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ABSTRACT

Regression Discontinuity Evidence on the Effectiveness of the Minimum Legal E-cigarette Purchasing Age*

Increases in youth vaping rates and concerns of a new generation of nicotine addicts recently prompted an increase in the federal minimum legal purchase age (MLPA) for tobacco products, including e-cigarettes, to 21 years. This study presents the first regression discontinuity evidence on the effectiveness of e-cigarette MLPA laws. Using data on 12th graders from Monitoring the Future, we obtain robust evidence that federal and state age-18 MLPAs decreased underage e-cigarette use by 15–20% and frequent use by 20–40%. These findings suggest that the age-21 federal MLPA could meaningfully reduce e-cigarette use among 18–20-year-olds.

JEL Classification: I12, I18, H51, H75

Keywords: e-cigarettes, minimum legal purchase age (MLPA), Monitoring the Future, regression discontinuity, vaping

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* This research was conducted with restricted access to Monitoring the Future data through the Inter-university Consortium for Political and Social Research (Miech et al. 2014, 2015, 2016, 2017). We thank Michael Pesko for helpful comments and discussing our paper. We also thank participants at the 10th Annual American Society of Health Economists Meeting for useful comments. We declare no conflict of interest. All remaining errors are our own.

1. Introduction

This study presents the first regression discontinuity (RD) evidence on the responsiveness of e-cigarette consumption to minimum legal purchase age (MLPA) laws.[1] We examine e-cigarette use from 2014 to 2017 using restricted-access Monitoring the Future (MTF) data on 12th graders in the U.S. During this period, 23 state-level e-cigarette MLPA laws of 18 years (MLPA-18) took effect, leading to the adoption of a federal MLPA-18 in August 2016.[2] Our findings indicate that MLPA-18 laws induced statistically significant and economically meaningful reductions in e-cigarette use among high school seniors approaching their 18th birthdays.

Over the decade of the 2010s, e-cigarette use increased substantially, from 1.5% to 20%, among high school students (U.S. Department of Health and Human Services 2016, Wang et al. 2020). In this same period—following a general trend in part because of aggressive public health measures—use of traditional cigarettes by high school students decreased from 20% to 5% (Wang et al. 2018, Gentzke et al. 2019, Gentzke et al. 2020). As public health scientists studied associated health risks of e-cigarette consumption and observers began to call the rise in vaping a "public health crisis" producing a new generation of young nicotine addicts (cf. Walley et al. 2019), the topic became an increasing focus of inquiry among health economists.[3] However, despite a long and rich literature on the effects of regulations in markets for traditional cigarettes (cf. DeCicca et al. 2022 for a comprehensive review), economists have largely focused

[1] MLPA laws can be interpreted as minimum *sales* age laws as they always prohibit selling these products whereas not all laws penalize the buyer. Henceforth, we will use the term MLPA throughout the paper.

[2] Table 1 provides dates of MLPA-18 implementation by state. The FDA issued a final rule setting a federal minimum age of 18 for purchasing any tobacco product, including e-cigarettes, on May 10, 2016 (Food and Drug Administration 2016, 21 CFR Parts 1100, 1140, and 1143). This became effective on August 8, 2016, but was binding in just the two states, Michigan and Pennsylvania, that had yet to adopt their own MLPA-18 laws.

[3] Many studies, such as Allcott and Rafkin (2022), point to the harm reduction potential of e-cigarettes.

on the potential impact of e-cigarette policy on the consumption of *traditional* rather than electronic cigarettes, that is, substitution and spillover effects.[4] In particular, economists have tested empirically whether imposing e-cigarette MLPAs might shift consumption by underage adolescents from electronic to traditional cigarettes.[5]

Friedman (2015) first estimated this relationship using state-level data on 12 to 17-year-olds from the 2002–2013 National Surveys on Drug Use and Health. She obtained evidence that e-cigarette MLPA laws increase smoking of traditional cigarettes. Subsequently, several analyses replicated this finding, including Pesko et al. (2016) and Dave et al. (2019) in biennial Youth Risk Behavior Survey (YRBS) data as well as Pesko & Currie (2019) in data on records of births to rural underage pregnant females.[6] In contrast, using data on high school seniors from the 2007–2014 waves of the MTF—the same data we use in this paper—Abouk & Adams (2017) found that e-cigarette MLPA-18 laws *reduce* traditional cigarette smoking. Rather than substitutes, this result is consistent with the two cigarette types being complements. All these studies identify MLPA effects using a difference-in-differences (DD) framework.

In one of the most comprehensive studies on optimal e-cigarette regulation, Allcott and Rafkin (2022) find that e-cigarette demand is elastic with respect to taxes, but cannot reject the

[4] One exception is the question of whether e-cigarette smokers misperceive the health risks of consumption, with evidence suggesting that consumers tend to *overestimate* these risks, in line with risk perceptions for traditional cigarettes (Viscusi 2016, 2021, Ziebarth 2018.)

[5] Absent such a shift, the overall cost-benefit assessment regarding population health effects would be more optimistic, given that e-cigarettes help traditional smokers to quit or at least switch (Bullen et al. 2013). In general, it is an accepted view that e-cigarettes are less harmful to humans' health than traditional cigarettes, but are nevertheless harmful (Polosa et al. 2020, Münzel et al. 2021).

[6] Pesko and Warman (2021) provide additional evidence of contemporaneous substitution of e-cigarette and combustible cigarette products. They also call into question the effectiveness of tobacco regulation more generally in reducing cigarette smoking among youth and suggest the potential harm of regulating e-cigarettes in terms of increased taxes given that cigarettes and e-cigarettes are economic substitutes.

null of zero substitution between traditional and e-cigarettes. Their study, however, remains largely silent on MLPAs as a regulatory tool.[7]

This paper empirically studies the effectiveness of MLPA-18 laws in restricting youth e-cigarette use, employing an RD design using MTF data from 2014 to 2017. Early research on e-cigarette MLPAs was precluded from estimating these important first-stage effects on vaping because survey data on adolescent e-cigarette consumption did not yet exist. The MTF, for example, did not collect information e-cigarette use until 2014, and likewise the YRBS did not until 2015. To our knowledge, we are the first to study the effectiveness of the most popular policy instrument to prevent youth from initiating vaping using data spanning multiple years and most U.S. states, nearly half of which adopted MLPA-18 laws during our study period, and the first to do so using RD. This approach follows seminal work on the minimum legal drinking age by Carpenter & Dobkin (2009, 2011).

Only three studies of which we are aware leverage e-cigarette consumption data to study MLPA-18 laws. Two of these, the aforementioned Dave et al. (2019) and Abouk & Adams (2017) analyses, each had access to only one year of e-cigarette use data. Both consequently focus on the impact that MLPA-18 laws have on *traditional* cigarette smoking rather than their effectiveness with respect to e-cigarette use. The third, Nguyen (2020), examines MLPAs enacted by 8 of the 10 Canadian provinces.

As one of their secondary results, Dave et al. (2019) estimate small negative effects of MLPA-18 laws on current and lifetime e-cigarette use which are significant only for the latter. The authors acknowledge that these are suggestive findings based solely on one cross-section. Abouk & Adams (2017) and Nguyen (2020), meanwhile, both find that MLPAs significantly

[7] Several other papers examine the impact of e-cigarette taxes on prices and use (Pesko et al. 2020, Cotti et al. 2022).

decrease past-month e-cigarette use, with very large associated effects on the order of 60% in the U.S. and 33–50% in Canada. Both again employ DD, identifying MLPA effects from variation in adoption timing across regions within a short time span.

While DD is generally an appropriate methodological choice (albeit prone to biases stemming from effect heterogeneity in staggered DD designs, e.g. Callaway and Sant'Anna 2021, Goodman-Bacon 2021, Sun and Abraham 2021), MLPAs provide a natural setting for alternative identification approaches using RD designs (cf. Carpenter & Dobkin 2009, 2011). More generally, with Abouk & Adams (2017) providing the sole relevant evidence but using variation from only 2014, we still know relatively little about how MLPAs-18 worked in the U.S. before the federal MLPA for all tobacco products was raised to age 21 on December 20, 2019.[8]

Our study leverages a MTF sample incorporating three subsequent years of restricted-access data, extending through 2017. These three additional years allow us to evaluate the enactment of e-cigarette MLPA-18 laws in the 25 states, including D.C., that had not already done so prior to spring 2014, as listed in Table 1.[9] Because many U.S. adolescents reach age 18 when they are high school seniors and the MTF surveys focus on this group, we are able to maintain a large sample while restricting attention to respondents who are within a relatively narrow radius of eight months from their 18th birthday.[10]

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[8] Further, Abouk & Adams (2017) discuss the important caveat that their e-cigarette effects are identified only by MLPA laws that took effect during a narrow spring 2014 time window. As such, their finding reflects behavior occurring shortly before and after law adoption, among underage respondents residing in at most the four states that "potentially provide identification." These four states appear to be Delaware, Kentucky, Louisiana, and Nebraska.

[9] Note that this count includes Michigan and Pennsylvania, which did not have state-level MLPAs before the federal law was implemented in August 2016.

[10] As our RD design captures changes in e-cigarette use along a different margin than DD, our work can be viewed as complementary to the previous literature.

The main result of our paper echoes the Abouk & Adams (2017) finding that U.S. MLPA-18 laws strongly reduced underage e-cigarette use, though by a more modest yet still substantial 15–20%.[11] We also expand the analysis beyond that of Abouk & Adams (2017) in two important directions. First, we further estimate that MLPAs significantly lower the prevalence of *frequent e-cigarette use*—that is, on at least 10 occasions during the past 30 days—by 20–40%. Our second extension estimates, as a falsification check, identical RD models among respondents in states without MLPA-18 laws between 2014 and 2016.[12] These placebo effects are indeed insignificant and small in magnitude for both *any* and *frequent use*. Further, results for both the actual and placebo MLPAs are robust to a wide variety of specification permutations. These involve triangular kernel weighting, varied bandwidths, local randomization, local quadratic estimates, omitting the month of the 18th birthday or the month that immediately follows, including or excluding subsets of control variables, and incorporating information on MLPAs implemented by local jurisdictions or based on older threshold ages of 19 or 21 years.

Our conclusions suggest that MLPA-18 laws are effective in substantially decreasing underage e-cigarette consumption. This is an important finding for policymakers and regulators around the world. According to Global Tobacco Control (2021), as of 2021, only 45 out of 195 countries had enacted e-cigarette MLPAs. One could argue that because of these externalities on their future selves, young smokers should face higher taxes than the population at large. However, while age-specific taxes may not be (politically) feasible, one may argue that age-specific purchase restrictions raise the cost of smoking for youth.

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[11] This is also consistent with Nguyen (2020), although most of the Canadian provincial MLPAs that he studies are 19 rather than 18.

[12] The federal MLPA-18 was in effect for the entire sample in 2017.

In addition, our results imply that MLPAs work despite geographic spillovers due to in person or online "cross-border" shopping (Lovenheim 2008). While e-cigarettes have emerged entirely in the internet era, it appears that online e-cigarette markets do not (fully) render MLPAs ineffective. This is also true for peer effects through "social markets" and older friends being a source of e-cigarette supply.[13]

Further, our results suggest that the recent federal adoption of an age 21 MLPA for tobacco products has the potential to meaningfully decrease e-cigarette use among newly underage 18–20-year-olds.[14] In fact, two very recent studies, Abouk et al. (2021) and Bryan et al. (2021), estimate effects of MLPA-21 laws adopted by states and localities in advance of the federal 2019 enactment. Both are of clear importance in light of what has become the current nationwide tobacco MLPA policy, but were constrained by a combination of MLPA-21 laws not becoming widespread before 2019 and limited data on respondents in the age 18–20 group that could previously legally purchase e-cigarettes under a MLPA-18 regime.[15] Further, a strength of our design is that the MLPA-18 variation we study did not alter MLPAs for traditional cigarettes. Moreover, we carefully investigate all substate MLPA-21 laws, excluding them to avoid confounding effects. Thus, research on the crucial first-stage effects of e-cigarette MLPA-18s

[13] We are grateful to a referee for suggesting these points about differential tax rates and "cross-border" shopping through the internet and friends.

[14] An important caveat is that our estimates are specifically local to age 18.

[15] Abouk et al. (2021) estimate positive, very small, and insignificant MLPA-21 effects on e-cigarette use among MTF high school seniors, but analyzed only respondents age 17 or younger. Moreover, only four states had age 21 MLPAs by their mid-2018 sample period; of the three surveyed by MTF, two had MLPAs-21 in effect only for the last sample year. Bryan et al. (2021) find that statewide MLPA-21 laws reduce e-cigarette consumption using biannual 2015–2019 YRBS and annual 2016–2018 BRFSS data. In contrast to Abouk et al. (2021), they study respondents who could legally purchase e-cigarettes in age 18 MLPA regimes, but because the YRBS samples high school students, this group includes only 18-year-olds. Like Abouk et al. (2021), their data encompass just four states with MLPA-21 laws enacted before July 2019. Their BRFSS results for *any e-cigarette use* are mixed.

remains highly valuable for future policy design domestically and around the world.

2. Data

Our study examines Monitoring the Future (MTF) data on high school seniors, which are nationally representative of the 12th grade population in the U.S (Miech et al. 2014, 2015, 2016, 2017). As required for a RD analysis of laws that phase out abruptly upon reaching age 18, these data include many teens surveyed close to their 18th birthdays.[16] Because the MTF did not ask participants about e-cigarette use until 2014, and 2017 already constitutes an entire sample year during which MLPA-18 was a federal law, our sample is confined to the years 2014–2017.

Measures of E-Cigarette Prevalence and Use

Throughout our sample period, the MTF collects information on e-cigarette use through the question *"During the last 30 days, on how many occasions have you used electronic cigarettes (V2546)?*" In 2017, some respondents are instead asked *"On how many occasions have you vaped nicotine in the past 30 days (V2569)?"* For each variant, five categorical answer choices are offered: zero, 1–2, 3–5, 6–9 and 10+ days (or occasions). We use information from both questions to construct our 2017 sample.[17]

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[16] The MTF also surveys 10th graders, but relatively few are close to turning 18 years old. Of those that are, most are still age 17, which means that 10th graders would disproportionately contribute to the treatment group. This potentially creates selection bias, as 10th graders close to age 18 are the oldest in their grade level and therefore unlikely to be representative of the more traditional-age students who comprise the 12th grade sample.

[17] Each year, only a subset of MTF respondents are asked these questions, even in 2017 when any given respondent is asked at most one of them. Reassuringly, the pattern of responses to the two questions in 2017, the year in which both were asked, is nearly identical. For V2546, 88.3% reported never using an e-cigarette, 4.7% used on 1–2 days, 1.9% on 3–5 days, 1.5% on 6–9 days, and 3.6% on 10+ days, while for V2569, 88.7% reported never vaping nicotine, 4.4% used on 1–2 occasions, 1.7% on 3–5 occasions, 1.0% on 6–9 occasions, and 4.1% on 10+ occasions. Our results, which are available upon request, are robust to using only respondents to question V2546.

From responses to these questions, we generate two binary measures of e-cigarette use that serve as our outcome variables. *Any e-cigarette use* indicates having used electronic cigarettes or vaped nicotine on at least one occasion in the past 30 days. Similarly, *frequent e-cigarette use* indicates having used electronic cigarettes or vaped nicotine on at least 10 days or occasions over the past 30 days.

Consistent with the estimation results we later show, sample means for each measure are higher for our placebo analysis of respondents living in areas without an e-cigarette MLPA than for our analysis of actual MLPAs. In the latter, 14.3% of respondents report *any e-cigarette use* while 3.9% report *frequent use*; in the former, the analogous rates are 16.8% and 4.3%, respectively. Our results tables display these means so that the relative sizes of our estimates can be readily assessed.

