

Long-Term Effects of Laws Governing Youth Access to Tobacco

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Both opponents of smoking and purveyors of cigarettes have recognized the significance of adolescence as the period during which smoking behaviors are typically developed.^{1,2} Accordingly, initiatives to curb youth smoking were among the first federal policy restrictions placed on cigarette sales in the United States.^{3,4} Adoption of these initiatives was accelerated by passage of the 1992 Synar Amendment to the Federal Alcohol, Drug Abuse, and Mental Health Administration Reorganization Act, which mandated withholding federal block-grant money from states that failed to prohibit distribution of tobacco products to persons younger than 18 years, or failed to enforce such prohibitions. Various measures were implemented by individual states to increase retailer compliance with de jure purchase ages, such as restricting physical access to cigarette vending machines and banning the sale of single cigarettes.^{5,6}

Adaptation of these policies was partly motivated by community intervention studies suggesting that more uniform compliance with legal purchase age policies would reduce smoking rates among youths.⁷⁻¹¹ Some post-implementation studies have suggested that youth access restrictions are effective at reducing smoking among adolescents,¹²⁻¹⁴ whereas other studies have cast doubt on their effectiveness.^{6,15-17} Furthermore, opponents argue that youth access policies could inadvertently glamorize smoking as a sophisticated adult behavior, thus reinforcing messages historically associated with tobacco advertising.¹⁸ Hence, some argue that youth access restrictions divert resources away from well-established and universally targeted tobacco control policies, such as clean indoor air policies, price increases, and media campaigns.^{16,19}

Youth access measures were not designed to merely delay smoking, but presumably to deter progression by delaying onset or reducing the intensity of smoking during adolescence. As such, reductions in smoking that persist into

Objectives. We sought to examine the association between policies governing access to tobacco during adolescence and subsequent adult smoking.

Methods. We analyzed adult smoking data from the 1998 through 2006–2007 administrations of the US Current Population Survey Tobacco Use Supplement by employing a quasi experimental approach. Participants (n = 105 519) were adults, aged 18 to 34 years at the time of the survey. Smoking outcomes included having ever smoked 100 cigarettes, smoking at the time of the survey, and having smoked 10 or more cigarettes a day conditioned on being an ever smoker. These were predicted from exposure to state youth access policies at age 17 years.

Results. Four of the 9 policies exhibited significant associations with reduced prevalence of 1 or more smoking outcomes, primarily among women. Lesser effects for other policies could not be ruled out.

Conclusions. Restrictions on youth access to tobacco might lead to reduction in smoking prevalence later in adulthood. The effect might be limited to women; we estimate that having all policies in place could be associated with a 14% reduction in lifetime smoking prevalence for women, and an additional 29% reduction in heavy smoking among ever smokers. (*Am J Public Health.* 2013;103:1493–1499. doi:10.2105/AJPH.2012.301123)

adulthood are the proper benchmarks by which these policies should be assessed. In the current study, we examined whether youths who face a restrictive policy environment are less likely to smoke as adults. We analyzed long-term associations between state youth access policies and subsequent adult smoking by taking advantage of state-by-state and year-by-year policy differences as states adopted various measures in response to the Synar Amendment. We expect that policies effective in mitigating youth smoking also influence subsequent adult smoking. It has been shown, for example, that exposure to stricter drinking age laws are associated with lower prevalence of alcohol problems in adulthood, but to our knowledge, parallel studies have not been conducted for tobacco policy.²⁰⁻²² Hence, we examined whether policy encountered during adolescence is associated with smoking behaviors during adulthood. This hypothesis is consistent with contemporary neurobiological views of adolescence as a critical period for the development of addiction.²³⁻²⁵

METHODS

Data from the Tobacco Use Supplement (TUS) of the Current Population Survey (CPS) were merged with data on the policy environment that adult participants would have encountered prior to age 18 years. Past policy exposures were used to predict smoking outcomes at time of survey. State of residence at age 17 years and younger would be required to precisely assess exposure; however, the TUS-CPS does not collect historical state of residence data, so we used state of residence at the time of the survey as a proxy. We describe the reliability of this approximation in detail in an appendix (available as a supplement to the online version of this article at <http://www.ajph.org>), wherein we estimate that policy exposure is correctly classified in 92% to 96% of cases. We estimate that 86% of the population lived in the same state at the time of observation as they lived during adolescence; an additional 6% to 10% lived in states that are concordant with state of emigration with respect to policy.

