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# Tobacco-21 laws and young adult smoking: quasi-experimental evidence

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## ABSTRACT

**Aims** To estimate the impact of tobacco-21 laws on smoking among young adults who are likely to smoke, and consider potential social multiplier effects. **Design** Quasi-experimental, observational study using new 2016–17 survey data. **Setting** United States. **Participants/cases** A total of 1869 18–22-year-olds who have tried a combustible or electronic cigarette. **Intervention and comparators** Tobacco-21 laws raise the minimum legal sales age of cigarettes to 21 years. Logistic regressions compared the association between tobacco-21 laws and smoking among 18–20-year-olds with that for 21–22-year-olds. The older age group served as a comparison group that was not bound by these restrictions, but could have been affected by correlated factors. Age 16 peer and parental tobacco use were considered as potential moderators. **Measurements** Self-reported recent smoking (past 30-day smoking) and current established smoking (recent smoking and life-time consumption of at least 100 cigarettes). **Findings** Exposure to tobacco-21 laws yielded a 39% reduction in the odds of both recent smoking [odds ratio (OR) = 0.61; 95% confidence interval (CI) = 0.42, 0.89] and current established smoking (OR = 0.61; 95% CI = 0.39, 0.97) among 18–20-year-olds who had ever tried cigarettes. This association exceeded the policy's relationship with smoking among 21–22-year-olds. For current established smoking, the tobacco-21 reduction was amplified among those whose closest friends at age 16 used cigarettes (OR = 0.50; 95% CI = 0.29, 0.87), consistent with peer effects moderating the policy's impact on young adult smoking. **Conclusions** Tobacco-21 laws appear to reduce smoking among 18–20-year-olds who have ever tried cigarettes.

**Keywords** Cigarettes, policy, smoking, tobacco control, tobacco-21, young adults.

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## INTRODUCTION

During the past 5 years, an increasing number of US states and localities have adopted tobacco-21 laws, raising the minimum legal sales age of cigarettes to 21 years. Such policies are promoted as a means to reduce youth smoking and improve population health [1]. Indeed, a 2015 Institute of Medicine report concluded that raising the tobacco sales age to 21 would reduce smoking rates and related mortality, emphasizing the importance of these policies for public health [1]. However, the report's simulations used hypothesized tobacco-21 effects on smoking initiation as an input to their model. Thus, the effects of tobacco-21 policies remain uncertain.

Estimates of the direct impact of tobacco-21 laws on smoking have focused on high school students and considered only a single policy-location at a time, with mixed findings [2,3]. Reducing access to tobacco can reduce youth tobacco use [4,5], and recent work shows that California's tobacco-21 policy reduced retailer sales to under-age individuals, with a retailer violation rate of 14.2% for traditional tobacco product sales to 18–19-year-olds after the law was implemented [6], yet results from studies of earlier youth access restrictions vary. Minimum legal sales ages have been effective in the United Kingdom and Sweden [7,8]. However, US studies of the relationship between retailer compliance and youth smoking show statistically insignificant effects [9,10].

As tobacco-21 laws may shape peer access and behavior, these policies could have both direct (own access) and indirect effects (e.g. reducing peer smoking) [11,12]. Figure 1 depicts this relationship, with direct effects as solid arrows and indirect impacts as striped arrows. Thus, when a given youth and their friends react to this

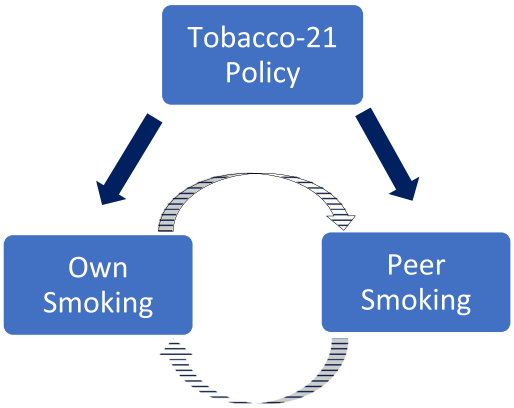


**Figure 1** Mechanism for tobacco-21 effect on young adult smoking. Solid arrows denote the policy's direct effects on young adult behavior, while striped arrows denote indirect effects. If the individual and their peers respond to both the policy and each other's behavior, these indirect effects will reinforce each other, such that the policy has an even greater impact on own and peer smoking. We refer to this as a social multiplier effect. [Colour figure can be viewed at wileyonlinelibrary.com]