Measuring Respondents' Age

To identify the age of respondents, we analyze the restricted use version of the MTF and combine birth month and interview month to construct a measure of *age in months*. This equals the age attained by the respondent during the interview month. For instance, among students surveyed in March 2017, age is coded as 18 × 12 = 216 months for those born in March 1999, 215 months for those born in April 1999, etc.

The inability to observe exact birth dates creates ambiguity about exact respondent age in the vicinity of the 18th birthday MLPA threshold. While we know that respondents coded as age 216 months turned 18 during their interview month, we cannot tell whether that had already occurred by the interview, that is, whether these respondents could legally buy e-cigarettes at the time of the interview. Likewise, we are certain that respondents aged 217 months are 18 years old, but not whether they have been for the entire 30-day reference period for the e-cigarette questions.

Our baseline specification resolves this ambiguity while preserving sample size by assuming that respondents aged 216 months are still treated, as they could not legally purchase e-cigarettes for at least part of the previous 30 days, while those aged 217 months are untreated because they are all 18 years old. We show robustness checks that omit respondents aged 216 months or, instead, aged 217 months.[18]

We also test for social desirability bias related to minimum age laws for vaping. Specifically, among those who live vs. do not live in an MLPA-18 jurisdiction, Appendix Table A4 shows that having smoked any cigarette in one's life does not vary discontinuously at the age 18 threshold. This test follows Carpenter and Dobkin (2009); the argument is that those who live in a state without a minimum age law will face different desirability bias than those living in a place with a law. For example, because it is illegal for vendors to sell the product to them, a 17-year-old in a state with an MLPA-18 may be less likely to report vaping than someone who lives in a state without one. This could create a discontinuity in vaping at the age threshold but would, in fact, simply reflect a social desirability bias in the survey responses. Additionally, because previous work has shown a relationship between smoking and vaping, any test of cigarette smoking in the past 30 days may similarly suffer from either desirability bias or a treatment effect caused by the law. Therefore, we focus on *ever smoking* cigarettes and show that there is no discontinuity in reporting before or after a law is implemented. In our view, this finding supports our claim that differential use of e-cigarettes, rather than differential reporting of use, drives our findings. An additional piece of evidence that supports our results involves the effects

[18] In these models, the MLPA threshold is recentered at the age that is excluded, since conceptually the reason for exclusion is because the age in question includes respondents on both sides of the threshold.

of implementing an age 19 or age 21 vaping law. In areas in which an MLPA-18 existed pre-implementation of age 19 or 21 laws, we find a similar discontinuity in vaping use. However, post-implementation, when age 18 no longer represents differential treatment under the law, we find no differential use by age right around the cutoff.

Sample Selection

Figure 1 depicts the distribution of respondents by *age in months* for our main sample. As MTF interviews occur in the spring semester, and U.S. students generally reach age 17 before their senior year begins, it is not surprising that the modal age range is within 3 months of turning 18, and the decline in respondents moving away from the threshold age is particularly steep in the younger direction. This dictates our decision to limit the sample to respondents within eight months of their 18th birthdays, a restriction we also impose for older students to maintain symmetry in potential ages around the MLPA-18 threshold.[19]

[Insert Figure 1 about here]

Geographically Matching MLPAs with Respondents

Table 1 displays implementation dates for state-level e-cigarette MLPA-18 laws. We coded these based on information from Abouk & Adams (2017), along with Pesko & Currie (2019) for city- and county-level laws and the Preventing Tobacco Addiction Foundation (2021) for age 21 laws. Of the 25 states, including D.C., that did not have MLPA-18 laws prior to our sample period, 13 implemented their law in 2014, 8 in 2015, and 2 more in 2016. The MLPA-18

[19] Because the restricted data use agreement precludes reporting the relatively low cell counts represented by the extreme age groups, we cannot show separate age distributions for the pre-law and post-law samples, though our tables do report overall sample sizes for each of these groups.

was enacted federally in August 2016, by which point only two states, Michigan and Pennsylvania, did not already have a state-level law in place.[20]

[Insert Table 1 about here]

We rely on the restricted use version of MTF to match state (and substate) MLPAs with respondents. The MTF samples students in schools that are chosen within each of a selected set of geographic areas. Based on the zip code of the school, we determine whether each respondent has been covered by an e-cigarette MLPA-18, either state or local, for the full 30-day pre-interview period covered by the e-cigarette questions, which is our criterion for categorizing a respondent as treated.[21]

As mentioned, several states and many localities had MLPA-21 laws during our sample period. This is clearly relevant for our research design: all our untreated respondents in MLPA states are age 18, so would in fact not have legal purchase status in these areas (or the few states with e-cigarette MLPA-19 laws). Our baseline sample consequently excludes respondents from schools located within a jurisdiction covered by a MLPA of 19 or 21 on the interview date, though we later confirm the robustness of our results to including various subsets of these schools.[22] In addition, we investigate potential cross-border shopping by splitting our sample

[20] In addition, three sample jurisdictions outside of Massachusetts, which we exclude from our main analysis as discussed below, adopted MLPA-18 laws prior to their states doing so: Lewisville, TX (January 27, 2014), Santa Fe, NM (February 12, 2014), and Philadelphia, PA (April 8, 2014). Respondents from the counties containing these jurisdictions are considered treated during the period the jurisdiction but not state had a MLPA-18.

[21] To reiterate, this means we code respondents as legally able to purchase e-cigarettes at the date of their interview if any portion of the past 30 days occurred prior to the adoption of a MLPA in their jurisdiction.

[22] Specifically regarding locations sampled by MTF, the state e-cigarette MLPA was 19 in Alabama, New Jersey, and Utah for the entire period, and was 21 in California during 2017, Kansas City (Kansas and Missouri) starting in 2016, and Chicago, Eugene (Oregon), and St. Louis (city and county) during 2017. We also omit the entire state of Massachusetts because over 150 jurisdictions had MLPA-21 laws throughout the sample period. Finally, NYC enacted a MLPA-21 law on May 18, 2014, and Cleveland did so on April 13, 2016, in each case while interviews were ongoing that year. We therefore include schools from the five NYC counties in 2014 but omit them starting in 2015, and likewise include Cleveland schools in 2016 but not 2017. No other state or locality with a MLPA-19 or -21 law contains a MTF school during 2014–2017. See Table 4 for sensitivity analyses that include and omit these states and localities.

based on whether the county is located within 100 miles of a state or country without an MLPA law.[23]

Beyond e-cigarette use, age, school location, and survey timing, the only other information we use from the MTF is gender and race/ethnicity (non-Hispanic Black, non-Hispanic White, Hispanic, with "other race" as omitted category). We include these controls in most of our models.[24]

3. Empirical Strategy

Following standard approaches in the literature on minimum legal drinking age (e.g. Carpenter & Dobkin 2009, 2011), we employ a regression discontinuity (RD) approach to estimate effects of MLPA-18 laws on e-cigarette use. Conceptually, this design compares respondents who are identical on average other than a slight difference in age across the 18th birthday threshold. The identifying assumption is that absent the MLPA, e-cigarette consumption would be the same for students regardless of whether they were about to turn 18 or had just done so.

We specifically estimate the following regression using a local linear regression model:[25]

$$y_{its} = \alpha + \beta D_i + \gamma_0(1 - D_i)f(z_i - c) + \gamma_1 D_i f(z_i - c) + X'_{it}\tau + \delta_t + \rho_s + e_{its}. \quad (1)$$

In this notation, y_{it} represents an indicator for *any* or *frequent e-cigarette use* by respondent *i* in survey year *t*. Our RD estimate of the impact of the MLPA-18 on e-cigarette use is the estimated

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[23] We include distance to Mexico and the nearest province in Canada without MLPA laws (Nguyen 2020). We also note that while cross-border shopping is a concern, this practice is generally used more often by older tobacco consumers (cf. Pesko et al. 2012).

[24] As a sensitivity analysis, we also estimate models with birth and survey month fixed effects in Table 3.

[25] For these estimates we use the STATA command rdrobust (Calonico et al. 2014, Calonico et al. 2017)

coefficient β on D_i, an indicator of whether the respondent is 18 years old, or in our case 217 months, and therefore can legally purchase e-cigarettes in the presence of a MLPA-18. We specifically use an age cutoff of 216.5 months to define D_i so that the boundary ages of 216 and 217 months are symmetric on either side.

The terms $\gamma_0(1 - D_i)f(z_i - c)$ and $\gamma_1 D_i f(z_i - c)$ allow for e-cigarette use to trend with the running variable age (z_i), differentially on either side of the MLPA age cutoff *c* = 216.5 months, separately from any abrupt effect of the MLPA, thus isolating the impact of reaching legal purchase age from smooth effects of age. Our baseline model also controls for indicators of respondent race/ethnicity and sex in X'_{it}, along with fixed effects for survey year, δ_t, and state of residence, ρ_s.[26] The error term is e_{its}, where the *s* notation reflects our use of standard errors clustered by state.[27]

All our regressions use MTF survey weights for each respondent. We also check robustness by applying several additional specification checks to our main estimates. In our main specification, we use a bandwidth of 8 months, a uniform kernel and local linear methods. In Appendix Table A1, we apply a triangular kernel, use data-driven methods to choose the bandwidth, use bandwidths from 4 to 12 months,[28] and report bias-corrected robust versions of the coefficient and standard errors using the STATA command rdrobust, which is equivalent to

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[26] However, Table 3 confirms that the inclusion of some or all of these covariates has little bearing on our estimates of β.

[27] Cunningham (2021) reports that despite clustering on the running variable being standard practice in the RD literature, recent theoretical and simulation evidence, particularly from Kolesár and Rothe (2018), suggests that doing so is "nearly an unambiguously bad idea." We thus do not cluster on the running variable. Appendix Table A3 presents robustness tests using randomization inference regression discontinuity methods (Cattaneo, Frandsen and Titiunuk 2015; Cattaneo, Titiunuk, Vazquez-Bare 2016, 2017) .

[28] We are limited in bandwidth by the MTF survey setup, which interviews students in grade 8, 10, and 12. This means that at the age 18 threshold, going beyond 8 months in either direction leads to small sample sizes. This also makes it difficult to perform any falsification analyses using a placebo cutoff such as age 17 or 19.

using a quadratic function for the running variable.[29] We also confirm that no discontinuity in density exists at the cutoff using both a density test for discrete variables (Cattaneo, Jansson, and Ma 2018) and a McCrary test in Appendix Figure A3. Additionally, we provide supporting evidence that the covariates are smooth at the cutoff. We do this, first, by showing robust results with and without covariates (Appendix Table A4) and, second, by performing our main specification but using covariates as dependent variables (Appendix Table A5).

For each choice of covariate set (Table 3) and for whether respondents from MLPA-19 or MLPA-21 jurisdictions are included (Table 4), we estimate four versions of the RD model above. These versions use both *any* or *frequent e-cigarette use* as the outcome and differentiate by whether we estimate actual MLPA-18 effects or placebo effects in jurisdictions without MLPA-18 laws in place. We also perform an additional falsification test in Appendix Table 5 using areas with an MLPA-18 in effect which then implemented an MLPA-21.[30]

If the identification assumptions hold, β identifies the *intent-to-treat* causal effect of MLPA-18 on youth vaping rates. Note that this effect precisely considers that youth may find ample opportunities to circumvent MLPA-18 laws, for instance, by obtaining access through older peers, purchasing products online, or due to a lack of enforcement. The coefficient estimate of β thus offers information about the real-world effectiveness of MLPA-18 laws.

[29] Our robustness checks rely on our main analysis as a baseline, making just one change at a time to functional form, estimation procedure, or bandwidth.

[30] This analysis is essentially the opposite of our main analysis. We expect these areas to show a discontinuity in vaping before the implementation of the MLPA-21 but not after the law goes into effect. This is because in the "pre-period" youths aged 18 are treated differently by the law than those aged 17, while post implementation, stores are not allowed to sell vaping products to those slightly above or slightly below age 18.

4. Results

Table 2 shows our main RD estimates of the impact of MLPA-18 in our baseline covariate set and geographic sample. Each column reflects a different specification along two dimensions, whether a triangular kernel is used and whether the sample excludes respondents aged 216 or 217 months (or neither).[31]

Column 1 of Table 2 contains the main results of our study, using models with a uniform kernel and including all students aged 216 or 217 months. In the top panel, reaching age 18 in areas not yet covered by a MLPA-18 is not associated with changes in either *any* or *frequent e-cigarette use*. Although the point estimates are positive, they are very small. By contrast, the bottom panel indicates that upon achieving legal purchase status in areas with a MLPA-18 law, the likelihood of e-cigarette consumption rises by a statistically significant 0.027 (19% of the sample mean rate) for *any e-cigarette use* and 0.015 (38% of the sample mean) for *frequent e-cigarette use*.

[Insert Table 2 about here]

Figures 2 and 3 depict graphical versions of these discontinuities for observed age-specific means of *any* and *frequent use*, respectively. Appendix Figures A1 and A2 do the same for the mean residuals from the regressions corresponding to Table 2, column 1, and conform more closely to those estimates.[32] While we observe hardly any discontinuity around age 18 in *any* or *frequent e-cigarette use* in localities and years absent an MLPA-18 law, the data show substantial discontinuities after the introduction of such laws. In line with regression results in

[31] To reiterate from Section 2, respondents aged 216 months are within a month on either side of their 18th birthdays, so may or may not be age 18 but have definitely not yet been legal to purchase e-cigarettes in MLPA-18 areas for a full 30 days, while respondents aged 217 months are age 18 with certainty but might not be a full month beyond their 18th birthdays.

[32] Analogous graphs for the remaining estimates in the tables are available from the authors upon request.

Table 2, the visual discontinuities are about 2.5 percentage points for *any use* and 1 percentage point for *frequent use*. In our view, the lack of visual evidence for a spike in use immediately upon turning 18, along with the robustness of our estimates to using bandwidths of up to 12 months, suggests that the discontinuity is unlikely to merely represent an age 18 "celebration" effect.

[Insert Figures 2 and 3 about here]

The second column of Table 2 shows estimates from the same model but using a triangular kernel. These are slightly more efficient in relying more heavily on observations closer to the 18th birthday and differ slightly in magnitude, but otherwise yield the same conclusions as column 1.

Columns 3 and 4 of Table 2 mimic columns 1 and 2, but remove from the sample respondents aged 216 months with ambiguous legal purchase status, and therefore also slide the MLPA cutoff to the left from 216.5 to 216 months. Columns 5 and 6 instead remove respondents aged 217 months who might not have had legal purchase status for the full 30 days prior to the survey. The decrease in sample size, particularly coming from the age groups closest to the threshold and with the most respondents, entails a slight loss of statistical power. Nevertheless, estimated discontinuities for the post-law sample are the same or larger—as one might expect if MLPAs are indeed effective and given the removal of partly misclassified observations. They are therefore still statistically significant. Pre-law effects remain statistically insignificant.

As an alternative to state-clustered standard errors, Appendix Table A3 demonstrates that our results are robust to using a randomized local inference regression discontinuity design.[33]

[33] We use the STATA command rdrandinf (Cattaneo, Frandsen and Titiunuk 2015; Cattaneo, Titiunuk, Vazquez-Bare 2016, 2017).

Our estimates are slightly attenuated using this method but still provide similar inferences to our main estimates in Table 2.

