on the extensive margin, we also explore measures that capture heavier drinking behaviors. For instance, we set *Binge Drinking* equal to 1 if the respondent reports having had five (four for females beginning in 2017) or more drinks in a row on a single occasion in the past 30 days and 0 otherwise. Along the same lines, *Frequent Binge Drinking* is set equal to 1 if the respondent reports binge drinking on three or more days in the past 30 days. We find that 25.5 percent (10.9 percent) of 16-year-olds in the national YRBS, reported binge drinking (frequent binge drinking). These means were comparable in the state and augmented YRBS.[20] We also explicitly examine heavier drinking on the intensive margin, including *Binge*

[17] To construct weights to make the augmented sample nationally representative, we use the sample weights described above for state-by-year cells and use the SEER data to calculate the state-by-year share of the youth population that falls in each age-by-gender-by-race/ethnicity bin *i*, s_{ist}. We then calculate each respondent's sample weight as $[s_{ist}/n_{ist}]$*StatePop14_18$_{st}$, where n_{ist} is the number of YRBSS sampled individuals in age-by-gender-by-race-ethnicity bin *i* in state *s* at year *t* and StatePop14_18$_{st}$ is the SEER estimated population of 14-to-18-year-olds in state *s* at year *t*.
[18] We also explore the effects if we pool together observations from both the state and national YRBS survey, a "fully combined YRBS" approach, with a very similar pattern to that produced using the augmented YRBS sample.
[19] In addition, over the same period (1991-2019), there are 86,576 17-to-18-year-old high school students in the national YRBS, 490,977 in the state YRBS, and 527,098 in the "combined" YRBS samples.
[20] In the state (augmented) YRBS, 21.7 percent (23.6 percent) of 16-year-olds reported binge drinking, and 8.9 percent (9.9 percent) reported frequent binge drinking.

Drinking | Alcohol Use = 1 and *Frequent Binge Drinking | Alcohol Use = 1*. We find that 61.6 percent of 16-year-old drinkers in the national YRBS reported binge drinking.

With respect to smoking, we measure *Everyday Smoking*, set equal to 1 if the respondent reports having smoked cigarettes on each of the past 30 days, and 0 otherwise; and set *Everyday Smoking | Cigarette Use = 1* equal to 1 if a self-reported smoker reports smoking cigarettes on every day of the past 30, and 0 otherwise. We find that 6.9 percent (5.4 percent) of 16-year-olds in the national (state) YRBS were everyday smokers and, conditional on any smoking 30.1 percent (31 percent) consumed cigarettes on every day of the prior 30 days.

3.3 Vertical Identification Laws

We code vertical identification laws (VILs) identically to Bellou and Bhatt (2013) over the 1991-2009 period and then augment our coding of the statute using our own searches of state statutes. Enactment years are shown in Appendix Table 1.

Panel (a) of Figure 1 shows states contributing identifying variation across the three YRBS datasets during the period from 1991-2009, the period studied by Bellou and Bhatt (2013). In panel a(i), we show that in the national YRBS, 33 states[21] contribute to identification over the 1991-2009 period. Over the same period, 31 states contributed to identification in the state YRBS (see panel a(ii)). Notably, however, the states that contributed to identification in the state YRBS were somewhat different than in the national YRBS, with nine (9) states that contribute to identification in the state, but not the national YRBS[22]; eleven (11) states that contribute to identification in the national, but not the state YRBS[23]; and 22 states that commonly contribute to identification in both datasets.[24] When we augment the state YRBS with the national YRBS over the 1991-2009 period, 41 states contribute to identification (panel a(iii)).

In panel (b) of Figure 1, we extend the analysis period to 1991-2019. A total of 42 states and the District of Columbia contribute to identification in the national YRBS (panel b(i)); in the state

[21] This count does not include the District of Columbia (policy year 2009), which appears only in the National YRBS for years 1995 and 2011 and thus does not contribute to any of the identifying variation for the 1991-2009 period.
[22] These states include Rhode Island (2002), Nebraska (2003), Alaska (2004), Wyoming (2005), North Dakota (2006), Montana (2008), New Hampshire (2008), North Carolina (2008), and South Dakota (2009).
[23] These states include Colorado (1994), Delaware (1996), Virginia (1999), Arizona (2001), Louisiana (2001), Pennsylvania (2001), Texas (2001), Washington (2001), Maryland (2003), Oklahoma (2003), and Kansas (2004)
[24] These states include West Virginia (1999), New Mexico (2000), Iowa (2001), Kentucky (2001), Mississippi (2001), Connecticut (2002), Idaho (2002), Nevada (2002), Ohio (2002), Michigan (2003), Vermont (2003), Florida (2004), Massachusetts (2004), New Jersey (2004), Alabama (2005), Hawaii (2005), Illinois (2005), Wisconsin (2005), Arkansas (2006), Utah (2006), Indiana (2007), and Georgia (2009).

YRBS, 36 states contribute to identification, and in the augmented YRBS, 48 states and the District of Columbia identify the treatment effect, as shown in panel c(iii).

4. Methods and Results

4.1 Benchmarking Estimates from 1991-2009 National YRBS

We begin by drawing data from the 1991-2009 national YRBS survey, the same period and dataset examined by Bellou and Bhatt (2013). Our approach is to estimate an identical two-way fixed effects (TWFE) specification via ordinary least squares (OLS):

$$Y_{ist} = \beta_0 + \beta_1 VIL_{st} + \beta_2 X_{ist} + \theta_s + \tau_t + \varepsilon_{ist} \quad (1)$$

where Y_{ist} is one of the above-mentioned outcomes for high school student *i* residing in state *s* in year *t*; VIL_{st} is an indicator for whether state s has enacted a vertical license law in year t; and $\mathbf{X}_{ist}$ is a vector of the controls used by Bellou and Bhatt (2013), including individual demographics (race, grade, sex, and, when applicable, age) and baseline state-specific, time-varying controls (graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of at least 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income (2019$).). In addition, θ_s is a time-invariant state effect, τ_t is a state-invariant year (wave) fixed effect, and ε_{ist} is the error term. All regressions are weighted using the national YRBS survey weight and standard errors are clustered at the state level (Bertrand et al. 2004).

In Table 2, we reproduce estimates from Bellou and Bhatt (2013) using the 1991-2009 national YRBS data. Columns (1) through (3) present findings for *Alcohol Use* and the columns (4) through (6) for *Cigarette Use*.

Controlling for state and wave (year of survey) fixed effects (column 1), we find that the enactment of VILs is associated with a statistically significant 3.2 percentage-point decline in alcohol consumption, representing a 6.3 percent decline relative to the pre-treatment mean of the dependent variable in VIL-enacting states (0.50). The addition of controls for individual demographic characteristics (column 2) and state-level policy controls (column 3) has very little impact on the

estimated treatment effect. In our fully specified model (column 3), we find that VIL enactment is associated with a 4.1 percentage-point (8.2 percent) reduction in the probability of alcohol use among 16-year-old high school students. Our estimated marginal effect is nearly identical to the estimate (-0.038) obtained by Bellou and Bhatt's analogous model (Table 1, column 2, page 358).[25]

Turning to tobacco use in the remaining columns, we also replicate the result of Bellou and Bhatt (2013) with regard to tobacco use. In our fully specified model (column 3), we find that VIL enactment is associated with a 3.1 percentage-point reduction in cigarette use among 16-year-olds. This represents an approximately 9.5 percent reduction in cigarette consumption relative to the pre-treatment mean in VIL-enacting states. Again, this estimate is nearly identical to that obtained by Bellou and Bhatt (2013).

To provide descriptive tests of the parallel trends assumption, we estimate the following event-study regression:

$$Y_{ist} = \sum_{j=-4}^{4} \pi_j D_{st}^{j} + X_{ist} + \theta_s + \tau_t + \varepsilon_{ist} \qquad (2)$$

where D^{j}_{st} it is a treatment indicator for VIL enactment *j* periods (waves) from *t*.[26] The πs are the coefficients on the VIL effect; the reference period is *j-1*, the year prior to VIL enactment.

An event-study analysis of alcohol use in the national YRBS, shown in panel a(i) of Figure 2, is consistent with a post-treatment decline in teenage drinking, with the largest effect observed one wave (2-3 years) after enactment.[27] However, an examination of the pre-treatment trend also provides suggestive evidence that teen drinking rates were rising faster in VIL states than non-VIL states prior to the policy's adoption. This could suggest a violation of the parallel trends assumption, but, as Bellou and Bhatt (2013) noted, none of the lead coefficients is statistically distinguishable from zero at conventional levels. Moreover, the enactment of the VIL does reflect a break in the trend in relative youth drinking rates across treatment and control states, particularly in the period 0 to 3 waves after VIL enactment.

[25] The small difference is likely due to missing observations from the 1993 national YRBS data available from the Centers for Disease Control and Prevention (CDC), since we are unable to recover state identifiers for approximately 20 percent of the available 1993 sample. In our correspondence with the CDC, it was indicated that such historical identifiers.

[26] We note that indicators are binned at the "endpoints" (the last lead and lag variables).

[27] Notably only the coefficient tied to the YRBS wave immediately following VIL enactment is statistically significant at the 5 percent level.

With regard to cigarette use (panel b(i) of Figure 2), the event study shows a similar pattern as for alcohol use. While none of the post-treatment coefficients is statistically distinguishable from zero at conventional levels, we do observe a reduction in smoking participation in the enactment wave (0-1 years post-treatment). This effect becomes smaller (in absolute magnitude) in subsequent waves. Next, continuing to follow the authors, we explore the sensitivity of estimates to the inclusion of state-specific time trends:

$$Y_{ist} = \beta_0 + \beta_1 VIL_{st} + \boldsymbol{\beta_2 X}_{ist} + \theta_s + \tau_t + g_s(t) + \varepsilon_{ist} \quad (3)$$

where $g_s(t) = \theta_s * t$ (state-specific linear time trend) or $g_s(t) = \theta_s * t + \theta_s * t^2$ (state-specific linear and quadratic time trend). A potential benefit of these additional right-hand side controls is that they may reduce omitted variable bias in the estimate of β_1 by controlling for state-level unobservables unfolding linearly (or quadratically) that are incidentally correlated with VIL enactment and teen alcohol use (or cigarette use). On the other hand, the inclusion of state-specific time trends may obscure dynamic effects of VILs (Wolfers 2006), or isolate variation in VILs that is less exogenous than in models excluding such trends (see, for example, Neumark, Salas, and Wascher 2014).

The results from these specifications, shown in Panel I of Appendix Table 2, continue to show a negative relationship between VILs and teen drinking (columns 1 and 2) and smoking (columns 3 and 4). The effect sizes largely mirror those reported by Bellou and Bhatt (2013).

4.2 Estimates Using State YRBS and Augmented YRBS Data, 1991-2009

In panel I of Table 3, we turn to our attention to the effects of VILs on alcohol (columns 1 through 3) and cigarette (columns 4 through 6) use in the state YRBS. We focus on the same 1991-2009 period and the same model specifications as in Table 2 but conduct our analyses using the state rather than the national YRBS. In sharp contrast to our findings from the national YRBS, results from the state YRBS reveals no evidence that the enactment of a VIL is associated with a statistically significant (or economically important) decline in drinking or smoking among 16-year-olds. In our fully specified model (column 3), we find that the adoption of a VIL is associated with a statistically insignificant 0.9 percentage-point decline in teenage drinking, and a statistically insignificant 0.4 percentage-point decline in teenage smoking. These null effects are sufficiently precisely estimated

such that we can, with 95 percent confidence, reject estimated treatment effects reported in columns (3) and (6) of Table 2 (-0.041 and -0.031, respectively).[28]

What could explain the differing results in the national versus state YRBS data? One answer could be heterogeneous treatment effects by state given that, as noted above, the national and state YRBS analysis identify treatment effects from different states (see Figure 1). Another could be measurement error in state-level trends in smoking or drinking in the national YRBS that are correlated with state policy changes (under the assumption that the state YRBS, which is designed to be representative of high school students' risky behaviors is measured with less error). In Appendix Table 3, we restrict the set of treatment states that identify treatment effects to those common to both datasets as well as the same set of never-adopting states. With respect to alcohol use (columns 1 and 2 of Appendix Table 3), the pattern of findings suggests that measurement error in drinking in the national YRBS may be correlated with VIL enactment. To the extent that we believe that the state YRBS provides more accurate estimates of state-level trends in teenage alcohol use, the estimated treatment effects from the national YRBS appear to be upwardly biased in a sample including the identical set of treatment and control states. With respect to cigarette use (columns 3 and 4 of Appendix Table 4), the findings are somewhat more ambiguous, with some of the difference in findings perhaps due to measurement error (column 3), but when the sample of treatment and control states are restricted to the identical waves, it is more suggestive of heterogeneous treatment effects.