policy in the same way, they may reinforce each other's responses, amplifying the policy's impact (i.e. a 'social multiplier effect').

To understand more clearly the relationship between tobacco-21 laws and young adult smoking this study considers two research questions, focusing on those who are likely to smoke: (1) how are tobacco-21 policies related to smoking; and (2) is this relationship moderated by peer-smoking?. 'Likely to smoke' is operationalized by selecting on those who have ever tried a combustible or electronic cigarette, as 87% of US smokers first tried cigarettes by age 18, and there is some concern that early e-cigarette users may be more likely to take up smoking later [13]. Specifically, newly collected survey data cover self-reported smoking behavior and demographics among 18–22-year-old 'ever-triers'.

To examine the relationship between tobacco-21 policies and smoking among young adult ever-triers, analyses compare current smoking between those residing in areas with versus without a tobacco-21 law at interview, among age groups that would, versus would not, have been bound by these policies (18–20 versus 21–22). Both unadjusted means and regressions adjusting for demographic differences are presented. A negative relationship between tobacco-21 exposure and current smoking is hypothesized.

Next, analyses consider whether peer effects amplify the tobacco-21 to smoking relationship. A vast literature provides theoretical and empirical support for the presence of peer effects in adolescent smoking [14–18]. This includes evidence for social multiplier effects, whereby peer responses to a policy reinforce the individual's response and vice-versa [18]. In the context of a tobacco-21 policy, a social multiplier would suggest that the law's impact on smoking should be strongest among those whose friends were likely to smoke absent a tobacco-21 restriction; i.e. where a tobacco-21 restriction could reduce smoking among one's friends. This hypothesized effect goes in the opposite direction of the expected impact from friend-selection. That is, if youths with the highest demand for cigarettes choose friends who are likely to smoke, friend-selection alone predicts less of a tobacco-21 response in this group than among those with non-smoking friends. To examine this, regressions use smoking among one's closest friends at age 16 as a proxy for friends' susceptibility to smoking in the absence of tobacco-21 policies (as few, if any, respondents were exposed to these policies at age 16).

### Project aims

Using survey data on 18–22-year-olds who have tried combustible and/or electronic cigarettes, this analysis aims to:

1 Estimate how tobacco-21 laws relate to current smoking among 18–20-year-olds who are otherwise likely to smoke; and
2 Test for evidence of a social multiplier effect in these relationships.

We expect (1) a differential reduction in smoking among 18–20-year-olds who are subject to tobacco-21 laws, relative to the trends among 21–22-year-olds in areas with the same policies; and (2) that this relationship will be strongest for those whose close friends at age 16 vaped or smoked, consistent with a social multiplier effect.

## METHODS

### Design

Quasi-experimental analyses use new, cross-sectional survey data to compare current smoking among 18–20-year-olds versus 21–22-year-olds, in areas that did, versus did not, have tobacco-21 laws at interview. To focus on those who were otherwise likely to smoke, the survey's sample is restricted to individuals who have ever tried a combustible or electronic cigarette.

### Participants

The authors commissioned an online survey of 2003 18–22-year-old US residents who had ever tried either an electronic or combustible cigarette ('ever-triers'), with questions focused on respondents' cigarette use and demographics. Qualtrics administered the survey from November 2016 to May 2017, with respondents recruited from standing panels used for academic and market research (see Supporting information, Appendix, for further details). To increase generalizability, sampling quotas were defined to match the 2015 National Health Interview

Survey's weighted distribution of respondents who reported ever-use of either combustible or electronic cigarettes, by year-of-age, sex, education and census region.