Figures 4 (*any use*) and 5 (*frequent use*) show the robustness of the results when varying the bandwidth of ages in months. We include estimates using bandwidths between 4 and 12 months and also perform a data-driven method for determining bandwidth (Calonico et al. 2014, Calonico et al. 2017). These estimates for post-law (pre-law) periods are plotted in the upper (lower) panels of Figure 4 and 5, respectively. As seen, the results are robust to

alternative bandwidths, with the post-period displaying a consistent increase in vaping at age 18, while the coefficient is close to zero for the pre-period.

Appendix Figures A4 to A6 repeat analyses for *any use* varying the bandwidths but using a triangular kernel (A4), using a quadratic specification (A5), and cutting demographic covariates (A6). The 60 additional models provide consistent results. We perform the same robustness checks using *frequent use* in Appendix Figures A7 to A9. In this case, either using a triangular kernel or cutting covariates provides consistent results across bandwidths, although they are a little noisier for the smallest bandwidths. Results using a quadratic specification, however, are not particularly robust.

[Insert Table 3 about here]

Table 3 chronicles estimates when we change the set of controls. All estimates use our original local linear specification that includes the age 216 months group but does not apply triangular kernel weighting. Column 1 restates column 1 of Table 2, emphasizing that our baseline model included fixed effects for states and years along with indicators for race/ethnicity and gender. Column 2 confirms that estimates are unchanged using a standard RD specification in which no other covariates are included. In column 3, effect sizes are slightly smaller and we lose statistical significance in *any e-cigarette use* when controlling just for temporal variation in e-cigarette use by including only year indicators. Columns 4 and 5 verify that estimates are nearly identical when we alternatively control only for geographic variation by including just state effects, or estimate a canonical two-way fixed effects model with state and year fixed effects but not survey month indicators. These specifications provide smaller standard errors and thus stronger statistical significance. In column 6, estimates remain quite similar when we omit gender from our baseline model, despite e-cigarette use varying by gender.

Columns 7 to 9 move in the opposite direction by adding indicators for birth and/or survey month to our baseline set of covariates. In column 7, holding constant survey month indicators again has little impact on our estimates, leading to the conclusion that any combination of our original controls and survey month fixed effects, including none of these, generates essentially the same set of results.

This changes slightly in columns 8 and 9, where we also control for birth month indicators. As opposed to survey month, which addresses potential seasonality, it is unclear why birth month would have much further impact on e-cigarette use, other than through age—our running variable—given the narrow range of survey months.[34] As age already incorporates birth month, from our perspective, this is a less-preferred specification. Nonetheless, it is reassuring that explicitly holding constant birth month does not alter the implication that the discontinuities at the MLPA-18 are statistically significant for *any use* post-law and are insignificant pre-law for both use measures. While the *frequent use* coefficient is no longer statistically significant at the 5% threshold, it remains substantively large at 25% of the sample mean and statistically significant at the 10% threshold.

[34] This is also the reason why we are unable to carry out heterogeneity tests by age; being among the oldest vs. youngest in the classroom may matter for risky behavior (Black et al. 2011).

[Insert Table 4 about here]

Table 4 again begins in column 1 with our estimates from the first columns of the previous two tables. From this starting point, we estimate seven additional models that vary whether schools are included based on the criteria listed in the five rows at the bottom, all concerning states and localities with MLPAs of 19 or 21. Moving from left to right, we alternatively include respondents from Massachusetts (column 2), categorizing them based on the state law (even though some are from localities with MLPA-21s); omit those from NYC in 2014 and Cleveland in 2016 (column 3), when MLPA-21s took effect during the interview window[35]; include schools in Massachusetts and California in 2017 as well as other zip codes within a locality that has a MLPA-21 (column 4); use this same sample but omit Massachusetts (column 5); include Massachusetts and MLPA-21 localities but not California in 2017 (column 6); replicate this sample but omit Massachusetts (column 7); and return to our baseline sample but add respondents from the three MLPA-19 states—Alabama, New Jersey, and Utah—to the pre-law sample, as both 17- and 18-year-olds have the same legal purchase status in these states (column 8).

In all of these samples, pre-law discontinuities remain small and insignificant, while post-law discontinuities are still large and significant. For the latter—that is, actual MLPA-18 effects—the one perceptible difference in coefficient estimates occurs in columns 4 to 7, where both decline in magnitude when we consider MLPA-21 laws. This decrease is roughly 5% of the sample mean for *any use*. However, as standard errors also shrink slightly, it has no impact on the significance level. For *frequent use*, the estimates shrink by around 10% of the sample mean, with statistical significance reduced to the 10% level in two cases. The common theme with

[35] All samples include Cleveland pre-2016 and exclude observations from NYC after 2014 and Cleveland in 2017.

these four samples is that, unlike those for all other models, they each include schools in localities that have MLPA-21 laws. Smaller discontinuities for these samples suggest that MLPA-21s are effective in reducing e-cigarette consumption among otherwise legal 18-year-olds, consistent with Bryan et al. (2021). Additionally, by including these groups, we are treating areas with MLPA-21 laws in the same way as those with MLPA-18 laws. Hence, we introduce misclassification into these analyses given that the sharp discontinuity in legality between age 17 and 18 years does not exist in these localities, which would bias our results towards zero.

In Appendix Table A6, we directly test for a discontinuity at age 18 in municipalities and states that enact MLPA-19 or MLPA-21 laws. We find that prior to the implementation of MLPA-19 or MLPA-21, these areas have discontinuities similar to our main results at age 18. Recall that these areas had MLPA-18 laws before implementing MLPA-21 so these results make sense. However, post MLPA-21 implementation, we find no discontinuity at age 18. This is consistent with our results in areas in which the law treats those just below and just above age 18 similarly.

In Appendix Table A7, we investigate the possibility of geographic spillovers by separating our post treatment sample into counties that are within 100 miles of an area without a law, and those more than 100 miles. Living near a jurisdiction with no MLPA-18 restriction may allow for cross-border shopping by those in MLPA-18 locations just below the MLPA cutoff and thus may dampen the effectiveness of these laws. For any use, we indeed estimate discontinuities that are smaller and less statistically insignificant in areas closer to states without a law, compared with areas farther than 100 miles, suggesting a potential role of cross-border shopping. These estimates are somewhat dependent on specification. However, the effects for frequent use are actually reversed, with larger effects for nearby areas and smaller effects for areas further away from a state without MLPA laws. While we cannot rule out that this result is due to noise, it would suggest different geographic spillovers for casual compared to frequent users.

Finally, when estimating our models separately for states that had laws in place for less vs. more than a year in Appendix Table A8, we find much smaller and non-significant point estimates during the first year of the law's implementation. This is a sensible finding suggestion that the roll-out of laws was not clear, information lacking, or enforcement was not instantaneous. However, discontinuities after the first year are strong and consistent with our main results. This is evidence of growth in the treatment effect over time. The results are available upon request.

5. Conclusion

Our analysis provides strong evidence that MLPA-18 laws—first enacted on a state-by-state basis and then at the federal level as of August 2016—substantially decreased underage e-cigarette consumption in the United States. Using an RD design that examines MTF high school seniors within eight months of their 18^{th} birthdays, we find that MLPA-18 laws reduce e-cigarette consumption markedly, both at the extensive and intensive margin. We specifically estimate statistically significant increases of at least 15% in *any use* and as much as 40% in *frequent use* immediately upon attaining legal purchase status. Contrastingly, changes in e-cigarette use upon turning 18 in states without MLPA-18 laws are statistically insignificant and either a fraction of the magnitude of those in states with laws or negatively signed (or both). These results hold for a variety of model specifications, covariate sets, and sample inclusion criteria.

Although the MLPA for all tobacco products increased to 21 nationwide in late 2019, our study contributes meaningfully to a better understanding of e-cigarette policy worldwide. Currently only 45 countries have MLPAs for e-cigarettes (Global Tobacco Control 2021). This paper is one of the first evaluations of the effectiveness of MLPA-18 laws in nationally representative data; it is also the first to apply an RD framework that has been used productively in the economics literature on minimum legal drinking age laws. Our investigation remains comparatively distinctive from the MLPA-21 literature in that the enactment of MLPA-18 laws for e-cigarettes did not alter the existing age restriction for traditional cigarette purchases, making it clear that any response in e-cigarette consumption operates strictly through reduced access to e-cigarettes.

A potential direction for tobacco MLPA research, therefore, involves implementing analogous RD frameworks to study the early state and recent federal MLPA-21 laws. While sample sizes might be small in the near term, such an approach could possibly be executed in a variety of data sets, including the longitudinal portion of the MTF, the BRFSS, and the National Health Interview Survey. A comparison of RD effects from before to after MLPA-21 laws were adopted would potentially isolate their effects from those of the longstanding U.S. drinking age of 21.

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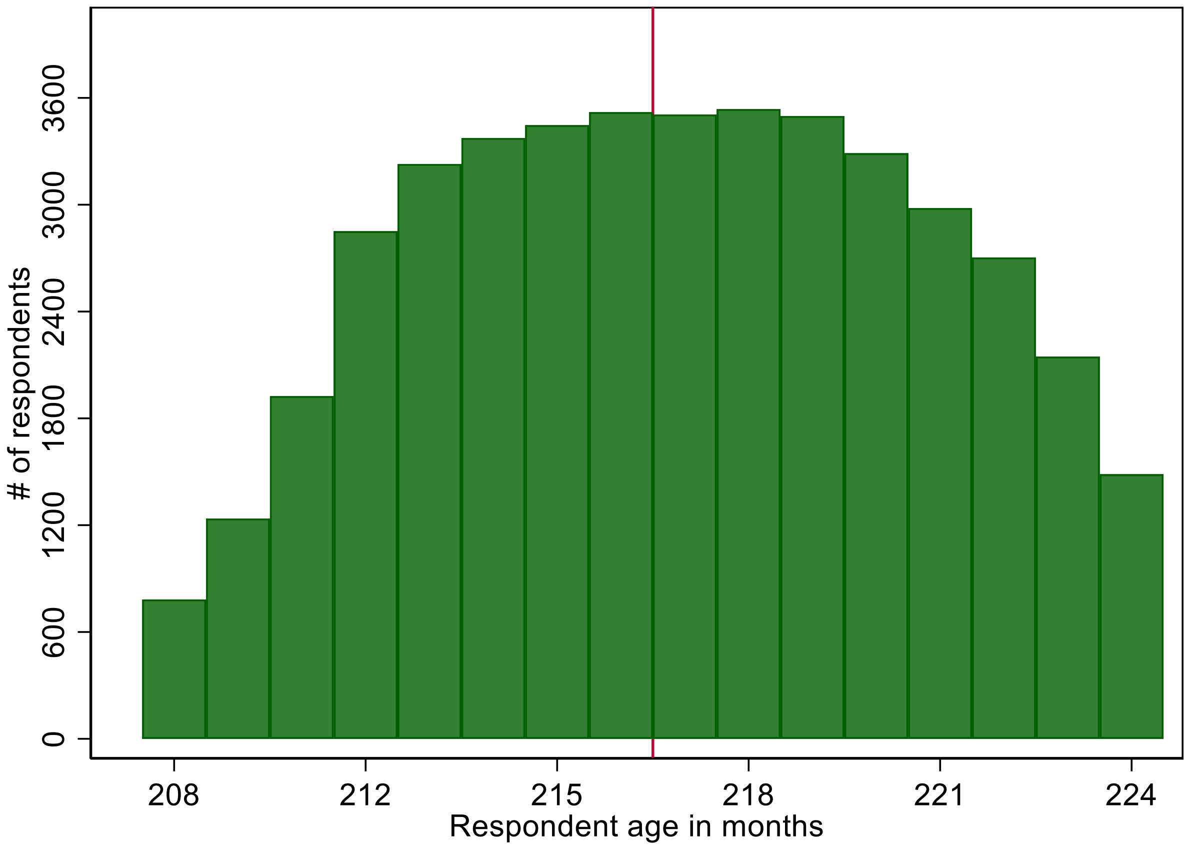
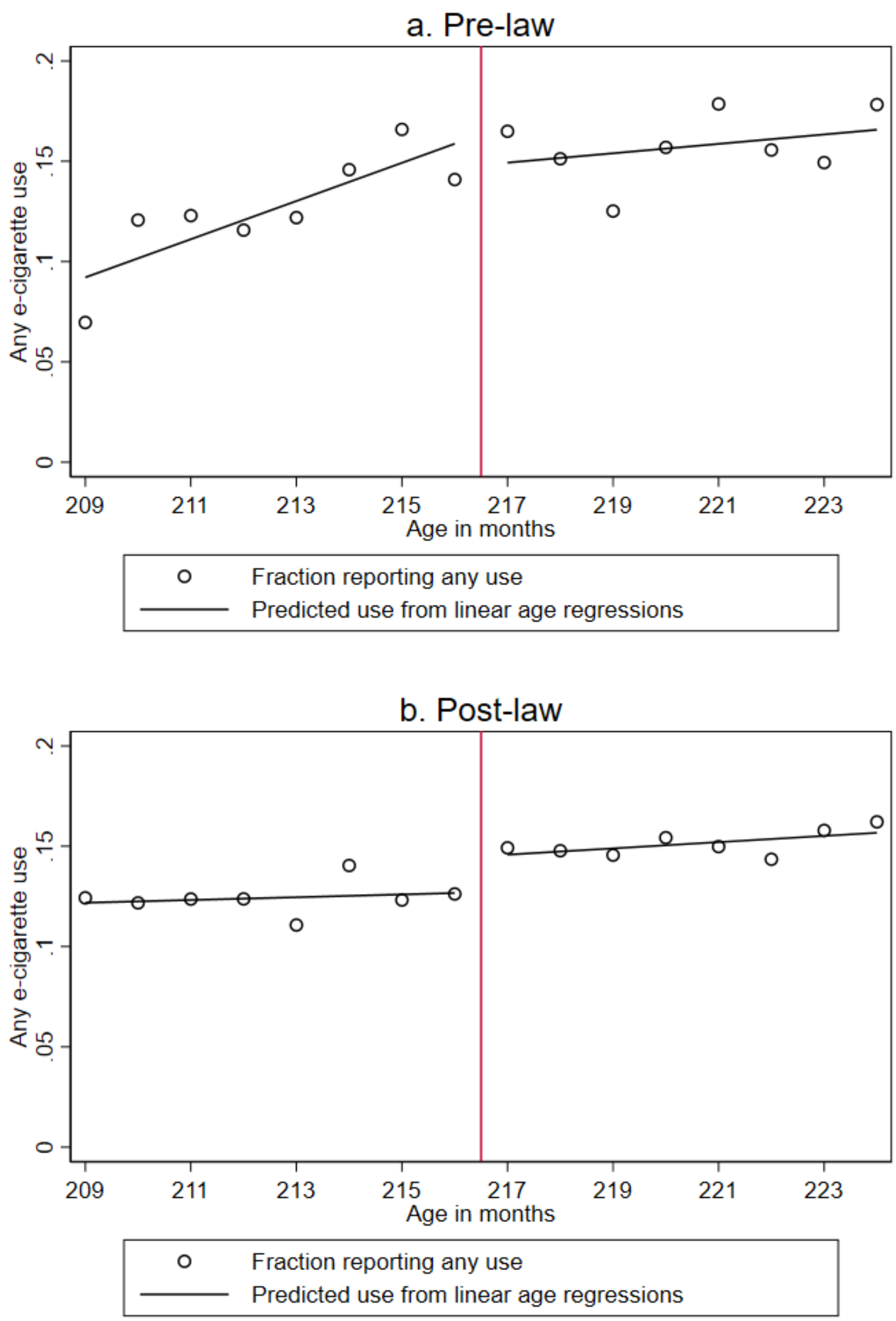
Figure 1 – Sample Respondents by Age