In panel II of Table 3, we augment data from the state YRBS with the national YRBS to maximize policy variation used to identify treatment effects. Our results provide little-to-no evidence that VILs are an effective policy tool to reduce teen drinking and smoking. In models that include the full set of controls, we find that the enactment of a VIL is associated with a statistically insignificant 0.9 percentage-point decrease in teenage drinking and a statistically insignificant 0.9 percentage-point *increase* in teenage smoking.[29] The 95 percent confidence intervals around these

[28] We do note, however, that there is overlap in the 95 percent confidence intervals from estimates in panels I and II. Our estimated treatment effects obtained from the state YRBS do not exclude the entire 95 percent confidence interval around the estimated treatment effects obtained from the national YRBS.

[29] In panels II and III of Appendix Table 2, we show results from the state YRBS and augmented YRBS samples in specifications that include state-specific linear (columns 1 and 3) and state-specific linear and quadratic (columns 2 and 4) time trends. The pattern of findings is qualitatively similar to those reported in Table 3.

estimates are such that we can rule out the point estimates obtained in columns (3) and column (6) of Table 2.[30]

Event-study analyses based on the state YRBS and augmented YRBS (panels a(iii) and b(iii)) show no evidence that VILs have reduced teen alcohol or cigarette use. There is neither evidence of a violation of the parallel trends assumption, nor evidence of post-treatment decline in alcohol or cigarette use in the post-treatment period. If anything, post-treatment longer-lagged effects are more positive than negative in the state and augmented YRBS samples.

4.3 Event Studies Accounting for Heterogeneous and Dynamic Treatment Effects, 1991-2009 National YRBS

New developments in the difference-in-differences literature have offered important insights into and critiques of TWFE estimates of the effect of state policies. The average treatment effect identified by the TWFE estimator is an average of two-by-two comparisons of "ever adopters vs never adopters," "early adopters vs later adopters," and "later adopters vs early adopters" (Goodman-Bacon 2021). In the presence of heterogeneous and dynamic treatment effects, the latter comparisons — especially if they receive substantial weight — TWFE estimates may produce biased estimates of the treatment effect (Goodman-Bacon 2021), as well as biased estimates of event study coefficients (Sun and Abraham 2021). With relatively few "never-adopters" in the above analysis of VILs (only 8 states had not adopted a VIL by 2009), we estimate an event study using the approach pioneered by Callaway and Sant'Anna (2021). To implement this method, we use not-yet-adopters (in each time period) as the counterfactuals for each treatment state.

Figure 3 shows the results of event studies using Callaway-Sant'Anna's estimates. While panel a(i) continues to show evidence of post-VIL decline in alcohol use using national YRBS data (consistent with TWFE estimates in Figure 2), Callaway-Sant'Anna (2021) estimates of the lead effects now show some evidence of a decline in alcohol use during the window beginning 3 waves (~6 years) *prior to the enactment of a VIL* and continuing through the wave prior to enactment. This pattern of estimates in the pre-treatment period suggests a possible violation of the parallel trends assumption and call into question whether the post-treatment drinking decline was, in part, an artifact of alcohol declines that were already underway before VILs were implemented. Moreover,

[30] The 95 percent confidence intervals for estimated effects on drinking and smoking in the 1991-2009 augmented YRBS are (-0.034,0.016) and (-0.024, 0.042), respectively. The corresponding point estimates for each of these models in the national YRBS fall outside of these confidence intervals at -0.0408 and -0.0304 for drinking and smoking, respectively.

evidence from the state and augmented YRBS samples using the Callaway and Sant'Anna (2021) estimates continue to provide no evidence that VILs reduce alcohol use.

Turning to cigarette use (panel b of Figure 3), Callaway and Sant'Anna (2021) estimates across all three datasets show no evidence that state VILs reduce teenage tobacco consumption. In fact, in the state YRBS, the pos-treatment effect is positive. Together, these results stand in stark contrast to those reported by Bellou and Bhatt (2013) and cast some doubt on the hypothesis that state VILs were effective at curbing underage teenage drinking over the 1991-2009 period.

4.4 Extending the Analysis Window to 1991-2019

Next, in Table 4, we extend the analysis period through 2019, using YRBS surveys collected from 1991 through 2019. We document three conclusions. First, we note that using the national YRBS and appending five additional waves of national YRBS data (2011, 2013, 2015, 2017, and 2019) to the analysis sample — and changing nothing else about the dataset or regression specification — renders the effects of VILs on drinking and smoking to be small and statistically indistinguishable from zero. Using the same set of observable controls used by Bellou and Bhatt (2013), we find that the enactment of a VIL was associated with a statistically insignificant 0.18 percentage-point decline in teen drinking (column 3) and a 0.16 percentage-point *increase* in teenage cigarette use (column 7). Again, the precision of these estimated nulls is such that, with 95 percent confidence, we can exclude the estimated treatment effects reported in columns (3) and (6) of Table 2.

In columns (4) and (8), we add additional alcohol and tobacco policy controls that studies in the recent literature suggest may affect teenage drinking or smoking rates: the presence of e-cigarette tax, false ID laws with scanner provisions, keg registration policies, minimum legal purchasing age for e-cigarettes of 18, and a minimum legal purchasing age for all tobacco products of 21. The estimated treatment effects are qualitatively unchanged and suggest that VILs have no effect on teenage drinking or smoking in the national YRBS dataset. Our findings using the 1991-2019 national YRBS stand in contrast to those obtained by Bellou and Bhatt (2013) over the 1991-2009 period and suggest that their results are not only sensitive to use of the national vs state YRBS, but also the time period under study.

An examination of the state YRBS (panel II) and augmented YRBS (panel III) show a similar pattern of results, with no evidence that VILs induce statistically significant or economically important declines in teenage drinking or smoking. In our preferred specifications, which uses the

augmented YRBS sample and includes the full set of controls (columns 4 and 8), our null findings are sufficiently precisely estimated such that we can, with 95 percent confidence, rule out the estimated treatment effects reported in Table 2.[31] Event-study analyses based on TWFE (Figure 4) and Callaway-Sant'Anna estimates (Figure 5) show no evidence that VILs reduced alcohol or cigarette use among 16-year-old high school students in either the 1991-2019 national, state, or augmented YRBS samples.[32,33]

4.4 Difference-in-Difference-in-Differences Estimates

In addition to difference-in-difference models, Bellou and Bhatt (2013) present results from a difference-in-difference-in-differences (DDD) model using 17-18-year-olds who were not issued a vertical ID as a within-state control group (see their Table 4). To calculate these estimated VIL effects, they restrict their sample to include (i) 16-to-18-year-olds from untreated states, (ii) 16-year-olds in treated states, and (iii) 17-to-18-year-olds in treated states who were coded as untreated because they were "grandfathered in" by the statute (by limiting the post-treatment period such that no 17-to-18-year-olds were treated with a lag).[34]

Bellou and Bhatt (2013) estimate a partially interacted DDD model such that an indicator for whether the individual was age 16 was interacted with the indicator for a VIL as well as with fixed effects (but not the covariates, which effectively forces their effects on drinking or smoking to be the same for 16 and 17-to-18-year-olds). In the spirit of Bellou and Bhatt (2013), we estimate:

$$Y_{ist} = \sigma_0 + \sigma_1 VIL_{st} + \sigma_2 VIL_{st} * Age16_i + \sigma_3 X_{ist} + \theta_s + \theta_s * Age16_i + \tau_t + \tau_t * Age16_i + \varepsilon_{ist} \quad (1)$$

Table 5 presents the results of this exercise. Consistent with Bellou and Bhatt (2013), we find that VIL enactment reduces smoking (column 1, panel I) and drinking (column 1, panel II) among 16-year-olds in the 1991-2009 period using the national YRBS data. However, when we use

[31] The 95 percent confidence intervals around the point estimates drawn from the augmented YRBS for drinking and smoking are (-0.024, 0.016) and (-0.008, 0.039), respectively.
[32] In Appendix Table 4, we fully combine the state and national YRBS. This could be problematic if the same students participate in both the national and state YRBS surveys. However, the findings are qualitatively similar to those obtained when using the state YRBS survey augmented with national YRBS observations when the state YRBS was missing.
[33] The findings in Appendix Table 5 show that the estimated impact of VILs on alcohol and cigarette use over the 1991-2019 period remains smaller (in absolute magnitude) than comparable estimates over the 1991-2009 period (Appendix Table 2), including in models with additional controls for state-specific time trends.
[34] That is, they restrict the post-treatment period such that no 17-to-18-year-olds received a vertical ID when they were 16 years old.

data from the state YRBS (column 2) or augmented YRBS (column 3) sample over the same time period, the DDD estimate of the effect of VILs on 16-year-olds relative to untreated 17-to-18-year-olds is much smaller in magnitude, occasionally of the opposite sign, and nowhere near statistically distinguishable from zero at conventional levels. Moreover, when we extend the analysis period to 1991-2019 (columns 4 through 6), our DDD estimates across all three samples (national YRBS, state YRBS, and combined YRBS) are not statistically different from zero. [35]

4.5 Alternative Measures of Alcohol Use and Smoking

To ensure that the null results are not masking important effects at the intensive margins of consumption, in Table 6 we explore alternate measures of drinking and smoking. We find no evidence that the enactment of a state VIL has a statistically significant or economically important effect on binge drinking, frequent binge drinking, or everyday smoking. Estimates from the augmented YRBS sample are sufficiently precisely estimated such that, we can, with 95 percent confidence, reject the hypothesis of binge drinking declines of greater than 0.7 percentage points (2.4 percent) and everyday smoking declines of greater than 0.4 percentage points (4.1 percent).

In Appendix Table 7, we estimate the effects of VILs on binge drinking conditional on any drinking (column 1), frequent binge drinking conditional on any drinking (column 2), and everyday smoking conditional on any smoking in the last 30 days (column 3). In no case do we find any evidence that the enactment of VILs is negatively related to these outcomes and in some cases we find a *positive* correlation between VIL enactment and heavier drinking.

4.6 Heterogeneity in VIL Effects Based on Youth Demographics

In Table 7, we explore whether there is any heterogeneity in the effects of VILs based on demographic characteristics such as gender, race/ethnicity, or age. We focus on the full sample period using the augmented YRBS for this exercise. We find no evidence that VIL enactment reduces teenage alcohol consumption (columns 1 through 3) or cigarette use (columns 4 and 5) for males (Panel I), females (Panel II), whites (Panel III), and non-whites (Panel IV).[36] The strongest

[35] In Appendix Table 6A, we present DDD estimates using untreated 17-year-olds as the control group and excluding 18-year-olds, for whom it is legal to purchase cigarettes in many states over many years over the sample period. The findings are qualitatively similar to those reported in Table 5.

[36] In Appendix Table 6B, we explore the lagged effects of VILs on treated 17-to-18-year-olds, that is those who were 16-years at the time of VIL enactment and whose behavior was measured with a lag when they were ages 17-to-18. We find no evidence that VILs were effective at reducing their drinking or smoking behavior.

evidence for alcohol declines is observed for non-white 16-year-olds (column 1, Panel IV), though the estimated coefficient is only marginally statistically significant at conventional levels and is about half the size of the average treatment effect on the treated reported in Table 2.