Qualtrics provided full data on 2710 US residents aged 18–22. The 707-person oversample was used to fill in sampling quotas in case of data quality issues. Specifically, 52 of the 2710 respondents were excluded based on the following quality checks: four for straight-lining (clicking the same response for a series of questions on a Likert scale), one for failing a minimum time threshold (i.e. speeding through the survey); and 47 for mutually exclusive age-responses (e.g. reported age at first cigarette use exceeds age-at-interview; reported age and year of birth conflict). Twenty-seven additional observations were dropped due to probable duplicate responses (where two interviews had the same IP address, sex, birth month and birth year). Using the 2631 remaining observations, the contracted sample of $n = 2003$ was populated with the latest survey-date responses until each quota was filled.

While the data cover all 50 states and the District of Columbia, analyses omit respondents from Massachusetts and New York. These states had the earliest tobacco-21 adopting localities, such that some 21–22-year-olds therein may have been bound by tobacco-21 restrictions since age 18. As prior policy-exposure could impact current smoking, including these respondents might bias the between-age-group comparisons towards a null result. Omitting them also ensures that findings are not driven by early-adopting regions, which may differ from later-adopters.

The resulting analytical sample contained 1869 respondents.

## Measures

### *Dependent variables*

Analyses focus on two binary, dependent variables: recent smoking (in the past 30 days) and current established smoking (recent smoking among those whose life-time smoking exceeds 100 combustible cigarettes). While recent smoking may include new experimenters, current established smoking ('established-smoking' for brevity) provides a clearer signal of regular use. [19]

### *Exposure*

The exposure of interest is a binary indicator for whether the respondent resided in a location with a tobacco-21 law in effect at interview. Specifically, the exposure variable equals 1 if (1) a respondent's state implemented a tobacco-21 law by their interview date or (2) they lived in the largest city in their state and that city was covered by a tobacco-21 policy at interview. Exposed respondents lived in Hawaii ($n = 3$), California ($n = 223$), New Jersey ($n = 46$), Chicago, IL ($n = 21$), Columbus, OH ($n = 8$) and Kansas City, MO ($n = 6$), with 16.4% of the sample exposed ($n = 307$) [20]. Most unexposed respondents faced tobacco minimum legal sales ages of 18, with the exception being those in Alaska, Alabama and Utah, where the minimum age was 19.

Substate locations are unavailable for individuals living outside their state's largest city. Thus, some respondents may be misclassified as unexposed to tobacco-21 laws if their town had a policy but their state did not. Such misclassification could bias estimates towards the null. However, this is not a problem for state-level laws, and two-thirds of substate tobacco-21 policies were in Massachusetts or New York and thus are not in the analytical sample [21]. This reduces the potential extent of exposure misclassification.

### *Control variables*

Binary indicators adjust for differential smoking by respondent demographics: sex, a binary 'under-age-21' indicator, race (black, other and multiple race; 'white race only' as the reference group), Hispanic ethnicity, current student status ('current student' and 'not currently a student but plans to enroll within next year'; non-student as the reference group), whether any parent attended college, and urbanicity (urban and suburban; rural as the reference group).

As peer and parental tobacco use affect adolescent smoking [11,12], two additional controls adjust for whether (a) any of the respondent's three closest friends at age 16 used combustible or electronic cigarettes at that time, and (b) a parent used combustible or electronic cigarettes when the respondent was age 16. As tobacco-21 laws were implemented after the analytical sample's respondents turned 16, these controls capture the relationship of peer and parental use to respondent smoking, separate from any influence of tobacco-21 laws on peer and parental behavior.

Tobacco-21 policies may be correlated with other tobacco policies. To address potential confounding, controls are included for state plus local combustible cigarette taxes and comprehensive smoke-free indoor air laws (i.e. covering restaurants, bars and private work-sites). As with the tobacco-21 variable, these controls are coded based on the respondent's state and residence in its largest city.

