Do Local Tobacco-21 Laws Reduce Smoking among 18 to 20 Year-Olds?

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ABSTRACT

Introduction: States and municipalities are increasingly restricting tobacco sales to those under age-21, in an effort to reduce youth and young adult smoking. However, the effectiveness of such policies remains unclear, particularly when implemented locally.

Methods: Analyses use 2011 - 2016 data from the Behavioral Risk Factor Surveillance System's Selected Metropolitan/Micropolitan Area Risk Trends dataset. Difference-in-differences and triple-difference regressions estimate the relationship between local tobacco-21 policies and smoking among 18 to 20 year-olds living in MMSAs (metropolitan/micropolitan statistical areas).

Results: Current smoking rates fell from 16.5 percent in 2011 to 8.9 percent in 2016 among 18-20 year-olds in these data. Regressions indicate that a tobacco-21 policy covering one's entire MMSA yields an approximately 3.1 percentage point reduction in 18 to 20 year-olds' likelihoods of smoking [CI: -0.0548, -0.0063]. Accounting for partial policy exposure — tobacco-21 laws implemented in some but not all jurisdictions within an MMSA — this estimate implies that the average exposed 18 to 20 year-old experienced a 1.2 percentage point drop in their likelihood of being a smoker at interview relative to unexposed respondents of the same age, all else equal.

Conclusions: Local tobacco-21 policies yield a substantive reduction in smoking among 18 to 20 year-olds living in metropolitan and micropolitan statistical areas. This finding provides empirical support for efforts to raise the tobacco purchasing age to 21 as a means to reduce young adult smoking. Moreover, it suggests that state laws preempting local tobacco-21 policies may impede public health.

IMPLICATIONS

While states and municipalities are increasingly restricting tobacco sales to under-21-year-olds, such policies' effectiveness remains unclear, particularly when implemented locally. Using quasi-experimental methods, this paper provides what may be the first evidence that sub-state tobacco-21 laws reduce smoking among 18 to 20 year-olds. Specifically, considering metropolitan and micropolitan areas from 2011 to 2016, the average 18 to 20 year-old who was exposed to these policies exhibited a 1.2 percentage point drop in their likelihood of being a current established smoker, relative to those who were unexposed. These findings validate local tobacco-21 laws as a means to reduce young adult smoking.

INTRODUCTION

Tobacco use is the leading cause of preventable death worldwide, with cigarette smoking in particular responsible for over 400,000 U.S. deaths annually. [1] Based on 2017 data, 54 percent of U.S. daily smokers aged-25 and older began smoking regularly prior to age 18, while 26 percent reported initiating regular use between the ages of 18 and 20. [2] Recognizing the latter age-group's contribution to smoking as well as the impact of age-21 minimum legal drinking ages on drunk driving [3], 14 states and over 400 sub-state jurisdictions passed laws to raise their tobacco sales age to 21 by June 1, 2019, above the federal minimum of 18. However, the impact of these "tobacco-21" policies on 18 to 20 year-old smoking has not been established. In particular, such policies' effects may be dampened when implemented at the local level, as older adolescents can travel to neighboring towns or counties to purchase cigarettes.

Research evaluating tobacco-21 policies' effects is limited. Schneider et al. (2016) found a greater reduction in past-30-day smoking among adolescents in Needham, Massachusetts following implementation of its tobacco-21 law, as compared to other adolescents in the Greater Boston area. [4] However, these results do not factor in how smoking was trending prior to the policy's implementation, and thus do not pinpoint a causal effect. An Institute of Medicine report concludes that tobacco-21 laws will reduce smoking as well as related mortality, based on simulation models that take hypothesized effects on smoking initiation as an input. [5] Since the authors do not estimate tobacco-21 effects on initiation, the overall impact remains unclear.

This study estimates the impact of local tobacco-21 restrictions on smoking among 18 to 20 year-olds in U.S. metropolitan statistical areas, metropolitan divisions, and micropolitan statistical areas (MMSAs). Quasi-experimental analyses use data from the Behavioral Risk Factor Surveillance System to estimate the policies' effects. Specifically, current established

smoking is compared in areas with varying levels of local tobacco-21 policy exposure, before versus after these policies were implemented, and among individuals who would versus would not have been bound by them (i.e., 18-20 year-olds versus 23-25 year-olds). A negative impact of sub-state tobacco-21 laws on current established smoking is hypothesized.