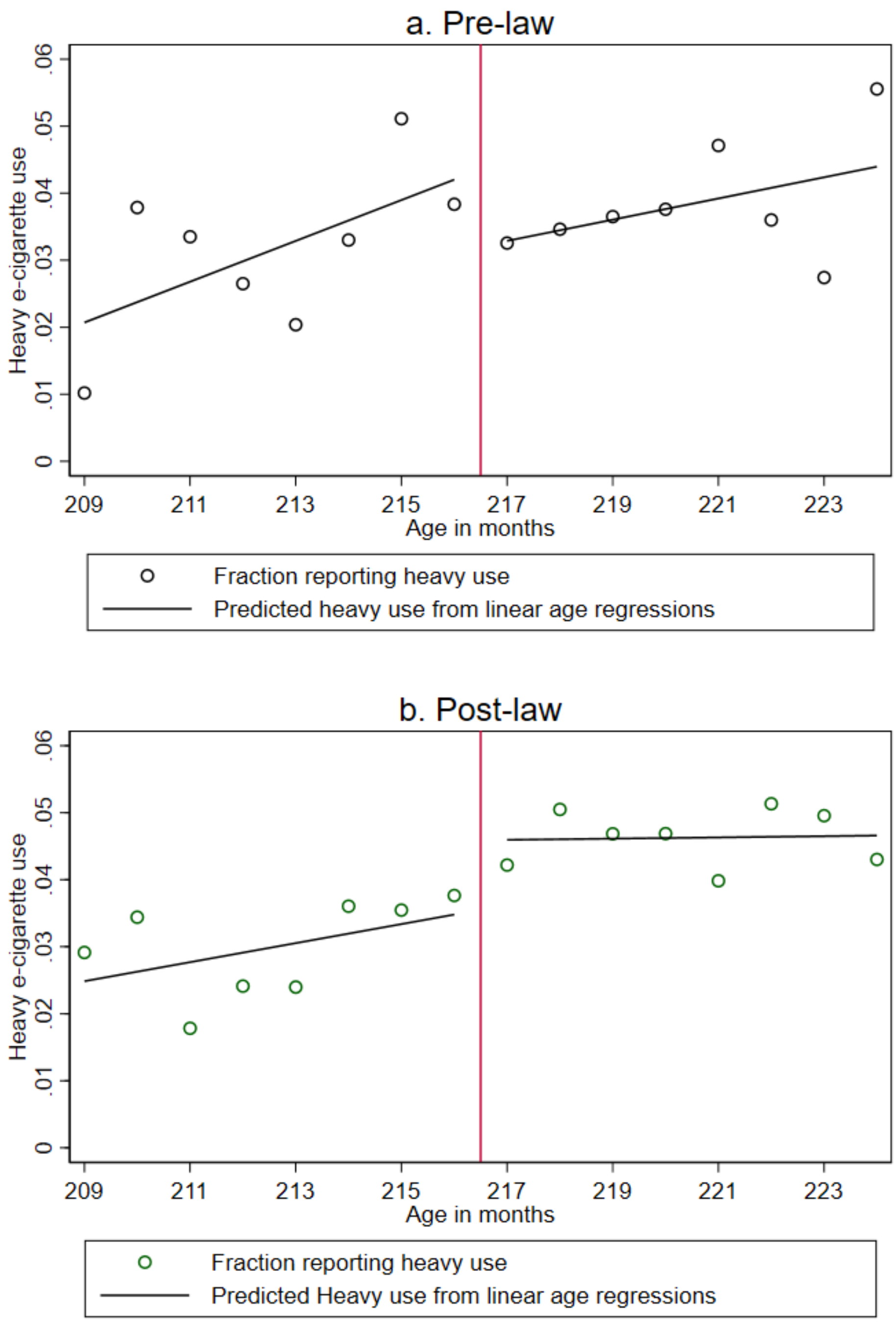
Counts represent the combined number of respondents in the pre-law and post-law samples at each age in months, where age is defined in terms of calendar months as of the interview month. For instance, a respondent born in March 1999 and interviewed in March 2017 is categorized as 216 months old. Assuming that birth and interview dates are distributed uniformly throughout their respective months, the assigned age is in fact the mean age of respondents at the day-of-age level within a two-month range: in this example, the youngest respondents (born March 31, interviewed March 1) are just over 215 months old, whereas the oldest respondents (born March 1, interviewed March 31) are just under 217 months old.

Figure 2 – Age 18 Discontinuities in *Any E-cigarette Use* Past Month



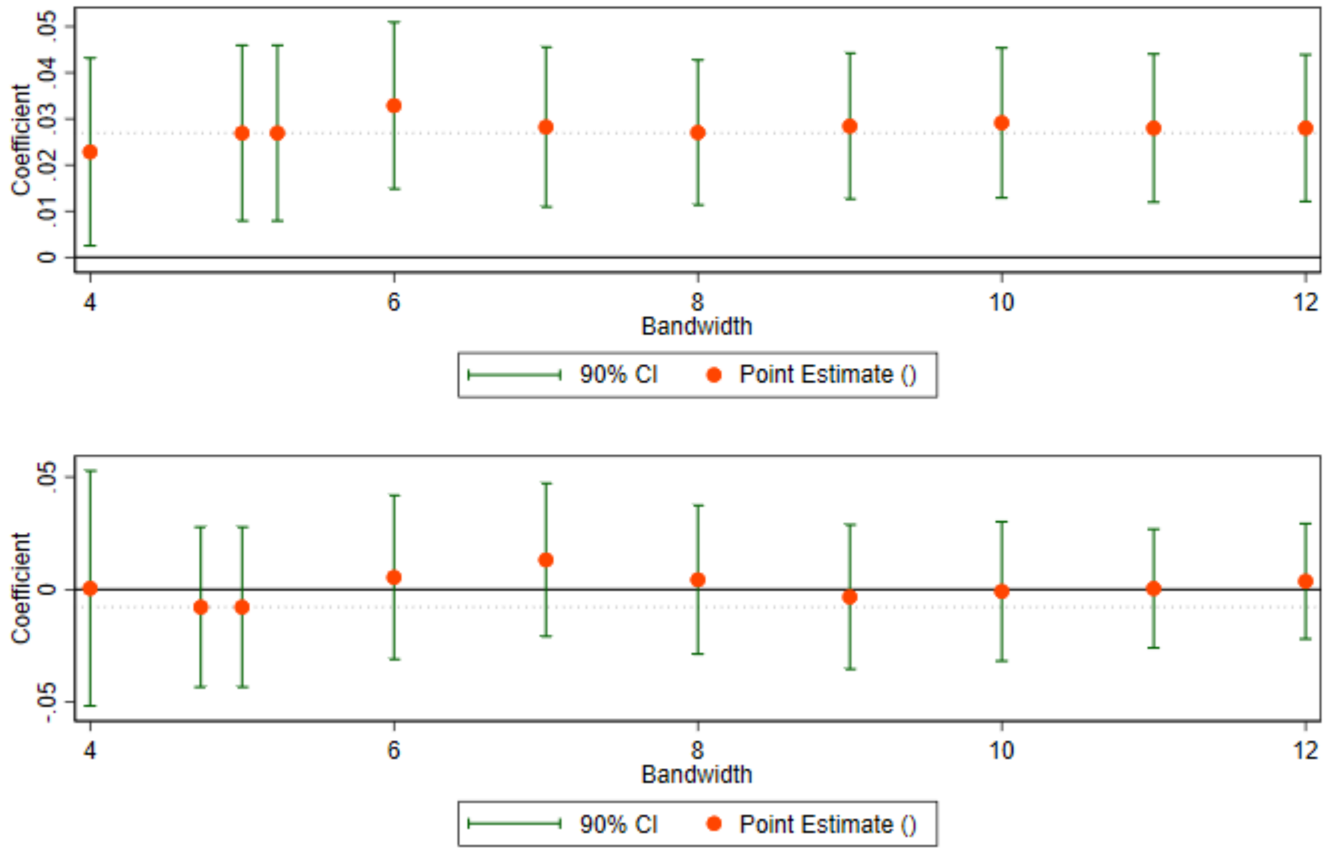
Data represent 5,075 pre-law and 17,106 post-law respondents from the 2014–2017 Monitoring the Future (MTF) surveys of 12th graders interviewed within 8 calendar months of their 18th birthdays, where pre-law (post-law) reflects being surveyed before (after) an age 18 e-cigarette MLPA was enacted in their state. Age represents the number of months in between birth month and interview month. Circles represent MTF survey-weighted mean e-cigarette use in the corresponding month-of-age group, while lines represent predicted use from separate linear age regressions on either side of the MLPA. In these graphs, the MLPA is assumed to affect past month use for respondents age 216 months, roughly half of whom are not yet 18 years old and none of whom are a full month beyond their 18th birthday, but not for respondents age 217 months, all of whom are 18 years old and roughly half of whom are at least a full month beyond their 18th birthday.

Figure 3 – Age 18 Discontinuities in *Frequent E-cigarette Use* Past Month



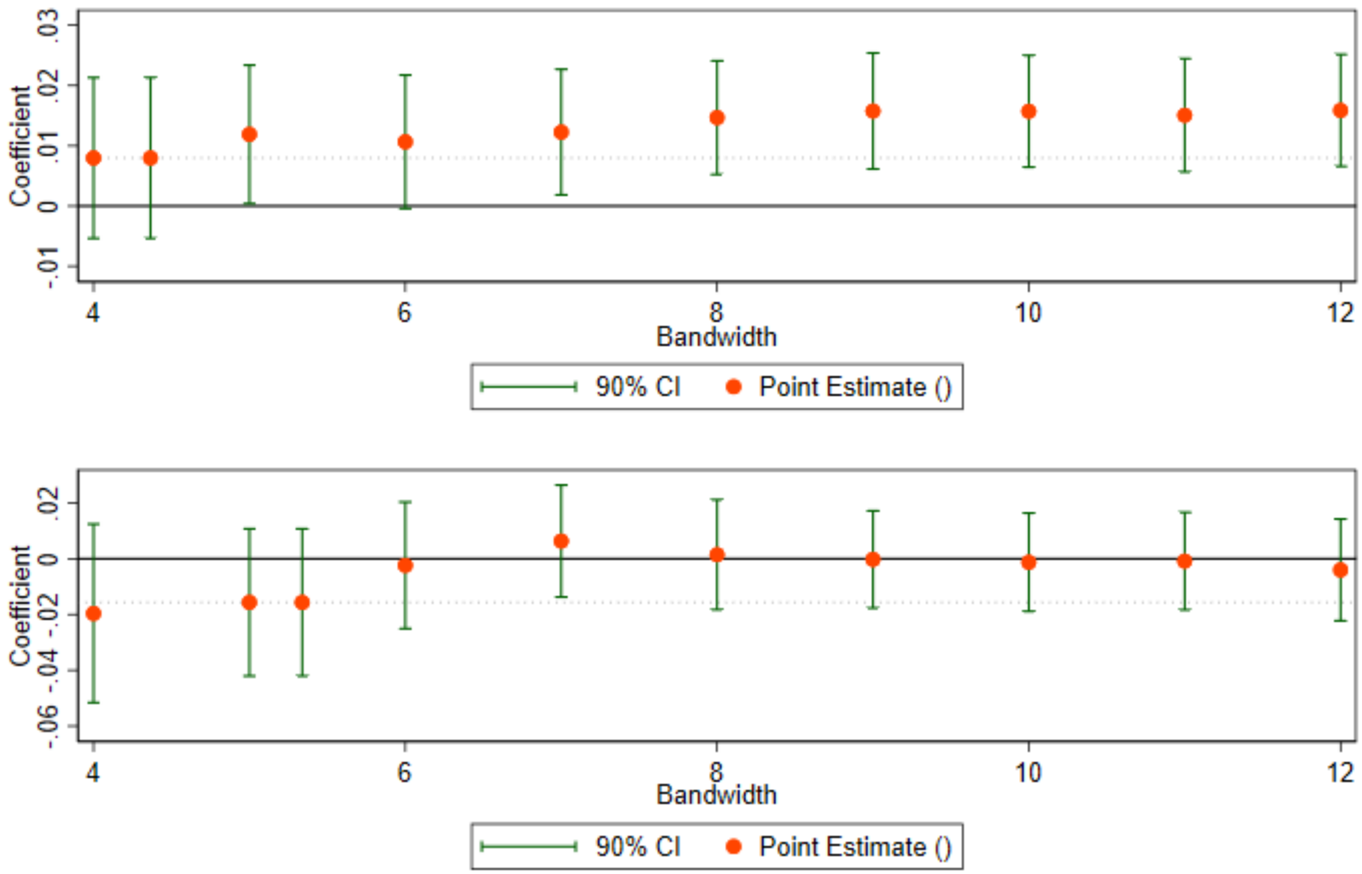
Data represent 5,075 pre-law and 17,106 post-law respondents from the 2014–2017 Monitoring the Future (MTF) surveys of 12th graders interviewed within 8 calendar months of their 18th birthdays, where pre-law (post-law) reflects being surveyed before (after) an age 18 e-cigarette MLPA was enacted in their state. Age represents the number of months in between birth month and interview month. Circles represent the MTF survey-weighted proportion of respondents in the corresponding month-of-age group who used e-cigarettes heavily (on 10+ days in the prior month), while lines represent predicted *frequent use* from separate linear age regressions on either side of the MLPA. In these graphs, the MLPA is assumed to affect past month *frequent use* for respondents age 216 months, but not respondents age 217 months, as explained in the footnote to Figure 2.

Figure 4. Age 18 Discontinuity in *Any E-Cigarette Use* Varying Bandwidth, Uniform Kernel



The upper panel represents post-law samples while the lower panel represents pre-law samples. *Any use* represents e-cigarette use or nicotine vaping on at least one of the past 30 days. Data are from the 2014–2017 Monitoring the Future (MTF) surveys of high school seniors. Pre-law observations are from states in years prior to MLPA-18 laws for e-cigarettes, either by the state during 2014–2016, or ultimately federally between the 2016 and 2017 surveys. Orange dots represent point estimates from local linear regressions varying the age in month bandwidth. Additionally, data driven bandwidth estimates are included (Calonico et al. 2014; Calonico et al. 2017). All estimates come from the STATA command rdrobust. Sample sizes, point estimates and standard errors are provided in Appendix Table A1. Additional controls include indicators for year, state, gender, and race, and are weighted using MTF sampling weights. Standard errors are clustered by state.

Figure 5. Age 18 Discontinuity in *Frequent E-Cigarette Use* Varying Bandwidth, Uniform Kernel



The upper panel represents post-law samples while the lower panel represents pre-law samples. *Frequent use* represents e-cigarette use or nicotine vaping on at least 10 of the past 30 days. Data are from the 2014–2017 Monitoring the Future (MTF) surveys of high school seniors. Pre-law observations are from states in years prior to MLPA-18 laws for e-cigarettes, either by the state during 2014–2016, or ultimately federally between the 2016 and 2017 surveys. Orange dots represent point estimates from local linear regressions varying the age in month bandwidth. Additionally, data driven bandwidth estimates are included (Calonico et al. 2014; Calonico et al. 2017). All estimates come from the STATA command rdrobust. Sample sizes, point estimates and standard errors are provided in Appendix Table A2. Additional controls include indicators for year, state, gender, and race, and are weighted using MTF sampling weights. Standard errors are clustered by state.