Survey question wording is given in the Supporting information Appendix.

## Analyses

Table 1 presents means for variables used in the analyses. Table 2 displays descriptive statistics of both recent and current established smoking rates by tobacco-21 exposure, stratified by age-group (panel A) and, for 18–20-year-olds,

**Table 1** Summary statistics.

| | *Full sample Percentage (n)* |
|---|---|
| Tobacco use | |
| Recent (past 30-day) smoking | 65.1% ($n$ = 1216) |
| Current established smoking | 54.7% ($n$ = 1022) |
| Any parent smoked or vaped when respondent was aged 16 years | 54.0% ($n$ = 1009) |
| Close friend smoked or vaped when respondent was aged 16 years | 67.1% ($n$ = 1254) |
| Tobacco policy exposure | |
| Tobacco-21 laws | 16.4% ($n$ = 307) |
| Comprehensive smoke-free indoor air restrictions | 54.9% ($n$ = 1026) |
| Combustible cigarette tax (state + local) | $1.56 ($n$ = 1869) |
| Demographics | |
| Age < 21 years | 48.7% ($n$ = 911) |
| Male | 58.6% ($n$ = 1095) |
| At least one parent attended college | 59.2% ($n$ = 1106) |
| Hispanic | 15.9% ($n$ = 298) |
| Race | |
| White only | 77.5% ($n$ = 1449) |
| Black | 11.7% ($n$ = 219) |
| Other | 11.5% ($n$ = 215) |
| Multiple | 3.9% ($n$ = 72) |
| Current student status | |
| Current student | 48.3% ($n$ = 902) |
| Not current student, plans to enroll within the next year | 24.5% ($n$ = 458) |
| Not a student | 27.2% ($n$ = 509) |
| Urbanicity | |
| Rural | 25.5% ($n$ = 477) |
| Suburban | 42.5% ($n$ = 794) |
| Urban | 32.0% ($n$ = 598) |
| Total number of observations | 1869 |

Means for demographic, tobacco use and policy exposure variables are based on newly collected survey data on 18–22-year-old 'ever-triers' of combustible and/or electronic cigarettes; *n*s give the number of non-zero observations for each variable in parentheses. Respondents from New York and Massachusetts are omitted.

by whether a close friend at age 16 smoked or vaped (panel B). Table 3 presents logistic regressions comparing smoking among 18–20- and 21–22-year-olds based on exposure to tobacco-21 laws at interview.

Specifically, binary indicators for recent smoking (models 1–3) and established-smoking (models 4–6) are regressed on demographic and policy controls, plus two exposure variables: the presence of tobacco-21 laws and an interaction between this variable and an under age-21 indicator. The uninteracted tobacco-21 coefficient captures the correlation between tobacco-21 laws and smoking in the general young adult population. The interaction term reflects any additional policy impacts specific to under 21-year-olds, beyond the general relationship. Analyses are run with and without state fixed-effects so that results can be compared across these specifications to verify that state-specific factors do not drive the coefficient estimates. In all cases, robust standard errors are clustered at the state level.

To consider whether peer effects shape the relationship between tobacco-21 laws and smoking, a third specification is assessed (columns 3 and 6). These regressions add two terms to the state fixed-effects analyses: interactions between the under 21-by-tobacco-21 term and binary indicators for (1) having close friends who smoked or vaped when the respondent was 16, and (2) having a parent who smoked or vaped when the respondent was 16. A low covariance (0.13) between the friend- and parental-use indicators allows assessment of their distinct relationships with current smoking. The friends' use interaction addresses whether the policy's impact is stronger among those whose close friends smoked or vaped as youths. The parental use interaction allows for the possibility of differential policy effects among those who grew up with easier access to or less stringent attitudes towards tobacco products in their household. If peer effects moderate tobacco-21's impacts, then the policy's association with smoking would be strongest among those whose friends might also have responded by reducing their smoking, holding parental behavior constant. Thus, a statistically significant peer use interaction term would support the hypothesis that tobacco-21 laws have indirect effects via peer effects.