METHODS

Data

Analyses use the 2011 to 2016 waves of the Behavioral Risk Factor Surveillance System's (BRFSS) Selected Metropolitan/Micropolitan Area Risk Trends (SMART) data. A yearly, cross-sectional survey providing nationally representative data on non-institutionalized U.S. adults, the BRFSS is administered by the Centers for Disease Control and Prevention. The SMART data are a subset of the full BRFSS, covering MMSAs wherein at least 500 BRFSS respondents were interviewed in a given year. Pre-2011 data are not considered, as these are not comparable to later waves due to changes in the survey's sampling structure and weighting scheme.[6] To ensure pre- and post-policy-implementation observations for each MMSA, this analysis restricts consideration to those covered in every year of the 2011 to 2016 SMART data with 18-20 year-old respondents in each survey year: 101 MMSAs in total, with every state represented except Wyoming, Mississippi, and Hawaii. Furthermore, analyses omit 9 MMSA-by-state areas where a component jurisdiction (e.g., a single county within the MMSA) changed its local cigarette tax during the period of analysis, as regressions cannot control for respondent exposure to these changes (See Data Appendix for further detail). This restriction excludes all represented localities in Virginia, Alaska, and Alabama, as well as the portion of the Chicago-

Naperville-Elgin MSA located in Illinois. Thus, the analytic sample includes data from 44 states and Washington, DC.

Consideration is limited to two young adult age-groups: 18 to 20 year-olds (i.e., those bound by concurrent tobacco-21 policies; N=25,066) and 23 to 25 year-olds (N=31,535). The latter group was neither bound by these policies at interview nor, in all sample MMSAs except the Boston Metropolitan Division, exposed to them prior to age 21. Thus, analyses considering both age-groups omit the Boston Metropolitan Division, so that the 23 to 25 year-olds provide a within-jurisdiction comparison for young adult smoking absent concurrent or lagged tobacco-21 effects. (Given prior tobacco-21 exposure, 21 and 22 year-olds are not considered as a comparison group.) This analytic approach separates the association between tobacco-21 laws and smoking among 18 to 20 year-olds into a portion that is also observed among older young adults (and thus not caused by the purchasing restriction per se) and a portion that is specific to the under-21 age-group.

The outcome of interest is current established smoking, captured by a binary indicator equal to one for respondents who have both consumed at least 100 cigarettes in their lifetime and report currently smoking either "some days" or "daily." This indicator is commonly used to distinguish current regular use from recent experimentation. [7] Since most US smoking initiation occurs prior to age-18 and the BRFSS data do not cover minors, analyses do not consider effects on first use or early experimentation. Smoking status is reported by 54,371 of the sample's 56,601 respondents, with 12% of 18 to 20 year-olds and 21% of 23 to 25 year-olds reporting current established smoking at interview (Table 1).

Tobacco-21 Policy Data

Each SMART data respondent is matched to their likelihood of being subject to a tobacco-21 restriction at interview based on their location and interview date. Since some MMSAs cross state boundaries, the smallest geographic unit a respondent can be linked to is the intersection of these two geographies; their MMSA-by-state, henceforth referred to as their "locality" (See Data Appendix for further detail). Omitting localities that either lack 18-20 year old respondents in any of the survey years considered here or contain a jurisdiction that changed its local cigarette tax between 2011 and 2016, the data cover 117 localities.

Local tobacco-21 implementation dates [8, 9] and census data on local population sizes [10, 11, 12, 13] are used to estimate each respondent's likelihood of being subject to a tobacco-21 restriction at interview. Specifically, this estimate is the percent of the population in their locality that was covered by a tobacco-21 policy on their interview date. Prior to 2017, such restrictions were in effect in 15 of the covered localities, across seven states —Kansas, Massachusetts, Missouri, New Jersey, New York, Ohio, Maine —with more than 100 distinct, non-zero levels of tobacco-21 exposure represented over time. Tobacco-21 exposure values range from 0 to 83 percent in this sample.

Data Analysis

First, a table of summary statistics describes the respondents by age-group and locality-type (i.e., those that did versus did not contain any jurisdictions with tobacco-21 policies during the period of analysis). Annual smoking rates and their confidence intervals are plotted separately for 18-20 year-olds whose MMSAs did versus did not include jurisdictions with a tobacco-21 law before January 1, 2017.