In Table 8, we explore whether the effects of VILs on tobacco and cigarette use differs by the strength of the tobacco or alcohol control environment. To do this, we create an index for the strength of the tobacco control environment an index for the strength of the alcohol control environment and interact each with a VIL.[37] While we find that stricter tobacco control policies are negatively related to cigarette use, there is no evidence that VILs are effective in combatting teen drinking and smoking in either weaker or stronger tobacco or alcohol control environments.

4.6 Usual Sources of Alcohol and Cigarettes, Drinking-and-Driving

Next, we examine whether our null findings are explained by the policy lacking "bite" on direct purchase of alcohol or cigarettes, or whether they can (in part or in whole) be explained by offsetting effects whereby teenagers turn to informal social sources of alcohol or tobacco in response to VIL adoption. To examine the impact of VILs on teenagers' usual sources of alcohol/cigarettes, we estimate a multinomial logistic (MNL) model of the form

$$\Pr(y_i = j) = \frac{exp^{\beta_j z_i}}{1+\sum_{j=1}^{J} exp^{\beta_j z_i}} \quad (5)$$

where *j* indexes the choice of how to obtain alcohol (or cigarettes) and z_i is the set of right-hand side variables described in equation (1). The categories for obtaining alcohol/cigarettes are: *Own Purchase* (from a bar, restaurant, store, or event); *Third-Party Purchase* (i.e., a teenager giving an adult money to buy the alcohol/cigarettes for them); *Some other method* (i.e., took from store/family, given by someone else, internet/vending machine purchase)*; No use* (the reference category).[38]

[37] The alcohol control index is set equal to the sum of the number of alcohol control policies that were in effect in each state and year (zero tolerance drunk driving laws; social host laws for minors; keg regulations; false ID laws with scanner provisions; real (2019$) increases in the beer tax rate of at least 10 percent); the tobacco control index is created analogously from tobacco policies (smokefree restaurant, bar, and workplace laws; punishments for minors attempting to purchase tobacco products; ID requirements for tobacco purchase; cigarette vending machine restrictions; tobacco MLPA of 18; tobacco MLPA of 21; presence of an e-cigarette tax; e-cigarette MLPA of 18; real (2019$) increases in the cigarette tax rate of at least 10 percent). We then create a set of dichotomous indicators for the top 25th percentile of the index distribution, the 25th to 75th percentile, and the bottom 25th percentile.

[38] There are differences in the YRBS survey wordings for how teens obtain alcohol and cigarettes. For alcohol, *Own purchase* refers to a respondent buying alcohol at a store (such as a liquor, grocery, or convenience store), bar, restaurant, nightclub, or public event (such as concert or sporting event); for cigarettes, *Own purchase* refers specifically to buying cigarettes in a store. For alcohol, *Third-Party Purchase* refers specifically to a transaction in which a respondent gives money to someone else to buy alcohol for them; for cigarettes, *Third-Party Purchase* also includes borrowing or "bumming" cigarettes from another person. For alcohol, *Other* refers to taking alcohol from a store or family, being

First, we note that very small shares of teenagers directly purchased their alcohol or cigarettes. Overall, we find that just 5.2 percent of 16-year-olds purchased cigarettes directly on their own and 2.3 percent directly purchased alcohol. The vast majority of 16-year-olds consumers of these products obtained them via third party purchase or in other ways (93.3 and 76.2 percent for alcohol and cigarettes, respectively). Thus, the margin through which VILs likely affect direct purchase is small.

Table 9 reports marginal effects from MNL regressions. Our results provide little support for the hypothesis that VILs reduce direct purchase of alcohol (Panel I) or tobacco (Panel II). We also find no evidence that VIL enactment affects consumption among those who typically rely on social sources for alcohol or tobacco produces (i.e., third party purchase or obtaining alcohol or cigarettes in some other way). Only for 17-to-18-year-olds do we uncover some evidence that VIL enactment could reduce cigarette use, but this is only for those who typically get their cigarettes in other ways (i.e., internet purchase, vending machine purchase, theft, or some other method). These findings tend to suggest that VILs did not appear to "bite" for most teenagers and did little to achieve their policy objective.[39,40]

Finally, in Appendix Table 10, we explore the effect of VILs on drinking and driving behavior, as measured in the YRBS, as well as alcohol-related traffic fatalities, following the recent study by Nesson and Shrestha (2021).[41] Our findings suggest that the null results we uncover for drinking are not masking drinking effects most strongly associated with problem drinking and associated negative externalities.

5. Conclusions

given alcohol by someone else, or any other method of obtaining alcohol; for cigarettes, *Other* refers to taking cigarettes from a store or family, purchasing via the internet or a cigarette vending machine, or some other method of obtaining cigarettes.

[39] In Appendix Table 8, we separate estimates for 17 and 18-year-olds given that the latter were able to legally purchase tobacco products during much of the sample period, while the former were not. The results are qualitatively similar for both age groups.

[40] Bellou and Bhatt (2013) also examined the relationship between VILs and the probability that a 16-year-old smoker was asked to show his/her ID when purchasing cigarettes. These data are only available during the period 1991-2005 in both the national and state YRBS. While we can largely replicate their positive VIL effects in the national YRBS (column 1, Appendix Table 9), we fail to find positive effects in the state YRBS (column 2). However, we treat these estimates as largely descriptive because fewer than 10 states identified the treatment effect in the state YRBS.

[41] For this analysis, we use FARS traffic fatality data available here: https://www.nhtsa.gov/file-downloads?p=nhtsa/downloads/FARS/ .

With the goal of lowering the cost of identifying underage youth trying to purchase alcohol and tobacco, all 50 states and the District of Columbia have adopted so-called vertical identification laws (VILs), which require state identification cards (drivers' licenses) issued to those under age 21 to be vertical in nature. In Volume 32, Issue 5 of this journal, Bellou and Bhatt (2013) used data from the 1991-2009 national Youth Risk Behavior Survey (YRBS) and found that VILs adoption reduced alcohol consumption and cigarette use among 16-year-old high school students. This study uses new data, novel difference-in-differences approaches, and additional policy variation that were not available to previous scholars to re-examine this question.

In contrast to Bellou and Bhatt (2013), our findings show little evidence that VILs are effective at curbing teen drinking or smoking. Specifically, we find that the negative drinking and smoking effects of VILs disappear (1) when the national YRBS data is extended through 2019, (2) when one uses the state YRBS or augmented state and national YRBS data from 1991-2009 or 1991-2009, and (3) use an alternate difference-in-differences estimator that accounts for heterogeneous and dynamic treatment effects. These results are consistent with (1) empirical evidence that few 16-year-olds directly purchased alcohol or cigarettes from a vendor, but instead relied on social sources for these products, and (2) the hypothesis that the supply of false IDs is relatively inelastic.

The cost of implementing VILs was not trivial. For instance, a 2002 analysis by the Michigan state legislature estimated that the implementation costs of Michigan's 2003 VIL would cost the state a one-time expenditure of about $1 million for the "program conversion costs for the contractor producing Michigan's driver's license and ID cards." (Michigan State Senate Analysis for S.B. 924 and S.B. 925). Given that policymakers sought the adoption of VILs to reduce the supply of false IDs and to reduce public health costs of underage purchases of cigarettes and alcohol, our updated estimates suggest that this policy strategy was largely ineffective.

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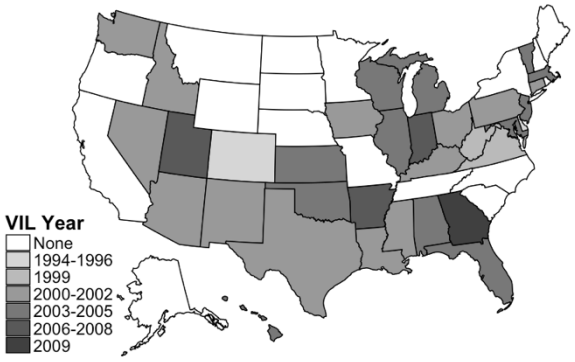
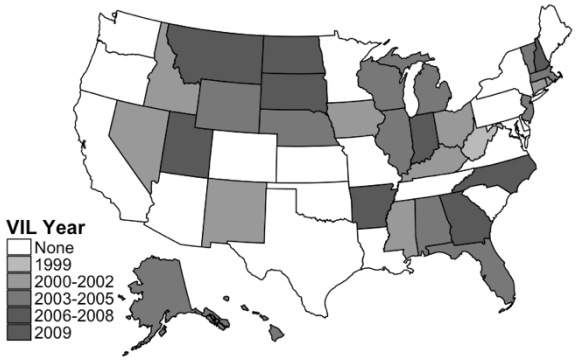
Figure 1. States Contributing to Identification of Effects of Vertical Identification Laws (VILs), by YRBS Survey and Years

Panel (a): 1991-2009

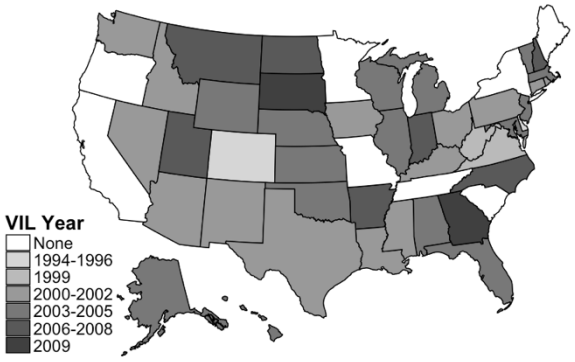
(i) National YRBS



(ii) State YRBS

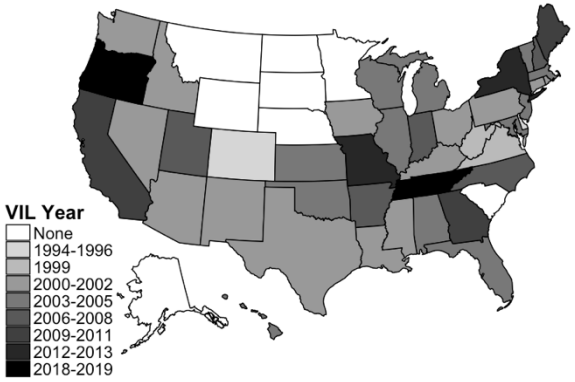


(iii) State & National Augmented YRBS

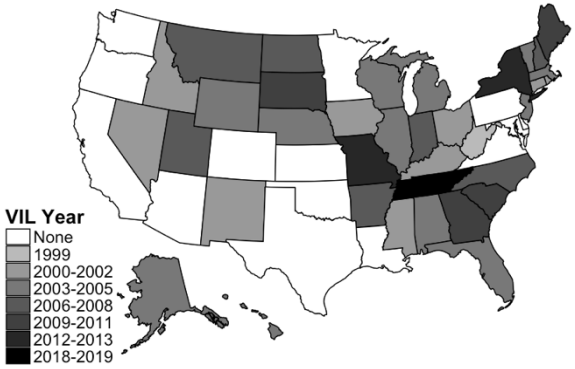


Panel (b): 1991-2019

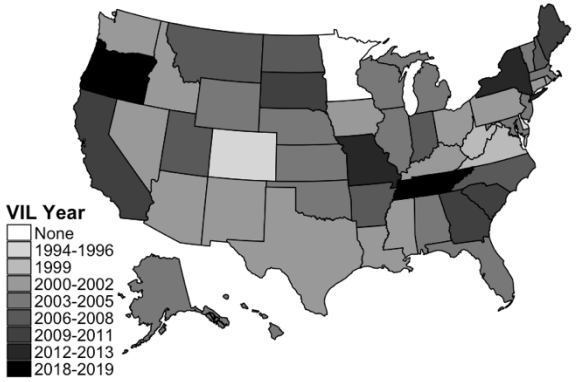
(i) National YRBS



(ii) State YRBS



(iii) State & National Augmented YRBS

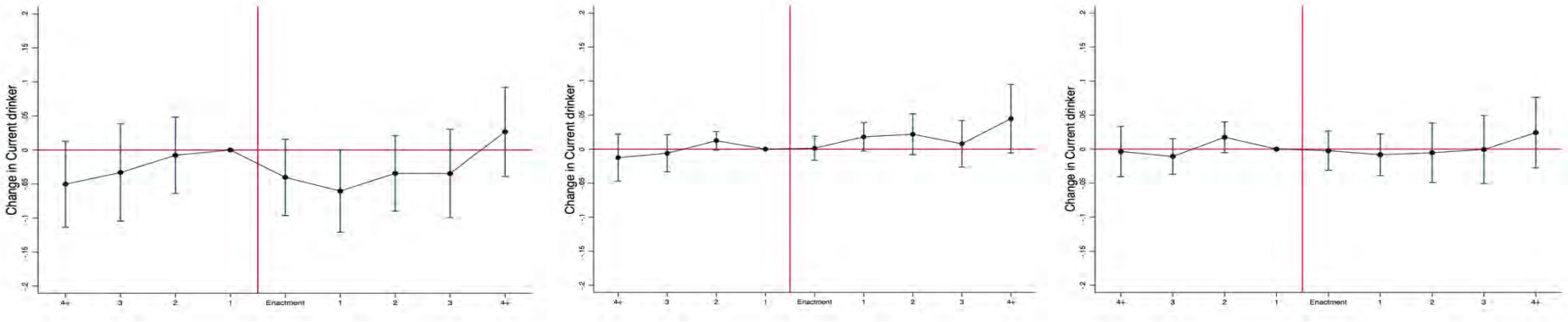


Sources: Appendix Table 1 shows sources for effective dates of policies, which include Bellou and Bhatt (2013) and our own searches of state statutes.