Specification checks repeat each regression using linear probability models instead of logistic regressions, as the latter may yield biased parameter estimates when heteroskedasticity is present. Analyses use Stata 14 statistical software. Yale University's Institutional Review Board approved this study (HIC Protocol no. 1307012384).

## RESULTS

Table 1 presents sample summary statistics. As the data are limited to electronic and combustible cigarette ever-triers, respondent smoking rates are high, at 65% for recent use ($n$ = 1216) and 55% for established-smoking ($n$ = 1022). Similarly, 54% report at least one parent having smoked or vaped when they were 16 ($n$ = 1009) and 67% report having a close friend who did so at that age ($n$ = 1254).

Table 2's cross-tabulations suggest potential impacts from tobacco-21 policies. Specifically, panel A shows that 18–20-year-olds who are not exposed to tobacco-21 laws have a recent smoking rate of 64%, compared to 46% for those who are exposed: an 18 percentage-point difference. Established smoking shows a similar gap, with rates of 50 versus 28%, respectively. However, these differences may not be fully attributable to the tobacco-21 laws. Indeed, 21–22-year-olds also exhibit differential smoking rates by tobacco-21 exposure, although they were not bound by

**Table 2** Smoking rates stratified by tobacco-21 exposure

*(A) By age group*

| | *Recent Smoking* | | | *Current established smoking* | | |
|---|---|---|---|---|---|---|
| *Ages (years)* | *No Tobacco-21* | *Tobacco-21* | *Smoking gap* | *No tobacco-21* | *Tobacco-21* | *Smoking gap* |
| 18–20 | 63.7% ($n$ = 763) | 45.9% ($n$ = 148) | 17.8 percentage points | 50.3% ($n$ = 763) | 28.4% ($n$ = 148) | 21.9 percentage points |
| 21–22 | 69.5% ($n$ = 799) | 67.3% ($n$ = 159) | 2.2 percentage points | 63.5 ($n$ = 799) | 56.0% ($n$ = 159) | 7.5 percentage points |

*(B) By whether closest friends at age 16 smoked or vaped, 18–20-year-olds only*

| | *Recent smoking* | | | *Current established smoking* | | |
|---|---|---|---|---|---|---|
| *Friends' Use* | *No Tobacco-21* | *Tobacco-21* | *Smoking gap* | *No Tobacco-21* | *Tobacco-21* | *Smoking Gap* |
| Yes | 67.9% ($n$ = 570) | 47.0 ($n$ = 117) | 20.9 percentage points | 57.4 ($n$ = 570) | 30.8% ($n$ = 117) | 26.6 percentage points |
| No | 51.0% ($n$ = 243) | 51.7% ($n$ = 60) | −0.7 percentage points | 31.3% ($n$ = 243) | 31.7% ($n$ = 60) | −0.4 percentage points |

This table presents average smoking rates by tobacco-21 exposure stratified by age-group (A) and, for 18–20-year-olds only, by whether any of the respondent's three closest friends at age 16 smoked or vaped. Respondents from New York and Massachusetts are omitted. 'Smoking gap' is the difference between the smoking rate among those with versus with tobacco-21 exposure, within a given stratum (i.e. row); that is, smoking gap = smoking rate $_{\text{no tobacco-21 exposure, row X}}$ − smoking rate $_{\text{tobacco-21 exposure, row X}}$.

these policies. This group's gaps—2 percentage points for recent smoking and 7 percentage points for established-smoking—may reflect variations in other factors correlated with both young adult smoking and tobacco-21 laws (e.g. cigarette tax rates).

