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# THE EFFECTS OF E-CIGARETTE MINIMUM LEGAL SALE AGE LAWS ON YOUTH SUBSTANCE USE

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## ABSTRACT

We use difference-in-differences models and individual-level data from the national and state Youth Risk Behavior Surveillance System (YRBSS) from 2005 to 2015 to examine the effects of e-cigarette Minimum Legal Sale Age (MLSA) laws on youth cigarette smoking, alcohol consumption, and marijuana use. Our results suggest that these laws increased youth smoking participation by about one percentage point, and approximately half of the increased smoking participation could be attributed to smoking initiation. We find little evidence of higher cigarette smoking persisting beyond the point at which youth age out of the laws. Our results also show little effect of the laws on youth drinking, binge drinking, and marijuana use. Taken together, our findings suggest a possible unintended effect of e-cigarette MLSA laws—rising cigarette use in the short term while youth are restricted from purchasing e-cigarettes.

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## 1. Introduction

Teenage substance use continues to be a major public health concern. Substance use has been linked with poor academic performance, impaired cognitive development, other mental and physical health problems, and motor-vehicle accidents (National Institute on Drug Abuse , National Institute on Alcohol Abuse and Alcoholism 2016). Tobacco, marijuana, and alcohol are among the most widely used substances by adolescents. Although rates of youth smoking are declining, each day more than 3,200 youth initiate cigarette consumption and more than 2,000 transition into daily smoking (US Department of Health Human Services 2014). Marijuana is the most commonly used illicit drug, with 22% of high school seniors reporting past month use. Moreover, alcohol use among youth is even more widespread than the use of tobacco or illicit drugs. Almost one out of three youth has consumed alcohol in the past month, and almost one out of five has participated in binge drinking (Centers for Disease Control and Prevention 2015).

Though a large economics literature has examined the cross-relationship between smoking, drinking and marijuana use (Dee 1999, Gruber, Sen, and Stabile 2003, Picone, Sloan, and Trogdon 2004, Chaloupka et al. 1999, Farrelly et al. 2001), the introduction of electronic cigarettes (e-cigarettes or e-cigs) presents youth with an alternative that could disrupt their use of these substances. E-cigarettes are a particular type of vaping device within the broader class of electronic nicotine delivery systems (ENDS), and differ primarily from conventional cigarettes by permitting the inhalation of nicotine that is heated rather than combusted, thereby substantially reducing the harm associated with combustion-related byproducts. Since their introduction into the U.S market, e-cigarettes have been advertised and positioned as alternatives to conventional cigarettes, and their popularity particularly among youth has risen exponentially.[1] Within a four-year period (2011-2015), e-cigarette use has increased from 1.5% to 16.0% among high school students and from 0.6% to 5.3% among middle school students,

[1] The Tobacco Control Act of 2009 gave the Food and Drug Administration (FDA) jurisdiction over tobacco products, and this "deeming" rule was finalized in 2016.

surpassing cigarettes as the most commonly used tobacco product among adolescents (Singh 2016).[2]

A heated policy debate concerning the regulation of e-cigarettes has ensued, at the heart of which are fundamental questions regarding the relative risks between e-cigarettes and conventional cigarettes and the potential for e-cigarettes to serve as a tool towards tobacco harm reduction. A recent report issued by the British government suggests that e-cigarettes are no more than five percent as harmful as conventional cigarettes (Tobacco Advisory Group of the Royal College of Physicians 2016). Other studies have suggested that e-cigarettes can direct smokers away from smoking and possibly help them quit (Hampton 2014, Abrams 2014, Brandon et al. 2015, McNeill et al. 2015). However, the 2016 Surgeon General's Report warns that e-cigarettes are dangerous to adolescents because they can interfere with cognitive development and can cause nicotine addiction (US Department of Health Human Services 2016). One particular concern is that e-cigarettes may act as a gateway towards the use of other addictive substances, such as cigarettes, alcohol, and marijuana (Gostin and Glasner 2014, Primack et al. 2015, Mammen, Rehm, and Rueda 2016). While the downward trend in youth smoking indicates a reduction in the number of new initiates, possibly because some of these youth are starting to use e-cigarettes instead, it is not clear that this trend is necessarily harm-reducing since youth who initiate nicotine with e-cigarettes may transition to smoking at some later point in time or transition to dual use. Polysubstance use is also quite prevalent among youth, which may lead to further spillovers from tobacco use into the use of other substances such as alcohol or marijuana.[3]

In response, state governments passed a wave of regulations limiting youth access to e-cigarettes. A popular initiative has been the adoption of Minimum Legal Sale Age (MLSA) laws on e-cigarettes analogous to those passed for conventional cigarettes decades ago. New Jersey became the first state to implement an e-cigarette MLSA law in March of 2010, followed by

[2] Among adults, the 2014 National Health Interview Survey shows that 12.6% had ever used e-cigarettes at least once and 3.7% currently use e-cigarettes (Schoenborn and Gindi 2015).

[3] Data from Wave 4 of the Add Health Survey indicated that 34% of youth reported either early use of both alcohol and marijuana, or alcohol, marijuana, and cigarettes (Moss, Cen, and Yi 2014).

four other states later within the same year.[4] Additional states adopted an e-cigarette MLSA law in each year subsequently, and by the time the Food and Drug Administration (FDA) mandated a federal e-cigarette MLSA law of 18 in August of 2016, all states but two had an e-cigarette MLSA law in place.[5]

Youth use of e-cigarettes, or vaping, is predicted to decline as costs and other components of the "full price" associated with the product rise due to restrictions on youth access. The effect of e-cigarette MLSA laws on smoking and the use of other addictive substances is a priori ambiguous. If e-cigarettes are economic substitutes with other tobacco products or other addictive substances, then e-cigarette restrictions may induce substitution toward smoking and other substance use, counteracting some of the intended public health gains. On the other hand, if these substances are economic complements, contemporaneously and intertemporally, then restricting e-cigarette access will additionally reduce smoking and the addictive stock of nicotine, and possibly drinking and marijuana use, among youth currently and as they transition into adulthood. Understanding such policy-driven spillovers and cross-price effects are integral towards informing the debate underlying e-cigarettes and designing optimal regulatory policy.

In this study we assess whether, and the extent to which, restricting youth access to e-cigarettes has affected their use of other addictive substances. We contribute to the limited literature on the effects of e-cigarette MLSA laws in several ways. First, the few studies that have explored the effect of e-cigarette MLSA laws on youth smoking have arrived at mixed conclusions, and our study attempts to provide further clarity to this conflicting evidence base (Friedman 2015, Pesko, Hughes, and Faisal 2016, Abouk and Adams 2017). Second, we extend the prior work and provide the first evidence on the intertemporal relationship between e-cigarette MLSA laws and youth smoking. In addition to any contemporaneous effects, by affecting the addictive nicotine stock, e-cigarette MLSA laws may also have dynamic effects. Our study informs whether a policy that makes vaping less attractive today makes future

[4] Utah, New Hampshire, Minnesota, and California enforced the law on May 11, July 31, August 1, and September 27, all in 2010, respectively.

[5] Appendix Table 1 provides a list of states that have implemented the e-cigarette MLSA laws over our sample period spanning 2005-2015.

smoking more or less likely when youth are no longer subject to the MLSA-based restriction. As noted above, this intertemporal transition from e-cigarette use to smoking among youth forms one of the key questions underlying the policy debate. Third, we broaden the lens to other addictive substances and provide some of the first evidence on potential spillover effects of e-cigarette MLSA laws on other substance use. Such spillover effects are plausible given the high co-occurrence of and transitions between alcohol, marijuana, and tobacco use among adolescents.[6]

The remainder of the study is organized as follows. Section 2 provides a brief background and a review of the relevant literature. Section 3 outlines a conceptual framework of the various channels through which e-cigarette MLSA laws may affect substance use, and motivates our empirical specifications. Section 4 describes the assembled data, followed by a description of the empirical approach in section 5. We present the findings in section 6, and the final section discusses some of the implications of these results.

## 2. Relevant Studies

Individual states have made several efforts in recent decades to tighten tobacco control regulations by prohibiting retailers from selling tobacco products to minors. Several studies have examined the efficacy of cigarette MLSA laws adopted between the 1980s and early 1990s in curbing youth smoking. Though many of these studies suggest that the laws have been effective in reducing youth smoking (Chaloupka and Pacula 1998, Gruber and Zinman 2001, Ahmad and Billimek 2007, DiFranza, Savageau, and Fletcher 2009), some find the law effects are limited. Chaloupka and Grossman (1996) find little effect of youth access restrictions on youth smoking, which they attribute to the weak enforcement of the laws. DeCicca and colleagues (2002), using indices of smoking restrictions that ranged from youth access restrictions to restrictions on smoking in public places, also find limited effects of the laws. A recent study by Yoruk and Yoruk (2015) revisits the effect of cigarette MLSA laws on youth

[6] Data from the 2014 National Survey of Drug Use and Health (NSDUH) suggest that, among youth ages 12-17 who have used tobacco products in the past year, 88% have also consumed alcohol and 56% have used marijuana over this period.

smoking using a regression discontinuity design. They find that gaining legal access to tobacco products once youth have aged out is associated with a 2 to 3 percentage point increase in the probability of smoking, though the effects are imprecisely estimated. By focusing on a subsample where youth had smoked in the prior wave, the authors find that cigarette MLSA laws lead to a statistically significant increase in the probability of smoking (five percentage points) and frequency of smoking (25% increase).

Several studies have focused specifically on the impact of e-cigarette MLSA laws. Friedman (2015) and Pesko et al. (2016), based on state-aggregated data spanning up to 2013, both find that e-cigarette MLSA laws have increased youth smoking by 0.8 to 0.9 percentage points.[7] These results are consistent with e-cigarettes and conventional cigarettes being economic substitutes, at least contemporaneously. In contrast, the results in Abouk and Adams (2017) suggest complementarity between e-cigarettes and cigarettes. Their study finds that e-cigarette MLSA laws have led to a reduction in smoking among high-school seniors (12th graders), based on individual-level data spanning 2007-2014 from Monitoring the Future (MTF). It is unclear whether the divergence in findings stems from the use of more granular individual-level data or from the addition of one more study period.[8]

Our study extends this seminal work and contributes to this limited literature in three important ways. First, we add to the thin evidence base by incorporating micro-level data from both the national and the state YRBSS spanning up to 2015. This yields a substantially larger sample size (over 700,000 observations) relative to Abouk and Adams 2017. Utilizing data up to 2015, just prior to the FDA's national ban on e-cigarette sales to minors, further maximizes policy variation (8 additional states had adopted these laws in 2015) and extends the post-policy window for the other states to disentangle the law's dynamic effects. Second-order policy responses on youth substance use (other than e-cigarettes) are likely to be small, and

[7] Friedman (2015) is based on 2-year state aggregated data from the NSDUH (spanning 2002-2013) and Pesko et al. (2016) is based on the state-aggregated data from the Youth Risk Behavioral Surveillance System (YRBSS; spanning 2007-2013).

[8] A recent study by Pesko et al. (2018), pairing cigarette prices from the Nielsen retail scanner data with the individual-level data from MTF, shows that higher cigarette prices are positively associated with youth use of e-cigarettes, consistent with the argument that electronic and conventional cigarettes are economic substitutes.

hence micro-level data with large sample sizes, more cleanly-defined affected groups, and longer time windows with greater policy variation may be necessary for maximizing precision. Second, prior work has focused only on the contemporaneous effects of e-cigarette MLSA laws on smoking behaviors. Our study is the first to consider how these laws may affect youth smoking rates once they have aged out of the restrictions and are able to purchase e-cigarettes. This is particularly relevant for assessing long-term effects on smoking rates and addressing public health concerns regarding the intertemporal transition from e-cigarettes to smoking. Finally, we also estimate whether e-cigarette MLSA laws have had any spillover effects into the use of other addictive substances. With the exception of Pesko et al. (2016), who studied and found no effects on marijuana use, prior work has mainly focused on cigarette smoking.

## 3. Conceptual Framework

The overall effect of e-cigarette MLSA laws on smoking, drinking, and marijuana use depends on the marginal direct and indirect costs of youth obtaining e-cigarettes, and the relationship between e-cigarettes and these other substances. Banning legal sales of e-cigarettes to minors is predicted to raise the indirect costs of obtaining the product through added inconvenience and/or associated time delays. The restrictions could also increase the direct costs of obtaining the product through additional markups or youth having to pay "friends" to purchase the product for them. E-cigarette MLSA laws will therefore raise the full price of e-cigarettes, leading to first-order effects in the form of a decline in e-cigarette consumption. However, the predicted decrease in e-cigarette consumption may be moderated to the extent that retailers do not abide by the law or that youth are able to bypass the law through online vendors.[9]

Any rise in the indirect or direct costs of purchasing e-cigarettes would cause a *relative* increase in the cost of e-cigarettes in comparison with conventional cigarettes, thereby affecting not just e-cigarette use but also potentially shifting smoking behaviors. E-cigarettes

[9] Data from the 2015 NYTS suggest however that only 1.1% of teens who used e-cigarettes in the past month obtained them through online vendors. When we restrict the sample to high school aged youth (16 or above), only 1.8% of them obtained e-cigarettes over the Internet in the past month.

and conventional cigarettes are both alternate modes of delivering nicotine, and youth may substitute to smoking if e-cigarettes become relatively more difficult to procure. Additionally, the e-cigarette MLSA laws may raise smoking if e-cigarettes are being used for smoking cessation or for cutting down on cigarette consumption. Losing access to e-cigarettes could therefore reduce a smoker's propensity to attempt cessation. Though some studies have documented the success of e-cigarettes in helping adult smokers quit (Etter and Bullen 2011, Brown et al. 2014, Adkison et al. 2013), the cessation margin may be less salient when it comes to youth. As most smokers initiate smoking prior to age 18, this initiation margin may be more relevant for adolescents, and policies that restrict access to e-cigarettes may induce some youth to initiate tobacco use with cigarettes instead.