Figure 2. Event-Study Analyses of VILs, Using TWFE Estimates, 1991-2009

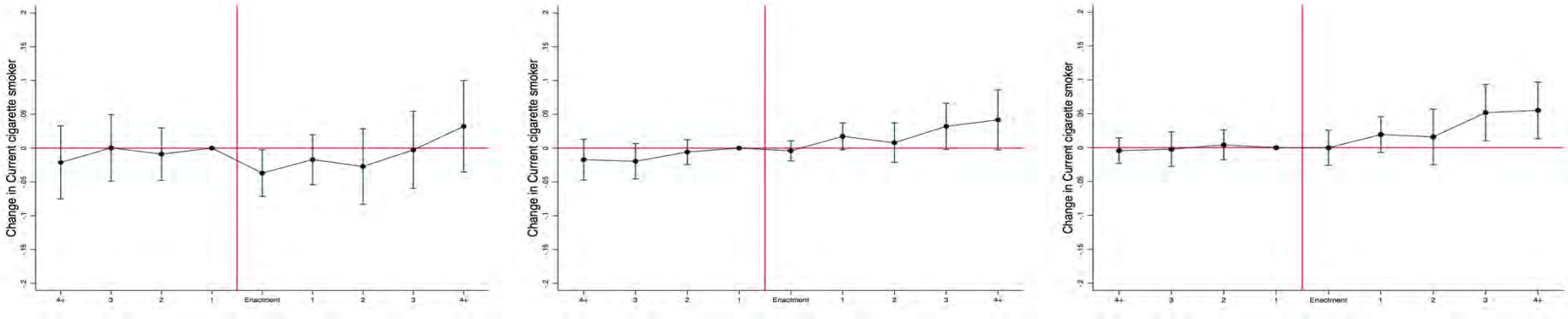
Panel (a): Alcohol Use

(i) National YRBS (ii) State YRBS (iii) State & National Augmented YRBS



Panel (b): Cigarette Use

(i) National YRBS (ii) State YRBS (iii) State & National Augmented YRBS

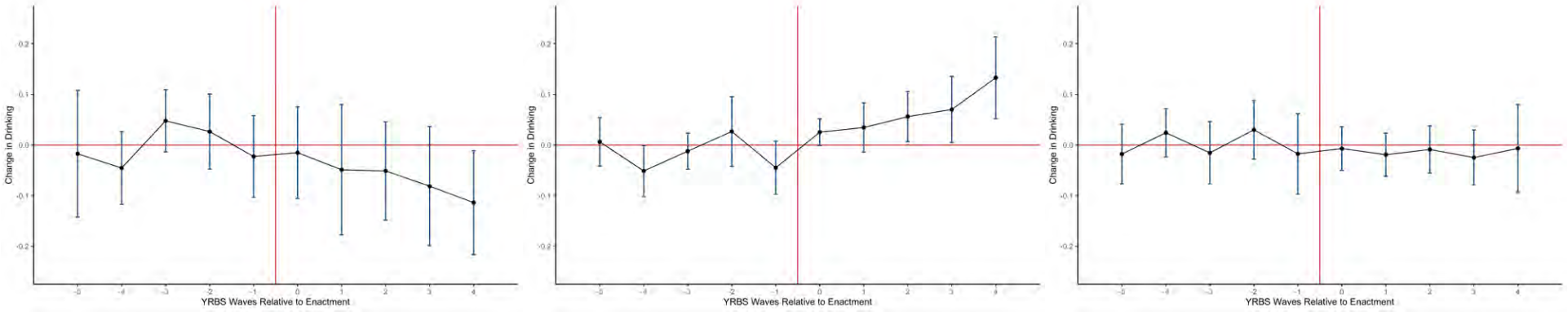


Notes: Estimates from weighted ordinary least squares (OLS) regressions and their 95% confidence intervals are shown. All models include state and year fixed effects, and the full set of individual and state controls listed in the notes to Table 2 and 3.

Figure 3. Event-Study Analysis of VILs, Using Callaway and Sant'Anna Estimates, 1991-2009

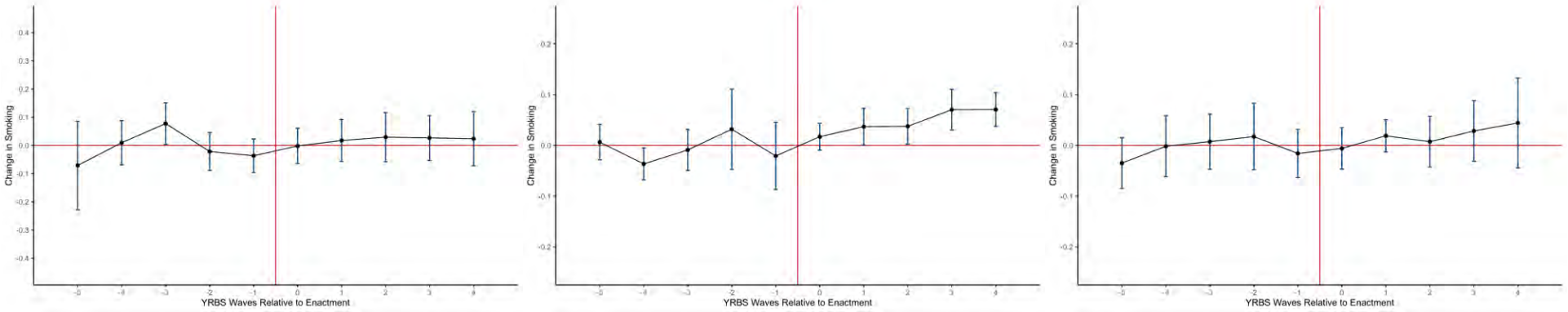
Panel (a): Alcohol Use

(i) National YRBS (ii) State YRBS (iii) State & National Augmented YRBS



Panel (b): Cigarette Use

(i) National YRBS (ii) State YRBS (iii) State & National Augmented YRBS

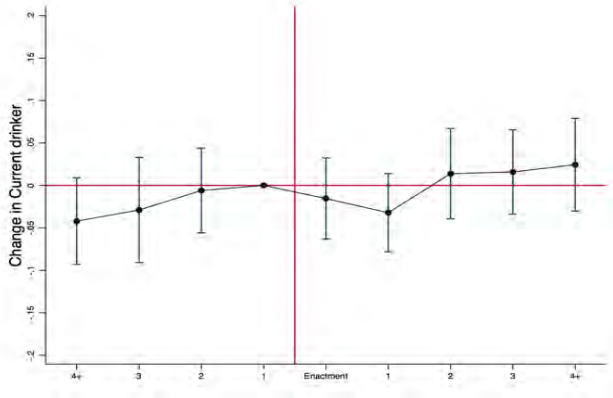


Notes: Estimates from weighted Callaway-Sant'Anna (2021) regressions and their 95% confidence intervals are shown.

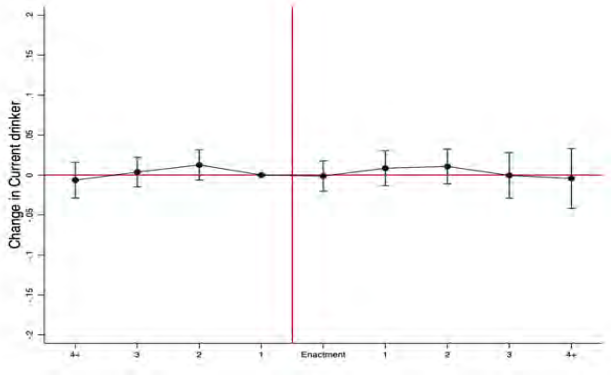
Figure 4. Event-Study Analysis of VILs, Using TWFE Estimates, 1991-2019