Subtracting the differential smoking rate observed among the older age group from that for the younger group excises differences in smoking rates that are not directly due to the age-21 restriction. Thus, the raw statistics suggest that tobacco-21 exposure may contribute to the 16 percentage-point difference in the recent smoking gap (18 minus 2%) and 15 percentage-point difference in the established smoking gap (22 minus 7%) between the 18–20- and 21–22-year-old age groups.

Panel B is similarly suggestive regarding a social multiplier effect. Among 18–20-year-olds whose closest friends at age 16 smoked or vaped, those exposed to tobacco-21 laws are more than 20 percentage points less likely to be recent and established smokers than those not exposed. Among those whose closest friends at age 16 did not smoke or vape, the corresponding smoking gaps are −0.7 and − 0.4, respectively. Thus, tobacco-21 exposure is associated with greater reductions in smoking among those most susceptible to a social multiplier effect.

Logistic regressions test these relationships more formally, controlling for individual demographics, tobacco policies and state fixed-effects. Comparing the main specification without and with state fixed-effects (Table 3, columns 1 and 4 versus 2 and 5) shows that including state fixed effects inflates the tobacco-21 odds ratios (ORs). Collinearity between the fixed-effects and state-level tobacco-21 laws explains this shift. Thus, while the non-interacted tobacco-21 indicator controls for the association between tobacco-21 exposure and smoking among 18–22-year-olds, its coefficient should not be interpreted as an estimate of the policy's across-age-group effect [22].

This collinearity does not compromise the tobacco-21 interaction variables. Specifically, the interaction terms' coefficients are identified by comparing policy responses between age groups within a state, and thus are not collinear with state fixed-effects. Indeed, the ORs on these terms are similar whether or not state fixed-effects are included: statistically significant at 0.61 in all baseline regression specifications, for recent and also established-smoking (Table 3, columns 1, 2, 4 and 5). Thus, exposure to a tobacco-21 law is associated with a 39% drop in the odds that an 18–20-year-old ever-trier will be a recent smoker [OR = 0.61, confidence interval (CI) = 0.42, 0.89; $P$-value = 0.01] or a current smoker [OR = 0.61; CI = 0.39, 0.97; $P$-value = 0.04] at interview, compared to 21–22-year-old ever-triers living in the same state.

Specifications 3 and 6 consider the mechanism behind this association, adding interaction terms for the under-21 by tobacco-21 indicator by (a) parental and (b) close friends' use of combustible or electronic cigarettes when the respondent was 16. Adding these controls yields a statistically insignificant odds ratio on the under-21 by tobacco-21 indicator for both recent smoking [OR = 1.07,