Next, difference-in-differences regressions consider 18 to 20 year-olds only, comparing current established smoking before and after local tobacco-21 policies went into effect, in localities with more versus less tobacco-21 coverage. Binary indicators for interview year and locality are used to control for both general time trends and time-invariant locality-effects. These fixed effects ensure that the tobacco-21 coefficient is identified off of within-locality changes in tobacco-21 exposure over time. A separate variable controls for California's tobacco-21 law—the only state-level tobacco-21 law covered by these data. This is both to ensure that the local tobacco-21 coefficient reflects sub-state policy effects only, and because California instituted several other changes alongside its tobacco-21 policy, including changes in tobacco seller licensing regulations and workplace smokefree indoor air laws, and initiating a campaign to promote the California Smokers' Helpline and educate residents about the state's tobacco-21 policy. [14] These other policies may have affected tobacco use, biasing the California tobacco-21 coefficient relative to the impact of its tobacco-21 law alone. (The only prior state-level policy was in Hawaii, which does not have an MMSA represented in these data.)

Additional controls for respondent demographics and tobacco policies adjust for differences in respondent characteristics and exposure to other regulations that may be correlated with tobacco-21 coverage. Demographic controls are fixed effects for year of age, female sex, race (Black, Asian, NHOPI, AIAN, Multiple race, Other race, with White as the reference group), Hispanic ethnicity, annual household income (a categorical variable in the BRFSS), and whether the respondent was a student at interview, had health insurance at interview, and completed the survey by cellular phone (a critical sampling variable in the BRFSS). Missing-observation indicators are included for race, Hispanic ethnicity, income, student status, and insurance coverage. Tobacco policy controls cover state cigarette taxes (in CPI-adjusted 2017

dollars) [15]; likelihood of exposure to comprehensive smoke-free indoor air restrictions [16] (i.e., covering bars, restaurants, and non-hospitality worksites) at the locality-level, calculated in the same manner as tobacco-21 exposure; and, a binary indicator for 18-year-old respondents in localities with tobacco purchasing ages of 19 (i.e., "tobacco-19" restrictions), as these individuals could not legally be sold tobacco at interview.

All regressions apply the SMART data's sampling weights and use robust standard errors, clustered at the state level. Given concerns about both coefficient bias and underestimated variance in logistic regressions with large numbers of fixed effects, linear probability models are used to evaluate these specifications. [17]

As a further check on the results, triple-difference regressions compare associations between tobacco-21 exposure and smoking among 18-20 year-olds versus 23-25 year-olds. The latter group would have been affected by other regulations and local attitudes towards smoking, but was not bound by the tobacco-21 policies. Thus, this approach separates out the local tobacco-21 policies' correlation with other local factors influencing young adult smoking from the estimate of its particular relationship to smoking among 18 to 20 year-olds.

Specification checks repeat the triple-difference analysis with controls for age-group-specific responses to cigarette taxes and smokefree indoor air laws, in order to ensure that age-differences in responsiveness to these policies do not drive the tobacco-21 coefficients. Sensitivity checks repeat all three analyses with the sample limited to localities that had at least ten 18 to 20 year-old respondents in every survey year, to ensure that single-digit sample sizes in a subset of the localities do not drive the results.

Finally, while the aforementioned regressions' tobacco-21 exposure coefficients estimate the association between full policy exposure and current established smoking, respondents in

these data were at most partially exposed (e.g., some but not all towns in their localities had tobacco-21 policies). Thus, to clarify the policy's impact in areas with partial tobacco-21 coverage, the estimated policy-effect for 18-20 year-olds is multiplied by the average probability of exposure among the exposed (i.e., those whose localities had at least one tobacco-21 policy at interview). The result provides an estimate of how local tobacco-21 exposure affects 18 to 20 year-olds' probability of being a current established smoker in the context of partial policy coverage.

The Yale Institutional Review Board deemed this study exempt from human subjects review. All analyses were conducted with Stata 14, StataCorp LP.

RESULTS

Table 1 gives summary statistics for the full analytic sample, as well as separately by age-group for localities containing at least one jurisdiction that had a tobacco-21 restriction before 2017 ("treatment localities") versus those that did not ("control localities"). Current established smoking rates are about one percentage point lower in treatment localities, consistent with stricter tobacco policies and higher socioeconomic status indicators therein. Specifically, treatment localities' state cigarette taxes are more than $1.50 higher than controls; their respondents are more than twice as likely to be exposed to a comprehensive smoke-free indoor air restriction; and, 18 to 20 year-olds therein are more likely to be current students and have health insurance than those in non-adopting localities. Thus, analyses of tobacco-21 policies' effects should control for other tobacco policies and respondent characteristics, as both may shape smoking rates.