While these channels underscore substitutability between e-cigarettes and conventional cigarettes and predict an increase in smoking as a result of the e-cigarette MLSA laws, it is also possible that e-cigarette restrictions may lead to a reduction in smoking. As with adults, concurrent use of e-cigarettes and conventional cigarettes is high among youth (Dutra and Glantz 2014).[10] If this pattern is reflective of economic complementarity, then policy-induced reductions in e-cigarette use could reduce cigarette consumption. Furthermore, lower e-cigarette use may reduce nicotine dependence, ceteris paribus, and make it less likely that youth may turn to tobacco products in general to satisfy nicotine-induced cravings. E-cigarette MLSA laws could also raise youth interest in the motive underlying the legal change and encourage them to search for health information related to e-cigarettes and possibly other substances, which could in turn change their attitudes toward consumption.

The e-cigarette MLSA laws may also impact dynamic transitions between e-cigarette use and smoking along channels similar to those discussed above. As with the contemporaneous effects, these intertemporal effects of the e-cigarette restrictions on smoking, for instance what happens to their smoking behaviors once adolescents have aged out of the restrictions, are also a priori indeterminate. It should be noted that once a youth turns 18, he is able to purchase

[10] Dutra and Glantz (2014) report that 76.3% of current e-cigarette users also concurrently used conventional cigarettes, based on the 2012 National Youth Tobacco Survey.

both e-cigarettes and conventional cigarettes legally.[11] In states that have enacted an e-cigarette MLSA law, youth who age out of the laws will therefore experience a decrease in the *relative* cost of obtaining e-cigarettes, which could lead to an increase in e-cigarette use and a decrease in smoking. However, if youth had turned to smoking when exposed to e-cigarette MLSA laws, the accumulation of the addictive stock of nicotine may make it difficult to cut down on smoking even when they are able to purchase e-cigarettes legally.

While effects on smoking are perhaps most highly indicated given the proximity between e-cigarettes and conventional cigarettes, the e-cigarette MLSA laws may also have second-order effects on the use of other addictive substances. Many youth concurrently smoke, drink, and use marijuana (Moss, Chen, and Yi 2014), and changes in tobacco consumption can affect the marginal utility of consuming these other substances. A large literature has explored this relationship between smoking, drinking, and marijuana use, based on variation stemming from cigarette excise taxes, medical marijuana laws, minimum legal drinking age, and other policies, though there still lacks a strong consensus in this literature regarding whether these substances are economic substitutes or complements.[12] Ultimately the question of how e-cigarette MLSA laws impact smoking, drinking, and marijuana use cannot be settled based on theory alone, and we bring empirical evidence to bear on this issue.

## 4. Data

Our analyses draw on the pooled national and state Youth Risk Behavior Surveillance System (YRBSS). The national YRBSS is conducted by the Centers for Disease Control and Prevention (CDC) and the state YRBSS, while coordinated by CDC, is usually administered by state health departments or education agencies.[13] Several studies note the advantages of using

[11] In most cases, youth aged 18 are old enough to legally purchase e-cigarettes and conventional cigarettes except for a few cases where states set the minimum age at 19 or 21.

[12] See, for instance, Crost and Guerrero (2012), Crost and Rees (2013), Dee (1999), Farrelly et al. (2001), Gruber et al. (2003), and Picone et al. (2004).

[13] State identifiers are not provided in the national YRBSS by default, but we obtained these from the CDC and use them in all analyses. We received the state-level data from either the CDC directly or from the states. Some states do not distribute their data due to low response rates, and so we did not receive these data nor use them in the analysis.

such pooled data over the national YRBSS alone, and we think the pooled YRBSS is especially well-suited for the analysis.[14] For one thing, very few datasets have requisite sample sizes and contain information on smoking and substance abuse patterns among adolescents over the time period when e-cigarette restrictions have been unfolding. The pooled YRBSS is one of the few that do, yielding sample sizes close to 800,000 person-year observations, which are 9 times larger than the national YRBSS and 15 times the MTF. Moreover, the pooled YRBSS maximizes the sample size for smaller states and thereby improves precision and state-trend controls. Most importantly, the policy effects being estimated under our model specifications are intention-to-treat (ITT) effects whose precision rely on sample sizes due to relatively low prevalence rates of youth substance use (smoking in particular), and that ITT estimates capture the average population effects. In that sense, large sample size will be necessary to reliably detect potentially small ITT effects. Note that some of these policy effects (for instance, on drinking or marijuana use) are third-order effects and would particularly benefit from large samples; even if they are insignificant, it is important to precisely document these null effects.

The YRBSS is conducted biennially, and we utilize data from the most recent six waves spanning 2005 through 2015. As the first set of states implemented e-cigarette MLSA laws in 2010, this ensures that our sample period includes a five-year pre-policy window at a minimum.[15] Although we have only three waves (5 years) of post-policy data given the biennial structure of the YRBSS, there is sufficient variation in the observed exposure to e-cigarette MLSA laws within states over time, which we exploit to identify the policy effects. Appendix Table 5 shows the states that are represented in the pooled YRBSS for each wave over the course of the study, as well as the number of observations in each state by year cell. We follow prior studies and use weights based on population, gender, race, and age at the state by year

[14] See, for instance, Carpenter and Cook (2008), Anderson et al. (2015), Sabia and Anderson (2016), and Hansen, Sabia, and Rees (2017).

[15] We do not extend our sample to previous years in order to minimize introducing confounding trends and trend breaks from periods prior to when e-cigarettes became available in the U.S. However, we note that our estimates are robust to utilizing all waves of the YRBSS (1991-2015) or to starting the analyses in 2007, the year when e-cigarettes entered the U.S. Results for these alternate sample periods are available from the authors upon request.

level retrieved from the National Cancer Institute's Surveillance Epidemiology and End Results Program for all analyses (Anderson, Hansen, and Rees 2015).[16]

The YRBSS data collection typically starts in March and ends in early June for each state. Our policy indicator for the e-cigarette MLSA law is therefore set to turn on (equal one) if the law has been effective by the end of February of the survey year and thereafter, and zero otherwise.[17] A battery of questions relating to youth risky behaviors such as smoking, drinking, and other substance use is consistently available in each wave of the YRBSS. We define dichotomous indicators for past month participation in smoking, alcohol consumption, binge drinking (consuming 5 or more drinks of alcohol in a row), and marijuana use. We also define an indicator for smoking initiation based on youth current age and the age they reported smoking a full cigarette for the first time; this indicator captures whether the respondent initiated smoking in a given wave if their current age matches their reported age of smoking onset.

To isolate the *ceteris paribus* relationship between e-cigarette MLSA laws and youth substance use, we control for an extensive set of confounding policy shifts over this period: federal and state cigarette excise taxes, state beer taxes, medical marijuana laws (MMLs), marijuana decriminalization laws, state unemployment rates, and the natural logarithm of state per capita income. To proxy for anti-smoking sentiment, we control for the presence of comprehensive smoke-free air laws covering four venues: government and private worksites, restaurants, and bars. We also account for anti-vaping sentiment by using an indicator variable for whether vaping in private workplaces is restricted.[18] We do not use e-cigarette taxes as a control because only Minnesota has levied taxes on e-cigarettes over the study period. Lastly, we control for a set of underage drinking regulations, ranging from zero-tolerance laws to laws related to alcohol possession, alcohol consumption, alcohol purchase, license suspension,

[16] Results from regressions without weights are very similar and are available from the authors upon request.
[17] Following this logic, we code four states as having e-cigarette MLSA laws by 2011, nine additional states by 2013, and 21 states in total (beyond the 13 previously) by 2015.
[18] No partial bans on vaping in private workplaces exist. We have also experimented with including smoke-free air laws in bars and restaurants to further control for state anti-smoking sentiment, which are highly collinear with private workplace laws. Our estimate of the impact of e-cigarette MLSA laws was not materially affected by adding these additional proxy variables for anti-smoking sentiment.

parties involving underage drinking, and keg registration, to account for the social norm against underage drinking.[19] Please see the Data Appendix for additional information on our control variables.

Table 1 presents descriptive statistics for all key variables over our study period, with their means weighted by the total underage population. Columns 1, 2, 3 present means for the full sample, and for youth under the age of 18, and for older youth respectively. While four states (Alabama, Alaska, New Jersey, and Utah) set the purchasing age of e-cigarettes at 19 years old, age in the YRBSS is top-coded at "18 or above" and we are unable to separate out youth who are 19 years of age.[20] As shown in Table 1, 17% of the sample are past-month smokers, 20% are marijuana users, 39% are past-month drinkers, and 23% have participated in binge drinking. The proportion of current smokers, drinkers, and marijuana users among those who are 18 or above is expectedly and significantly higher than that among youth 17 or younger. Questions related to youth e-cigarette use are first included in the YRBSS in 2015, and using data from this wave, we find that 45% of high-school students have tried e-cigarettes in their lifetime and 24% are current (past 30-day) e-cigarette users. The final two columns present means of all variables during the pre-policy window, separately for states that have implemented e-cigarette MLSA laws at any time over the sample period (MLSA or treated states) and states that have not yet (non-MLSA or control states). Baseline youth substance use rates are slightly higher among the control states (by about 2-3 percentage points, or about 10%), though the differences are insignificant.

## 5. Empirical Approach

[19] The full set of underage drinking regulations is listed in the summary statistics table (Table 1), and we obtain all the information from the Alcohol Policy Information System (APIS).

[20] This will result in some individuals "18 and above" being subject to e-cigarette MLSA laws, that is, some individuals in the control group may be treated. We found that moving these youth into column 2 does not at all change the means. Based on the 2016 American Community Survey, among current high-school enrollees nationally between the ages of 12-19, only about 2% are 19, and only 4.3% (based on the share of the population of the affected states, AK, AL, NJ and UT) of these 19-year olds would be misclassified as being not treated. Hence, any attenuation bias from this misclassification is negligible. We show later that our results are not sensitive to excluding these four states from the analyses.

Our baseline model employs the standard difference-in-differences (DD) framework, exploiting variation in the timing of policy change within states over time to identify the effects of e-cigarette MLSA laws on youth substance use behaviors. Specifically, we estimate the following reduced-form demand function, relating substance use behaviors for youth $i$ residing in state $s$ and surveyed at time $t$ directly to e-cigarette MLSA laws.

$$(1)\quad P\left(DV_{i,s,t} > 0\right) = \sum_i \Gamma \mathbf{X}_{\mathrm{i,s,t}} + \mathbf{b_1}\mathrm{MLSA}_{\mathrm{s,t}} + \mathbf{b_2}\mathbf{Z}_{s,t} + \boldsymbol{\gamma}_s + \boldsymbol{\lambda}_t\ [+\boldsymbol{\gamma}_s t]\ [+\boldsymbol{\gamma}_s t_{Pre}] \dagger\ \varepsilon_{i,s,t},$$

where $DV_{i,s,t}$ is one of the four indicator outcome variables for the youth's past month substance use behavior. For instance, when $DV_{i,s,t}$ indicates smoking, $P(DV_{i,s,t} > 0)$ denotes the probability that the youth is a current smoker. Our key variable of interest, $\mathrm{MLSA}_{\mathrm{s,t}}$, is an indicator variable for whether state *s* had an e-cigarette MLSA law in place by the end of February of the survey year and thereafter. The vector $\mathbf{X}_{\mathrm{i,s,t}}$ contains a full set of youth demographic characteristics and the vector $\mathbf{Z}_{\boldsymbol{s,t}}$ contains the time-varying state policy controls (inflation-adjusted cigarette and beer taxes expressed in 2015 dollars, a set of indicator variables for MMLs, restrictions on vaping in private workplaces and smoking in public places, a set of indicator variables for underage drinking regulations, state unemployment rates, and the natural logarithm transformed state per capita income). All specifications include state and year fixed effects, denoted by $\boldsymbol{\gamma}_s$ and $\boldsymbol{\lambda}_t$, to account for the time-invariant state heterogeneity and unobserved national trends. All specifications are estimated as linear probability models via OLS.[21] By convention, we cluster standard errors at the state level to account for correlated errors across individuals and over time within each state (Bertrand, Duflo, and Mullainathan 2004).

The parameter of interest $\mathbf{b_1}$ captures the average reduced-form effects of e-cigarette MLSA laws on youth smoking, drinking, or marijuana use, including through all reinforcing and/or competing pathways as discussed earlier. Identification of policy effects comes from comparing changes in youth substance use rates within states that have implemented e-cigarette MLSA laws to changes in states that have not yet done so. The DD estimates will yield

[21] Our results and conclusions are not materially affected if the specification is estimated via a logit or probit regression.

the causal effect if outcome trends for the control states (states that have not yet adopted e-cigarette MLSA laws) are valid counterfactual to outcome trends for the treatment states (those that have implemented the restrictions) in the absence of the policy (Colman and Dave 2015). We investigate this "parallel trends" assumption in Figure 1, generated using data from the pooled YRBSS and weighted by the total underage population.