**Table 3** Logistic regression analysis of tobacco-21 laws and smoking, odds ratio/(*t*-statistic)/*P*-value

| | *Recent smoking* | | | *Current established smoking* | | |
|---|---|---|---|---|---|---|
| | *(1)* | *(2)* | *(3)* | *(4)* | *(5)* | *(6)* |
| Tobacco-21 law | 1.0551 | 1.7146 | 1.7475 | 0.8517 | 1.4094 | 1.4359 |
| | (0.353) | (1.148) | (1.127) | (−1.031) | (0.884) | (0.876) |
| | *P* = 0.72 | *P* = 0.25 | *P* = 0.26 | *P* = 0.30 | *P* = 0.38 | *P* = 0.38 |
| Age < 21 years | 0.8470 | 0.8413 | 0.8366 | 0.6459** | 0.6280** | 0.6251** |
| | (−1.498) | (−1.459) | (−1.501) | (−4.542) | (−4.443) | (−4.483) |
| | *P* = 0.13 | *P* = 0.14 | *P* = 0.13 | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 |
| Tobacco-21 law* Age < 21 years | 0.5931** | 0.6125* | 1.0698 | 0.6015* | 0.6116* | 0.9360 |
| | (−2.770) | (−2.566) | (0.235) | (−2.279) | (−2.103) | (−0.217) |
| | *P* = 0.01 | *P* = 0.01 | *P* = 0.81 | *P* = 0.02 | *P* = 0.04 | *P* = 0.83 |
| Tobacco-21 law *Age < 21* Close friend smoked or vaped when R was 16 | | | 0.5039 | | | 0.5364** |
| | | | (−1.669) | | | (−3.467) |
| | | | *P* = 0.10 | | | *P* = 0.00 |
| Tobacco-21 law *Age < 21* Parent smoked or vaped when R was 16 | | | 0.8294 | | | 1.0253 |
| | | | (−1.039) | | | (0.134) |
| | | | *P* = 0.30 | | | *P* = 0.89 |
| Any parent smoke or vape when R was 16 | 1.4231** | 1.4262** | 1.4491** | 1.6612** | 1.6885** | 1.6847** |
| | (2.822) | (2.722) | (2.579) | (5.026) | (4.957) | (4.601) |
| | *P* = 0.00 | *P* = 0.01 | *P* = 0.01 | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 |
| Any close friend smoke or vape when R was 16 | 1.5683** | 1.5755** | 1.6744** | 1.7157** | 1.6789** | 1.7526** |
| | (3.610) | (3.452) | (4.234) | (3.892) | (3.677) | (4.229) |
| | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 |
| Constant | 2.5893** | 3.1304** | 2.9618** | 2.1179** | 2.6513** | 2.5756** |
| | (3.674) | (4.376) | (3.881) | (3.214) | (4.081) | (3.849) |
| | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 | *P* = 0.00 |
| State fixed-effects | No | Yes | Yes | No | Yes | Yes |
| *n* | 1869 | 1857 | 1857 | 1869 | 1857 | 1857 |
| Adjusted $R^2$ | 0.059 | 0.083 | 0.084 | 0.109 | 0.129 | 0.129 |
| Dependent variable mean | 0.651 | 0.648 | 0.648 | 0.547 | 0.544 | 0.544 |

Logistic regression models consider how age 21 tobacco sales restrictions impact smoking at interview. Models compare 18–20-year-olds with 21–22-year-olds, and omit respondents (R) from New York and Massachusetts, as local restrictions in those states' restrictions are old enough that some 21- and 22-year-old respondents therein could have been bound by the restrictions when they were age 18. Controls not indicated in the table are the (state+local) combustible cigarette tax and fixed-effects for the presence of comprehensive smoke-free indoor air laws, male sex, race (black, multiple, other, with white as the reference group), Hispanic ethnicity, urbanicity (suburban, urban, with rural as the reference group), whether any parent attended college and student status (current student, planning to enroll in the coming year, with non-student as the reference group). Columns 1 and 4 give main specification results; 2 and 5 add controls for state fixed-effects; 3 and 6 add additional interaction terms listed in the table. Standard errors are clustered by state. *(**) denote statistical significance at the 0.05 (0.01) level.

confidence interval (CI) = 0.610, 1.877; *P*-value = 0.814] and established smoking (OR = 0.94, CI = 0.515, 1.701; *P*-value = 0.828). The parental use interaction is statistically insignificant and close to 1 in all cases. However, the friends' use interaction term is statistically significant in the established-smoker regression (OR = 0.54, CI = 0.377, 0.763; *P*-value = 0.001). Thus, tobacco-21 policies are associated with a 50% drop in the odds of established smoking among 18–20-year-old ever-triers whose friends smoked or vaped at age 16: OR = exp [ln(0.54) + ln(0.94)] ≈ 0.5020, CI = 0.289, 0.871; *P*-value = 0.014). The elevated tobacco-21 effect in this subgroup is consistent with a social multiplier effect in young adults' responses to tobacco-21 policies.

Evaluating these specifications with linear probability models yields similar findings (see Supporting information Appendix, Table S1). These analyses are repeated with recent and established vaping as the dependent variables, yielding statistically insignificant results across the board (see Supporting information, Appendix Table S2). Vaping regression results are not presented here due to concerns about statistical power given lower vaping rates and relatively high standard errors.