Table 1 – State-Level Age 18 E-Cigarette MLPA Adoption Dates

State	Date Law Adopted	State	Date Law Adopted
New Jersey	March 12, 2010 (age 19)	**Nebraska**	**April 9, 2014**
Utah	May 11, 2010 (age 19)	**Kentucky**	**April 10, 2014**
New Hampshire	July 31, 2010	**Louisiana**	**May 28, 2014**
Minnesota	August 1, 2010	**West Virginia**	**June 6, 2014**
California	September 27, 2010	**Delaware**	**June 12, 2014**
Colorado	March 25, 2011	**Rhode Island**	**June 30, 2014**
Tennessee	July 1, 2011	**Florida**	**July 1, 2014**
Wisconsin	April 20, 2012	**Georgia**	**July 1, 2014**
Idaho	July 1, 2012	**Iowa**	**July 1, 2014**
Kansas	July 1, 2012	**South Dakota**	**July 1, 2014**
Alaska	August 22, 2012	**Virginia**	**July 1, 2014**
Maryland	October 1, 2012	**Ohio**	**August 2, 2014**
New York	January 1, 2013	**Missouri**	**October 10, 2014**
Wyoming	March 13, 2013	**Oklahoma**	**November 1, 2014**
South Carolina	June 7, 2013	**New Mexico**	**June 19, 2015**
Hawaii	June 27, 2013	**Maine**	**July 4, 2015**
Indiana	July 1, 2013	**North Dakota**	**August 1, 2015**
Mississippi	July 1, 2013	**Massachusetts**	**September 25, 2015**
Vermont	July 1, 2013	**Connecticut**	**October 1, 2015**
Washington	July 28, 2013	**District of Columbia**	**October 1, 2015**
Alabama	August 1, 2013 (age 19)	**Nevada**	**October 1, 2015**
North Carolina	August 1, 2013	**Texas**	**October 1, 2015**
Arkansas	August 16, 2013	**Montana**	**January 1, 2016**
Arizona	September 13, 2013	**Oregon**	**January 1, 2016**
Illinois	January 1, 2014	**Michigan**	**August 8, 2016 (U.S.)**
		Pennsylvania	**August 8, 2016 (U.S.)**

Localities in our sample outside of Massachusetts (which we exclude from our main analysis) that adopted MLPA-18 laws prior to their states doing so are Lewisville, TX (January 27, 2014), Santa Fe, NM (February 12, 2014), and Philadelphia, PA (April 8, 2014). Michigan and Pennsylvania did not have state-level e-cigarette MLPAs until the federal MLPA was adopted. Alabama, New Jersey, and Utah MLPA laws applied to those under age 19. California additionally adopted an MLPA-21 law June 9, 2016. **States in bold and underlined** enacted policies during our sample period.

Table 2 – Age 18 Discontinuities in E-Cigarette Use

	(1)	(2)	(3)	(4)	(5)	(6)
<u>Pre-law</u>						
Any use	0.004	0.007	0.009	0.012	–0.018	–0.027
[Mean = 0.168]	(0.020)	(0.019)	(0.031)	(0.031)	(0.023)	(0.023)
Frequent use	0.001	–0.005	0.008	0.0002	0.004	–0.003
[Mean = 0.043]	(0.012)	(0.013)	(0.012)	(0.015)	(0.013)	(0.015)
Sample size	5,075		4,683		4,647	
<u>Post-law</u>						
Any use	0.027***	0.029***	0.029***	0.033***	0.027**	0.029***
[Mean = 0.143]	(0.010)	(0.009)	(0.010)	(0.010)	(0.011)	(0.010)
Frequent use	0.015**	0.011*	0.018***	0.014*	0.018***	0.016***
[Mean = 0.039]	(0.006)	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)
Sample size	17,106		15,715		15,746	
Triangular kernel?	No	Yes	No	Yes	No	Yes
Omit age 216 months?	No	No	Yes	Yes	No	No
Omit age 217 months?	No	No	No	No	Yes	Yes

Any use represents e-cigarette use or nicotine vaping on at least one of the past 30 days while *frequent use* represents use on at least 10 of the past 30 days. Data are from the 2014–2017 Monitoring the Future (MTF) surveys of high school seniors. Pre-law observations are from states in years prior to MLPA-18 laws for e-cigarettes, either by the state during 2014–2016, or ultimately federally between the 2016 and 2017 surveys. Samples include all respondents interviewed within eight months of their 18th birthdays, with the exception of those from localities for which the e-cigarette MLPA is 19 years or older by the survey year. Estimates are coefficients of an indicator that respondents are age 217 months or older, using local linear regressions that control for separate linear functions of age in months through and beyond age 216 months, along with indicators for year, state, gender, and race, and are weighted using MTF sampling weights. All estimates come from the STATA command rdrobust (Calonico et al. 2017). Standard errors clustered by state are in parentheses, with *, **, and *** signifying significance at the 10%, 5%, and 1% levels, respectively.

Table 3 – Age 18 Discontinuities in E-Cigarette Use across Covariate Sets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pre-law (N = 5,075)									
Any use	0.004	0.001	0.002	0.002	0.001	0.004	0.003	0.017	0.014
[Mean = 0.168]	(0.020)	(0.023)	(0.023)	(0.020)	(0.020)	(0.020)	(0.020)	(0.019)	(0.019)
Frequent use	0.001	0.001	0.001	0.0007	0.0003	0.001	0.0008	–0.011	–0.012
[Mean = 0.043]	(0.012)	(0.013)	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)
Post-law (N = 17,106)									
Any use	0.027^{***}	0.026^{*}	0.023	0.024^{**}	0.023^{**}	0.025^{**}	0.027^{***}	0.030^{***}	0.030^{***}
[Mean = 0.143]	(0.010)	(0.015)	(0.014)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)
Frequent use	0.015^{**}	0.014^{*}	0.013^{*}	0.013^{**}	0.013^{**}	0.013^{**}	0.014^{**}	0.010^{*}	0.010^{*}
[Mean = 0.039]	(0.006)	(0.008)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Includes indicators for:									
Year?	Yes	No	Yes	No	Yes	Yes	Yes	Yes	Yes
State?	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Race?	Yes	No	No	No	No	Yes	Yes	Yes	Yes
Gender?	Yes	No	No	No	No	No	Yes	Yes	Yes
Survey month?	No	No	No	No	No	No	Yes	No	Yes
Birth month?	No	No	No	No	No	No	No	Yes	Yes

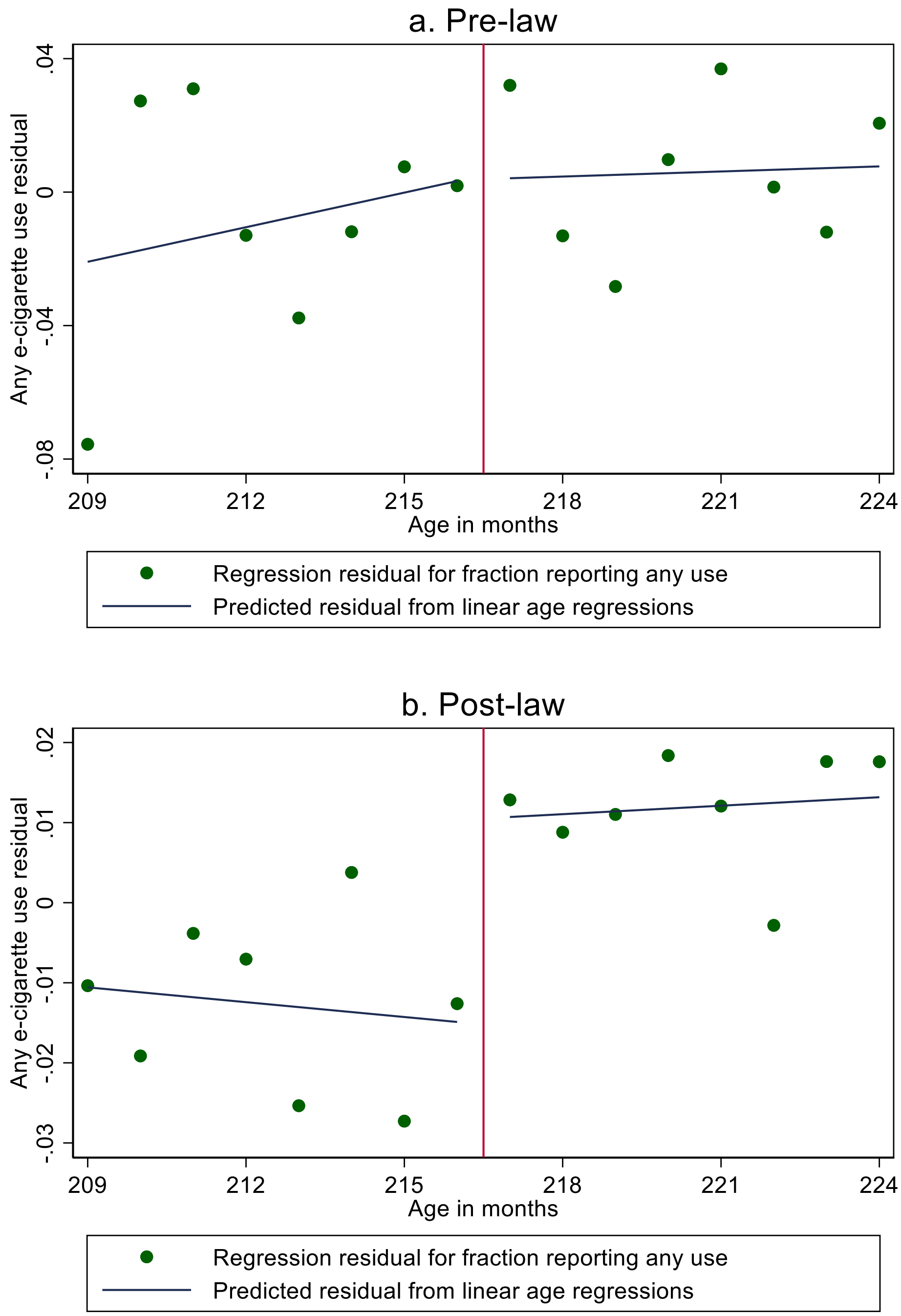
See the Table 2 footnote for details. Regressions correspond to the model used for column 1 of Table 2, so neither exclude respondents ages 216 or 217 months nor specify a triangular kernel.

Table 4 – Age 18 Past Discontinuities in E-Cigarette Use across Samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-law								
Any use	0.004	0.0004	0.004	0.0002	0.004	0.0002	0.004	–0.002
[Mean = 0.168]	(0.020)	(0.019)	(0.020)	(0.019)	(0.020)	(0.019)	(0.020)	(0.018)
Frequent use	0.001	0.003	0.001	0.003	0.001	0.003	0.001	–0.002
[Mean = 0.043]	(0.012)	(0.011)	(0.012)	(0.010)	(0.012)	(0.010)	(0.012)	(0.011)
Sample size	5,075	5,494	5,075	5,524	5,075	5,524	5,075	5,603
Post-law								
Any use	0.027***	0.026***	0.027***	0.021***	0.022***	0.020**	0.021**	0.027***
[Mean = 0.143]	(0.010)	(0.009)	(0.010)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)
Frequent use	0.015**	0.014**	0.015**	0.009*	0.009**	0.011**	0.011**	0.015**
[Mean = 0.039]	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Sample size	17,106	17,437	17,029	19,618	19,287	18,697	18,366	17,179
States with age 19 laws?	Omit	Omit	Omit	Omit	Omit	Omit	Omit	Pre-law
Omit California in 2017?	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Omit zip codes with age 21 laws?	Yes	Yes	Yes	No	No	No	No	Yes
Omit Massachusetts?	Yes	No	Yes	No	Yes	No	Yes	Yes
Omit NYC in 2014 & CLE in 2016?	No	No	Yes	No	No	No	No	No

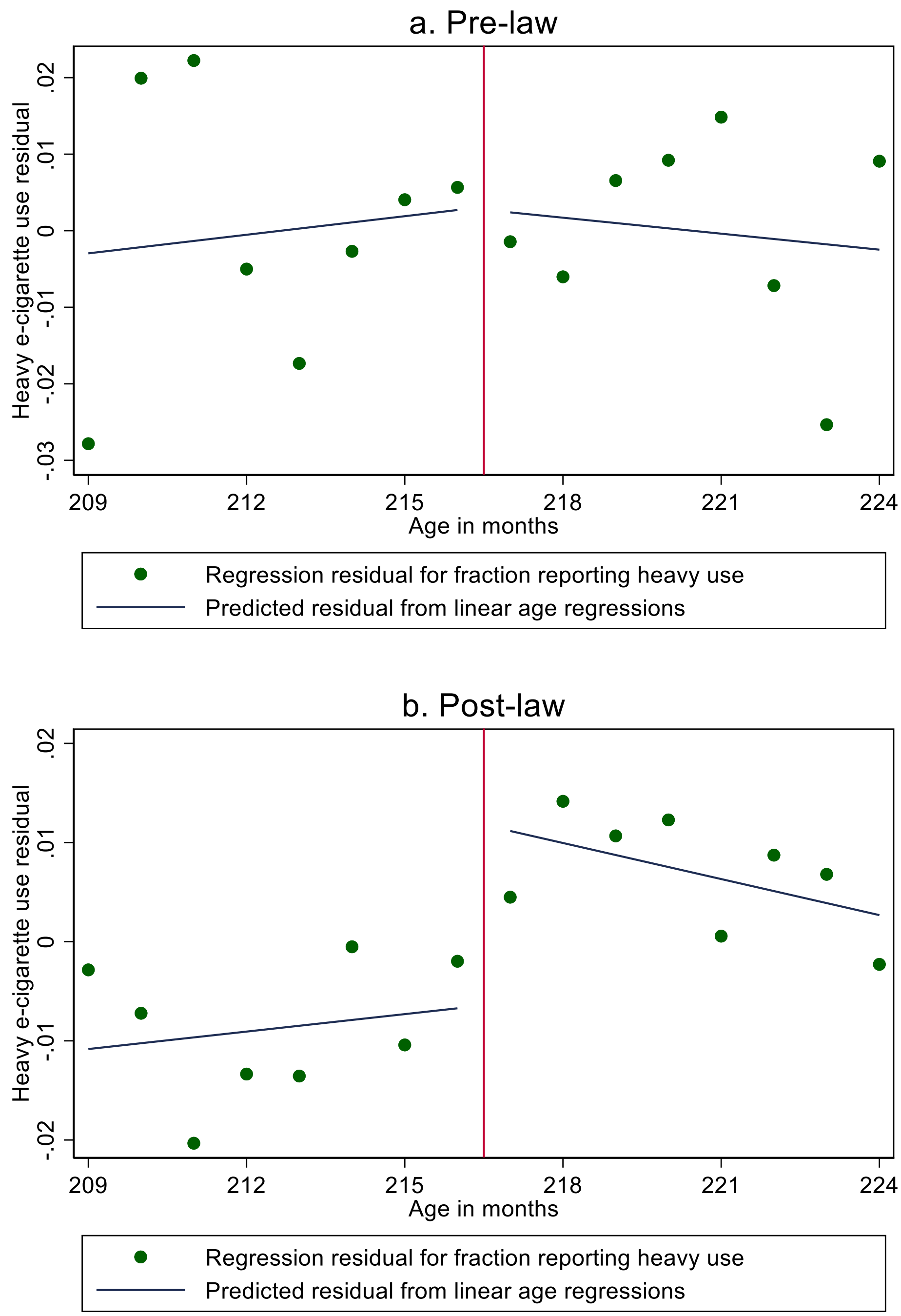
See the Table 2 footnote for details. Regressions correspond to the model used for column 1 of Table 2, so neither exclude respondents ages 216 or 217 months nor specify a triangular kernel. E-cigarette use means are for the column 1 samples; these vary slightly across samples in the remaining columns from 0.162 to 0.168 for *any use* pre-law, 0.141 to 0.143 for *any use* post-law, and 0.041 to 0.043 for *frequent use* pre-law (but are 0.039 for all *frequent use* post-law samples). With particular regard to sample schools, the state e-cigarette MLPA was 19 years in Alabama, New Jersey, and Utah for the entire period, and was 21 years in California for 2017. The MLPA was also 21 in over 150 jurisdictions in Massachusetts throughout the study period, New York City starting in mid-2014, Kansas City (Kansas and Missouri) starting in 2016, Cleveland starting in mid-2016, and Chicago, Eugene (Oregon), and St. Louis (city and county) during 2017.