Using the sensitivity check sample, Figure 1 plots annual smoking rates and their confidence intervals for 18-20 year-olds whose MMSAs did versus did not include jurisdictions with a tobacco-21 law before January 1, 2017. These groups' smoking rates overlap closely prior to 2014, consistent with parallel pre-trends. From 2014-on, when localities began adopting tobacco-21 policies, the two trends diverge slightly, though remaining within each other's confidence intervals. A regression-based parallel trends test concurs: analyses of pre-2014 data find no statistically significant difference in smoking trends for 18 to 20 year-olds in control versus treatment localities prior to tobacco-21 adoption, suggesting that the former is a valid counterfactual for the latter (see Appendix Table A1, Columns 1 and 2).

Difference-in-differences analyses are presented in Table 2, columns 1 and 2. The full sample regression finds that living in a tobacco-21 MMSA yields a statistically significant 3.1 percentage point drop in the probability that an 18 to 20 year-old is a current established smoker (CI [-0.0548, -0.0063]). Restricting the sample to localities with at least ten 18-20 year-old respondents in every survey year yields a similar estimate (-3.1 percentage points; CI [-0.0553, -0.0065]).

Triple-difference analyses disaggregate the tobacco-21 effect into a general association between these laws and smoking across age-groups, as well as an additional relationship between the policies and smoking among 18 to 20 year-olds, above and beyond the general association. Results show that living in an MMSA with a tobacco-21 law is associated with a general 3.0 to 3.1 percentage point increase in the probability of current established smoking across age-groups, alongside a relative 5.5 to 5.7 percentage point decrease in this probability for 18 to 20 year-olds (Table 2, columns 3 and 4). That is, areas with these policies exhibit general increases in young adult smoking, alongside a countervailing decrease among 18 to 20 year-olds that more

than compensates for this trend. The net effect is a 2.5 to 2.6 percentage point drop in smoking among 18 to 20 year-olds (see Table 2's "Full Tobacco-21 Effect, ages 18-20"). This finding is consistent with the difference-in-differences results.

Allowing under-21-year-olds to respond differently to cigarette taxes and smokefree indoor air laws than 23 to 25 year-olds yields similar point estimates across the full and sensitivity check samples (Table 2, columns 5 and 6). Coefficients on other policy variables — cigarette taxes, likelihood of exposure to smokefree indoor air laws, and being age-18 in a tobacco-19 locality — are not discussed due to high variance inflation factors indicative of multicollinearity, and, in the case of California's state-level tobacco-21 law, likely overidentification as well as the concurrent implementation of other tobacco-related policies (See Data Appendix). While it is appropriate to include these variables as controls, their coefficients should not be interpreted causally. [18]

These regressions suggest that living in a locality with complete tobacco-21 coverage would yield a 2.4 to 3.1 percentage point reduction in an 18 to 20 year-olds' likelihood of being an established smoker. However, locality-level tobacco-21 exposure is at most partial in these data, with policy-coverage ranging from 0 to 83%. Thus, the policy's impact on current established smoking among the average exposed respondent is the estimated effect for 18 to 20 year-olds (Table 3, Row A) times the average probability of policy-exposure among those with non-zero exposure (Table 3, Row B). This calculation suggests that, on average, tobacco-21 exposure between 2011 and December 31, 2016 reduced the probability of current established smoking by 1.2 to 1.1 percentage points among 18 to 20 year-olds exposed to these laws (Table 3, Row C).

DISCUSSION

Using quasi-experimental methods, this analysis provides what may be the first direct estimates of the relationship between local tobacco-21 restrictions and current smoking among 18 to 20 year-olds in U.S. metropolitan areas. Specifically, results indicate that sub-state tobacco-21 policies yield a statistically significant reduction in smoking among 18 to 20 year-olds residing in MMSAs. Analyses find a 1.1 - 1.2 percentage point reduction in the average treated respondents' likelihood of current established smoking in response to local tobacco-21 policies.