Panel (a): Alcohol Use

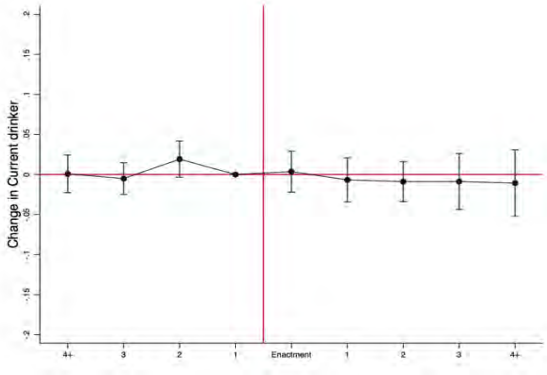
(i) National YRBS



(ii) State YRBS

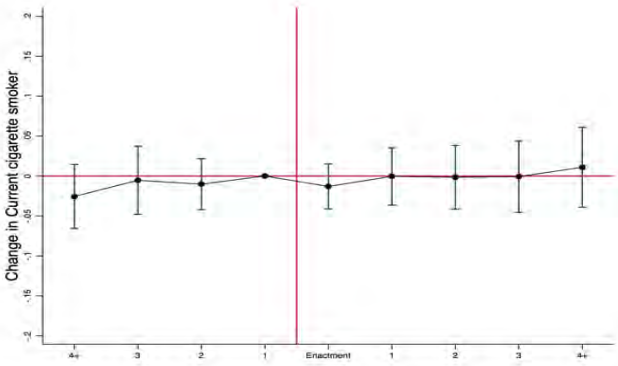


(iii) State & National Augmented YRBS

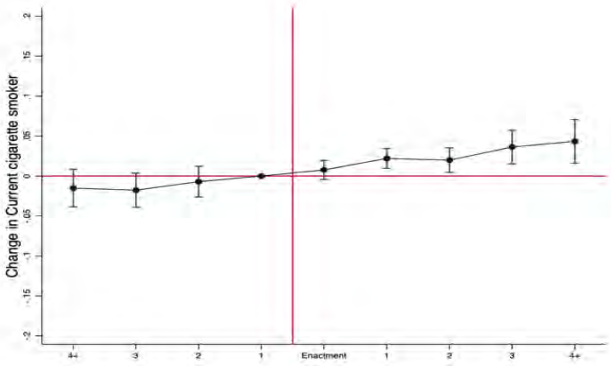


Panel (b): Cigarette Use

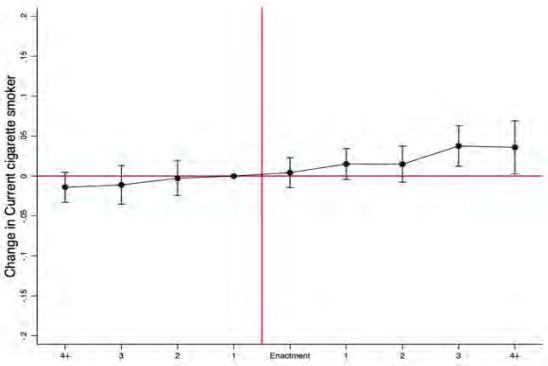
(i) National YRBS



(ii) State YRBS



(iii) State & National Augmented YRBS

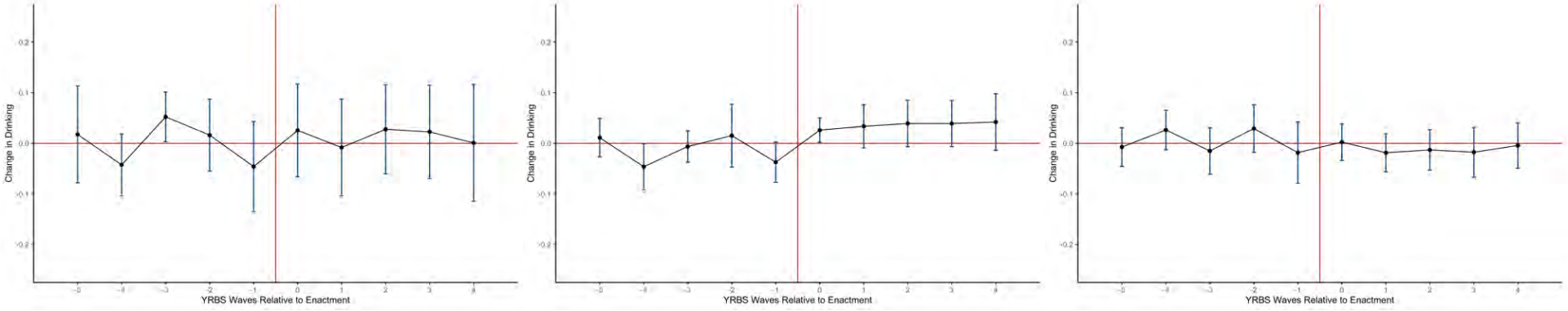


Notes: Estimates from weighted ordinary least squares (OLS) regressions and their 95% confidence intervals are shown. All models include state and year fixed effects, and the full set of individual and state controls listed in the notes to Table 4.

Figure 5. Event-Study Analysis of VILs, Using Callaway and Sant'Anna (2021) Estimates, 1991-2019

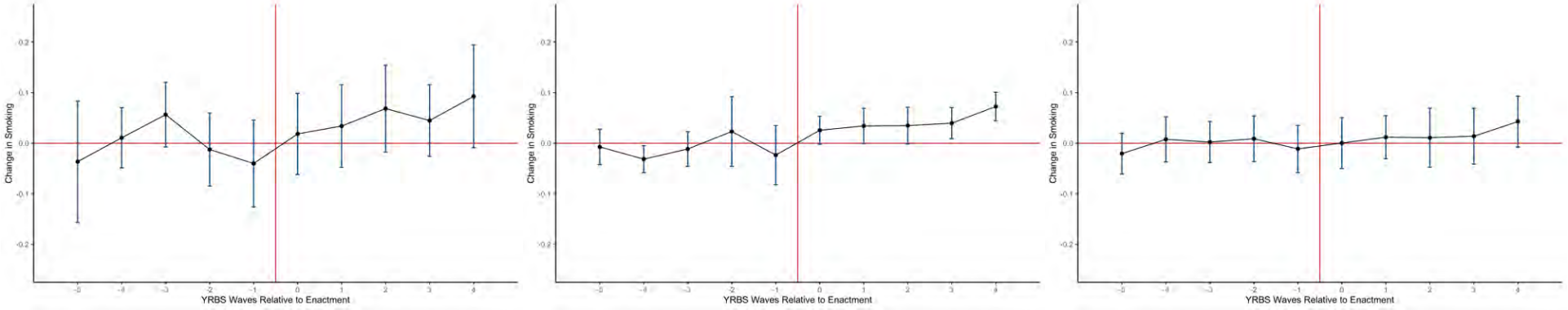
Panel (a): Alcohol Use

(i) National YRBS (ii) State YRBS (iii) State & National Augmented YRBS



Panel (b): Cigarette Use

(i) National YRBS (ii) State YRBS (iii) State & National Augmented YRBS



Notes: Estimates from weighted Callaway-Sant'Anna (2021) regressions and their 95% confidence intervals are shown.

Table 1. Descriptive Statistics

	1991-2009			1991-2019		
	National YRBS	State YRBS	State & National Augmented YRBS	National YRBS	State YRBS	State & National Augmented YRBS
Dependent Variables						
Any Drinking	0.474	0.436	0.454	0.430	0.376	0.403
Binge Drinking	0.294	0.267	0.278	0.255	0.217	0.236
Binge Drinking | Drinking	0.632	0.614	0.617	0.616	0.593	0.602
Frequent Binge Drinking	0.130	0.115	0.122	0.109	0.089	0.099
Frequent Binge Drinking | Drinking	0.280	0.265	0. 270	0.265	0.243	0.253
Smoking Participation	0.281	0.237	0.258	0.228	0.175	0.201
Frequent Smoking	0.121	0.108	0.115	0.093	0.073	0.084
Everyday Smoking	0.089	0.080	0.087	0.069	0.054	0.063
Everyday Smoking | Smoking	0.319	0.340	0.332	0.301	0.310	0.311
Individual-Level Controls						
9th Grade	0.078	0.142	0.108	0.069	0.106	0.090
10th Grade	0.496	0.536	0.517	0.501	0.522	0.514
11th Grade	0.415	0.316	0.367	0.418	0.363	0.386
12th Grade	0.011	0.006	0.008	0.012	0.009	0.009
Ungraded	0.0002	0.0002	0.0003	0.0001	0.0001	0.0002
White, Non-Hispanic	0.674	0.664	0.634	0.615	0.602	0.596
Black, Non-Hispanic	0.149	0.169	0.153	0.154	0.166	0.153
All Other Races	0.177	0.167	0.213	0.231	0.232	0.251
Male	0.523	0.500	0.508	0.517	0.502	0.508
State-Level Controls						
Graduated Drivers' License Law	0.824	0.920	0.868	0.882	0.957	0.918
Smoke-free Workplace Law	0.092	0.161	0.101	0.247	0.349	0.267
Smoke-free Restaurant Law	0.181	0.199	0.204	0.339	0.414	0.376
Smoke-free Bar Law	0.138	0.113	0.156	0.279	0.315	0.307
Zero Tolerance Law	0.169	0.887	0.799	0.845	0.940	0.874

	1991-2009			1991-2019		
	National YRBS	State YRBS	State & National Augmented YRBS	National YRBS	State YRBS	State & National Augmented YRBS
Social Host Law	0.614	0.786	0.645	0.643	0.760	0.668
Punishments for Minors	0.516	0.562	0.539	0.634	0.697	0.661
ID Requirement for Tobacco	0.382	0.382	0.398	0.425	0.470	0.469
Restrictions on Tobacco Vending	0.556	0.682	0.611	0.692	0.817	0.746
MLPA for Tobacco of 18 or 19	0.993	0.994	0.995	0.995	0.997	0.997
Cigarette Tax (2019$ per pack)	0.44	0.57	0.49	0.80	1.04	0.89
	(0.44)	(0.53)	(0.47)	(0.84)	(0.97)	(0.89)
Beer Tax (2019$ per gallon)	0.18	0.21	0.19	0.20	0.248	0.22
	(0.17)	(0.19)	(0.17)	(0.19)	(0.254)	(0.23)
Unemployment Rate	5.847	5.793	5.890	5.955	5.796	5.94
	(1.837)	(1.969)	(1.857)	(2.000)	(2.017)	(2.01)
Median Personal Income (2019$)	28,389.52	31,942.42	30,034.81	36,752.22	41,944.21	38,874.52
	(9,123.87)	(8,826.01)	(9,418.01)	(15,431.14)	(14,506.16)	(15,012.07)
Tobacco 21 Law	0	0	0	0.398	0.066	0.049
Any E-cigarette Tax	0	0	0	0.056	0.078	0.058
Keg Registration Policy	0.321	0.292	0.327	0.387	0.404	0.409
ID Scanner Policy	0.126	0.237	0.166	0.170	0.280	0.225
E-Cigarette MLPA of 18	0	0	0	0.234	0.314	0.264
N	35,488	166,556	186,154	53,454	385,353	408,580

Table 2. Two-Way Fixed Effects (TWFE) Estimates of the Effect of VILs on Alcohol and Cigarette Use among 16-Year-Olds, 1991-2009 National YRBS

	Alcohol Use			Cigarette Use		
	(1)	(2)	(3)	(4)	(5)	(6)
VIL	-0.0316**	-0.0349**	-0.0409**	-0.0202	-0.0254*	-0.0306**
	(0.0152)	(0.0151)	(0.0184)	(0.0151)	(0.0139)	(0.0136)
	[33,326]	[33,326]	[33,326]	[33,881]	[33,881]	[33,881]
Pre-Treatment Mean DV	0.500	0.500	0.500	0.322	0.322	0.322
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	No	Yes	Yes
State Controls	No	No	Yes	No	No	Yes

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the state population of 16-year-olds, and standard errors are clustered at the state level. Sample sizes are in brackets. Individual-level controls include race, grade, sex, an indicator for whether an individual is not in a particular grade in school, and an indicator for whether the state-wave combination for a given observation exists in both contributing YRBS samples. Baseline state-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income (2019$).

Table 3. Sensitivity of Estimates to Use of State and Augmented YRBS Samples, 1991-2009

	Alcohol Use			Cigarette Use		
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel I: State YRBS			
VIL	-0.0092	-0.0102	-0.0093	-0.0004	-0.0011	-0.0044
	(0.0076)	(0.0074)	(0.0090)	(0.0083)	(0.0086)	(0.0081)
	[165,059]	[165,059]	[165,059]	[161,666]	[161,666]	[161,666]
Pre-Treatment Mean DV	0.452	0.452	0.452	0.284	0.284	0.284
			Panel II: State and National Augmented YRBS			
VIL	-0.0132	-0.0131	-0.0090	0.0099	0.0094	0.0094
	(0.0105)	(0.0111)	(0.0123)	(0.0152)	(0.0157)	(0.0164)
	[182,567]	[182,567]	[182,567]	[179,299]	[179,299]	[179,299]
Pre-Treatment Mean DV	0.476	0.476	0.476	0.308	0.308	0.308
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	No	Yes	Yes
State Controls	No	No	Yes	No	No	Yes

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the state population of 16-year-olds and standard errors are clustered at the state level. Sample sizes are in brackets. Individual-level controls include race, grade, sex, an indicator for whether an individual is not in a particular grade in school, and an indicator for whether the state-wave combination for a given observation exists in both contributing YRBS samples. Baseline state-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income (2019$). Regressions in the Augmented YRBS include a binary control indicating whether an observation comes from the State or National YRBS. The augmented YRBS sample in Panel II is created by augmenting the state YRBS sample with national YRBS observations if state YRBS observations are not available in a state-year cell.