Figure A1 – Age 18 Discontinuities in *Any E-cigarette Use* Regression Residuals



Data represent 5,075 pre-law and 17,106 post-law respondents from the 2014–2017 Monitoring the Future (MTF) surveys of 12th graders interviewed within 8 calendar months of their 18th birthdays, where pre-law (post-law) reflects being surveyed before (after) an age 18 e-cigarette MLPA was enacted in their state. Age represents the number of months in between birth month and interview month. Circles represent month-of-age group means for residuals from a MTF survey-weighted regression of *any past month e-cigarette use* on indicators for year, state, race, and gender, while lines represent predicted residuals from separate linear regressions of these means on age on either side of the MLPA. In these graphs, the MLPA is assumed to affect past month use for respondents age 216 months, but not respondents age 217 months, as explained in the footnote to Figure 2.

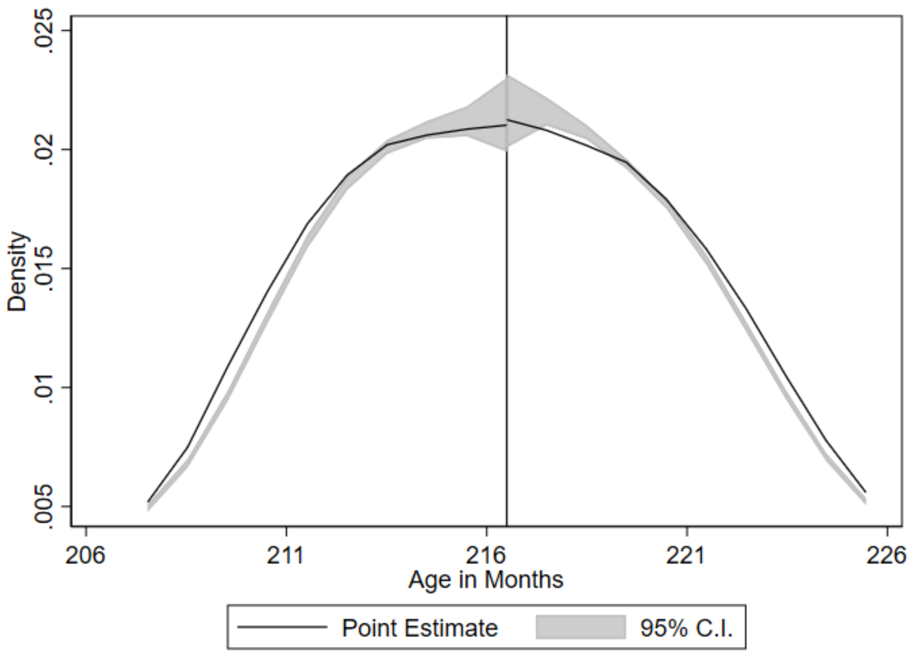
Figure A2 – Age 18 Discontinuities in *Frequent E-cigarette Use* Regression Residuals



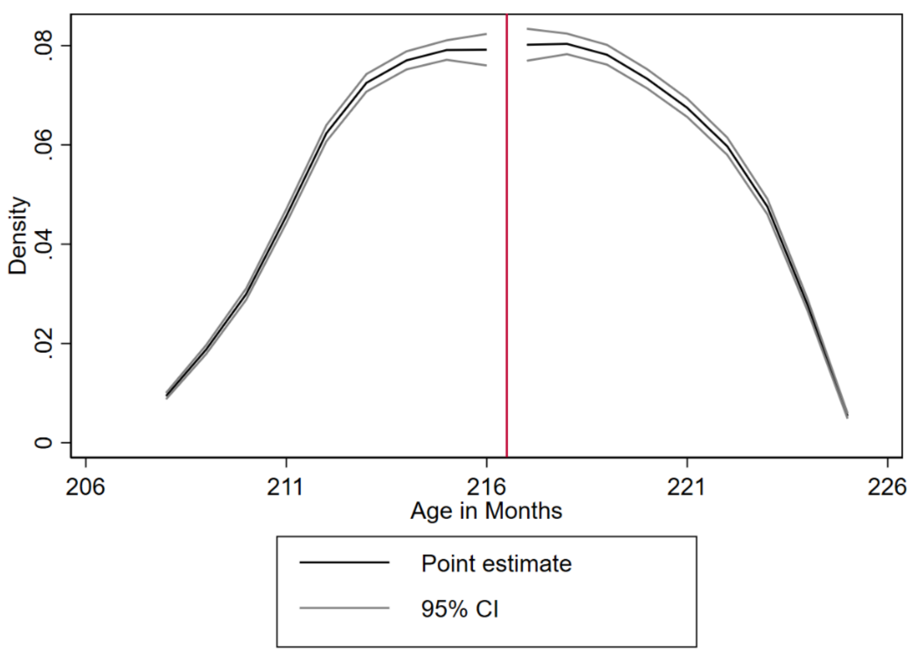
Data represent 5,075 pre-law and 17,106 post-law respondents from the 2014–2017 Monitoring the Future (MTF) surveys of 12th graders interviewed within 8 calendar months of their 18th birthdays, where pre-law (post-law) reflects being surveyed before (after) an age 18 e-cigarette MLPA was enacted in their state. Age represents the number of months in between birth month and interview month. Circles represent month-of-age group means for residuals from a MTF survey-weighted regression of *frequent past month e-cigarette use* (10+ days) on indicators for year, state, race, and gender, while lines represent predicted residuals from separate linear regressions of these means on age on either side of the MLPA. In these graphs, the MLPA is assumed to affect past month *frequent use* for respondents age 216 months, but not respondents age 217 months, as explained in the footnote to Figure 2.

Appendix Figure A3. Discrete Density and McCrary Density Test around Age 18 Cutoff

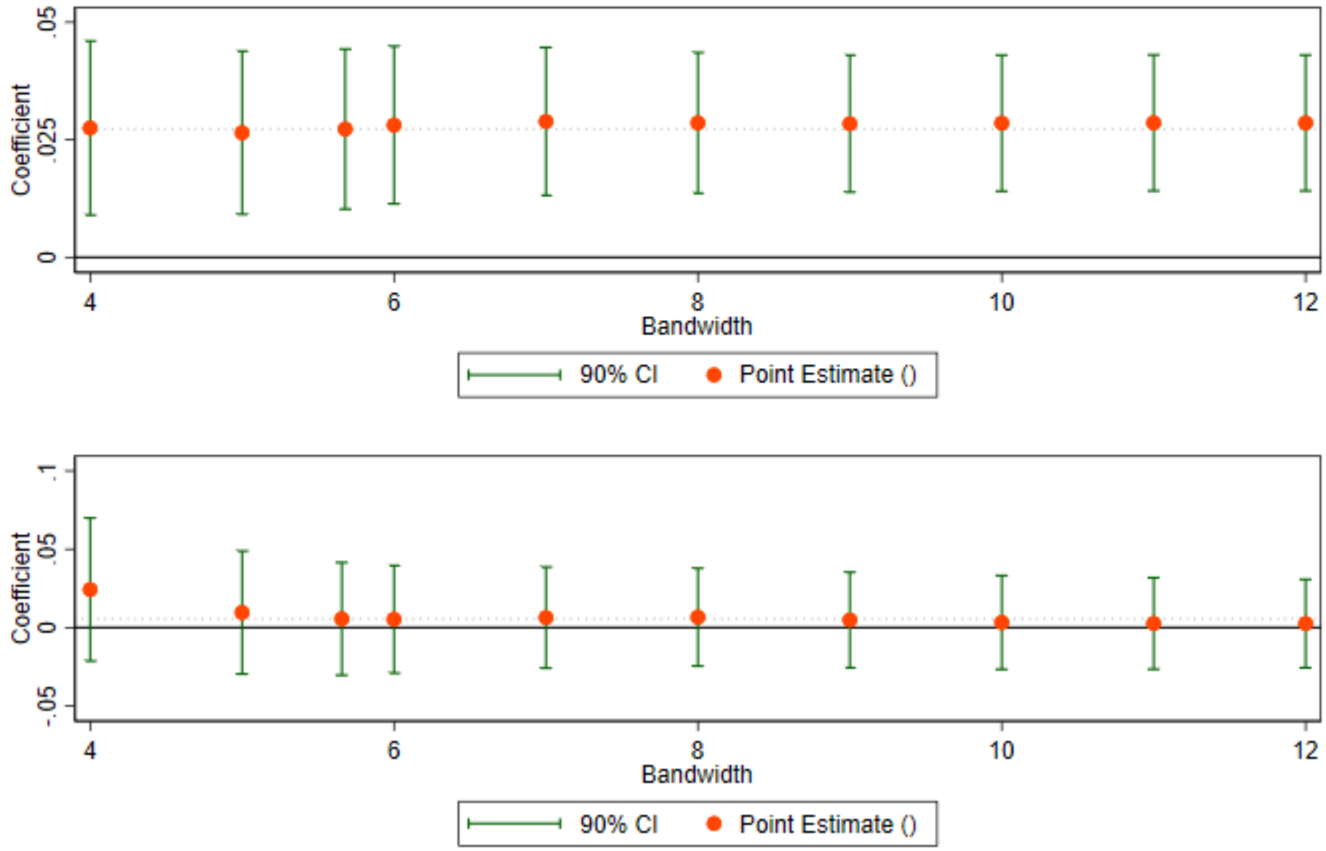
a. Discrete Density Test



b. McCrary Test

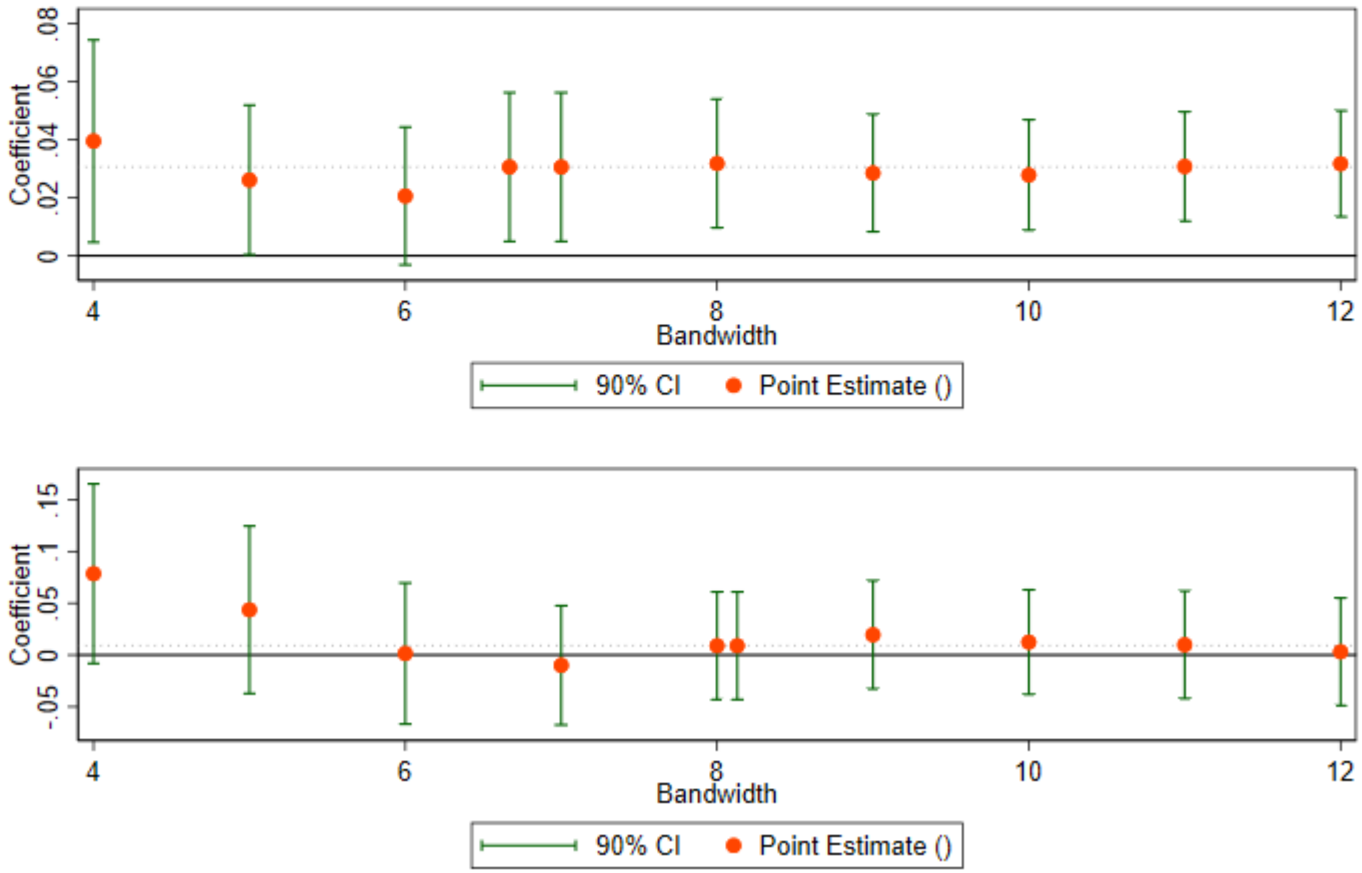


Appendix Figure A4. Age 18 Discontinuity in *Any E-Cigarette Use* Varying Bandwidth, Using Triangular Kernel



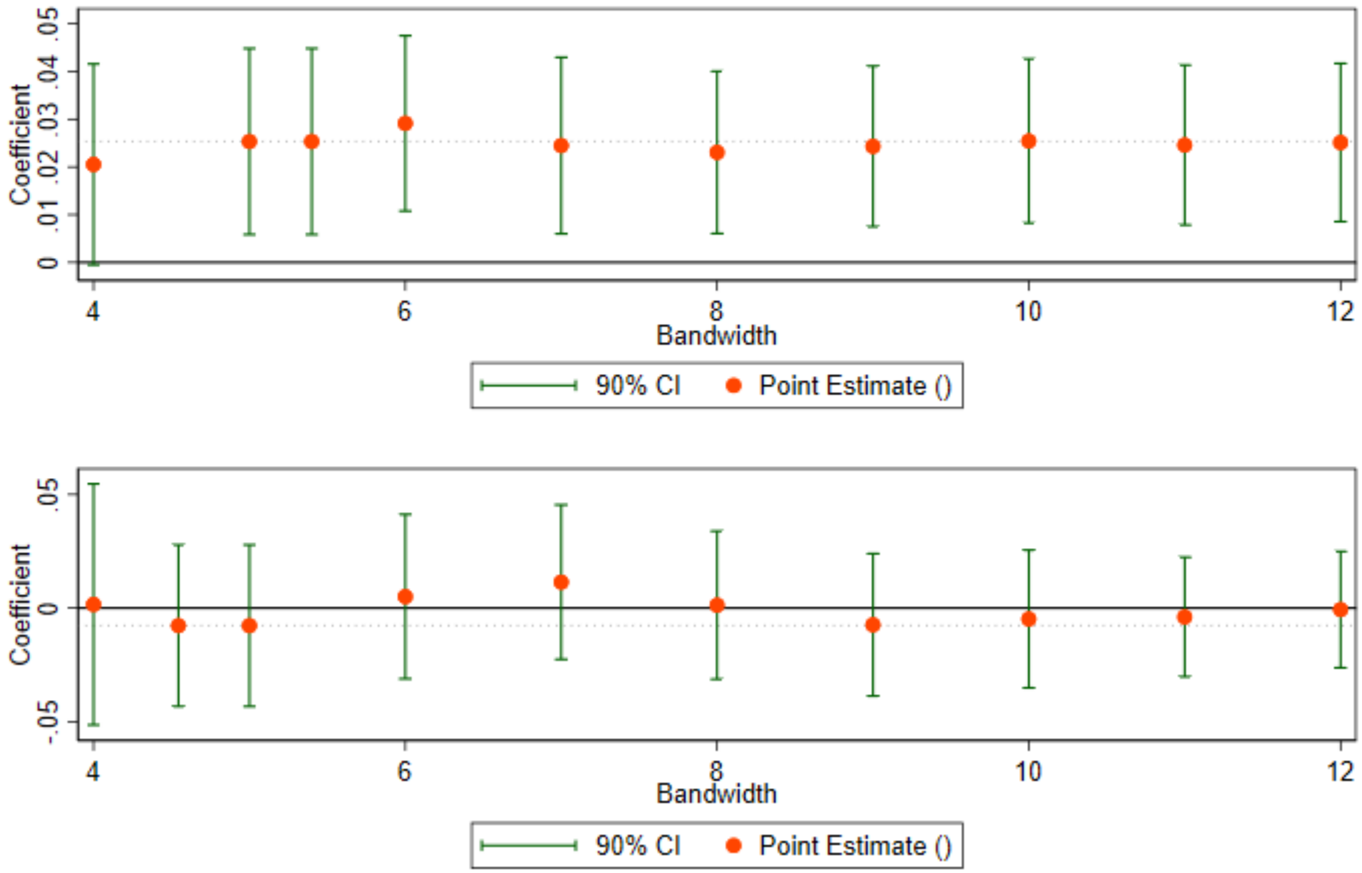
Estimates are the same as in Figure 4, except local linear regressions include a triangular kernel. The upper panel represents post-law samples while the lower panel represents pre-law samples.