With average smoking rates of 11.9 percent in this age-group, these full-exposure estimates span the gap between the Institute of Medicine Report's hypothesized "medium effect" and "large effect" ranges (reductions of 15 to 20 percent versus 25 to 30 percent, respectively). [5] However, these results cannot be directly compared to that model, as the latter is based on hypothesized initiation effects (i.e., having smoked at least 100 cigarettes), not current established smoking.

Similar implications from difference-in-differences and triple-difference analyses support a causal interpretation of these findings. Moreover, the data cover metropolitan areas across 44 states and Washington, D.C., increasing confidence about their generalizability to other U.S. metropolitan areas. This evidence is critical to informing policymaking.

Tobacco-21 policies may influence smoking through a variety of mechanisms, including access to cigarettes, attitudes towards smoking, and/or consequent peer effects (i.e., if shifts in cigarette access or attitudes towards smoking alter peer behavior). Any or all of these factors could explain the relationships estimated here. Further work is needed to pinpoint whether

impacts on own- or peer-access (for example) are more responsible for associated reductions in young adult smoking.

An array of research indicates that consumers circumvent local tobacco policies by crossing borders to buy cigarettes in lower tax areas. [19, 20, 21, 22, 23] While such policy avoidance would be expected to reduce tobacco-21 restrictions' efficacy, this study finds a substantive reduction in smoking among 18-20 year-olds associated with local tobacco-21 laws. This result not only indicates potential for an even greater impact from statewide measures to raise the minimum legal sales age, but also provides an argument for rescinding preemption laws that forbid local jurisdictions from passing such restrictions. According to U.S. Census data for 2017, 19,611,698 civilians under age-21 reside in the 16 states that have such preemption laws but had not passed state-wide tobacco-21 laws as of June 1, 2019, accounting for about 23% of the US population in that age-group. [24, 25] Thus, with local tobacco-21 policies offering a means to reduce smoking among 18 to 20 year-olds, state preemption laws stand in the way of potential health gains.

Critically, the electorate broadly supports tobacco-21 policies. Estimates indicate that two-thirds to three-quarters of U.S. adults favor raising the minimum age of tobacco sales to 21. [26, 27, 28] A five-state survey of adults' attitudes towards these policies found even higher support, with the lowest level at 78 percent (Texas), and the highest at 85 percent (New York). [29] While 14 states had passed tobacco-21 laws as of June 1, 2019, 16 of the non-adopting states have laws preempting local governments from implementing such regulations. The combination of this study's findings and broad electoral support present a strong argument for tobacco-21 policies at both the state and local level.

Limitations

This study has several limitations. First, changes in BRFSS sampling and weighting procedures preclude the inclusion of pre-2011 data. [6] Short pre-trends are not ideal for difference-in-differences models. Reassuringly, findings are consistent between difference-in-differences and triple-difference analyses; and, a parallel trends test cannot reject the null hypothesis that smoking rates in localities with versus without tobacco-21 laws before January 1, 2017 were moving in parallel prior to these laws' adoption (Appendix Table A1). Still, while statistically insignificant, those pre-trend coefficients average to -2.2 percentage points, less than three-quarters the size of the difference-in-differences analyses' tobacco-21 coefficients (Table 2, columns 1 and 2). Those who find the pre-trend coefficients' sizes to be of concern may prefer to interpret this paper's findings as evidence of an association more so than causation.

Second, self-reported smoking status data present a potential under-reporting issue. BRFSS smoking responses are largely consistent with other nationally representative surveys. [30] However, if tobacco-21 policies reduce under-21-year-olds' willingness to report smoking in the context of an anonymous survey, the estimated policy effects may be biased upwards. Future studies should use data with biochemically verified abstinence to assess this possibility.

The third limitation concerns generalizability: these results may not apply to minors or more rural areas. The analysis cannot test for lasting impacts of past tobacco-21 exposure, as the policies are sufficiently new that few young adults in their mid-twenties and above were exposed to them before turning 21. Similarly, due to data limitations, this analysis does not consider how tobacco-21 laws influence use of other tobacco products such as e-cigarettes, or substitution between products.

Fourth, statistically insignificant, positive coefficients for the local tobacco-21 impact on 23 to 25 year-olds may suggest that localities passed these laws in response to rising young adult smoking. If so, the difference-in-differences estimates will be biased towards the null whereas the triple-difference coefficient on the tobacco-21 by under-age-21 interaction term would give a more accurate estimate of the policy's impact.