## DISCUSSION

This study finds that tobacco-21 policies are associated with a 39% reduction in the odds of recent and established smoking among 18–20-year-olds who have ever tried a combustible or electronic cigarette, compared to similar 21–22-year-olds. Sensitivity checks indicate that, for established smoking, this association is differentially stronger among 18–20-year-olds whose close friends vaped or smoked at age 16. Specifically, such youths exhibit a 46%

reduction in their odds of established smoking in response to tobacco-21 exposure, above and beyond the full age group's policy-response. These findings are consistent with a social multiplier effect.

This research adds to the literature in several ways: it constitutes one of the first studies to estimate the relationship between tobacco-21 policies and smoking among 18–20-year-olds; uses a quasi-experimental analysis to examine new survey data; and provides both simple comparative statistics and controlled regression analyses to consider smoking among ever-triers of combustible and/or electronic cigarettes. The findings provide critical empirical support for tobacco-21 policies, as well as evidence for a social multiplier effect.

### Limitations

Limitations stem primarily from the data. First, the sample's restriction to ever-triers means that analyses do not reflect the full population's policy responses. Results should be interpreted as applying to those who are particularly susceptible to tobacco use: a critical group. Similarly, as Qualtrics recruited respondents from a standing panel, the analysis is not a probability sample and may under-represent some populations. While sampling quotas increase the sample's representativeness of the general population in terms of age, sex, education and census region, they do not ensure representativeness for specific states with versus without tobacco-21 laws, or address race or income differences between these areas. This may affect the results' generalizability, particularly to under-represented groups. Generalizability may also be constrained by the exclusion of Massachusetts and New York residents, and coverage of only six tobacco-21 policies (three state-level, three local). Critically, self-reported smoking data may underestimate true smoking rates [23]. However, given that respondents are self-admitted ever-triers, they may be less inclined to misrepresent their current smoking status. Because tobacco-21 laws have been in place for relatively few years, we cannot estimate the impact of exposure throughout adolescence.

Due to potential misclassification of policy exposure, regression results should be interpreted as lower bounds on the true tobacco-21 effect. Specifically, some respondents living outside their state's largest city may be misclassified as unexposed to tobacco-21 laws if their locality has such policies and their state does not, potentially biasing parameter-estimates towards the null. Omitting Massachusetts and New York from the analyses reduces this concern.

Finally, as this analysis is not a randomized controlled trial, we cannot ascertain causality. The quasi-experimental approach used here compares smoking among 18–20-year-olds versus 21–22-year-olds, who were, versus were not, living in areas with tobacco-21 policies at interview, controlling for other tobacco policies as well as demographics and state fixed-effects. These regressions effectively hold determinants of smoking that are common to these two age groups constant, assuming that the older group is a valid counterfactual for the younger absent tobacco-21 exposure. Regressing current smoking rates by state for 18–20-year-olds on rates for 21–22-year-olds among those not exposed to tobacco-21 laws yields an $R^2$ of 0.89, consistent with this assumption (results not shown). As no other tobacco policies were simultaneously implemented that applied differently to these two age groups, the findings reported here provide strong evidence suggesting a negative impact of tobacco-21 policies on 18–20-year-olds' smoking.

## CONCLUSIONS

In summary, this study finds that tobacco-21 restrictions are associated with a 39% reduction in the odds of smoking among 18–20-year-olds who ever tried a combustible or electronic cigarette. Moreover, this relationship is strongest among those whose close friends smoked or vaped at age 16, consistent with a social multiplier effect wherein tobacco-21 restrictions influence young adult smoking both directly and indirectly, via peer responses to the policy. These results provide support for efforts to implement tobacco-21 laws as a means to reduce young adult smoking, producing long-term benefits for public health.

### Declaration of interests

None.

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## Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

**Appendix** SI Additional Analyses.
**Table S2** Logistic Regression Analysis of Tobacco-21 Laws and Vaping, Odds Ratio/ (t-statistic) /*p-value.*