Appendix Figure A5. Age 18 Discontinuity in *Any E-Cigarette Use* Varying Bandwidth, Local Quadratic Regressions



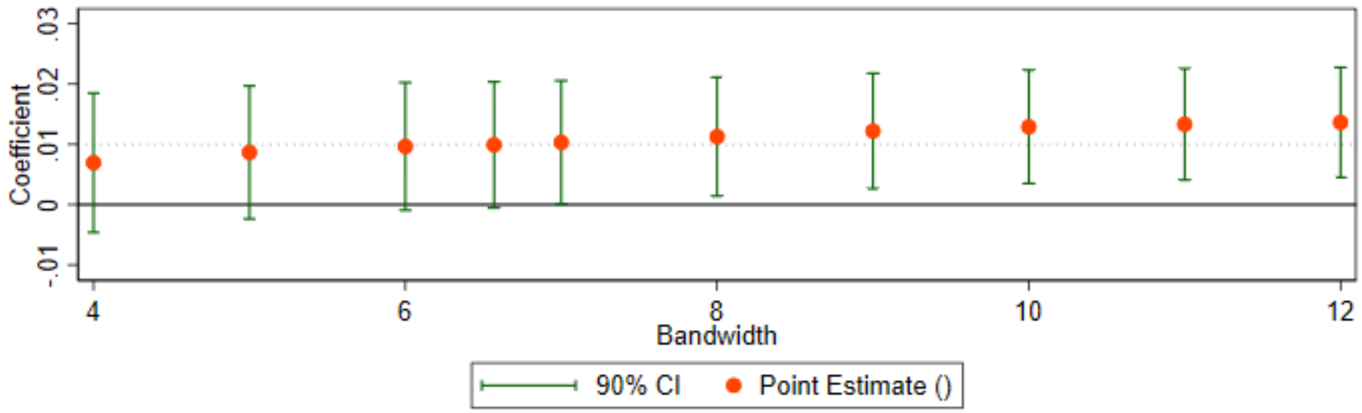
Estimates are the same as in Figure 4, except they are from local quadratic regressions. The upper panel represents post-law samples while the lower panel represents pre-law samples.

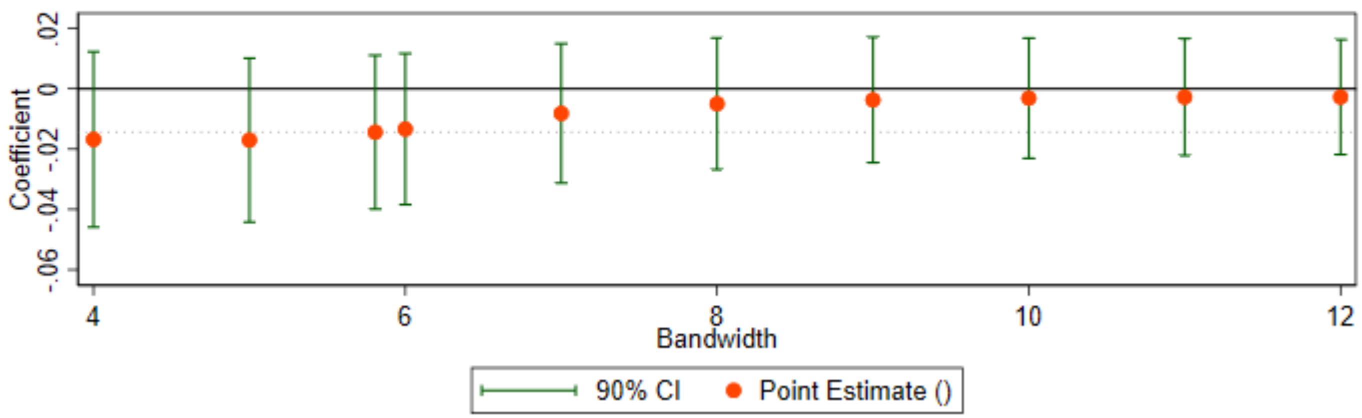
Appendix Figure A6. Age 18 Discontinuity in *Any E-Cigarette Use* Varying Bandwidth, No Covariates



Estimates are the same as in Figure 4, except local linear regressions do not include covariates for race and sex. The upper panel represents post-law samples while the lower panel represents pre-law samples.

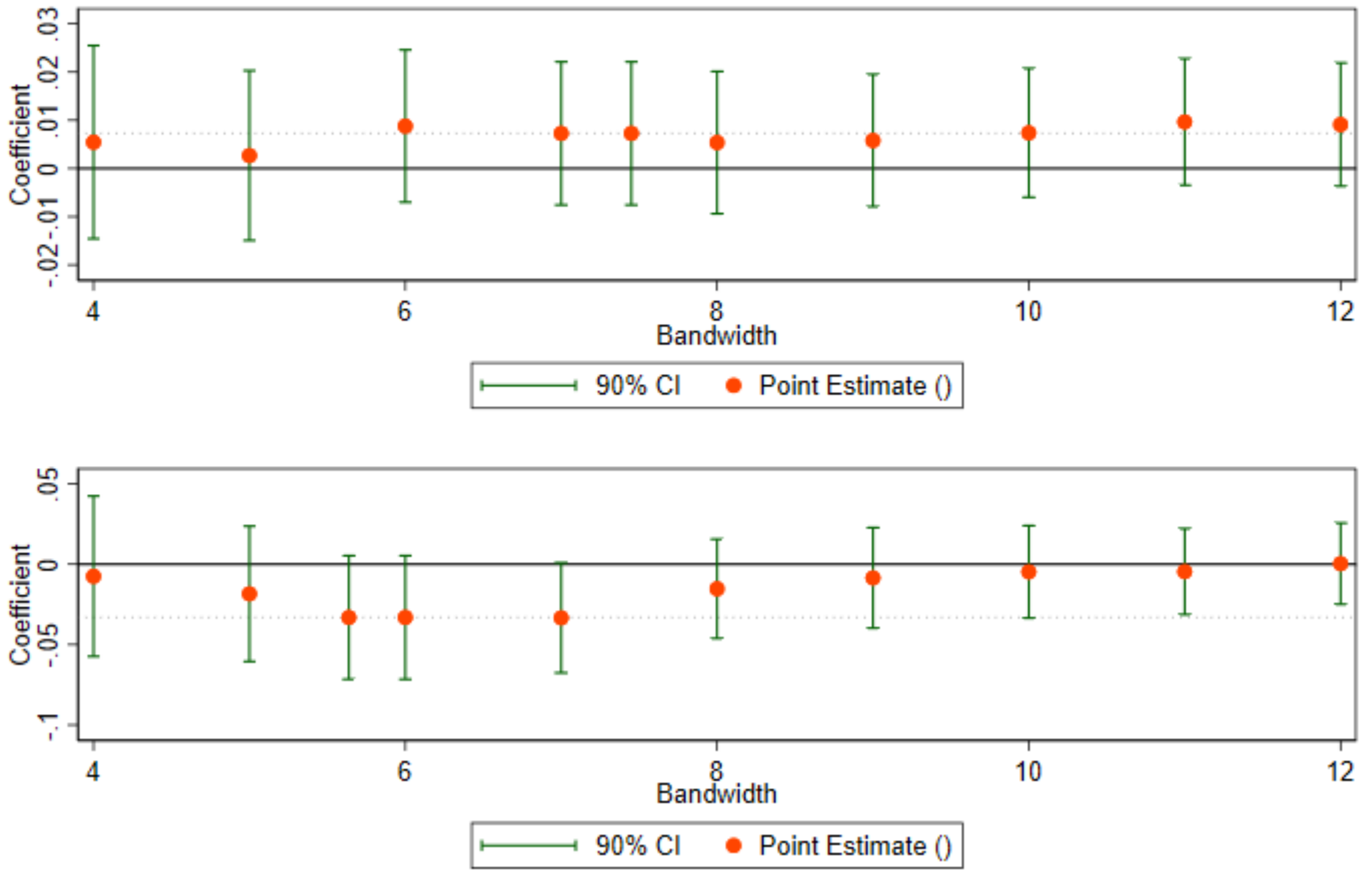
Appendix Figure A7. Age 18 Discontinuity in *Frequent E-Cigarette Use* Varying Bandwidth, Using Triangular Kernel





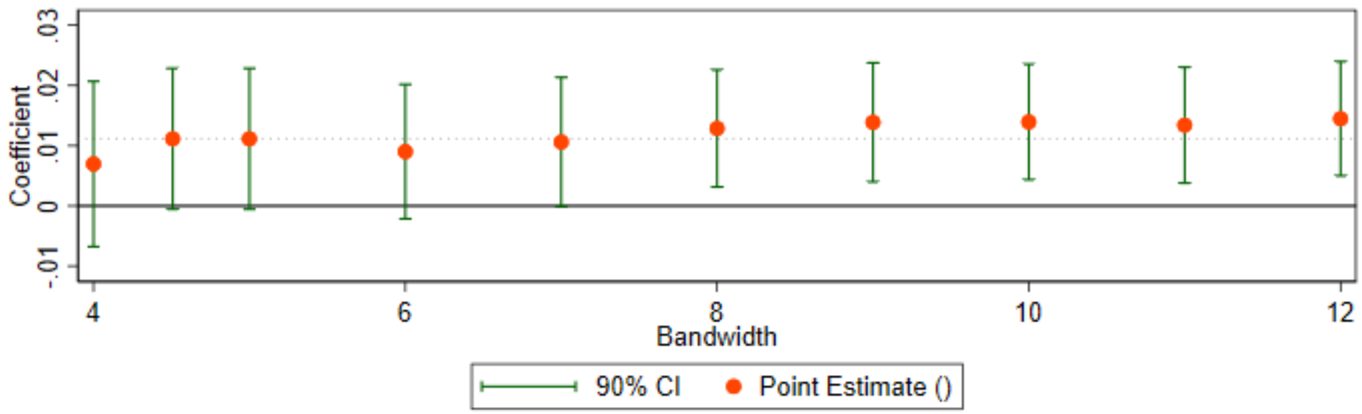
Estimates are the same as in Figure 5, except local linear regressions include a triangular kernel. The upper panel represents post-law samples while the lower panel represents pre-law samples.

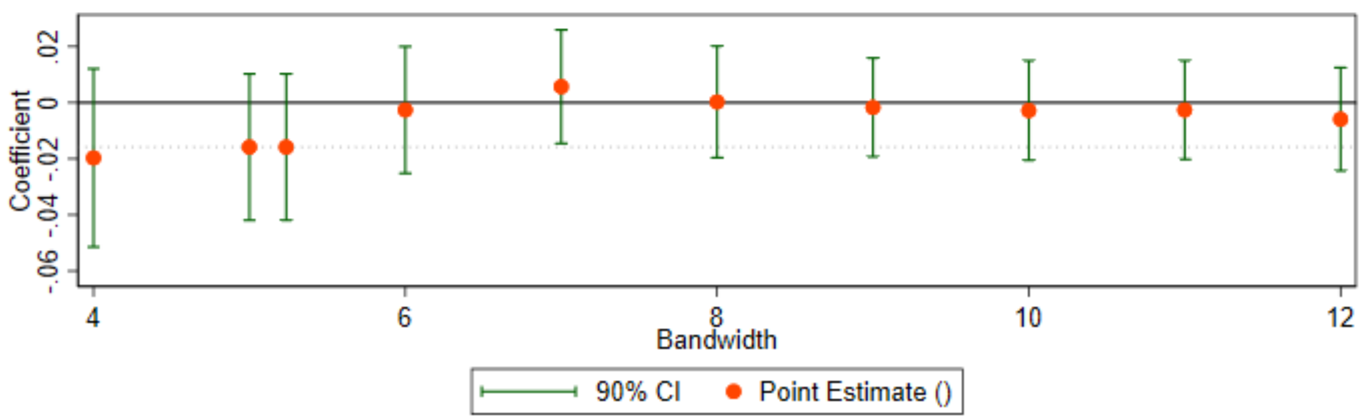
Appendix Figure A8. Age 18 Discontinuity in *Frequent E-Cigarette Use* Varying Bandwidth, Local Quadratic Regressions



Estimates are the same as in Figure 5, except they are from local quadratic regressions. The upper panel represents post-law samples while the lower panel represents pre-law samples.

Appendix Figure A9. Age 18 Discontinuity in *Frequent E-Cigarette Use* Varying Bandwidth, No Covariates





Estimates are the same as in Figure 5, except local linear regressions do not include covariates for race and sex. The upper panel represents post-law samples while the lower panel represents pre-law samples.