Finally, a quasi-experimental analysis is not a randomized controlled trial. The ability to identify causal tobacco-21 effects using such methods depends on several assumptions, specifically: (1) smoking in localities without tobacco-21 policies constitutes an appropriate counterfactual for smoking in localities with these policies, holding all control variables constant; (2) trends in smoking among 23 to 25 year-olds are a reasonable counterfactual for trends in smoking among 18 to 20 year-olds absent these policies (level-differences are not a problem, as these are directly controlled for via year-of-age fixed effects); and, (3) no other changes occurred concurrently with the tobacco-21 policies that differentially affected smoking among 18 to 20 year-olds relative to 23 to 25 year-olds. These assumptions appear to hold: the analysis passes a parallel trends test to validate assumption 1; and, locality-level current established smoking rates among 23-25 year-olds are statistically significant predictors of the corresponding rates for 18-20 year-olds prior to tobacco-21 policies being implemented, consistent with assumption 2 (See Appendix Table A2). The third assumption — that no concurrent changes differentially affected smoking among 18 to 20 year-olds relative to 23 to 25 year-olds — cannot be proven with certainty but is supported by the evidence, as other tobacco policies that changed during this period (e.g., taxes, smokefree air laws) applied to both age-groups. Allowing these policies to have differential impacts by age-group yields similar tobacco-21 results to the main analyses (Table 2, Columns 5 and 6). Still, the failure these assumptions

would mean that the coefficients on local tobacco-21 exposure estimated here should be viewed as associations rather than estimates of a causal impact.

Conclusions

Local tobacco-21 policies yield substantive reductions in smoking among 18 to 20 year-olds. Given the addictiveness and health consequences of cigarette smoking, this finding supports such policies' implementation as a means to promote public health.

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DECLARATION OF INTERESTS

The authors have no conflicts of interest, financial or otherwise, related to this research.

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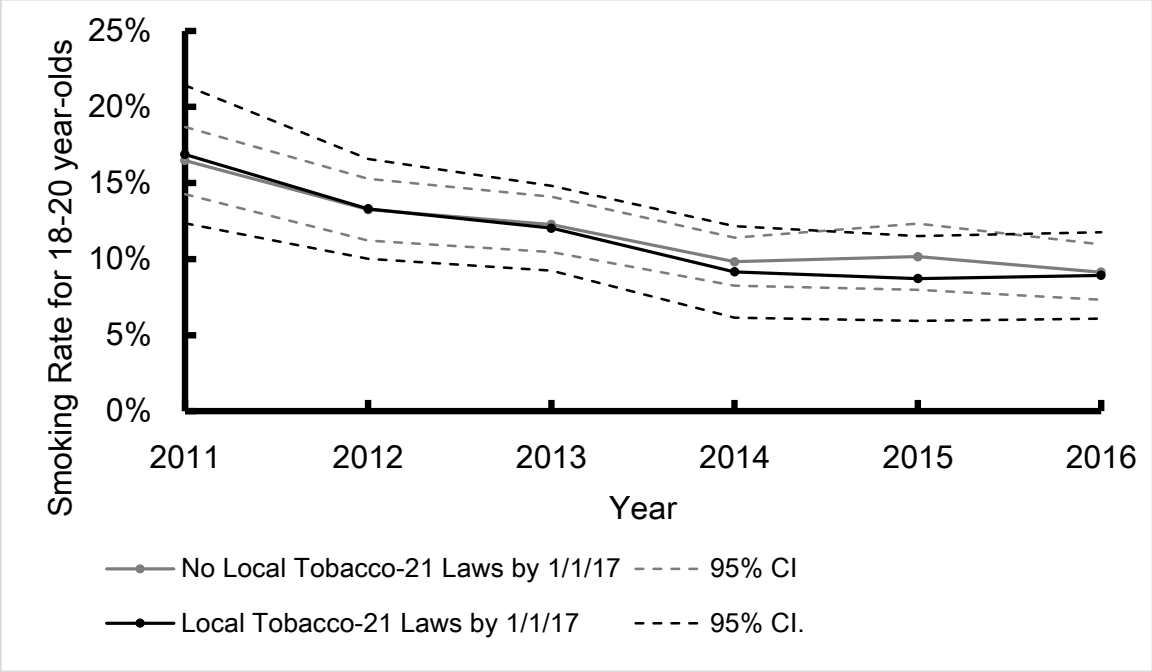
Figures

Figure 1: Trends in 18-20 year-olds' Smoking Rates, by Local Tobacco-21 Adoption



Notes: Data on 18-20 year-old respondents to the 2011-2016 BRFSS SMART dataset are used to calculate annual, sample-weighted smoking rates and 95% confidence intervals for those living in localities that did versus did not adopt a tobacco-21 policy before January 1, 2017. Figures restrict consideration to the sensitivity-check sample (i.e., localities with at least ten 18 to 20 year-old respondents in every survey year, to ensure that single-digit sample sizes do not drive results).