Table 4. TWFE Estimates of Effects of VILs on Alcohol and Cigarette Use, Extending Analysis Window to 1991-2019

	Alcohol Use				Cigarette Use			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel I: National YRBS				
VIL	-0.0005	-0.0029	-0.0018	-0.0053	0.0004	-0.0032	0.0016	0.0014
	(0.0151)	(0.0148)	(0.0150)	(0.0141)	(0.0149)	(0.0142)	(0.0126)	(0.0113)
	[49,627]	[49,627]	[49,627]	[49,627]	[51,003]	[51,003]	[51,003]	[51,003]
Pre-Treatment Mean DV	0.478	0.478	0.478	0.478	0.294	0.294	0.294	0.294
				Panel II: State YRBS				
VIL	0.0011	-0.0009	0.0006	0.0026	0.0112*	0.0098	0.0124*	0.0127*
	(0.0055)	(0.0055)	(0.0063)	(0.0065)	(0.0062)	(0.0066)	(0.0069)	(0.0066)
	[367,084]	[367,084]	[367,084]	[367,084]	[373,617]	[373,617]	[373,617]	[373,617]
Pre-Treatment Mean DV	0.443	0.443	0.443	0.443	0.262	0.262	0.262	0.262
				Panel III: State and National Augmented YRBS				
VIL	-0.0033	-0.0039	-0.0037	-0.0038	0.0154	0.0139	0.0147	0.0151
	(0.0087)	(0.0088)	(0.0097)	(0.0098)	(0.0110)	(0.0113)	(0.0112)	(0.0116)
	[387,459]	[387,459]	[387,459]	[387,459]	[394,322]	[394,322]	[394,322]	[394,322]
Pre-Treatment Mean DV	0.462	0.462	0.462	0.462	0.275	0.275	0.275	0.275
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Baseline State Controls	No	No	Yes	Yes	No	No	Yes	Yes
Expanded State Controls	No	No	No	Yes	No	No	No	Yes

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the state population of 16-year-olds and standard errors are clustered at the state level. Sample sizes are included in brackets. Individual-level controls include race, grade, sex, an indicator for whether an individual is not in a particular grade in school, and an indicator for whether the state-wave combination for a given observation exists in both contributing YRBS samples. Baseline state-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income (2019$). Expanded state controls include presence of e-cigarette tax, ID scanner policies, keg registration policies, minimum legal purchasing age for e-cigarettes of 18, and minimum tobacco purchasing age of 21. Regressions in the Augmented YRBS include a binary control indicating whether an observation comes from the State or National YRBS. The augmented YRBS sample is created by augmenting the state YRBS sample with national YRBS observations if state YRBS observations are not available in a state-year cell.

Table 5. Difference-in-Difference-in-Difference Estimates of the Effects of VILs on Alcohol and Cigarette Use for 16-Year-Olds Relative to Untreated 17-to-18-Year-Olds

	1991-2009			1991-2019		
	National YRBS	State YRBS	State and National Augmented YRBS	National YRBS	State YRBS	State and National Augmented YRBS
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel I: Alcohol Use			
VIL * Age 16	-0.0414**	0.0016	-0.0119	-0.0144	0.053	-0.0100
	(0.0184)	(0.0092)	(0.0082)	(0.0176)	(0.0075)	(0.0073)
VIL	0.0122	-0.0059	0.0079	0.0157	-0.0052	0.0086
	(0.0159)	(0.0082)	(0.0093)	(0.0112)	(0.0089)	(0.0095)
Observations	[88,992]	[384,199]	[430,895]	[129,904]	[832,694]	[886,990]
Pre-Treatment Mean DV for Age 16	0.500	0.452	0.476	0.478	0.443	0.462
			Panel II: Cigarette Use			
VIL * Age 16	-0.0379*	0.0067	-0.0052	-0.0239	0.0083*	-0.0078
	(0.0203)	(0.0066)	(0.0124)	(0.0187)	(0.0049)	(0.0098)
VIL	0.0233	-0.0080	0.0211*	0.0290***	0.0039	0.0239**
	(0.0167)	(0.0082)	(0.0120)	(0.0121)	(0.0067)	(0.0097)
Observations	[90,392]	[376,531]	[423,638]	[133,517]	[846,482]	[901,694]
Pre-Treatment Mean DV for Age 16	0.322	0.284	0.308	0.294	0.262	0.275

***Significant at 1% **at 5% *at 10%

Notes: All regressions use a sample of 16-to-18-year-olds and are weighted using the relevant state population and standard errors are clustered at the state level. Sample sizes are included in brackets. Individual controls include race, grade, and sex. State-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; median income; presence of e-cigarette tax; ID scanner policies; keg registration policies; minimum legal purchasing age for e-cigarettes of 18; and minimum tobacco purchasing age of 21. All regressions include age-specific state fixed effects, age-specific year fixed effects, and dummies for whether the respondent is age 16. They key policy variable of interest is the interaction of the treated age (16) and the VIL indicator. For the period of 1991-2009, the controls for presence of e-cigarette tax, minimum tobacco purchasing age of 21, and minimum legal purchasing age for e-cigarettes of 18 are omitted, since they did not come into effect until after 2009. Regressions in the Augmented YRBS include a binary control indicating whether an observation comes from the State or National YRBS. The augmented YRBS sample is created by augmenting the state YRBS sample with national YRBS observations if state YRBS observations are not available in a state-year cell.

Table 6. Effects of VILs on Alternate Measures of Drinking and Smoking, 1991-2019

	Binge Drinking	Frequent Binge Drinking	Everyday Smoking
	(1)	(2)	(3)
	Panel I: National YRBS		
VIL	0.0070	0.0015	0.0075
	(0.0131)	(0.0115)	(0.0077)
	[51,638]	[51,638]	[51,003]
Pre-Treatment Mean DV	0.296	0.131	0.095
	Panel II: State YRBS		
VIL	0.0118*	0.0039	0.0036
	(0.0063)	(0.0049)	(0.0037)
	[353,613]	[353,613]	[373,617]
Pre-Treatment Mean DV	0.269	0.119	0.095
	Panel III: State and National Augmented YRBS		
VIL	0.0107	0.0100*	0.0065
	(0.0087)	(0.0055)	(0.0052)
	[374,776]	[374,766]	[394,322]
Pre-Treatment Mean DV	0.282	0.125	0.094

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the state population of 16-year-olds and standard errors are clustered at the state level. Sample sizes are in brackets. All regressions include state fixed effects and year fixed effects. Individual-level controls include race, grade, sex, an indicator for whether an individual is not in a particular grade in school, and an indicator for whether the state-wave combination for a given observation exists in both contributing YRBS samples. State-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income; presence of e-cigarette tax; ID scanner policies; keg registration policies; minimum tobacco purchasing age of 21; minimum legal purchasing age for e-cigarettes of 18. Regressions in the Augmented YRBS include a binary control indicating whether an observation comes from the State or National YRBS. The augmented YRBS sample is created by augmenting the state YRBS sample with national YRBS observations if state YRBS observations are not available in a state-year cell

Table 7. Exploring Heterogeneity in Effects of VILs, by Gender, Race, and Age

	Alcohol Use	Binge Drinking	Frequent Binge Drinking	Smoking Participation	Everyday Smoking
	(1)	(2)	(3)	(4)	(5)
			Panel I: Males, Age 16		
VIL	0.0030	0.0174	0.0132	0.0165	0.0046
	(0.0125)	(0.0120)	(0.0087)	(0.0139)	(0.0065)
	[187,335]	[181,321]	[181,321]	[190,894]	[190,894]
Pre-Treatment Mean DV	0.458	0.308	0.149	0.263	0.095
			Panel II: Females, Age 16		
VIL	-0.0112	0.0035	0.0067	0.0141	0.0085
	(0.0104)	(0.0087)	(0.0056)	(0.0119)	(0.0061)
	[200,124]	[193,445]	[193,445]	[203,428]	[203,428]
Pre-Treatment Mean DV	0.436	0.250	0.099	0.260	0.087
			Panel III: Non-Hispanic Whites, Age 16		
VIL	0.0047	0.0107	0.0098	0.0300**	0.0195**
	(0.0143)	(0.0113)	(0.0071)	(0.0127)	(0.0077)
	[233,297]	[223,522]	[223,522]	[235,233]	[235,233]
Pre-Treatment Mean DV	0.474	0.317	0.146	0.298	0.113
			Panel IV: Non-Whites, Age 16		
VIL	-0.0175	0.0102	0.0094	-0.0130	-0.0171
	(0.0131)	(0.0115)	(0.0069)	(0.0177)	(0.0124)
	[154,162]	[151,244]	[151,244]	[159,089]	[159,089]
Pre-Treatment Mean DV	0.397	0.207	0.083	0.197	0.052
			Panel V: Ages 17-to-18 (Treated with a Lag)		
VIL	0.0010	0.0018	0.0022	0.0225**	0.0116
	(0.0091)	(0.0107)	(0.0062)	(0.0105)	(0.0071)
	[499,531]	[481,551]	[481,551]	[507,372]	[507,372]
Pre-Treatment Mean DV	0.505	0.336	0.169	0.295	0.118

***Significant at 1% **at 5% *at 10%

Notes: All regressions use the augmented YRBS sample and are weighted using the relevant state population, and standard errors are clustered at the state level. Sample sizes are in brackets. Individual controls include race, grade, and sex. State-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; median income; presence of e-cigarette tax; ID scanner policies; keg registration policies; minimum legal purchasing age for e-cigarettes of 18; and minimum tobacco purchasing age of 21. All regressions include state and year fixed effects. Panel IV, "Non-Whites" includes respondents who identified as Black, Hispanic, or some other race.

Table 8. Exploring Heterogeneity in the Effects of VILs on Alcohol and Cigarette Use by the Strength of the Alcohol and Tobacco Control Policy Environment

	Age 16		Ages 17-to-18	
	Alcohol Use	**Cigarette Use**	**Alcohol Use**	**Cigarette Use**
	(1)	(2)	(3)	(4)
VIL	0.0008	0.0166	-0.0102	0.0393
	(0.0156)	(0.0176)	(0.0241)	(0.0443)
VIL * 25th to 75th Percentile Tobacco Policy Index	0.0152	0.0143	0.0323	-0.0098
	(0.0161)	(0.0170)	(0.0244)	(0.0452)
VIL * Top Quartile of Tobacco Policy Index	0.0085	0.0454*	-0.0041	-0.0213
	(0.0239)	(0.0268)	(0.0329)	(0.0496)
VIL * 25th to 75th Percentile of Alcohol Policy Index	-0.0195	-0.0218	-0.0159	-0.0083
	(0.0127)	(0.0176)	(0.0115)	(0.0147)
VIL * Top Quartile Alcohol Policy Index	-0.0338	-0.0473	-0.0180	-0.0009
	(0.0237)	(0.0349)	(0.0190)	(0.0228)
25th to 75th Percentile of Tobacco Policy Index	-0.0065	-0.0256*	-0.0354**	-0.0370**
	(0.0128)	(0.0134)	(0.0133)	(0.0127)
Top Quartile of Tobacco Policy Index	-0.0138	-0.0661**	0.0107	-0.0441**
	(0.0159)	(0.0193)	(0.0230)	(0.0211)
25th to 75th Percentile of Alcohol Policy Index	0.0117	0.0020	0.0039	0.0141
	(0.0130)	(0.0113)	(0.0074)	(0.0136)
Top Quartile of Alcohol Policy Index	0.0471**	0.0101	0.0028	0.0109
	(0.0136)	(0.0223)	(0.0187)	(0.0233)
	[387,459]	[394,322]	[499,531]	[507,372]
Pre-Treatment Mean DV	0.462	0.275	0.505	0.295

***Significant at 1% **at 5% *at 10%

Notes: All regressions are based on the augmented YRBS sample and include state and year fixed effects, are weighted using the relevant state population, and use standard errors clustered at the state level. Sample sizes are in brackets. All regressions include the vector of individual demographic controls of age (omitted in columns 1-2), race, grade, and sex. Alcohol and tobacco policy indices are equal to the total number of relevant policies in place in a state in a given year. To account for increases in relevant taxes for alcohol and cigarettes, a real beer or cigarette tax (2019$) increase of at least 10 percent is coded as an enactment of an alcohol (or tobacco) control policy. Policies that make up the alcohol policy index are: zero tolerance drunk driving laws; social host laws for minors; keg regulations; false ID laws with scanner provisions; real (2019$) increases in the beer tax rate. Policies that make up the tobacco policy index are: smokefree restaurant, bar, and workplace laws; punishments for minors attempting to purchase tobacco products; ID requirements for tobacco purchase; cigarette vending machine restrictions; tobacco MLPA of 18; tobacco MLPA of 21; presence of an e-cigarette tax; e-cigarette MLPA of 18; real (2019$) increases in the cigarette tax rate.