Appendix Table A1. Age 18 Discontinuity in *Any E-Cigarette Use* Varying Bandwidth, Kernel, and Local Quadratic Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<u>Pre-law</u>										
Uniform	-0.00782	0.000498	-0.00782	0.00540	0.0132	0.00434	-0.00332	-0.0008	0.000439	0.00366
	(0.0216)	(0.0317)	(0.0216)	(0.0221)	(0.0207)	(0.0201)	(0.0195)	(0.0188)	(0.0160)	(0.0156)
Observations	3893	3189	3893	4453	4820	5075	5248	5362	5441	5515
Triangular	0.00554	0.0243	0.00959	0.00526	0.00635	0.00661	0.00483	0.00326	0.00261	0.00254
	(0.0218)	(0.0277)	(0.0239)	(0.0209)	(0.0196)	(0.0190)	(0.0185)	(0.0182)	(0.0178)	(0.0171)
Observations	4453	3189	3893	4453	4820	5075	5248	5362	5441	5515
Quadratic	0.00893	0.0785	0.0436	0.00140	-0.01000	0.00893	0.0195	0.0124	0.00982	0.00308
	(0.0316)	(0.0527)	(0.0492)	(0.0414)	(0.0350)	(0.0316)	(0.0318)	(0.0307)	(0.0316)	(0.0316)
Observations	5075	3189	3893	4453	4820	5075	5248	5362	5441	5515
<u>Post-law</u>										
Uniform	0.0269**	0.0228*	0.0269**	0.0329***	0.028***	0.027***	0.028***	0.029***	0.028***	0.028***
	(0.0116)	(0.0124)	(0.0116)	(0.0110)	(0.0105)	(0.00955)	(0.00960)	(0.00988)	(0.00976)	(0.00967)
Observations	13074	10669	13074	14868	16227	17106	17650	18013	18246	18453
Triangular	0.027***	0.028**	0.027**	0.028***	0.029***	0.029***	0.028***	0.029***	0.029***	0.029***
	(0.0103)	(0.0113)	(0.0106)	(0.0102)	(0.00957)	(0.00909)	(0.00886)	(0.00880)	(0.00879)	(0.00878)
Observations	14868	10669	13074	14868	16227	17106	17650	18013	18246	18453
Quadratic	0.0306**	0.0395*	0.0261*	0.0206	0.0306**	0.0318**	0.0285**	0.0278**	0.031***	0.032***
	(0.0156)	(0.0212)	(0.0157)	(0.0144)	(0.0156)	(0.0135)	(0.0123)	(0.0116)	(0.0115)	(0.0111)
Observations	16227	10669	13074	14868	16227	17106	17650	18013	18246	18453
Bandwidth in Months	Data Driven	4	5	6	7	8	9	10	11	12

This table provides sample sizes, point estimates and standard errors for Figure 4, Appendix Figure A4, and Appendix Figure A5. All specifications are the same as in column (1) of Table 2 with the following exceptions: Uniform and Triangular refer to the kernel used in local linear regressions, Quadratic refers to results estimated using a local quadratic function, and the bandwidth varies as specified in the final row.

Appendix Table A2. Age 18 Discontinuity in *Frequent E-Cigarette Use* Varying Bandwidth, Kernel, and Local Quadratic Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre-law										
Uniform	-0.0157	-0.0196	-0.0157	-0.00238	0.00632	0.00143	-0.0003	-0.00127	-0.0009	-0.00402
	(0.0160)	(0.0195)	(0.0160)	(0.0138)	(0.0122)	(0.0120)	(0.0106)	(0.0107)	(0.0106)	(0.0111)
Observations	3893	3189	3893	4453	4820	5075	5248	5362	5441	5515
Triangular	-0.0144	-0.0168	-0.0170	-0.0134	-0.00816	-0.00498	-0.00371	-0.00313	-0.00277	-0.00276
	(0.0155)	(0.0177)	(0.0165)	(0.0152)	(0.0140)	(0.0133)	(0.0126)	(0.0121)	(0.0118)	(0.0116)
Observations	4453	3189	3893	4453	4820	5075	5248	5362	5441	5515
Quadratic	-0.0331	-0.00748	-0.0184	-0.0331	-0.0334	-0.0153	-0.00848	-0.00473	-0.00458	0.000356
	(0.0234)	(0.0303)	(0.0256)	(0.0234)	(0.0208)	(0.0188)	(0.0190)	(0.0175)	(0.0163)	(0.0154)
Observations	4453	3189	3893	4453	4820	5075	5248	5362	5441	5515
Post-law										
Uniform	0.00797	0.00797	0.0119*	0.0107	0.0123*	0.0147**	0.0158***	0.0157***	0.0151***	0.0159***
	(0.00811)	(0.00811)	(0.00699)	(0.00673)	(0.00634)	(0.00573)	(0.00585)	(0.00564)	(0.00572)	(0.00565)
Observations	10669	10669	13074	14868	16227	17106	17650	18013	18246	18453
Triangular	0.00992	0.00694	0.00867	0.00966	0.0103*	0.0113*	0.0122**	0.0129**	0.0133**	0.0136**
	(0.00636)	(0.00702)	(0.00671)	(0.00644)	(0.00622)	(0.00599)	(0.00583)	(0.00571)	(0.00562)	(0.00555)
Observations	16227	10669	13074	14868	16227	17106	17650	18013	18246	18453
Quadratic	0.00727	0.00546	0.00268	0.00879	0.00727	0.00537	0.00580	0.00739	0.00966	0.00910
	(0.00902)	(0.0122)	(0.0107)	(0.00961)	(0.00902)	(0.00897)	(0.00834)	(0.00812)	(0.00798)	(0.00777)
Observations	16227	10669	13074	14868	16227	17106	17650	18013	18246	18453
Bandwidth in Months	Data Driven	4	5	6	7	8	9	10	11	12

This table provides sample sizes, point estimates and standard errors for Figure 5, Appendix Figure A7, and Appendix Figure A8. All specifications are the same as in column (1) of Table 2 with the following exceptions: Uniform and Triangular refer to the kernel used in local linear regressions, Quadratic refers to results estimated using a local quadratic function, and the bandwidth varies as specified in the final row.

Appendix Table A3. Age 18 Discontinuities in E-Cigarette Use using Randomized Local Inference

	(1)	(2)	(3)	(4)
Pre-law				
Any use	-0.000	0.013	0.009	0.012
[Mean = 0.168]	(0.983)	(0.550)	(0.031)	(0.031)
Frequent use	0.005	0.002	0.008	0.0002
[Mean = 0.043]	(0.657)	(0.885)	(0.012)	(0.015)
Sample size		5,075		5,075
Post-law				
Any use	0.019*	0.022**	0.029***	0.033***
[Mean = 0.143]	(0.067)	(0.050)	(0.010)	(0.010)
Frequent use	0.011*	0.009	0.018***	0.014*
[Mean = 0.039]	(0.061)	(0.157)	(0.007)	(0.008)
Sample size		17,106		17,106
Triangular kernel?	No	Yes	No	Yes
Polynomial		Linear		Quadratic

Any use represents e-cigarette use or nicotine vaping on at least one of the past 30 days while *frequent use* represents use on at least 10 of the past 30 days. Data are from the 2014–2017 Monitoring the Future (MTF) surveys of high school seniors. Pre-law observations are from states in years prior to MLPA-18 laws for e-cigarettes, either by the state during 2014–2016, or ultimately federally between the 2016 and 2017 surveys. Samples include all respondents interviewed within eight months of their 18th birthdays, with the exception of those from localities for which the e-cigarette MLPA is 19 years or older by the survey year. Estimates are coefficients of an indicator that respondents are age 217 months or older, using local linear (cols 1 and 2) or quadratic regressions (cols 3 and 4) that control for separate linear or quadratic functions of age in months through and beyond age 216 months. All estimates come from the STATA command rdrandinf (Cattaneo, Frandsen and Titiunuk 2015; Cattaneo, Titiunuk, Vazquez-Bare 2016, 2017**).** Standard errors clustered by state are in parentheses, with *, **, and *** signifying significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A4. Age 18 Discontinuity in E-Cigarette Use Without Demographic Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre-law										
Any use	-0.008	0.002	-0.008	0.005	0.011	0.001	-0.007	-0.005	-0.004	-0.001
[Mean = 0.168]	(0.0216)	(0.0323)	(0.0216)	(0.0220)	(0.0207)	(0.0198)	(0.0191)	(0.0184)	(0.0160)	(0.0156)
Observations	3893	3189	3893	4453	4820	5075	5248	5362	5441	5515
Frequent use	-0.016	-0.020	-0.020	-0.003	0.006	0.0003	-0.002	-0.003	-0.003	-0.006
[Mean = 0.043]	(0.0159)	(0.0193)	(0.0159)	(0.0137)	(0.0123)	(0.0121)	(0.0107)	(0.0108)	(0.0107)	(0.0112)
Observations	3893	3189	3893	4453	4820	5075	5248	5362	5441	5515
Post-law										
Any use	0.025**	0.021	0.025**	0.029***	0.025**	0.023**	0.024**	0.025**	0.025**	0.025**
[Mean = 0.143]	(0.0119)	(0.0128)	(0.0119)	(0.0112)	(0.0112)	(0.0103)	(0.0102)	(0.0105)	(0.0102)	(0.0101)
Observations	13074	10669	13074	14868	16227	17106	17650	18013	18246	18453
Frequent use	0.011	0.007	0.011	0.009	0.011	0.013**	0.014**	0.014**	0.013**	0.015**
[Mean = 0.039]	(0.00712)	(0.00835)	(0.00712)	(0.00679)	(0.00653)	(0.00592)	(0.00601)	(0.00583)	(0.00585)	(0.00577)
Observations	13074	10669	13074	14868	16227	17106	17650	18013	18246	18453
Bandwidth in Months	Data Driven	4	5	6	7	8	9	10	11	12

This table provides sample sizes, point estimates and standard errors for Appendix Figure A6 and Appendix Figure A9. Specifications are the same as in column (1) of Table 2 except they do not include covariates for race and sex, and the bandwidth varies as specified in the final row.

Appendix Table A5. Demographic Characteristics as Dependent Variable

	Male	Black	White	Hispanic	Ever Smoked Cigarettes
<u>Pre-law</u>					
Point estimate	0.00881	0.0135	0.0286	-0.00568	-0.0115
	(0.0238)	(0.0120)	(0.0189)	(0.0131)	(0.0230)
<u>Post-law</u>					
Point estimate	-0.00536	-0.0102	0.00769	-0.00118	-0.00122
	(0.0141)	(0.0137)	(0.0184)	(0.00887)	(0.0161)

See the Table 2 footnote for details. Regressions correspond to the model used for column (1) of Table 2. Each regression uses the column name as the dependent variable and age in months as the running variable. The lack of statistical significance is consistent with demographic characteristics of respondents being smooth at the age cutoff. Results for ever smoke cigarettes provide evidence that there is no social desirability bias in reporting tobacco use. All standard errors are clustered at the state level.

Appendix Table A6. Age 18 Discontinuity in *Any E-Cigarette Use* Pre- and Post-MLPA-21 Laws

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre MLPA-21 laws in places that have MLPA-18										
Any use	0.049***	0.034**	0.058***	0.069***	0.040***	0.030***	0.031***	0.036***	0.040***	0.043***
	(0.017)	(0.015)	(0.012)	(0.010)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Observations	1275	2537	3023	3398	3692	3907	4023	4079	4114	4148
Post MLPA-21 laws in places that have MLPA-18										
Any use	-0.003	-0.003	-0.013	-0.024	-0.020	-0.024	-0.014	-0.011	-0.011	-0.013
	(0.045)	(0.045)	(0.029)	(0.026)	(0.025)	(0.030)	(0.030)	(0.027)	(0.022)	(0.022)
Observations	1663	1663	1999	2295	2494	2640	2743	2783	2821	2845
Bandwidth in Months	Data Driven	4	5	6	7	8	9	10	11	12

Any use represents e-cigarette use or nicotine vaping on at least one of the past 30 days. Data are from the 2014–2017 Monitoring the Future (MTF) surveys of high school seniors. "Pre MLPA-21 laws in places that have MLPA-18" refers to places that currently have an MLPA-18 law in place, while "Post MLPA-21 laws in places that have MLPA-18" are from states that have implemented MLPA-21 laws and previously had an MLPA-18 law in place. Samples include all respondents interviewed within eight months of their 18th birthdays and meet the MLPA law distinctions listed above. Estimates are coefficients of an indicator that respondents are age 217 months or older, using local linear regressions that control for separate linear functions of age in months through and beyond age 216 months, along with indicators for year, state, gender, and race, using a uniform kernel and are weighted using MTF sampling weights. All estimates come from the STATA command rdrobust (Calonico et al. 2017). Standard errors clustered by state are in parentheses, with *, **, and *** signifying significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A7. Age 18 Discontinuity in *Any E-Cigarette Use* Post-MLPA-18 Laws Separately by Distance to the Nearest State without MLPA-18 Laws

	(1)	(2)	(3)	(4)	(5)	(6)
Post-law						
Any use	0.027***	0.029***	0.016	0.035**	0.023	0.034**
[Mean = 0.143]	(0.010)	(0.009)	(0.015)	(0.015)	(0.015)	(0.014)
Frequent use	0.015**	0.011*	0.021**	0.011	0.023***	0.006
[Mean = 0.039]	(0.006)	(0.006)	(0.008)	(0.008)	(0.009)	(0.008)
			6366	10740	6366	10740
Sample size		17,106		17,106		17,106
Triangular kernel?	No	Yes	No	No	Yes	Yes
Within 100 Miles?			Yes	No	Yes	No

Any use represents e-cigarette use or nicotine vaping on at least one of the past 30 days while *frequent use* represents use on at least 10 of the past 30 days. Data are from the 2014–2017 Monitoring the Future (MTF) surveys of high school seniors. We focus only on Post-law observations and determine whether individuals live within 100 miles of the nearest municipality (state, county, or country) without MLPA-18 laws, or areas to which a person under 18 could conceivably travel to buy e-cigarette products legally. Cols 1 and 2 are reprinted from Table 2. Samples include all respondents interviewed within eight months of their 18th birthdays, with the exception of those from localities for which the e-cigarette MLPA is 19 years or older by the survey year. Estimates are coefficients of an indicator that respondents are age 217 months or older, using local linear regressions that control for separate linear or quadratic functions of age in months through and beyond age 216 months. All estimates come from the STATA command rdrobust (Calonico et al. 2017). Standard errors clustered by state are in parentheses, with *, **, and *** signifying significance at the 10%, 5%, and 1% levels, respectively.

Appendix Table A8. Age 18 Discontinuity in E-Cigarette Use Post-MLPA-18 Laws Separately by Survey within One Year of MLPA Implementation

	(1) Baseline	(2) Baseline	(3) Law up to one year in place	(4) Law more than a year in place
Post-law				
UNIFORM				
Any use	0.027***		0.008	0.031***
[Mean = 0.143]	(0.010)		(0.020)	(0.011)
Frequent use	0.015**		0.013	0.015**
[Mean = 0.039]	(0.006)		(0.010)	(0.008)
			4345	12761
Sample size		17,106	17,106	
TRIANGULAR				
Any use		0.029***	0.003	0.036***
[Mean = 0.143]		(0.009)	(0.020)	(0.011)
Frequent use		0.011*	0.013	0.011
[Mean = 0.039]		(0.006)	(0.010)	(0.008)
			4345	12761
Triangular kernel?	No	Yes		
Year Plus			No	Yes

Any use represents e-cigarette use or nicotine vaping on at least one of the past 30 days while *frequent use* represents use on at least 10 of the past 30 days. Data are from the 2014–2017 Monitoring the Future (MTF) surveys of high school seniors. We focus only on Post-law observations and determine whether individuals were surveyed within one year of MLPA implementation. Cols 1 and 2 are reprinted from Table 2. Col 3 provides the immediate effect of the law while col 4 provides the effect after one year. Samples include all respondents interviewed within eight months of their 18th birthdays, with the exception of those from localities for which the e-cigarette MLPA is 19 years or older by the survey year. Estimates are coefficients of an indicator that respondents are age 217 months or older, using local linear regressions that control for separate linear or quadratic functions of age in months through and beyond age 216 months. All estimates come from the STATA command rdrobust (Calonico et al. 2017). Standard errors clustered by state are in parentheses, with *, **, and *** signifying significance at the 10%, 5%, and 1% levels, respectively.