Tables

Table 1: Summary Statistics

	All	Ages 18 to 20			Ages 23 to 25		
Localities:	All	All	Treatment	Control	All	Treatment	Control
Age	21.4	18.9	18.9	18.9	24.0	24.0	24.0
Female	48.3%	48.1%	50.1%	47.5%	48.5%	48.9%	48.4%
Current established smoker	16.2%	11.9%	11.3%	12.0%	20.8%	19.7%	21.1%
Current student	33.3%	50.0%	55.8%	48.5%	15.3%	17.0%	14.8%
Has health insurance	76.4%	77.2%	83.4%	75.6%	75.6%	80.1%	74.4%
Cell phone survey	74.0%	69.6%	66.2%	70.4%	78.7%	70.4%	81.1%
Race & Ethnicity							
White	62.5%	62.0%	59.0%	62.8%	63.1%	58.2%	64.5%
Black	15.7%	15.8%	17.7%	15.3%	15.7%	16.8%	15.3%
Asian	8.5%	8.7%	11.6%	8.0%	8.3%	12.1%	7.3%
Native Hawaiian/ Pacific Islander	0.7%	0.7%	1.0%	0.7%	0.6%	0.8%	0.6%
American Indian/ Alaska Native	2.3%	2.4%	1.3%	2.7%	2.1%	1.6%	2.2%
Multiple races	2.6%	2.8%	1.9%	3.0%	2.3%	1.9%	2.4%
Other race	4.0%	4.1%	3.2%	4.4%	3.9%	3.1%	4.1%
Hispanic	23.9%	23.6%	17.3%	25.2%	24.3%	20.7%	25.3%
Policies							
Pr(Local Tobacco-21 Exposure)	3.4%	3.1%	15.2%	-	3.7%	16.8%	-
State cigarette tax	$1.75	$1.73	$3.01	$1.40	$1.78	$3.17	$1.39
Pr(Comprehensive smoke-free indoor air law exposure)	52.3%	52.3%	92.9%	41.8%	52.3%	93.9%	40.7%
Age 18 in Tobacco-19 locality	1.6%	3.1%	10.0%	1.3%	-	-	-
N	56,601	25,066	4,813	20,253	31,535	6,709	24,826

Notes: Summary statistics are sample-weighted means using data on 18-20 and 23-25 year-old respondents to the 2011-2016 BRFSS SMART dataset. Consideration is limited to MMSA-by-state localities with 18-20 year-old respondents interviewed in every survey year, that did not change their local cigarette taxes during the period of analysis. "Treatment Localities" refer to MMSA-by-state areas containing at least one jurisdiction with a tobacco-21 policy prior to 2017. "Control Localities" are MMSA-by-state areas wherein no jurisdictions were subject to tobacco-21 policies by that date. Comprehensive smoke-free indoor air laws are those covering restaurants, bars, and non-hospitality worksites. Pr(Local Tobacco-21 Exposure) and Pr(Comprehensive smoke-free indoor air law exposure) are constructed at the locality-level (See Data Appendix). The following variables have some missing-observations: current established smoker (3.9% in the full analytic sample), race (3.7%), Hispanic (0.8%), student status (0.3%), health insurance (2.5%), and annual household income (25.8%).