Figure 1 shows trends for youth smoking, drinking, binge drinking, and marijuana use before and after the enactment of e-cigarette MLSA laws in the context of an unadjusted event study design. The *x*-axis of the figure is the survey year relative to the year MLSA laws turned on, so that year 0 represents the first year MLSA laws are coded as 1. For states that do have the laws by February 2015, we assign each a randomly selected pseudo-MLSA date by respecting the true distribution of effective dates among the MLSA states, and then normalize them to time zero. We use solid lines to track the mean substance use rates among the MLSA states and dashed lines for the non-MLSA states. Appendix Table 1 shows that several states have adopted MLSA laws over the sample period, and thus we generate Figure 1 by netting out these state fixed effects.[22] Figure 1 suggests a few things. Most apparently, the pre-policy trends for all outcomes track each other closely between the MLSA and non-MLSA states, providing visual evidence for the "parallel trends" assumption. We also statistically test for pre-policy differentials by regressing the outcome measure on an indicator for being the MLSA states interacted with the linear pre-policy trends, controlling for a set of individual- and state-level covariates listed in specification (1). This allows us to assess whether there exists any remaining systematic differences in trends prior to policy exposure between the MLSA and non-MLSA states in a specification analogous to our main models. Appendix Table 2 reports the point estimates for the interaction term, which suggest little evidence of differences in pre-policy trends, consistent with Figure 1.

Second, we see clear trend breaks in youth smoking, drinking, and binge drinking around the MLSA restrictions, suggesting positive behavioral responses to the policy, but little or no break around the MLSA restrictions in youth marijuana use. Although these diverging trends

[22] For scaling purposes, we added back the mean youth substance use rate across the whole sample to each adjusted substance use rate (adjusted for state fixed effects). We also hold *y*-axis fixed for ease of comparison.

appear to point out the positive impacts of e-cigarette MLSA laws on youth smoking and drinking, many confounding factors have yet to be adjusted in Figure 1. In the analyses that follow, we take care to account for a multitude of confounders (vector $\mathbf{Z}$), and, in alternate specifications, add state-specific linear time trends (denoted by $\boldsymbol{\gamma}_s t$) or state-specific pre-policy linear trends ($\boldsymbol{\gamma}_s t_{Pre}$)[23] to allow for systematically different policy trends across the MLSA and non-MLSA states and adjust for the less than perfect nature of the natural experiment.

We further extend the baseline specification in (1) in several ways to address some other issues. First, to examine the dynamic impacts of the policy on youth substance use behaviors and alternatively assess the "parallel trends" assumption between the MLSA and the non-MLSA states after conditioning on covariates, we transform the specification in (1) into a fully-specified event study design. In particular, we decompose $\mathrm{MLSA}_{\mathrm{s,t}}$ in (1) into a series of policy "leads", or "placebo" laws, and policy lags, which takes the form:

(2) $$P\left(DV_{i,s,t} > 0\right) = \sum_i \Gamma \mathbf{X}_{\mathrm{i,s,t}} + \boldsymbol{\alpha_1}\mathrm{MLSA}_{\mathrm{s,-2}} + \boldsymbol{\alpha_2}\mathrm{MLSA}_{\mathrm{s,0}} + \boldsymbol{\alpha_3}\mathrm{MLSA}_{\mathrm{s,1}} + \mathbf{b_2}\mathbf{Z}_{s,t} + \boldsymbol{\gamma}_s + \boldsymbol{\lambda}_t\ [+\boldsymbol{\gamma}_s t] + \varepsilon_{i,s,t},$$

where all variables except MLSA are defined in the same way as in (1). For the full event of MLSA, our reference (control) group indicates that the laws will not be turned on in another survey year.[24] The parameter $\boldsymbol{\alpha_2}$ captures the contemporaneous policy effect on teen substance use and $\boldsymbol{\alpha_3}$ captures the lagged policy effect one or more survey years after the law's implementation. Hence, $\boldsymbol{\alpha_1}$ provides evidence of parallel or differential pre-policy trends. If this coefficient is statistically distinguishable from zero, it would suggest that the treatment and control states had differential trends prior to policy adoption, which may undermine the interpretation of the DD effect as causal. Explicitly controlling for the lead effects as in the event study design can also help to partly net out any non-parallel trends.

Next, we assess transitions into/out of smoking once youth are no longer subject to the e-cigarette purchase restrictions. Specifically, we estimate the inter-temporal relation

[23] State-specific pre-trends are created by subtracting survey year from the year MLSA switched on. We use only the negative values and set all the positive values to zero. We convert all the negative values to positive by multiplying -1.

[24] We use survey year instead of the calendar year to define event time in order to respect the biennial structure of the YRBSS data. Our results are robust to using the calendar year in defining event time.

associated with how being exposed to an e-cigarette MLSA law when the youth were underage affects their smoking behaviors once they have aged out and are able to purchase e-cigarettes. We do so by restricting the sample to those who are currently 18 or older and thus not subject to the e-cigarette MLSA law, and then estimate the following specification:

$$(3)\quad P\left(Smk_{i,s,t} > 0\right) = \sum_i \Gamma \mathbf{X}_{\mathrm{i,s,t}} + \mathbf{b_1}\mathrm{MLSA\_Minor} + \mathbf{b_2}\mathbf{Z}_{s,t} + \boldsymbol{\gamma}_s + \boldsymbol{\lambda}_t\ [+\boldsymbol{\gamma}_s t] + \varepsilon_{i,s,t}$$

Here, $\mathrm{MLSA\_Minor}$ is an indicator for whether an e-cigarette MLSA law was effective in the individual's state of residence at any point in time when he was underage.[25] For instance, an e-cigarette MLSA law was effective on January 1st, 2013 in the state of New York. Therefore, a youth aged 18 in 2014 from New York would have been exposed to the law in 2013. Analogously, a youth aged 18 or 19 in 2015 would have also been exposed to the law two years prior. Because age in the YRBSS is top-coded at 18, our strategy might erroneously subsume someone aged 19 or 20 in the treatment group who are in fact not subject to the law in our hypothetical examples. While this may possibly moderate the treatment effects, any attenuation bias is likely to be small.[26] We confirm this by dropping the four states where the age limit of legally purchasing e-cigarettes is set at 19 and find that the results are virtually unchanged. The parameter $\mathbf{b_1}$ captures how youth exposure to the e-cigarette purchase restrictions, at any point in time when he was underage, affects his substance use behavior once he has aged out of the restrictions.

We also build upon the above specifications and assess the margin at which smoking is potentially affected. Specifically, we consider whether, and to what extent, e-cigarette MLSA laws have impacted youth smoking initiation and take-up as well as their impacts on the other sections of the smoking distribution besides the extensive margin focused on above. In alternate specifications, we conduct additional checks to assess heterogeneous responses

[25] For states where no e-cigarette MLSA laws were enacted during the study period, this variable equals zero.

[26] Among current high-school enrollees nationwide between the ages of 12-21, only about 2% are 19, and less than 1% are 20 or 21 (based on the 2015 American Community Survey). Thus, at most 3% of the sample who may be untreated may be erroneously classified as being treated, and this would lead the treatment effect to be understated by at most a factor of 3% (for instance, an estimated treatment effect of 2.9 percentage points when the true treatment effect is 3 percentage points). This attenuation factor assumes that all 19-year olds are untreated, when most of them would have been treated if they lived in a state that had enacted an e-cigarette MLSA law in the past; hence in practice the attenuation bias is likely to be even smaller.

across gender and grade. We also implement a falsification check, assessing effects of the e-cigarette MLSA laws on youth who should not be constrained or affected by the policy.

Lastly, to check whether our estimates are driven by unobserved differential pre-policy trends, we undertake a synthetic control design following Abadie, Diamond, and Hainmueller (2010) to ensure that the treatment (MLSA) and control (non-MLSA) states shared common pre-treatment trends in youth smoking and other substance use outcomes. We then follow the approach developed by Donald and Lang (2007) and described in Bedard and Kuhn (2015) in deriving synthetic DD estimates with multiple treatment assignments and compute standard errors using the Donald and Lang's two-step estimator. Note that this synthetic DD estimates approach has appeared in several other studies (Choi, Dave, and Sabia 2016, Sabia, Swigert, and Young 2017).

## 6. Results

### *A. Effects on Smoking*

Table 2 presents estimates of the effects of e-cigarette MLSA laws on youth smoking participation among the underage adolescents. Panel A reports baseline effects from the difference-in-differences (DD) model specified in Equation (1). Model 1 suggests a significant 1.1 percentage point (pp) increase in smoking participation among youth exposed to an e-cigarette MLSA law, which translates to about 7 percent increase relative to the baseline means for the control states. We introduce state-specific linear pre-policy trends in Model 2 to net out any systematic differential trends in smoking across treatment and control states prior to the enactment of the e-cigarette MLSA restrictions.[27] The effect magnitude remains significant, continuing to suggest about a 1 pp increase in smoking participation. The policy effect is also robust to controlling for a full set of state-specific linear trends in Model 3, allowing the trends to persist both pre- and post-policy enactment. State-specific time trends capture systematic time-varying state heterogeneity and adjust for the potential endogeneity of the e-cigarette

[27] State-specific pre-trends allow only the pre-policy trends to differ and therefore attribute any potential break in trends at $t = 0$ to the policy.

MLSA restrictions. One possible limitation of using state-specific time trends is that it reduces the amount of identifying variation (Neumark, Salas, and Wascher 2014). Furthermore, fitting such state-specific linear trends may exacerbate bias, particularly for sample periods and pre-policy windows where trends in smoking (or other substance use) are far from linear. Wolfers (2006) also cautions against adding state-specific linear trends in timing analyses where the policy is modeled as pre-post implementation since such trends may confound both the state-specific time-varying unobservable as well as any dynamic effects of the policy itself. We therefore exercise care in using state-specific linear trends, though it is notable that adding state-specific linear trends does not dilute the estimated effects. If anything, the point estimates are slightly larger. The stability of estimates bolster the plausibility of our research design.

Panel B decomposes the timing of the DD effects and presents estimates from a formal event study design as specified in Equation (2). In keeping with the biennial sampling scheme of the YRBSS, these models control for indicators for the full year of policy enactment, one or more survey years post-policy enactment, one survey year before enactment (reference category) and two or more survey years before enactment. The results from the event study design underscore three points. First, e-cigarette MLSA laws appear to have a significant "contemporaneous" effect during the full year of enactment, about 1.4 pp on average. Owing to the biennial sampling frame of the YRBSS and data collection typically starting in March of a given year, the enactment year indicator is defined such that it turns on if the policy took effect anytime since March of the previous survey year and February of the current year.[28] This suggests that the policy could be active for over 12 months, picking up some lag in the policy effect but only for up to 2 years. Second, as the lag increases, there is some suggestive evidence that the response to policy becomes larger, on the order of 2-3 pp across all models, though estimates in models with the state pre-policy trends are not significant. This possible compounding of the policy effects over time is consistent with an interactive age response. Smoking participation generally increases with age among adolescents; current smoking

[28] For instance, for respondents interviewed in the 2013 YRBSS, the enactment indicator would equal 1 in 2013 if the state they lived in adopted the policy anytime between March 2011 and February 2013.

participation among 16-year-olds is 10.2% compared to 5.0% among 14-year-olds. Hence, an e-cigarette MLSA law in effect when the adolescent was for instance 14 years of age would be expected to have a stronger "bite" as he ages and becomes more likely to contemplate smoking (or use other forms of tobacco) in the future. Third, the lead effects are small in magnitude and insignificant, providing validation to the research design and confirming that the policy is orthogonal to pre-adoption trends in smoking.

While our conceptual framework is agnostic about the direction of the effects given the potential for cigarette smoking to either substitute or complement e-cigarette use, the pattern of results that we find – suggesting an increase in smoking participation – is ex post validating when contrasted with the breaking trends in youth smoking around the MLSA restrictions. As shown in Figure 1, pre-policy trends suggest a decrease in youth smoking as e-cigarettes entered the market in 2007 and e-cigarette MLSA laws proliferated across states (starting in 2010). Thus, if our models are simply reflecting this decline in smoking as states enacted more e-cigarette MLSA laws, then the DD effects would have suggested (possibly spuriously) a deterrent effect of the laws on youth smoking. However, finding increases in smoking from the policy, despite the declining pre-policy trends, adds confidence that these estimates are not just reflecting the falling smoking rates.

Together, estimates in Table 2 suggest that when faced with e-cigarette MLSA laws, underage youth are more likely to turn to cigarette smoking. This may prima facie seem counter-intuitive since they are also restricted from purchasing cigarettes; hence, it would appear that underage youth are turning from one restricted substance to another. However, since all youth face purchasing restrictions for cigarettes over the sample period, the implementation of e-cigarette MLSA laws would increase the *relative* costs of accessing e-cigarettes (relative to cigarettes), affecting the demand for these substances. Because cigarettes have been in the market for a long time, most youth who smoke may have found alternative ways to bypass the purchase restrictions and obtain their cigarettes through secondary sources, such as "bumming" or borrowing from a friend or adult (Katzman,

Markowitz, and McGeary 2007, Hansen, Rees, and Sabia 2013).[29] Thus, it is conceivable that these youth are increasing their participation in the secondary cigarette market when purchasing e-cigarettes is prohibited. The secondary market for e-cigarettes, however, may be less well-developed, particularly when recent estimates suggest that only 3.7% of adults vape (Schoenborn and Gindi 2015), thus reducing a source of e-cigarettes for teenagers in secondary markets.[30]

The smoking participation margin among adolescents in Table 2, columns 1-3 combines first-time smoking, smoking experimentation, regular or heavy smoking, and use of multiple tobacco products. Most smokers initiate smoking in their teens, and hence the initiation margin is the most salient for adolescents and also very relevant from a policy stance since it may determine future transitions and paths to nicotine dependence. Models 4-6 in Table 2 specifically look at how exposure to an e-cigarette MLSA law affects smoking initiation. For these analyses, we restrict the sample to youth who have initiated smoking in the given survey year or are non-smokers; thus youth who are current smokers but had initiated smoking habits in the past are excluded. As noted earlier, we define youth as a first-time smoker if his age at the time of interview matches the reported age when he first tried smoking. These results should be interpreted with care since smoking initiation in the YRBSS is likely coupled with recall errors in the reported age at which smoking was initiated as well as the mismatch between age and survey year.[31] These estimates nevertheless suggest that exposure to an e-cigarette MLSA law significantly increases the probability of initiating smoking, on the order of 0.7 pp. The event study design in Panel B also suggest similar magnitudes during the full year of enactment (0.7 pp, capturing significant effects within 12 months of enactment and possibly up

[29] A dollar increase in cigarette taxes is estimated to decrease the probability of youth getting cigarettes through a secondary market by 5 or 6 percent, but cigarette taxes had little impact on youth obtaining cigarettes through borrowing or taking from a store or family member. This may suggest that they have alternative ways to bypass the rising costs of cigarettes.