Table 9. Multinomial Logit Estimates (Marginal Effects) of the Effects of VILs on Usual Sources of Alcohol and Cigarettes

	Age 16	Ages 17-to-18
	(1)	(2)
	Panel I: Usual Sources of Alcohol (2007-2019)	
Own purchase	0.0029	-0.0049
	(0.0038)	(0.0038)
Pre-Treatment Mean	0.025	0.055
Third-party	-0.0043	0.0079
	(0.0056)	(0.0069)
Pre-Treatment Mean	0.093	0.125
Other	0.0173	0.0095
	(0.0097)	(0.0095)
Pre-Treatment Mean	0.269	0.286
Observations	[179,792]	[228,412]
	Panel II: Usual Sources of Cigarettes (1995-2015)	
Own purchase	0.0016	0.0177
	(0.0051)	(0.0104)
Pre-Treatment Mean	0.078	0.175
Third-party	0.0032	0.0035
	(0.0088)	(0.0049)
Pre-Treatment Mean	0.153	0.108
Other	0.0002	-0.0044**
	(0.0038)	(0.0030)
Pre-Treatment Mean	0.044	0.028
Observations	[210,792]	[278,311]

***Significant at 1% **at 5% *at 10%

Notes: All regressions are based on the augmented YRBS sample and include state and year fixed effects, are weighted using the state population of 16-year-olds, and use standard errors clustered at the state level. Sample sizes are in brackets. All regressions include the full vector of controls included in the notes to Table 4. The reference outcome category for multinomial logit regressions is "Didn't drink" for Panel I and "Didn't smoke" for Panel II. Definitions of outcomes differ slightly across Panels I and II due to YRBS survey composition and changes thereof over time. "Bought in person" in Panel I refers to a respondent buying alcohol at a store (such as a liquor, grocery, or convenience store), bar, restaurant, nightclub, or public event (such as concert or sporting event), but in Panel II refers specifically to buying cigarettes in a store. "Third-party" in Panel I refers specifically to a third-party purchase in which a respondent gives money to someone else to buy alcohol for them, but in Panel II refers to that type of third-party purchase for cigarettes in addition to borrowing or "bumming" cigarettes from another person. "Other" in Panel I refers to taking alcohol from a store or family, being given alcohol by someone else, or any other method of obtaining alcohol, but in Panel II refers to taking cigarettes from a store or family, purchasing via the internet or a cigarette vending machine, or some other method of obtaining cigarettes. Sample waves for the regressions above are determined by YRBS waves in which the questions appear.

Appendix Table 1. Vertical Identification Law (VIL) Effective Years

State	Effective Year	Source
Alabama	2005	Bellou and Bhatt (2013)
Alaska	2004	Bellou and Bhatt (2013)
Arizona	2001	Bellou and Bhatt (2013)
Arkansas	2006	Bellou and Bhatt (2013)
California	2010	Bellou and Bhatt (2013)
Colorado	1994	Bellou and Bhatt (2013)
Connecticut	2002	Bellou and Bhatt (2013)
District of	2004	Bellou and Bhatt (2013)
Delaware	1996	Bellou and Bhatt (2013)
Florida	2004	Bellou and Bhatt (2013)
Georgia	2009	Bellou and Bhatt (2013)
Hawaii	2005	Bellou and Bhatt (2013)
Idaho	2002	Bellou and Bhatt (2013)
Illinois	2005	Bellou and Bhatt (2013)
Indiana	2007	Bellou and Bhatt (2013)
Iowa	2001	Bellou and Bhatt (2013)
Kansas	2004	Bellou and Bhatt (2013)
Kentucky	2001	Bellou and Bhatt (2013)
Louisiana	2001	Bellou and Bhatt (2013)
Maine	2011	Bellou and Bhatt (2013)
Maryland	2003	Bellou and Bhatt (2013)
Massachusetts	2004	Bellou and Bhatt (2013)
Michigan	2003	Bellou and Bhatt (2013)
Minnesota	2018	Minnesota Department of Public Safety and Driver Services (https://dps.mn.gov/divisions/dvs/Pages/new-driver-licenses-id-cards.aspx)
Mississippi	2001	Bellou and Bhatt (2013)
Missouri	2012	Missouri Department of Revenue (https://atc.dps.mo.gov/enforcement/mo-drivers-
Montana	2008	Bellou and Bhatt (2013)
Nebraska	2003	Bellou and Bhatt (2013)
Nevada	2002	Bellou and Bhatt (2013)
New Hampshire	2008	Bellou and Bhatt (2013)
New Jersey	2004	Bellou and Bhatt (2013)
New Mexico	2000	Bellou and Bhatt (2013)
New York	2013	New York Governor's Press Release (https://www.governor.ny.gov/news/governor-cuomo-announces-initiative-educate-bar-owners-about-revamped-id-cards ; https://talkofthesound.com/2015/08/22/governor-cuomo-announces-initiative-to-educate-bar-owners-about-revamped-id-cards/)
North Carolina	2008	Bellou and Bhatt (2013)
North Dakota	2006	Bellou and Bhatt (2013)
Ohio	2002	Bellou and Bhatt (2013)
Oklahoma	2003	Bellou and Bhatt (2013)
Oregon	2018	Oregon Department of Transportation (https://content.govdelivery.com/bulletins/gd/ORDOT-2221165?wgt_ref=ORDOT_WIDGET_10b)
Pennsylvania	2001	Bellou and Bhatt (2013)
Rhode Island	2002	Bellou and Bhatt (2013)

South Carolina	2011	Bellou and Bhatt (2013)

State	**Effective Year**	**Source**
South Dakota	2009	Bellou and Bhatt (2013)
Tennessee	2018	Tennessee Department of Safety and Homeland Security (https://legiscan.com/TN/text/SB0384/id/1801695 ; https://www.tn.gov/safety/news/2018/6/28/tennessee-department-of-safety-and-homeland-security-to-roll-out-vertical-licenses-to-people-under-21-years-old.html)
Texas	2001	Bellou and Bhatt (2013)
Utah	2006	Bellou and Bhatt (2013)
Vermont	2003	Bellou and Bhatt (2013)
Virginia	1999	Bellou and Bhatt (2013)
Washington	2001	Bellou and Bhatt (2013)
West Virginia	1999	Bellou and Bhatt (2013)
Wisconsin	2005	Bellou and Bhatt (2013)
Wyoming	2005	Bellou and Bhatt (2013)

Appendix Table 2. Sensitivity of Estimated Effects to Inclusion of State-Specific Linear and Quadratic Time Trends, 1991-2009

	Alcohol Use		Cigarette Use	
	(1)	(2)	(3)	(4)
		Panel I: National YRBS		
VIL	-0.0696**	-0.0616	-0.0534**	-0.0450
	(0.0335)	(0.0435)	(0.0224)	(0.0330)
	[33,318]	[33,318]	[33,872]	[33,872]
Pre-Treatment Mean DV	0.500	0.500	0.321	0.321
		Panel II: State YRBS		
VIL	-0.0099	0.0009	-0.0119	-0.0157
	(0.0099)	(0.0106)	(0.0095)	(0.0113)
	[165,059]	[165,059]	[161,666]	[161,666]
Pre-Treatment Mean DV	0.452	0.452	0.284	0.284
		Panel III: State and National Augmented YRBS		
VIL	-0.0166	-0.0251	0.0013	0.0071
	(0.0133)	(0.0203)	(0.0151)	(0.0165)
	[182,567]	[182,567]	[179,299]	[179,299]
Pre-Treatment Mean DV	0.476	0.476	0.308	0.308
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes
State-Specific Quadratic Time Trend	No	Yes	No	Yes

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the state population of 16-year-olds and standard errors are clustered at the state level. Sample sizes are in brackets. All regressions include state and year fixed effects. All regressions include individual controls of race, grade, and sex, and original state-level controls of: graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income (2019$). Regressions in the Augmented YRBS include a binary control indicating whether an observation comes from the State or National YRBS. The augmented YRBS sample is created by augmenting the state YRBS sample with national YRBS observations if state YRBS observations are not available in a state-year cell.

Appendix Table 3. Sensitivity of Estimates to Restrictions in National and State YRBS Samples to those that Identify Common Treatment Effects in Both Datasets

	Alcohol Use		Cigarette Use	
	(1)	(2)	(3)	(4)
	Panel I: National YRBS			
VIL	-0.0252	-0.0449	-0.0225	-0.0102
	(0.0389)	(0.0365)	(0.0232)	(0.0274)
	[13,845]	[12,328]	[14,143]	[12,631]
Pre-Treatment Mean DV	0.496	0.478	0.321	0.315
	Panel II: State YRBS			
VIL	-0.0097	-0.0036	-0.0124	-0.0140
	(0.0103)	(0.0091)	(0.0089)	(0.0089)
	[85,735]	[76,932]	[82,040]	[73,132]
Pre-Treatment Mean DV	0.452	0.461	0.286	0.287
Restrict to States that Identify Treatment Effects in National and State YRBS	Yes	Yes	Yes	Yes
Restrict to State-Wave Cells with Non-Missing Data from National and State YRBS	No	Yes	No	Yes

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the state population of 16-year-olds and standard errors are clustered at the state level. Sample sizes are in brackets. All regressions include state and year fixed effects. All regressions include individual controls of race, grade, and sex, and baseline state-level controls of: graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income (2019$).

Appendix Table 4. Sensitivity of Estimates to Use of Fully Combined National and State YRBS

	Alcohol Use				Cigarette Use			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel I: 1991-2009				
VIL	-0.0120	-0.0099	-0.0092	-0.0096	0.0090	0.0098	0.0073	0.0044
	(0.0119)	(0.0122)	(0.0133)	(0.0133)	(0.0167)	(0.0167)	(0.0164)	(0.0153)
	[198,385]	[198,385]	[198,385]	[198,385]	[195,547]	[195,547]	[195,547]	[195,547]
Pre-Treatment Mean DV	0.470	0.470	0.470	0.470	0.283	0.283	0.283	0.283
				Panel II: 1991-2019				
VIL	-0.0043	-0.0034	-0.0042	-0.0050	0.0131	0.0124	0.0126	0.0128
	(0.0090)	(0.0092)	(0.0098)	(0.0101)	(0.0118)	(0.0117)	(0.0108)	(0.0114)
	[416,711]	[416,711]	[416,711]	[416,711]	[424,620]	[424,620]	[424,620]	[424,620]
Pre-Treatment Mean DV	0.465	0.465	0.465	0.465	0.278	0.278	0.278	0.278
State and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Baseline State Controls	No	No	Yes	Yes	No	No	Yes	Yes
Expanded State Controls	No	No	Yes	Yes	No	No	No	Yes

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the state population of 16-year-olds and standard errors are clustered at the state level. Sample sizes are in brackets. All regressions include state and year fixed effects. Individual-level controls include race, grade, sex, an indicator for whether an individual is not in a particular grade in school, and an indicator for whether the state-wave combination for a given observation exists in both contributing YRBS samples. Baseline state-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income (2019$). Expanded state controls include presence of e-cigarette tax, ID scanner policies, keg registration policies, minimum legal purchasing age for e-cigarettes of 18, and minimum tobacco purchasing age of 21. Regressions in the Combined YRBS include a binary control indicating whether an observation comes from the State or National YRBS.