Table 2: Tobacco-21 Laws and Current Established Smoking, Coefficient/(Standard Error)

	Difference-in-Differences (Ages 18-20)		Triple-Difference (Ages 18-20 vs. 23-25), Drop Boston Metropolitan Division			
Sample	Full	Sensitivity	Full	Sensitivity	Full	Sensitivity
	(1)	(2)	(3)	(4)	(5)	(6)
Pr(Local Tobacco-21 Exposure)	-0.0306*	-0.0309*	0.0309	0.0299	0.0307	0.0296
	(0.012)	(0.012)	(0.019)	(0.018)	(0.019)	(0.018)
Pr(Local Tobacco-21 Exposure) * Age < 21			-0.0568**	-0.0548**	-0.0559*	-0.0540*
			(0.020)	(0.020)	(0.021)	(0.021)
Other Policy Variables						
Pr(Comprehensive smoke-free indoor air law)	-0.0814	-0.0973	-0.0693	-0.0893	-0.1287	-0.1404
	(0.076)	(0.079)	(0.145)	(0.149)	(0.132)	(0.135)
State cigarette tax	-0.0068	0.0054	0.0018	0.0212	0.0058	0.0247
	(0.013)	(0.011)	(0.039)	(0.037)	(0.042)	(0.041)
Age 18 in Tobacco-19 locality	0.0080	0.0088	0.0054	0.0056	0.0055	0.0057
	(0.026)	(0.027)	(0.030)	(0.031)	(0.030)	(0.031)
State cigarette tax * Age < 21					-0.0079	-0.0067
					(0.017)	(0.019)
Pr(Comprehensive smoke-free air law) * Age < 21					0.1254*	0.1090
					(0.062)	(0.056)
Constant	0.3428**	0.3293**	0.3044**	0.2809**	0.2253	0.2024
	(0.043)	(0.046)	(0.098)	(0.102)	(0.142)	(0.143)
Fixed Effects: Year, Locality, Age	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects: Year*Age <21, Locality*Age<21, Year*Locality	No	No	Yes	Yes	Yes	Yes
N	24067	21851	53139	48327	53139	48327
Adjusted R^2	0.081	0.083	0.087	0.085	0.087	0.085
Full Tobacco-21 Effect, ages 18-20	-0.0306*	-0.0309*	-0.0259	-0.0250	-0.0252	-0.0243

Notes: Data cover 18-20 and 23-25 year-olds in the 2011-2016 BRFSS SMART dataset, restricting consideration to MMSA-by-state localities with data on 18-20 year-olds in every survey year, where no jurisdictions changed their local cigarette tax during this period (the "Full" sample). Survey-weighted linear probability models consider how current established smoking among 18-20 year-olds varies with different levels of exposure to tobacco-21 laws over time. The "Sensitivity" sample includes only those localities with 10 or more 18-20 year-old respondents in every year. Controls not indicated above are fixed effects for race, ethnicity, income group, current student status, and current health insurance coverage; missing-observation indicators for each of these; and, binary indicators for California's state-level tobacco-21 law as well as an

interaction between that variable and an under-age-21 indicator. The "Full tobacco-21 effect, ages 18-20" is the row-1-coefficient for difference-in-differences analyses, and the sum of the overall exposure (row 1) and under-21 exposure coefficients (row 2) for triple-difference analyses. SEs are clustered by state. *(**) denote statistical significance at the 0.05(0.01) level. For full output, see Appendix Table A3.

Table 3: Impact of Local of Tobacco-21 Laws on Current Established Smoking among Partially-Exposed 18 to 20 year-olds

Sample:	Difference-in-Differences Analyses		Triple-Difference Analyses			
	Full (1)	Sensitivity (2)	Full (3)	Sensitivity (4)	Full (5)	Sensitivity (6)
A. Local Tobacco-21 Effect $_{\text{Ages 18-20}}$ *	-0.0306*	-0.0309*	-0.0259	-0.0250	-0.0252	-0.0243
B. Average Pr(Local Tobacco-21 Exposure | Non-zero exposure & Age < 21) =	0.4018	0.4018	0.4546	0.4546	0.4546	0.4546
C. Estimated Impact of Full Exposure * Average Exposure among Exposed	-1.2 percentage points	-1.2 percentage points	-1.2 percentage points	-1.1 percentage points	-1.1 percentage points	-1.1 percentage points

Notes: Analyses use data on 18-20 and 23-25 year-old respondents to the 2011-2016 BRFSS SMART dataset. The estimated impact of tobacco-21 policy exposure on current established smoking among 18 to 20 year-olds is presented in row A (Table 2's "Full Tobacco-21 Effect, ages 18-20"). Multiplying these by the mean probability of exposure among those exposed (Row B) yields the estimated policy effect on exposed individuals in these data (Row C). *(**) denote statistical significance at the 0.05(0.01) level for the coefficient estimates.