[30] See https://www.cdc.gov/nchs/products/databriefs/db217.htm. Furthermore, while it may be relatively easier for a youth to borrow or "bum" a combustible cigarette from a friend or adult, which by definition is disposed after use, the long-lasting properties of e-cigarettes (e.g. even one disposable e-cigarette can last up to 400 puffs or equivalent to one pack of cigarettes) makes it more difficult to borrow or bum from another user.

[31] For instance, a 15-year-old surveyed in 2013 who reported that they initiated smoking at age 15 would be coded as having initiated smoking in 2013. However, the youth may have initiated smoking in 2012 while still 15 years of age.

to 24 months of enactment, as noted above) and some positive effects thereafter, though these lagged effects are not statistically significant. The magnitudes for smoking initiation represent a little over half of the smoking participation effect identified in models 1-3. Thus, the caveats regarding measurement error notwithstanding, which is likely to bias the initiation effect downward, it appears that some of the positive effects of e-cigarette MLSA laws on smoking participation among underage youth may reflect an increase in smoking initiation and remainder reflects movement across smoking and vaping in former initiates.[32]

In Table 3, we assess the distributional effects of e-cigarette MLSA laws on youth smoking, but, in the interest of space, we focus on the policy effects on the upper tail of the smoking distribution.[33] Following Pesko, Hughes, and Faisal (2016), we define youth as a regular smoker if he smoked cigarettes 20 or more days in the past month and a heavy smoker if he smoked cigarettes every day. Table 3, mirroring Table 2, reports estimates using the specification in (1) (Panel A) and estimates using a fully adjusted event-study design (Panel B). Turning to Panel A, we find that youth exposed to the e-cigarette MLSA restrictions are 0.8 pp, or 18 percent relative to the baseline means, more likely to be regular and heavy smokers. In Panel B, we find that the law's impact continues to be larger in the lagged period than the "contemporaneous" period. While these results are not statistically significant across model specifications, they are economically significant in magnitude. In earlier analyses (Panel A, columns 1-3 of Table 2), we find that e-cigarette MLSA laws increased youth smoking participation by about 7 percent and; in comparison, results here suggest that youth increased regular or heavy cigarettes smoking by 18%, suggesting greater effects along the "intensive" margin when exposed to the e-cigarette MLSA laws.

In Table 4, we evaluate whether the increase in smoking persists after youth are no longer constrained by the e-cigarette purchasing restrictions. Thus, we estimate specification (3) for youth, 18 and above, who have aged out of the e-cigarette MLSA laws. Since age in the

[32] It should be noted that adolescents aged 14-17 who are current smokers are likely to have initiated very recently; hence, any change in the smoking margin for this age group may still reflect initiation, experimentation, and trying out different substances.

[33] Results for the MLSA treatment effects on the remaining part of the distribution are very similar to what is reported in Table 3 below and are available from the authors upon request.

YRBSS is top-coded as 18 or above and four states (AL, AK, NJ, and UT) set the age for legally purchasing e-cigarettes at 19, our sample may still include a few who are not old enough to buy e-cigarettes. We therefore present models for all states (models 1-3) and after excluding these four states (models 4-6). We discuss here the latter set of models that bypass the potential misclassification, though estimates remain virtually identical whether we include or exclude the states that had set the e-cigarette MLSA at age 19.

There is little evidence from Table 4 to suggest that exposure to an e-cigarette MLSA law when underage is associated with increased smoking behaviors when he has aged out. Hence, we do not find any strong evidence that the increase in smoking persists as youth age out of e-cigarette MLSA laws. These models suggest that any effects on underage smoking, among youth exposed to an e-cigarette MLSA law, fade when they aged out of the law and are able to purchase e-cigarettes legally.[34]

*B. Magnitude of the Smoking Effect*

Our estimates thus far suggest that when faced with e-cigarette MLSA laws, underage youth are more likely to turn to cigarette smoking, at least until they age out of these laws. Results in Table 2 suggest about a 1.3 pp increase in smoking post-policy adoption, which is consistent with findings reported by Friedman (2015) and Pesko et al. (2016).[35] To place this magnitude in context, it should be noted that the DD effect we estimate is an intention-to-treat (ITT) effect since our sample includes youth that do not use e-cigarettes. It is unlikely that e-cigarette MLSA laws would have a direct effect on smoking behaviors, independent of their effect on e-cigarette use. If e-cigarette MLSA laws had no effect on e-cigarette use, we should expect no effects on other substance use behaviors as well.

[34] Most smokers initiate smoking during adolescence, with 16 years of age being the mode among ever-smokers (based on data from the 2013 National Survey on Drug Use and Health). Hence, accumulation of the addictive smoking stock is still relatively low.

[35] Both studies find about a 1 pp increase in smoking among underage youth, based on data up to 2013. Our slightly larger estimate (up to 1.5 pp in some model specifications) reflect two additional years of data (YRBSS spanning up to 2015) in conjunction with some evidence that the lagged policy response are slightly larger over time.

Hence, establishing the first-stage effect of how e-cigarette MLSA laws may have impacted youth e-cigarette use can help frame what the maximal effect should be for spillover responses into smoking (and other substance use) given that these individuals represent the affected group. However, estimating effects on e-cigarette use due to these policies has been a challenge because of data limitations; youth-based surveys, including the YRBSS and the MTF, have only started asking respondents if they use e-cigarettes in 2014 or 2015. Abouk and Adams (2017), for instance, estimate that the e-cigarette MLSA law is associated with a significant 10 pp decline in e-cigarette use among high school seniors in 2014, based on cross-sectional evidence from the 2014 MTF wave.

The YRBSS started fielding questions on e-cigarette use in the latest 2015 wave. For suggestive evidence, we estimate a similar specification to that in (1) for outcomes related to e-cigarette use (ever use and current use) based only on the 2015 YRBSS.[36] Table 5 suggests that, among underage youth, e-cigarette MLSA laws reduced current use by about 1 pp (5% decline relative to the baseline mean of 21% vaping participation), and ever use (as a proxy for initiation) by about 4.3 pp (10% decline relative to the baseline mean of 44% ever vaping). Similar to Abouk and Adams (2017), the effects (not shown) are somewhat larger for older adolescents (11th and 12th graders). We previously found evidence that the laws increased youth smoking by about 1.3 pp, and so we calculate a back-of-the-envelope treatment-on-the-treated (TOT) effect of 0.3 using the law's impact on ever vaping. We use ever vaping for this calculation to better match the longer duration of data available for smoking. In other words, about 3 in every 10 youth may have increased their smoking as they reduced e-cigarette use in response to the e-cigarette MLSA restrictions. These estimates should be interpreted with considerable caution and are meant to be suggestive due to the inherent difficulties in obtaining the first-stage effect of the laws on e-cigarette use with only a single wave of data. Nevertheless, they can prove useful in gauging the credibility of the magnitudes on the second-order effects.

[36] Given the single wave of data, we are not able to control for state fixed effects, and year fixed effects are not necessary. Instead, we include census division fixed effects to account for unobserved heterogeneity at this geographic level. Models are saturated with all other state-level policy controls.

*C. Effects on Drinking and Marijuana Use*

Next we examine whether exposure to e-cigarette MLSA laws has any spillover effects on other substance use behaviors among underage youth. In Table 6, baseline DD estimates and dynamic effects from the event study are presented separately for past month drinking and binge drinking, showing little evidence of any consistent effect on alcohol use. Though Figure 1 suggests that e-cigarette MLSA laws may have increased youth drinking and binge drinking, the estimated policy impacts turn out to be sensitive to model specifications and are never statistically distinguishable from zero. For example, there is some suggestive evidence of a lagged increase in drinking (on the order of about 0.7-1.4 pp, or 2-4% relative to the baseline mean) in the event study specifications (models 2 and 3). But, standard errors are large and we cannot reject the null. Table 7 presents estimates of the effect of e-cigarette MLSA laws on past month marijuana use, and here we do not find any statistically significant effects. We note that the e-cigarette MLSA effects on substances other than tobacco are third-order effects, and so it is unsurprising that they are quite weak. Hence, while our results suggest that restricting the purchase of e-cigarettes among underage youth may have spilled over into higher smoking, we find little evidence of additional substitution into drinking or marijuana use.

*D. Results from Alternative Samples*

Of concern that our DD estimates may not be consistent due to the unbalanced nature of the YRBSS, we re-run specification (1) using a strongly balanced sample (i.e. states without gaps in data collection) shown in Appendix Table 6, and report the estimated policy effects on youth cigarette smoking, alcohol consumption, and marijuana use in Appendix Table 3. It is validating that all our estimates are not sensitive across analysis samples and are highly similar in terms of magnitudes and significance. For instance, we continue to find that the enactment of e-cigarette MLSA laws is associated with a 1-1.7 pp increase in youth smoking participation, a 0.6-0.8 pp increase in smoking initiation, and a 0.8-1.1 pp increase in regular and heavy

smoking. As above, we find little effects of e-cigarette MLSA laws on youth drinking, binge drinking, and marijuana use.

We also re-run the specification in (1) using only the state YRBSS that are representative of the sampled states. The estimated MLSA treatment effects on youth smoking, drinking, and marijuana use (not reported but are available from the authors upon request) are consistent in magnitude with findings from the full sample; however, due to lower sample size statistical power is attenuated somewhat. The conclusion that e-cigarette MLSA laws increase youth smoking behaviors remains unchanged when using only the state YRBSS data.

### *E. Results from the Synthetic Control Method*

As the last set of robustness checks, we undertake a synthetic control design following Abadie, Diamond, and Hainmueller (2010) to ensure that the treatment (MLSA) and control (non-MLSA) states shared common pre-treatment trends in youth smoking and other substance use outcomes. A detailed explanation of synthetic control method (SCM) is outside the scope of this study, but its essence can be viewed as follows: information on youth substance use in a few pre-policy periods coupled with the means of state-level covariates (Table 1) across the entire pre-policy period are utilized to form a "best" linear combination of control states in which e-cigarette MLSA laws have not been implemented over the study period (2005-2015). The algorithm underlying this method assigns weight to each donor (non-MLSA) state so that any pre-treatment differences in outcomes and state-level covariates between the treatment and the synthetically matched state are minimized. Hence, by expressly forcing the e-cigarette MLSA counterfactuals to have more similar pre-treatment trends, SCM raises the likelihood of satisfying the "parallel trends" assumption (Sabia, Swigert, and Young 2017).

Building on this logic, we run SCM on each MLSA state by excluding all the other MLSA states from the estimation sample, and iterate this process for youth cigarette smoking, alcohol consumption, binge drinking, and marijuana use. For each substance use outcome, we then pool the individually created synthetic samples and form one larger SCM-weighted sample, keeping the synthetic weights unchanged. Following the approach developed by Donald and

Lang (2007) and described in Bedard and Kuhn (2015), we derive the synthetic DD estimates with multiple treatment assignments and compute standard errors using Donald and Lang's two-step estimator. In the interest of space, we present three graphs generated using an event-study design analogous to those in Figure 1 but using the synthetically weighted sample.[37]

Figure 2 presents visual analyses for youth smoking, drinking, binge drinking, and marijuana use. Like in Figure 1, we use solid lines to denote MLSA states and dashed lines for their synthetically matched states, with the vertical dashed lines representing the year e-cigarette MLSA laws turned on. Unlike in Figure 1, we perform SCM on an extended sample period (1999-2015) to allow for a better match of the pre-policy trends. All four figures show that the treatment and synthetic control states have overlaid trends in the pre-policy periods, with clear divergence since the policy enactment.

Appendix Table 4 reports the estimates of policy effects using the pooled synthetically weighted sample and inferential statistics based on Donald and Lang's two-step estimator. Similar to the baseline DD estimates, we find a significant increase in smoking among underage youth exposed to the e-cigarette MLSA laws and no effects on the use of other substances. The point estimate of a 1 pp increase in youth smoking remains robust and is similar in magnitude to that from our standard DD models (Panel A, column1 of Table 2). The weight of the evidence across a battery of checks and alternate model specifications, in conjunction with the synthetic control estimates, give us confidence that the policy effects are not confounded by differential pre-policy trends.

*F. Placebo Checks and Heterogeneous Effects*

Given that e-cigarette MLSA laws are by definition binding only for underage youth, this presents a natural falsification test. The policy should have no causal effect on any addictive behaviors among youth who have aged out and were not exposed to the policy while underage.

[37] The resulting trends for youth cigarette smoking, alcohol consumption, and marijuana use in each MLSA state and its synthetically matched states are available from the authors upon request.

That is, even if a state enacted an e-cigarette MLSA law of 18 in 2010, youth aged 18 or older in 2010 (19 years of age or older in 2011; etc.) should not be affected since they were never exposed to the restriction even when they were underage. Table 8 carries out this falsification test for each specification and substance use outcome, defining the sample as youth that have aged out of e-cigarette MLSA laws and were never exposed while underage. All estimates, most notably for smoking participation, which earlier models suggested a significant effect among affected underage youth, are statistically insignificant and generally small in magnitude relative to the baseline means.