Appendix Table 5. Sensitivity of Estimated Effects to Inclusion of State-Specific Linear and Quadratic Time Trends, 1991-2019

	Alcohol Use		Cigarette Use	
	(1)	(2)	(3)	(4)
	Panel I: National YRBS			
VIL	-0.0213	-0.0377	-0.0008	-0.0215
	(0.0178)	(0.0311)	(0.0124)	(0.0205)
	[49,614]	[49,614]	[50,988]	[50,988]
Pre-Treatment Mean DV	0.478	0.478	0.293	0.293
	Panel II: State YRBS			
VIL	-0.0030	0.0026	0.0023	-0.0041
	(0.0055)	(0.0078)	(0.0075)	(0.0087)
	[367,084]	[367,084]	[373,617]	[373,617]
Pre-Treatment Mean DV	0.443	0.443	0262	0.262
	Panel III: State and National Augmented YRBS			
VIL	-0.0048	-0.0023	0.0127	0.0018
	(0.0106)	(0.0113)	(0.0115)	(0.0096)
	[387,459]	[387,459]	[394,322]	[394,322]
Pre-Treatment Mean DV	0.462	0.462	0.275	0.275
State-Specific Linear Time Trend	Yes	Yes	Yes	Yes
State-Specific Quadratic Time Trend	No	Yes	No	Yes

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the state population of 16-year-olds and standard errors are clustered at the state level. Sample sizes are in brackets. All regressions include state and year fixed effects. All regressions include individual controls of race, grade, and sex, and original state-level controls for: graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income. Expanded state controls include presence of e-cigarette tax, ID scanner policies, keg registration policies, minimum legal purchasing age for e-cigarettes of 18, and minimum tobacco purchasing age of 21. Regressions in the Augmented YRBS include a binary control indicating whether an observation comes from the State or National YRBS. The augmented YRBS sample is created by augmenting the state YRBS sample with national YRBS observations if state YRBS observations are not available in a state-year cell.

Appendix Table 6A. Difference-in-Difference-in-Difference Estimates of the Effects of VILs on Cigarette Use for 16-Year-Olds Relative to Untreated 17-Year-Olds

	1991-2009			1991-2019		
	National YRBS	State YRBS	State and National Augmented YRBS	National YRBS	State YRBS	State and National Augmented YRBS
	(1)	(2)	(3)	(4)	(5)	(6)
			Cigarette Use			
VIL * Age 16	-0.0429*	0.0043	0.002	-0.0274	0.0085*	-0.0015
	(0.0239)	(0.0069)	(0.0117)	(0.0218)	(0.0050)	(0.0086)
VIL	0.0315	-0.0059	0.0128	0.0342***	0.0031	0.0176*
	(0.0208)	(0.0085)	(0.0137)	(0.0154)	(0.0061)	(0.0103)
Observations	[68,471]	[300,422]	[336,157]	[102,265]	[693,539]	[735,548]
Pre-Treatment Mean DV for Age 16	0.322	0.284	0.308	0.294	0.262	0.275

***Significant at 1% **at 5% *at 10%

Notes: All regressions are obtained using a sample of 16-to-17-year-olds and are weighted using the relevant state population and standard errors are clustered at the state level. Sample sizes are included in brackets. Individual controls include race, grade, and sex. State-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; median income; presence of e-cigarette tax; ID scanner policies; keg registration policies; minimum legal purchasing age for e-cigarettes of 18; and minimum tobacco purchasing age of 21. All regressions include age-specific state fixed effects, age-specific year fixed effects, and dummies for whether the respondent is age 16. They key policy variable of interest is the interaction of the treated age (16) and the VIL indicator. Regressions in the Augmented YRBS include a binary control indicating whether an observation comes from the State or National YRBS. The augmented YRBS sample is created by augmenting the state YRBS sample with national YRBS observations if state YRBS observations are not available in a state-year cell.

Appendix Table 6B. Lagged Effect of VILs on 17-to-18-Year-Olds Who Were Treated When Age 16

	Alcohol Use	Binge Drinking	Frequent Binge Drinking	Smoking Participation	Everyday Smoking
	(1)	(2)	(3)	(4)	(5)
VIL	0.0010	0.0018	0.0022	0.0225**	0.0116
	(0.0091)	(0.0107)	(0.0062)	(0.0105)	(0.0071)
	[499,531]	[481,551]	[481,551]	[507,372]	[507,372]
Pre-Treatment Mean DV	0.505	0.336	0.169	0.295	0.118

***Significant at 1% **at 5% *at 10%

Notes: All regressions use the augmented YRBS sample and are weighted using the relevant state population, and standard errors are clustered at the state level. Sample sizes are in brackets. Individual controls include race, grade, and sex. State-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; median income; presence of e-cigarette tax; ID scanner policies; keg registration policies; minimum legal purchasing age for e-cigarettes of 18; and minimum tobacco purchasing age of 21. All regressions include state and year fixed effects.

Appendix Table 7. Sensitivity of Estimates to Intensive Margin of Alcohol and Cigarette Use, 1991-2019

	Binge Drinking | Drinking	Frequent Binge Drinking | Drinking	Everyday Smoking | Smoking
	(1)	(2)	(3)
		Panel I: National YRBS	
VIL	0.0395	0.0134	0.0329
	(0.0241)	(0.0247)	(0.0285)
	[20,550]	[20,550]	[10,545]
Pre-Treatment Mean DV	0.626	0.280	0.323
		Panel II: State YRBS	
VIL	0.0153**	0.0015	0.0067
	(0.0069)	(0.0082)	(0.0148)
	[124,842]	[124,842]	[60,010]
Pre-Treatment Mean DV	0.611	0.271	0.367
		Panel III: State and National Augmented YRBS	
VIL	0.0271**	0.0226**	0.0023
	(0.0122)	(0.0109)	(0.0170)
	[134,034]	[134,034]	[65,061]
Pre-Treatment Mean DV	0.614	0.272	0.340

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the state population of 16-year-olds and standard errors are clustered at the state level. Sample sizes are in brackets. All regressions include state and year fixed effects. Individual controls include race, grade, and sex. State-level controls include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income; presence of e-cigarette tax; ID scanner policies; keg registration policies; minimum tobacco purchasing age of 21; minimum legal purchasing age for e-cigarettes of 18. Regressions in the Augmented YRBS include a binary control indicating whether an observation comes from the State or National YRBS. The augmented YRBS sample is created by augmenting the state YRBS sample with national YRBS observations if state YRBS observations are not available in a state-year cell.

Appendix Table 8. Multinomial Logit Estimates (Marginal Effects) of the Effects of VILs on Usual Sources of Alcohol and Cigarettes for Individual Ages 17 and 18

	Age 17	Age 18
	(1)	(2)
	Panel I: Usual Sources of Alcohol (2007-2019)	
Own purchase	-0.0012	-0.0064
	(0.0031)	(0.0071)
Pre-Treatment Mean	0.044	0.067
Third-party	0.0065	0.0095
	(0.0077)	(0.0116)
Pre-Treatment Mean	0.117	0.134
Other	0.0192	-0.0171
	(0.0118)	(0.0140)
Pre-Treatment Mean	0.288	0.284
Observations	[155,478]	[72,934]
	Panel II: Usual Sources of Cigarettes (1995-2015)	
Own purchase	0.0145	0.0352*
	(0.0078)	(0.0157)
Pre-Treatment Mean	0.119	0.239
Third-party	-0.0002	0.0051
	(0.0070)	(0.0057)
Pre-Treatment Mean	0.149	0.061
Other	-0.0054	-0.0023
	(0.0030)	(0.0026)
Pre-Treatment Mean	0.035	0.021
Observations	[182,853]	[95,458]

***Significant at 1% **at 5% *at 10%

Notes: All regressions are based on the augmented YRBS sample and include state and year fixed effects, are weighted using the relevant state population, and use standard errors clustered at the state level. Sample sizes are in brackets. All regressions include the full vector of controls included in the notes to Table 4. The reference outcome category for multinomial logit regressions is "Didn't drink" for Panel I and "Didn't smoke" for Panel II. Definitions of outcomes differ slightly across Panels I and II due to YRBS survey composition and changes thereof over time. "Bought in person" in Panel I refers to a respondent buying alcohol at a store (such as a liquor, grocery, or convenience store), bar, restaurant, nightclub, or public event (such as concert or sporting event), but in Panel II refers specifically to buying cigarettes in a store. "Third-party" in Panel I refers specifically to a third-party purchase in which a respondent gives money to someone else to buy alcohol for them, but in Panel II refers to that type of third-party purchase for cigarettes in addition to borrowing or "bumming" cigarettes from another person. "Other" in Panel I refers to taking alcohol from a store or family, being given alcohol by someone else, or any other method of obtaining alcohol, but in Panel II refers to taking cigarettes from a store or family, purchasing via the internet or a cigarette vending machine, or some other method of obtaining cigarettes. Sample waves for the regressions above are determined by YRBS waves in which the questions appear.

Appendix Table 9. Estimated Effect of VILs on the Probability that a 16-Year-Old Cigarette Smoker who Purchases their Cigarettes Themselves is Asked by a Vendor Show Identification for the Cigarette Purchase

	National YRBS (Replication of Bellow and Bhatt)	**State YRBS** (Extension to State YRBS)
	(1)	(2)
VIL	0.0979 (0.0592) [3,102]	-0.0483* (0.0238) [5,067]
Pre-Treatment Mean DV	0.344	0.346

***Significant at 1% **at 5%

Notes: Regressions are obtained using data from 1991-2005 due to data availability. All regressions are weighted using the relevant state population and standard errors are clustered at the state level. Sample sizes are in brackets. All regressions include state and year fixed effects. All regressions include individual controls of race, grade, and sex, and baseline state-level controls of: graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; and median income (2019$). All regressions include state and year fixed effects.

Appendix Table 10. Estimates of Effects of VILs on Drunk Driving and Traffic Fatalities, 1991-2019

	National YRBS	State YRBS	State and National Augmented YRBS	FARS			
	Driving After Drinking			**Fatalities**	**BAC>0 Fatalities**	**BAC>0.10 Fatalities**	**Weekend Fatalities**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel I: Age 16			
VIL	-0.0072	0.0012	-0.0022	-0.0058	-0.0175	-0.0118	0.0291
	(0.0082)	(0.0052)	(0.0058)	(0.0291)	(0.0951)	(0.1619)	(0.0441)
	[51,647]	[299,698]	[320,890]	[1,479]	[1,479]	[1,479]	[1,479]
Pre-Treatment Mean DV	0.122	0.081	0.100	33.42	3.00	1.72	12.05
				Panel II: Ages 17-to-18			
VIL	0.0006	-0.0064	-0.0078	0.0020	-0.0104	0.0007	0.0050
	(0.0109)	(0.0045)	(0.0063)	(0.0358)	(0.0377)	(0.0563)	(0.0380)
	[83,672]	[377,051]	[412,333]	[2,958]	[2,958]	[2,958]	[2,958]
Pre-Treatment Mean DV	0.206	0.134	0.170	45.23	7.16	4.54	17.62

***Significant at 1% **at 5% *at 10%

Notes: All regressions are weighted using the relevant state population and use standard errors clustered at the state level. Sample sizes are in brackets. All regressions include state and year fixed effects. Columns 1-3 report results of OLS regressions on the indicated YRBS sample. Individual-level controls for columns 1-3 include age (omitted in Panel I), race, grade, and sex. State-level controls for columns 1-3 include graduated driver's license programs; smoke free workplace, restaurant, and bar laws; zero tolerance laws; punishments for minors who attempt to buy tobacco; ID requirements for tobacco purchase; tobacco vending machine placement restrictions; minimum tobacco purchasing age of 18; real cigarette tax (2019$); real beer tax (2019$); annual unemployment rate; median income (2019$); presence of e-cigarette tax; ID scanner policies; keg registration policies; minimum tobacco purchasing age of 21; and minimum legal purchasing age for e-cigarettes of 18. Regressions in the Augmented YRBS include a binary control indicating whether an observation comes from the State or National YRBS. Columns 4-6 report results of Poisson regressions on FARS data. Age is included as an individual level control for columns 4-8 of Panel II. State-level controls for columns 1-4 include the full vector of controls for columns 1-3, plus additional controls for: texting-and-driving bans; seatbelt laws; medical and recreational marijuana laws; and real gasoline taxes (2019$).