In Tables 9 and 10, we assess whether the response in smoking behaviors is different across gender and grade in school. Models 1-3 in Table 9 present estimates of being exposed to an e-cigarette MLSA law on smoking participation among underage boys, and Models 4-6 present estimates for underage girls. We find that most of the positive effects on smoking is being driven by boys; specifically, these models suggest a significant 1.3 to 2 pp increase in smoking participation among boys who are exposed to an e-cigarette MLSA law, which translates to about a 10% increase relative to their baseline means. Use of e-cigarettes and conventional cigarettes is significantly lower among adolescent girls relative to boys, and hence it is not altogether surprising that the policy effects are substantially larger among boys. Models in Table 10 present differential effects across 9th and 10th graders vs. 11th and 12th graders. We generally find significant and positive effects of the e-cigarette MLSA restrictions on smoking participation for both groups. The event study analyses with state trends are somewhat suggestive of a slightly larger response among the older adolescents relative to the younger adolescents. However, standard errors are relatively large, and we are not able to reject the null that these effects are similar across both groups.

## 7. Conclusion:

Economic theory suggests that e-cigarette MLSA laws may reduce e-cigarette use, and we find suggestive evidence of this using a single cross-section of data. Using MTF data, Abouk and Adams (2017) reached a similar conclusion. We also find strong evidence that e-cigarette

MLSA laws increased the probability of youth smoking conventional cigarettes by approximately 1.1 pp (7% relative to the mean smoking rates). In particular, youth who have not smoked in the past but initiated their first cigarettes due to the e-cigarette MLSA restrictions may have contributed to a little over half of the increase in smoking participation. Our estimates of the policy effects on youth smoking are slightly larger than those of Friedman (2015) and Pesko et al. (2016), who both found that the laws increased smoking participation by roughly 0.9 pp. Our slightly larger estimates reflect two additional waves of data in conjunction with some evidence of a stronger lagged policy response. However, our finding that e-cigarette MLSA laws increased cigarette use contrasts from findings by Abouk and Adams (2017) who suggested that the laws decreased smoking among underage seniors (but not among other underage youth). Given that both our study and Abouk and Adams (2017) use individual-level data, this alone does not appear to account for the differences in results. Our study employs one additional year of data and utilizes data from the pooled YRBSS which yields a sample size approximately 14 times that of the MTF sample employed by Abouk and Adams (2017). Restricting our analyses to the same periods as their study does not alter our results or conclusions. Hence, it is possible that differences between the MTF and the YRBSS sampling schemes and their respective sample sizes may underlie some of the differences in our results. While it has been argued that the YRBSS may be more representative at the state level (Carpenter and Cook 2008), and hence may provide more stable estimates of changes in smoking within states over time, further research exploring these differences is warranted.

Our models also suggest that the increase in youth smoking caused by e-cigarette MLSA laws appears to fade once youth age out of the law. Additionally, we do not find any evidence that the laws affect the use of other addictive substances such as alcohol or marijuana use.

While federal regulations require all states to have a cigarette MLSA law of at least 18, some states have made the age limit for purchasing both cigarettes and e-cigarettes higher. As of the 1st quarter of 2018, three states had an MLSA law of 19 and five states (California, D.C., Hawaii, New Jersey, and Oregon) had MLSA laws of 21. Our results suggest some caution in raising MLSA laws for both cigarettes and e-cigarettes to 21. It may be preferable to raise cigarette MLSA laws to 21, but maintain e-cigarette MLSA laws at 18 to encourage youth to quit

smoking using e-cigarettes. Preventing youth from legally buying e-cigarettes until age 21 may harden preferences for cigarettes and make quitting at that age more difficult.

In sum, it is unclear from our results if e-cigarette MLSA laws have a positive impact on public health. It appears that some portion of the decrease in e-cigarette use, about 30% based on crude TOT estimates, may come at the cost of higher conventional cigarette use, at least in the short-term until the youth has aged out of the restrictions. If e-cigarettes are only 5% as harmful as traditional cigarettes (Tobacco Advisory Group of the Royal College of Physicians 2016), then e-cigarette MLSA laws leading to increased smoking may cause greater harm than benefits. However, such net costs need to be balanced against other considerations such as the potential use of e-cigarettes for smoking cessation among older youth and among longer-term smokers.

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Table 1 — Summary Statistics of Key Response Variables, Individual Demographic Characteristics, and State-level Policy Controls

| | Full Sample | Youth younger than 18 | Youth 18 or older | States **with** E-Cig MLSA Laws; Pre-policy periods | States **without** E-Cig MLSA Laws; Pre-policy periods |
|---|---|---|---|---|---|
| ***Youth substance use*** | | | | | |
| Current smoker | 0.17 [0.37] | 0.15 [0.36] | 0.25 [0.43] | 0.18 [0.39] | 0.20 [0.41] |
| Current drinker | 0.39 [0.49] | 0.36 [0.48] | 0.51 [0.50] | 0.41 [0.49] | 0.43 [0.50] |
| Current binge drinker | 0.23 [0.42] | 0.22 [0.41] | 0.35 [0.48] | 0.25 [0.43] | 0.28 [0.45] |
| Current marijuana user | 0.20 [0.40] | 0.19 [0.39] | 0.26 [0.44] | 0.20 [0.40] | 0.20 [0.40] |
| ***Youth demographic characteristics*** | | | | | |
| Female | 0.51 [0.50] | 0.52 [0.50] | 0.46 [0.50] | 0.51 [0.50] | 0.51 [0.50] |
| White | 0.56 [0.50] | 0.56 [0.50] | 0.56 [0.60] | 0.56 [0.50] | 0.58 [0.49] |
| Black | 0.14 [0.35] | 0.14 [0.35] | 0.15 [0.36] | 0.16 [0.37] | 0.08 [0.26] |
| Hispanics | 0.16 [0.37] | 0.16 [0.37] | 0.16 [0.36] | 0.16 [0.37] | 0.18 [0.39] |
| Other races | 0.14 [0.34] | 0.14 [0.34] | 0.13 [0.33] | 0.11 [0.32] | 0.16 [0.37] |
| 9th grade | 0.28 [0.45] | 0.31 [0.46] | 0.01 [0.08] | 0.28 [0.45] | 0.28 [0.45] |
| 10th grade | 0.27 [0.44] | 0.30 [0.46] | 0.01 [0.11] | 0.27 [0.44] | 0.27 [0.44] |
| 11th grade | 0.25 [0.43] | 0.27 [0.44] | 0.10 [0.30] | 0.25 [0.43] | 0.24 [0.43] |
| 12th grade | 0.21 [0.41] | 0.12 [0.33] | 0.88 [0.32] | 0.20 [0.40] | 0.21 [0.41] |
| ***Merged state-level covariates*** | | | | | |
| E-cigarette MLSA Laws | 0.24 [0.43] | 0.27 [0.44] | 0.01 [0.11] | 0.00 [0.00] | 0.00 [0.00] |
| Real cigarette taxes | 2.35 [1.18] | 2.37 [1.19] | 2.21 [1.11] | 2.00 [1.20] | 2.22 [0.72] |
| Comprehensive smoke-free air laws | 0.41 [0.49] | 0.42 [0.49] | 0.34 [0.47] | 0.34 [0.47] | 0.24 [0.43] |
| Bans on e-cigarette use in private work places | 0.01 [0.09] | 0.01 [0.09] | 0.01 [0.10] | 0.00 [0.00] | 0.00 [0.00] |
| Real beer taxes | 0.27 [0.24] | 0.27 [0.24] | 0.29 [0.25] | 0.32 [0.27] | 0.24 [0.13] |
| Zero-tolerance law | 0.41 [0.49] | 0.41 [0.49] | 0.41 [0.49] | 0.22 [0.41] | 0.77 [0.42] |

| | | | | | |
|---|---|---|---|---|---|
| Underage drinking: No possession of alcohol | 0.15 [0.36] | 0.16 [0.37] | 0.10 [0.29] | 0.05 [0.23] | 0.10 [0.30] |
| Underage drinking: No consumption of alcohol | 0.08 [0.27] | 0.08 [0.27] | 0.07 [0.25] | 0.11 [0.31] | 0.02 [0.12] |
| Underage drinking: No internal consumption of alcohol | 0.18 [0.38] | 0.18 [0.39] | 0.13 [0.34] | 0.12 [0.32] | 0.00 [0.00] |
| Underage drinking: No purchase of alcohol | 0.21 [0.41] | 0.21 [0.40] | 0.25 [0.43] | 0.31 [0.46] | 0.09 [0.28] |
| Underage drinking: Suspense or revoke driving privileges | 0.36 [0.48] | 0.36 [0.48] | 0.33 [0.47] | 0.35 [0.48] | 0.13 [0.33] |
| Underage drinking: Against underage drinking party | 0.14 [0.34] | 0.13 [0.34] | 0.15 [0.36] | 0.13 [0.34] | 0.11 [0.31] |
| Underage drinking: Keg registration law | 0.21 [0.40] | 0.21 [0.41] | 0.20 [0.40] | 0.21 [0.41] | 0.12 [0.32] |
| Medical Marijuana Laws | 0.26 [0.44] | 0.26 [0.44] | 0.22 [0.41] | 0.09 [0.28] | 0.26 [0.44] |
| Medical Marijuana Laws: home cultivation | 0.12 [0.33] | 0.12 [0.33] | 0.13 [0.33] | 0.07 [0.25] | 0.16 [0.36] |
| Medical Marijuana Laws: legal dispensary | 0.12 [0.33] | 0.12 [0.33] | 0.13 [0.33] | 0.06 [0.25] | 0.10 [0.30] |
| Medical marijuana Laws: non-specific pains | 0.19 [0.40] | 0.20 [0.40] | 0.16 [0.37] | 0.08 [0.27] | 0.17 [0.38] |
| Medical Marijuana Laws: registry | 0.15 [0.36] | 0.15 [0.36] | 0.11 [0.31] | 0.03 [0.18] | 0.11 [0.32] |
| Marijuana decriminalization law | 0.37 [0.48] | 0.38 [0.49] | 0.31 [0.46] | 0.40 [0.49] | 0.10 [0.30] |
| State unemployment rates | 6.66 [2.00] | 6.65 [1.99] | 6.80 [2.06] | 6.82 [2.20] | 6.78 [2.15] |
| Natural logarithm of state per capita personal income | 10.65 [0.18] | 10.66 [0.18] | 10.62 [0.17] | 10.59 [0.15] | 10.53 [0.12] |

Notes: Means and standard deviation (in bracket) are reported. The statistics are weighted by the total underage population at the state by year level obtained from the National Cancer Institute's Surveillance Epidemiology and End Results Program.
Definitions of youth substance use are defined in the text.
E-cigarette MLSA laws and cigarette excise taxes come from CDC State Tobacco Activities Tracking and Evaluation System.
State-level policies related to underage drinking come from Alcohol Policy Information System.
State unemployment rates and per capita personal income come from Bureau of Labor Statistics.
Comprehensive smoke-free air laws consist of four venues: government and private workplaces, restaurants, and bars.
Cigarette and beer taxes are inflation-adjusted to 2015 dollars using CPI-U.

Table 2 — E-cigarette MLSA Law and Youth Smoking
National and State YRBSS: 2005-2015

| | *DV: Youth is a current smoker* | | | *DV: Youth is a first-time smoker* | | |
|---|---|---|---|---|---|---|
| **Panel A** | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA Law | 0.011*** | 0.012** | 0.015*** | 0.007** | 0.007* | 0.007** |
| | (0.004) | (0.005) | (0.004) | (0.003) | (0.004) | (0.003) |
| **Panel B** | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA ≤2 Waves Pre | -0.006 | -0.004 | -0.006 | -0.002 | -0.000 | -0.001 |
| | (0.006) | (0.008) | (0.004) | (0.003) | (0.005) | (0.003) |
| E-cigarette MLSA 1 Wave Pre (Ref.) | – | – | – | – | – | – |
| E-cigarette MLSA Wave of Enactment | 0.010*** | 0.015 | 0.018*** | 0.007** | 0.007* | 0.008** |
| | (0.004) | (0.009) | (0.005) | (0.003) | (0.004) | (0.003) |
| E-cigarette MLSA ≥1 Wave Post | 0.020*** | 0.026 | 0.022** | 0.006 | 0.005 | 0.009 |
| | (0.005) | (0.016) | (0.011) | (0.005) | (0.014) | (0.014) |
| Full Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State-specific linear pre-trends | | ✓ | | | ✓ | |
| State-specific linear trends | | | ✓ | | | ✓ |
| Mean of dep. var. in the control states | 0.15 | 0.15 | 0.15 | 0.09 | 0.09 | 0.09 |
| Observations | 752,332 | 752,332 | 752,332 | 551,232 | 551,232 | 551,232 |

Notes: Standard errors, clustered at the state level, are shown in parentheses.
* p < 0.10, ** p < 0.05, *** p < 0.01
All models include dummy variables for gender, race, age, and grade levels. State-level covariates listed in Table 1 are included.
In columns 1-3, we define youth as current smokers if any days of smoking over the past month are reported. The analysis sample there is restricted to youth younger than 18.
In columns 4-6, we define youth as first-time smokers if their age at the time of the survey matches the age of first-time smoking.
Youth who never smoke a cigarette are coded zero and youth who initiated smoking prior to the survey are excluded.
Youth younger than 13 or older than 17 are dropped as they cannot be first-time smokers when exposed to an e-cigarette MLSA law.
E-cigarette MLSA law, the leads, and the lags are defined in the text. One wave means one survey year.

Table 3 — E-cigarette MLSA Law and Youth Smoking at Different Margins
National and State YRBSS: 2005-2015

| | *DV: Youth is a regular smoker* | | | *DV: Youth is a heavy smoker* | | |
|---|---|---|---|---|---|---|
| **Panel A** | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA Law | 0.007** | 0.010** | 0.008** | 0.008*** | 0.010** | 0.009*** |
| | (0.003) | (0.005) | (0.003) | (0.003) | (0.004) | (0.003) |
| **Panel B** | 4 | 5 | 6 | 4 | 5 | 6 |
| E-cigarette MLSA ≤2 Waves Pre | -0.005 | -0.005 | -0.004 | -0.003 | -0.003 | -0.002 |
| | (0.003) | (0.006) | (0.003) | (0.002) | (0.004) | (0.003) |
| E-cigarette MLSA 1 Wave Pre (Ref.) | – | – | – | – | – | – |
| E-cigarette MLSA Wave of Enactment | 0.007** | 0.012* | 0.010** | 0.007*** | 0.011** | 0.010*** |
| | (0.003) | (0.006) | (0.005) | (0.002) | (0.005) | (0.004) |
| E-cigarette MLSA ≥1 Wave Post | 0.025*** | 0.011 | 0.019 | 0.022*** | 0.010 | 0.016* |
| | (0.004) | (0.009) | (0.012) | (0.003) | (0.007) | (0.010) |
| Full Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State-specific linear pre-trends | | ✓ | | | ✓ | |
| State-specific linear trends | | | ✓ | | | ✓ |
| Mean of dep. var. in the control states | 0.05 | 0.05 | 0.05 | 0.04 | 0.04 | 0.04 |
| Observations | 752,332 | 752,332 | 752,332 | 752,332 | 752,332 | 752,332 |

Notes: Standard errors, clustered at the state level, are shown in parentheses.
* p < 0.10, ** p < 0.05, *** p < 0.01
All models include dummy variables for gender, race, age, and grade levels. State-level covariates listed in Table 1 are included.
In columns 1-3, we define youth as regular smokers if they smoked cigarettes at least 20 days over the past month.
In columns 4-6, we define youth as heavy smokers if they smoked cigarettes every day over the past month.
The analysis sample is restricted to youth younger than 18.
E-cigarette MLSA law, the leads, and the lags are defined in the text. One wave means one survey year.

Table 4 — The Intertemporal Relationship Between E-cigarette MLSA Law and Youth Smoking
National and State YRBSS: 2005-2015

| ***DV: Youth is a current smoker*** | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| Exposed to E-cigarette MLSA Law While Underage | 0.002 | 0.005 | 0.010 | 0.001 | 0.006 | 0.009 |
| | (0.013) | (0.015) | (0.014) | (0.014) | (0.016) | (0.015) |
| Full Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State-specific linear pre-trends | | ✓ | | | ✓ | |
| State-specific linear trends | | | ✓ | | | ✓ |
| Mean of dep. var. in the control states | 0.24 | 0.24 | 0.24 | 0.24 | 0.24 | 0.24 |
| Observations | 93,716 | 93,716 | 93,716 | 93,716 | 93,716 | 93,716 |

Notes: Standard errors, clustered at the state level, are shown in parentheses.
All models include dummy variables for gender, race, age, and grade levels. State-level covariates listed in Table 1 are included.
The analysis sample is restricted to youth aged 18 or above.
In columns 1-3, we include Alabama, Alaska, New Jersey, and Utah, where the age limits of purchasing e-cigarettes are set at 19.
In columns 4-6, we exclude Alabama, Alaska, New Jersey, and Utah.
The definition of the key regressor, "Exposed to E-cigarette MLSA Law While Underage," is in the text.

Table 5 — E-cigarette MLSA Law and Youth E-cigarette Use
National and State YRBSS: 2015

| | Ever Used E-cigarettes | Current Vapor |
|---|---|---|
| E-cigarette MLSA Law | -0.043*** | -0.009 |
| | (0.015) | (0.015) |
| Mean of dep. var. in the control states | 0.44 | 0.21 |
| Full controls | Yes | Yes |
| Census Division FEs | Yes | Yes |
| Observations | 145,950 | 178,444 |

Notes: Standard errors, clustered at the state level, are shown in parentheses.
*** $p < 0.01$
Both models include dummy variables for gender, race, age, and grade levels.
State-level covariates listed in Table 1 are also included.
Youth aged 18 or above are excluded.
E-cigarette MLSA law is defined in the text.
We define youth as a current vapor if any day of e-cigarette use is reported in the past month.

Table 6 — E-cigarette MLSA Law and Youth Alcohol Use
National and State YRBSS: 2005-2015

| | *DV: Youth is a current drinker* | | | *DV: Youth is a current binge drinker* | | |
|---|---|---|---|---|---|---|
| **Panel A** | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA Law | -0.001 | 0.005 | 0.010 | 0.001 | -0.002 | 0.000 |
| | (0.008) | (0.009) | (0.009) | (0.006) | (0.005) | (0.005) |
| **Panel B** | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA ≤2 Waves Pre | -0.003 | -0.007 | -0.008 | -0.002 | 0.004 | 0.001 |
| | (0.006) | (0.009) | (0.007) | (0.005) | (0.009) | (0.005) |
| E-cigarette MLSA 1 Wave Pre (Ref.) | – | – | – | – | – | – |
| E-cigarette MLSA Wave of Enactment | -0.001 | 0.011 | 0.013 | 0.001 | -0.004 | -0.002 |
| | (0.008) | (0.011) | (0.008) | (0.006) | (0.009) | (0.006) |
| E-cigarette MLSA ≥1 Wave Post | -0.010 | 0.007 | 0.014 | 0.009 | -0.014 | -0.009 |
| | (0.009) | (0.013) | (0.014) | (0.008) | (0.010) | (0.012) |
| Full Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State-specific linear pre-trends | | ✓ | | | ✓ | |
| State-specific linear trends | | | ✓ | | | ✓ |
| Mean of dep. var. in the control states | 0.35 | 0.35 | 0.35 | 0.21 | 0.21 | 0.21 |
| Observations | 711,220 | 711,220 | 711,220 | 711,220 | 711,220 | 711,220 |

Notes: Standard errors, clustered at the state level, are shown in parentheses.
All models include dummy variables for gender, race, age, and grade levels. State-level covariates listed in Table 1 are included.
In columns 1-3, we define youth as current drinker if any days of drinking over the past month are reported.
In columns 4-6, we define youth as current binge drinker if any days of binge drinking (drank 5 or more drinks of alcohol in a row within a couple of hours) over the past month are reported.
The analysis sample is restricted to youth younger than 18.
E-cigarette MLSA Law, the leads, and the lags are defined in the text. One wave means one survey year.

Table 7 – E-cigarette MLSA Law and Youth Marijuana Use
National and State YRBSS: 2005-2015

| **Panel A** | | | |
|---|---|---|---|
| ***DV: Youth is a current marijuana user*** | 1 | 2 | 3 |
| E-cigarette MLSA Law | -0.000 | -0.007 | -0.002 |
| | (0.008) | (0.009) | (0.009) |
| **Panel B** | | | |
| ***DV: Youth is a current marijuana user*** | 1 | 2 | 3 |
| E-cigarette MLSA ≤2 Waves Pre | -0.010 | -0.006 | -0.012 |
| | (0.006) | (0.010) | (0.009) |
| E-cigarette MLSA 1 Wave Pre (Ref.) | – | – | – |
| E-cigarette MLSA Wave of Enactment | -0.001 | -0.004 | 0.002 |
| | (0.006) | (0.012) | (0.010) |
| E-cigarette MLSA ≥1 Wave Post | 0.008 | 0.017 | 0.015 |
| | (0.007) | (0.011) | (0.017) |
| Full Controls | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ |
| State-specific linear pre-trends | | ✓ | |
| State-specific linear trends | | | ✓ |
| Mean of dep. var. in the control states | 0.20 | 0.20 | 0.20 |
| Observations | 760,063 | 760,063 | 760,063 |

Notes: Standard errors, clustered at the state level, are shown in parentheses.
All models include dummy variables for gender, race, age, and grade levels. State-level covariates listed in Table 1 are included.
We define youth as current marijuana users if any days of marijuana use over the past month are reported.
The analysis sample is restricted to youth younger than 18.
E-cigarette MLSA Law is defined in the text.
One wave means one survey year.

Table 8 — Falsification Tests
National and State YRBSS: 2005-2015

| **Panel A** | | | |
|---|---|---|---|
| ***DV: Youth is a current smoker*** | 1 | 2 | 3 |
| E-cigarette MLSA Law | 0.012 | -0.006 | -0.001 |
| | (0.013) | (0.011) | (0.014) |
| Mean of dep. var. in the control states | 0.23 | 0.23 | 0.23 |
| *N* | 93,716 | 93,716 | 93,716 |
| **Panel B** | | | |
| ***DV: Youth is a current drinker*** | 1 | 2 | 3 |
| E-cigarette MLSA Law | -0.008 | -0.011 | -0.013 |
| | (0.018) | (0.024) | (0.019) |
| Mean of dep. var. in the control states | 0.47 | 0.47 | 0.47 |
| *N* | 88,992 | 88,992 | 88,992 |
| **Panel C** | | | |
| ***DV: Youth is a current binge drinker*** | 1 | 2 | 3 |
| E-cigarette MLSA Law | -0.009 | 0.003 | -0.002 |
| | (0.014) | (0.012) | (0.009) |
| Mean of dep. var. in the control states | 0.31 | 0.31 | 0.31 |
| *N* | 88,992 | 88,992 | 88,992 |
| **Panel D** | | | |
| ***DV: Youth is a current marijuana user*** | 1 | 2 | 3 |
| E-cigarette MLSA Law | -0.006 | -0.019 | -0.009 |
| | (0.015) | (0.013) | (0.015) |
| Mean of dep. var. in the control states | 0.26 | 0.26 | 0.26 |
| *N* | 95,906 | 95,906 | 95,906 |
| Full Controls | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ |
| State-specific linear pre-trends | | ✓ | |
| State-specific linear trends | | | ✓ |

Notes: Standard errors, clustered at the state level, are shown in parentheses.
All models include dummy variables for gender, race, age, and grade levels. State-level covariates listed in Table 1 are included.
Definitions of current smokers, drinkers, binge drinkers, and marijuana users are in the text.
E-cigarette MLSA Law is defined in the text.
Sample is restricted to youth who have aged out and were not exposed to the e-cigarette MLSA laws while underage.

Table 9 — E-cigarette MLSA Law and Youth Smoking; Stratified by Gender
National and State YRBSS: 2005-2015

| ***DV: Youth is a current smoker*** | Boys | | | Girls | | |
|---|---|---|---|---|---|---|
| **Panel A** | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA Law | 0.013** | 0.016** | 0.019*** | 0.006 | 0.008 | 0.012** |
| | (0.006) | (0.006) | (0.005) | (0.005) | (0.005) | (0.005) |
| **Panel B** | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA ≤2 Waves Pre | -0.001 | -0.003 | -0.006 | -0.003 | -0.005 | -0.007 |
| | (0.005) | (0.007) | (0.005) | (0.004) | (0.005) | (0.004) |
| E-cigarette MLSA 1 Wave Pre (Ref.) | – | – | – | – | – | – |
| E-cigarette MLSA Wave of Enactment | 0.013** | 0.019** | 0.023*** | 0.006 | 0.011* | 0.014*** |
| | (0.006) | (0.008) | (0.007) | (0.005) | (0.007) | (0.005) |
| E-cigarette MLSA ≥1 Wave Post | 0.027** | 0.045*** | 0.032* | 0.018*** | 0.018* | 0.013 |
| | (0.011) | (0.016) | (0.017) | (0.006) | (0.010) | (0.010) |
| Full Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State-specific linear pre-trends | | ✓ | | | ✓ | |
| State-specific linear trends | | | ✓ | | | ✓ |
| Mean of dep. var. in the control states | 0.16 | 0.16 | 0.16 | 0.14 | 0.14 | 0.14 |
| Observations | 359,044 | 359,044 | 359,044 | 393,288 | 393,288 | 393,288 |

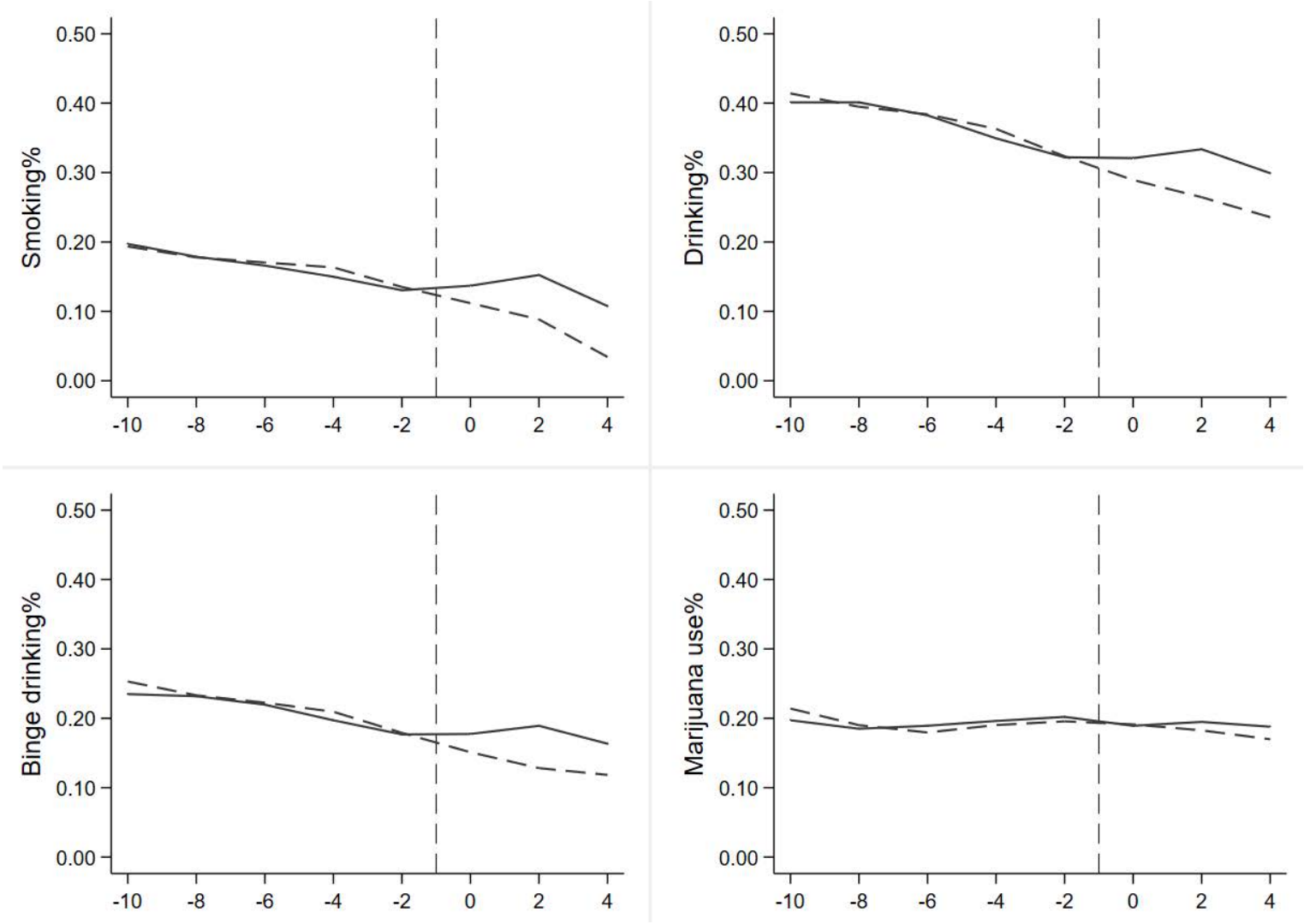
Notes: Standard errors, clustered at the state level, are shown in parentheses.
* p < 0.10, ** p < 0.05, *** p < 0.01
All models include dummy variables for race, age, and grade levels. State-level covariates listed in Table 1 are included.
We define youth as current smokers if any days of smoking over the past month are reported. The analysis sample is restricted to youth younger than 18.
E-cigarette MLSA Law, the leads, and the lags are defined in the text. One wave means one survey year.

Table 10 — E-cigarette MLSA Law and Youth Smoking; Stratified by Grade
National and State YRBSS: 2005-2015

| ***DV: Youth is a current smoker*** | 9 & 10th graders | | | 11 & 12th graders | | |
|---|---|---|---|---|---|---|
| **Panel A** | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA Law | 0.012** | 0.014 | 0.016** | 0.008* | 0.016* | 0.018*** |
| | (0.005) | (0.009) | (0.007) | (0.005) | (0.009) | (0.005) |
| **Panel B** | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA ≤2 Waves Pre | -0.003 | 0.003 | -0.001 | -0.011 | -0.016 | -0.017* |
| | (0.005) | (0.009) | (0.006) | (0.008) | (0.011) | (0.007) |
| E-cigarette MLSA 1 Wave Pre (Ref.) | – | – | – | – | – | – |
| E-cigarette MLSA Wave of Enactment | 0.012** | 0.012 | 0.016** | 0.007* | 0.024** | 0.025*** |
| | (0.005) | (0.012) | (0.008) | (0.004) | (0.011) | (0.008) |
| E-cigarette MLSA ≥1 Wave Post | 0.022*** | 0.021 | 0.016 | 0.018*** | 0.036 | 0.046* |
| | (0.007) | (0.016) | (0.016) | (0.005) | (0.022) | (0.024) |
| Full Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State-specific linear pre-trends | | ✓ | | | ✓ | |
| State-specific linear trends | | | ✓ | | | ✓ |
| Mean of dep. var. in the control states | 0.12 | 0.12 | 0.12 | 0.16 | 0.16 | 0.16 |
| Observations | 461,560 | 461,560 | 461,560 | 290,772 | 290,772 | 290,772 |

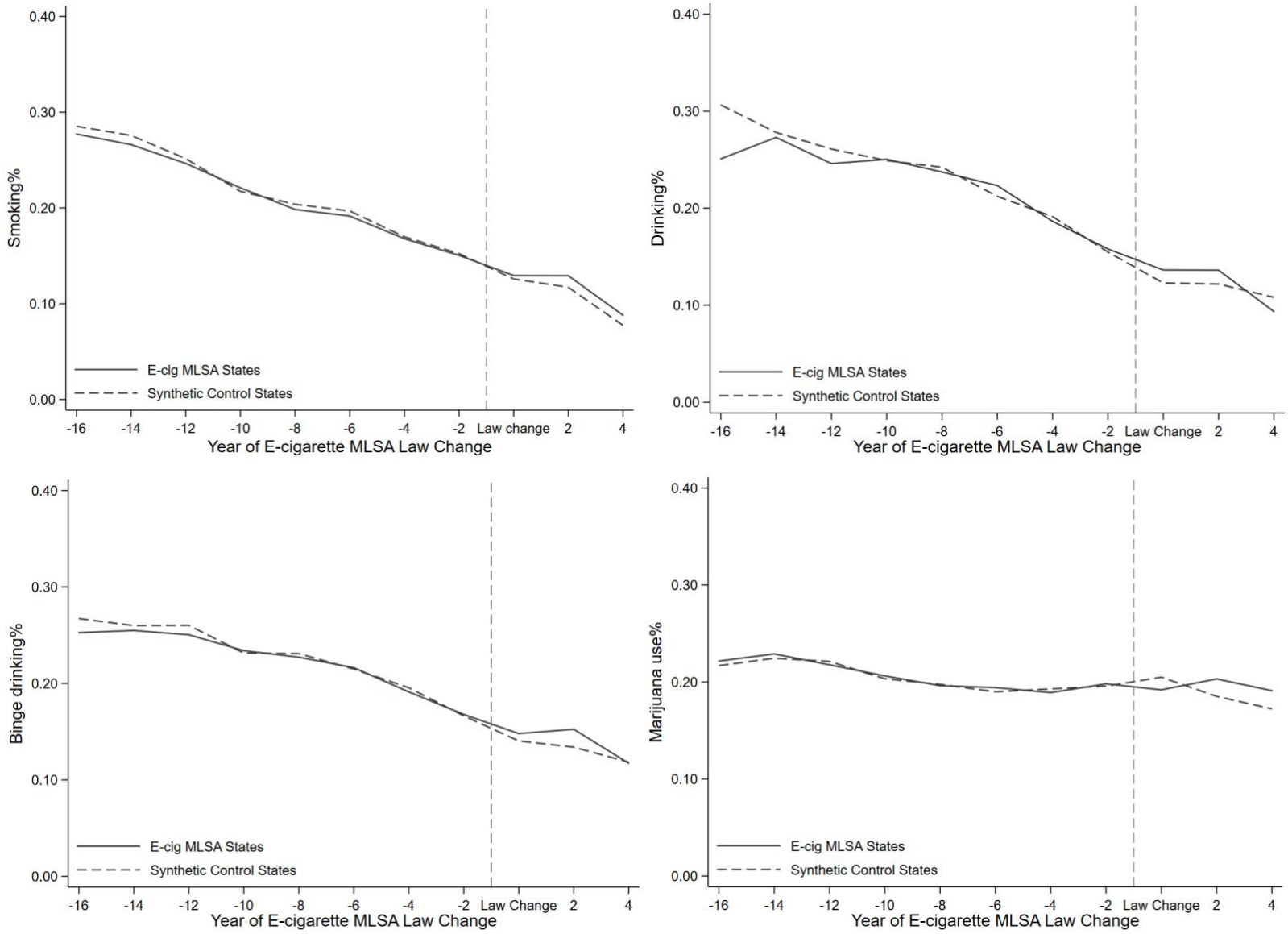
Notes: Standard errors, clustered at the state level, are shown in parentheses.
* p < 0.10, ** p < 0.05, *** p < 0.01
All models include dummy variables for gender, race, age, and grade levels. State-level covariates listed in Table 1 are included.
We define youth as current smokers if any days of smoking is reported in the past month. The analysis sample is restricted to youth younger than 18.
E-cigarette MLSA Law, the leads, and the lags are defined in the text. One wave means one survey year

Figure 1 – Youth Substance Use Rates Between E-cigarette MLSA and Non-MLSA States
National and State YRBSS: 2005-2015




Notes: the *x*-axis indicates the survey year relative to the year e-cigarette MLSA laws turned on, and thus negative values represent periods before the law change and positive values represent periods after the change. Year 0 represents the first year MLSA laws are coded as 1 based on our coding scheme. A randomly selected pseudo-MLSA date drawn from the true distribution of effective dates among the MLSA states is assigned to each non-MLSA state and then normalized to time 0. The graph plots the mean youth substance use rates between the MLSA (solid lines) and non-MLSA states (dashed lines) after netting out the state fixed effects. For scaling purposes, we added the mean youth substance use rate calculated over the entire sample to each adjusted substance use rate (adjusted for state fixed effects only). Sample statistics are weighted by the total underage population.

Figure 2 – Youth Substance Use Rates Between E-cigarette MLSA States and Synthetic Control States




Notes: as in Figure 1, the *x*-axis indicates the survey year relative to the year of e-cigarette MLSA law change. The graph plots the mean youth substance use rates between the MLSA and synthetic control states after netting out the state fixed effects. For scaling purposes, we added the mean substance use rate calculated over the pooled SCM-weighted sample to each adjusted substance use rate (adjusted for state fixed effects). Sample statistics are weighted by the total underage population.

Appendix Table 1 — E-Cigarette Minimum Legal Sale Age Laws, 2005 – 2015

| State | Effective Date | State | Effective Date |
|---|---|---|---|
| Alabama | August 1, 2013 | Montana | January 1, 2016 |
| Alaska | August 22, 2012 | Nebraska | April 9, 2014 |
| Arizona | September 13, 2013 | Nevada | October 1, 2015 |
| Arkansas | August 16, 2013 | New Hampshire | July 31, 2010 |
| California | September 27, 2010 | New Jersey | March 12, 2010 |
| Colorado | March 25, 2011 | New Mexico | June 9, 2015 |
| Connecticut | October 1, 2014 | New York | January 1, 2013 |
| Delaware | June 12, 2014 | North Carolina | August 1, 2013 |
| District of Columbia | October 1, 2015 | North Dakota | August 1, 2015 |
| Florida | July 1, 2014 | Ohio | August 2, 2014 |
| Georgia | July 1, 2014 | Oklahoma | November 1, 2014 |
| Hawaii | June 27, 2013 | Oregon | January 1, 2016 |
| Idaho | July 1, 2012 | Pennsylvania | August 8, 2016 |
| Illinois | January 1, 2014 | Rhode Island | January 1, 2015 |
| Indiana | July 1, 2013 | South Carolina | June 7, 2013 |
| Iowa | July 1, 2014 | South Dakota | July 1, 2014 |
| Kansas | July 1, 2012 | Tennessee | July 1, 2011 |
| Kentucky | April 10, 2014 | Texas | October 1, 2015 |
| Louisiana | May 28, 2014 | Utah | May 11, 2010 |
| Maine | July 4, 2015 | Vermont | July 1, 2013 |
| Maryland | October 1, 2012 | Virginia | July 1, 2014 |
| Massachusetts | September 25, 2015 | Washington | July 28, 2013 |
| Michigan | August 8, 2016 | West Virginia | June 6, 2014 |
| Minnesota | August 1, 2010 | Wisconsin | April 20, 2012 |
| Mississippi | July 1, 2013 | Wyoming | March 13, 2013 |
| Missouri | October 10, 2014 | | |

Notes: By the end of August 2016, all states except Pennsylvania and Michigan have implemented E-Cigarette MLSA Laws.

Appendix Table 2 – Test for the Parallel Trends Assumption
National and State YRBSS: 2005-2015

| | Current Smoker | Current Drinker | Current Binge Drinker | Current Marijuana User |
|---|---|---|---|---|
| Treated × Pre-trends | -0.001 | 0.001 | -0.001 | -0.001 |
| | (0.002) | (0.002) | (0.002) | (0.001) |
| Full Controls | ✓ | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ | ✓ |
| *N* | 459,784 | 436,271 | 436,271 | 467,754 |

Notes: Standard errors, clustered at the state level, are shown in parenthesis.
Pre-trends refer to the time periods before the implementation of e-cigarette MLSA laws, shown on the *x*-axis in Figure 1 as negative values.
We convert these negative values to positive by multiplying -1.
Full controls include dummy variables for gender, age, race, and grade levels, as well as all the state-level covariates listed in Table 1.
Youth aged 18 or above are excluded.
Definitions of youth substance use are in the text.

Appendix Table 3 — E-cigarette MLSA Law and Youth Substance Use
National and State YRBSS: 2005-2015 (Strongly Balanced Sample)

| **Panel A** | ***DV: Youth is a current smoker*** | | | ***DV: Youth is a first-time smoker*** | | |
|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA Law | 0.010** | 0.014** | 0.017*** | 0.006** | 0.007** | 0.008*** |
| | (0.004) | (0.006) | (0.005) | (0.003) | (0.004) | (0.003) |
| *N* | 625,719 | 625,719 | 625,719 | 455,908 | 455,908 | 455,908 |

| **Panel B** | ***DV: Youth is a regular smoker*** | | | ***DV: Youth is a heavy smoker*** | | |
|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA Law | 0.008* | 0.011** | 0.009*** | 0.008** | 0.011*** | 0.009*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.004) | (0.003) |
| *N* | 625,719 | 625,719 | 625,719 | 625,719 | 625,719 | 625,719 |

| **Panel C** | ***DV: Youth is a current drinker*** | | | ***DV: Youth is a binge drinker*** | | |
|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 |
| E-cigarette MLSA Law | -0.002 | 0.003 | 0.009 | 0.002 | -0.003 | -0.001 |
| | (0.009) | (0.010) | (0.010) | (0.007) | (0.006) | (0.006) |
| *N* | 589,491 | 589,491 | 589,491 | 589,491 | 589,491 | 589,491 |

| **Panel D** | ***DV: Youth is a marijuana user*** | | |
|---|---|---|---|
| | 1 | 2 | 3 |
| E-cigarette MLSA Law | 0.002 | -0.005 | 0.000 |
| | (0.009) | (0.010) | (0.010) |
| *N* | 632,304 | 632,304 | 632,304 |
| Full Controls | ✓ | ✓ | ✓ |
| State FEs | ✓ | ✓ | ✓ |
| Year FEs | ✓ | ✓ | ✓ |
| State-specific linear pre-trends | | ✓ | |
| State-specific linear trends | | | ✓ |

Notes: Standard errors, clustered at the state level, are shown in parentheses.
* p < 0.10, ** p < 0.05, *** p < 0.01
All models include dummy variables for gender, race, age, and grade levels. State-level covariates listed in Table 1 are included.
E-cigarette MLSA law, the leads, and the lags are defined in the text.
The definitions of youth substance use are in the text.

Appendix Table 4 — E-cigarette MLSA Law and Youth Substance Use
SCM-weighted Sample

| | Current Smoker | Current Drinker | Current Binge Drinker | Current Marijuana User |
|---|---|---|---|---|
| E-cigarette MLSA Law | 0.010* | 0.008 | 0.008 | -0.004 |
| | (0.005) | (0.009) | (0.006) | (0.009) |
| State FEs | ✓ | ✓ | ✓ | ✓ |
| *N* | 245 | 245 | 245 | 245 |

Notes: Standard errors, calculated using Donald and Lang's (2007) two-step estimator, are shown in parentheses.
* $p < 0.10$
We run SCM on each MLSA state by excluding all the other MLSA states from the estimation sample. We then pool these individually created synthetic samples, thereby forming one larger SCM-weighted sample, and keep the synthetic weights unchanged. Lastly, we regress the difference of youth substance use rates between the MLSA states and synthetic control states on an indicator variable for the enactment of e-cigarette MLSA laws and control for a set of state dummy variables.
Youth aged 18 or above are excluded from creating such SCM-weighted sample.

## Data Appendix

Our analysis sample uses data from the pooled national and state YRBSS, spanning 2005-2015. The national YRBSS is conducted by CDC and the state YRBSS, while coordinated by CDC, is administered by each state health department or education agency. Unlike the state YRBSS, the national YRBSS does not provide state identifiers by default but we obtain this information from CDC. States that have administered YRBSS may not distribute data for secondary analyses due to low response rates, and we do not include them in analyses. Appendix Tables 5 and 6 display the number of observations at each state by year cell from the pooled national and state YRBSS.

Our control for medical marijuana laws follow Choi, Dave, and Sabia (2016) by creating a set of indicator variables tracking the law's overall legislative decision and its separate statutes related to home cultivation, legal dispensaries, allowance for non-specific pain, and state registry. Home cultivation allows qualified patients and their caregivers to grow cannabis plants at home. Legal dispensaries offer protection to legal marijuana supply through retail dispensaries. Allowance for non-specific pain relaxes the constraint that medical marijuana is reserved for particular medical symptoms. And state registry requires medical marijuana users to register with a state or local authority.

The cigarette tax data come from the CDC STATE System and the beer tax data come from the National Institute on Alcohol Abuse and Alcoholism. We use tax rates as of March for both variables to match the study period over which surveys were conducted. We obtain state unemployment rates and per capita income from the Bureau of Labor Statistics. Both cigarette and beer taxes are inflation-adjusted to 2005 dollars using the Consumer Price Index for All Urban Consumers (CPI-U), and we transform the per capita income using a natural logarithm.

Appendix Table 5 — National and State YRBSS State by Year Observation Counts

| State | 2005 | 2007 | 2009 | 2011 | 2013 | 2015 |
|---|---|---|---|---|---|---|
| Alabama | 1,026 | 483 | 2,528 | 1,654 | 1,845 | 1,810 |
| Alaska | | 1,268 | 1,218 | 1,279 | 1,183 | 1,343 |
| Arizona | 3,502 | 3,545 | 2,846 | 3,876 | 1,744 | 2,698 |
| Arkansas | 1,503 | 1,979 | 1,927 | 1,327 | 1,802 | 2,746 |
| California | 1,553 | 2,110 | 2,802 | 1,877 | 2,463 | 5,779 |
| Colorado | 1,475 | | 1,684 | 1,721 | 304 | 270 |
| Connecticut | 2,442 | 1,997 | 2,319 | 2,000 | 2,377 | 2,429 |
| Delaware | 2,633 | 2,357 | 2,257 | 2,421 | 2,590 | 2,638 |
| District of Columbia | | | | 316 | | |
| Florida | 4,982 | 5,098 | 5,591 | 7,409 | 6,840 | 6,854 |
| Georgia | 3,579 | 2,744 | 3,146 | 2,033 | 2,278 | 402 |
| Hawaii | 1,627 | 1,148 | 1,692 | 4,172 | 4,467 | |
| Idaho | 1,667 | 1,384 | 2,102 | 1,921 | 2,090 | 2,050 |
| Illinois | 492 | 2,956 | 4,432 | 4,500 | 3,793 | 4,022 |
| Indiana | 1,682 | 2,653 | 1,473 | 3,062 | 824 | 2,057 |
| Iowa | 1,588 | 1,666 | | 1,513 | | |
| Kansas | 1,909 | 1,692 | 2,196 | 2,133 | 2,089 | |
| Kentucky | 3,766 | 3,842 | 1,726 | 1,973 | 2,257 | 2,465 |
| Louisiana | 158 | 1,299 | 1,437 | 1,115 | 1,063 | |
| Maine | 1,325 | 1,267 | 8,445 | 9,079 | 8,343 | 9,112 |
| Maryland | 1,398 | 1,486 | 1,590 | 2,793 | 51,769 | 54,356 |
| Massachusetts | 3,598 | 3,745 | 2,624 | 2,915 | 2,630 | 3,238 |
| Michigan | 3,479 | 3,723 | 3,636 | 4,711 | 4,627 | 4,879 |
| Minnesota | 95 | | 188 | | 292 | 745 |
| Mississippi | | 1,923 | 1,763 | 1,846 | 2,144 | 2,040 |
| Missouri | 1,963 | 1,865 | 1,681 | 344 | 1,825 | 1,594 |
| Montana | 2,987 | 3,846 | 1,785 | 4,022 | 4,745 | 4,308 |
| Nebraska | 3,706 | | | 3,719 | 1,824 | 1,634 |
| Nevada | 1,529 | 1,729 | 2,403 | 207 | 2,069 | 1,787 |
| New Hampshire | 1,249 | 1,581 | 1,450 | 1,359 | 1,590 | 14,310 |
| New Jersey | 1,800 | 689 | 2,203 | 1,730 | 2,027 | 208 |
| New Mexico | 5,417 | 2,780 | 5,495 | 5,685 | 5,325 | 8,486 |
| New York | 9,939 | 13,688 | 15,335 | 13,161 | 10,409 | 10,406 |
| North Carolina | 4,466 | 3,975 | 5,550 | 3,324 | 2,171 | 5,891 |
| North Dakota | 1,710 | 1,722 | 1,767 | 1,863 | 1,919 | 2,064 |
| Ohio | 1,663 | 2,433 | | 1,358 | 1,578 | 227 |
| Oklahoma | 1,923 | 2,842 | 1,397 | 1,136 | 1,465 | 1,934 |
| Oregon | 268 | | 247 | | | |
| Pennsylvania | 423 | 210 | 3,104 | 450 | 264 | 3,278 |
| Rhode Island | 2,316 | 2,133 | 3,106 | 3,814 | 2,357 | 4,004 |

| | | | | | | |
|---|---|---|---|---|---|---|
| South Carolina | 1,567 | 1,206 | 1,070 | 1,437 | 1,553 | 1,311 |
| South Dakota | 1,567 | 1,577 | 2,122 | 1,502 | 1,273 | 1,257 |
| Tennessee | 1,924 | 2,182 | 2,176 | 2,874 | 1,847 | 4,371 |
| Texas | 5,821 | 4,906 | 4,766 | 5,841 | 3,479 | 1,226 |
| Utah | 1,710 | 2,097 | 1,544 | 1,657 | 2,118 | |
| Vermont | 6,997 | 5,744 | 8,190 | 8,267 | | 20,151 |
| Virginia | 349 | 439 | 98 | 1,603 | 7,776 | 4,310 |
| Washington | 101 | | 246 | 167 | 195 | 102 |
| West Virginia | 1,549 | 1,598 | 2,071 | 2,375 | 1,753 | 1,803 |
| Wisconsin | 2,593 | 2,234 | 3,074 | 3,615 | 2,776 | |
| Wyoming | 2,455 | 2,174 | 2,802 | 2,439 | 2,924 | 2,317 |

Appendix Table 6 — National and State YRBSS State by Year Observation Counts (Strongly Balanced Sample)

| State | 2005 | 2007 | 2009 | 2011 | 2013 | 2015 |
|---|---|---|---|---|---|---|
| Alabama | 1,026 | 483 | 2,528 | 1,654 | 1,845 | 1,810 |
| Arizona | 3,502 | 3,545 | 2,846 | 3,876 | 1,744 | 2,698 |
| Arkansas | 1,503 | 1,979 | 1,927 | 1,327 | 1,802 | 2,746 |
| California | 1,553 | 2,110 | 2,802 | 1,877 | 2,463 | 5,779 |
| Connecticut | 2,442 | 1,997 | 2,319 | 2,000 | 2,377 | 2,429 |
| Delaware | 2,633 | 2,357 | 2,257 | 2,421 | 2,590 | 2,638 |
| Florida | 4,982 | 5,098 | 5,591 | 7,409 | 6,840 | 6,854 |
| Georgia | 3,579 | 2,744 | 3,146 | 2,033 | 2,278 | 402 |
| Idaho | 1,667 | 1,384 | 2,102 | 1,921 | 2,090 | 2,050 |
| Illinois | 492 | 2,956 | 4,432 | 4,500 | 3,793 | 4,022 |
| Indiana | 1,682 | 2,653 | 1,473 | 3,062 | 824 | 2,057 |
| Kentucky | 3,766 | 3,842 | 1,726 | 1,973 | 2,257 | 2,465 |
| Maine | 1,325 | 1,267 | 8,445 | 9,079 | 8,343 | 9,112 |
| Maryland | 1,398 | 1,486 | 1,590 | 2,793 | 51,769 | 54,356 |
| Massachusetts | 3,598 | 3,745 | 2,624 | 2,915 | 2,630 | 3,238 |
| Michigan | 3,479 | 3,723 | 3,636 | 4,711 | 4,627 | 4,879 |
| Missouri | 1,963 | 1,865 | 1,681 | 344 | 1,825 | 1,594 |
| Montana | 2,987 | 3,846 | 1,785 | 4,022 | 4,745 | 4,308 |
| Nevada | 1,529 | 1,729 | 2,403 | 207 | 2,069 | 1,787 |
| New Hampshire | 1,249 | 1,581 | 1,450 | 1,359 | 1,590 | 14,310 |
| New Jersey | 1,800 | 689 | 2,203 | 1,730 | 2,027 | 208 |
| New Mexico | 5,417 | 2,780 | 5,495 | 5,685 | 5,325 | 8,486 |
| New York | 9,939 | 13,688 | 15,335 | 13,161 | 10,409 | 10,406 |
| North Carolina | 4,466 | 3,975 | 5,550 | 3,324 | 2,171 | 5,891 |
| North Dakota | 1,710 | 1,722 | 1,767 | 1,863 | 1,919 | 2,064 |
| Oklahoma | 1,923 | 2,842 | 1,397 | 1,136 | 1,465 | 1,934 |
| Pennsylvania | 423 | 210 | 3,104 | 450 | 264 | 3,278 |
| Rhode Island | 2,316 | 2,133 | 3,106 | 3,814 | 2,357 | 4,004 |
| South Carolina | 1,567 | 1,206 | 1,070 | 1,437 | 1,553 | 1,311 |
| South Dakota | 1,567 | 1,577 | 2,122 | 1,502 | 1,273 | 1,257 |
| Tennessee | 1,924 | 2,182 | 2,176 | 2,874 | 1,847 | 4,371 |
| Texas | 5,821 | 4,906 | 4,766 | 5,841 | 3,479 | 1,226 |
| Virginia | 349 | 439 | 98 | 1,603 | 7,776 | 4,310 |
| West Virginia | 1,549 | 1,598 | 2,071 | 2,375 | 1,753 | 1,803 |
| Wyoming | 2,455 | 2,174 | 2,802 | 2,439 | 2,924 | 2,317 |