Participants

Smoking data from the 1998 to 1999, 2001 to 2002, 2003, and 2006 to 2007 releases of the TUS-CPS were analyzed. The CPS is a continuing monthly survey—cosponsored by the US Census Bureau and the US Bureau of Labor Statistics—that collects data on topics related to employment, economic, and demographic characteristics of the US civilian, noninstitutionalized population aged 15 years and older. The TUS is sponsored by National Cancer Institute (NCI) and the Centers for Disease Control and Prevention (CDC), and is administered along with the CPS during selected months. The TUS-CPS uses a nationally representative sample to provide national- and state-level data on smoking and other tobacco use. The survey content, stratified sampling strategy, and detailed methods of the TUS-CPS are described elsewhere.²⁶

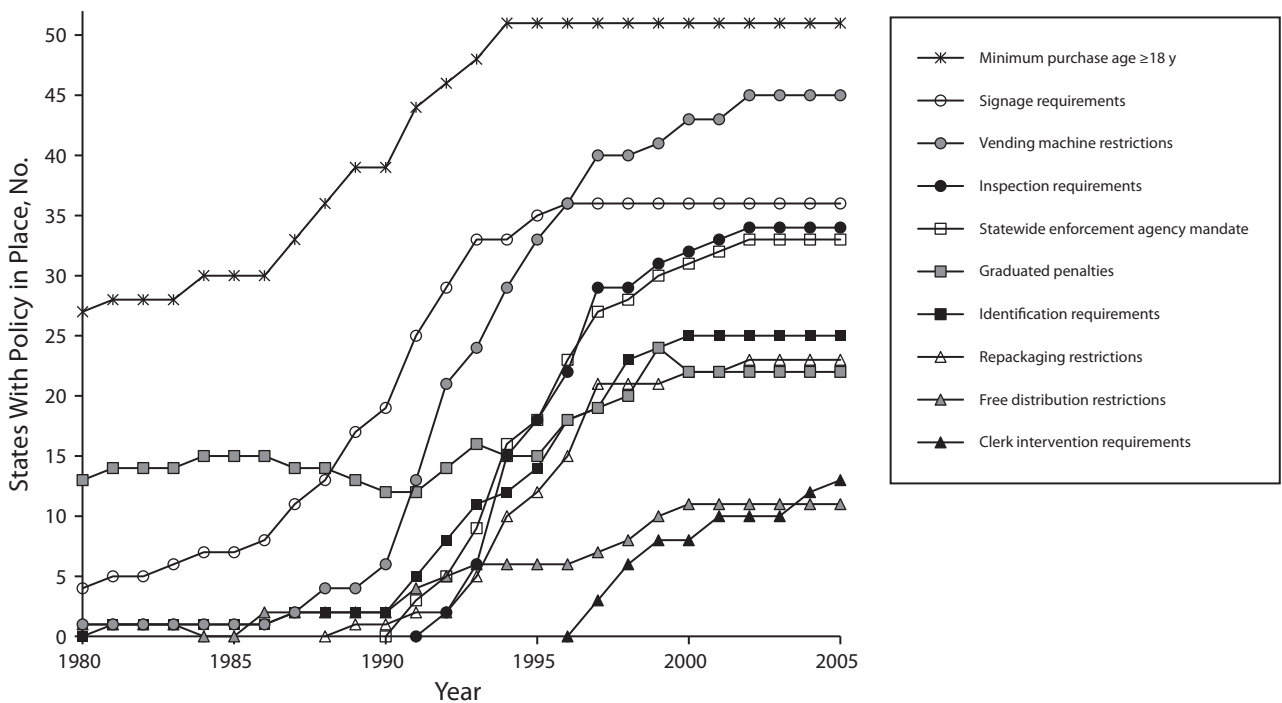
Because our interest is in adult smoking, analyses were focused on participants aged 18 years and older at the time of the survey. Because our analytical approach relied on differences across time and state to estimate

policy effects, we restricted the analyses to those born during 1973 through 1986 (n = 110 069); these were the individuals who reached legal purchase age during the period in which youth access policies were changing rapidly (1990 through 2003; Figure 1). We further restricted the sample to individuals who were living in states where the minimum purchase age for tobacco was 18 years or older. By 2003 all states had purchase ages of 18 years or older, but a few states did not adopt this criterion until the early 1990s. As a result, the analysis sample comprised n = 105 519 individuals, or 95.9% of the sample of participants who met the birth year criteria described previously. This eliminated heterogeneity with respect to the legal purchase age for tobacco, allowing us to focus on policies that further restrict access to tobacco by youths younger than 18 years.

Smoking Outcomes

Our first outcome of interest was whether participants had smoked 100 or more cigarettes in their lifetime (“ever smoking”), versus

those who never smoked, or had smoked fewer than 100 cigarettes. Our second outcome was whether participants smoked any cigarettes at the time of the survey. Specifically, participants were asked whether they “now smoke cigarettes every day, some days, or not at all.” Those participants who reported smoking some days or every day were counted as current smokers, contrasted with those who did not smoke at the time of the survey. This outcome is not independent from the ever smoking outcome because TUS-CPS participants are only asked about current smoking if they reached the 100-cigarette threshold. Nonetheless, it is included because it is the most relevant measure for public health. Finally, among ever smokers only, we analyzed “current heavy smoking,” defined as smoking 11 or more cigarettes per day at the time of the survey. For “every day” smokers, cigarettes per day was queried directly; for “some days” smokers, cigarettes per day was determined by multiplying number of days smoked over the past 30 days by number of cigarettes per smoking days and dividing by 30. Heavy



Source. US Census Bureau²⁶ and ImpacTeen.²⁸

FIGURE 1—Analysis of the number of states adopting each of the 9 target policies and a minimum purchase age of 18 years or older for tobacco: ImpacTeen Tobacco Control Policy and Prevalence Data and National Cancer Institute State Cancer Legislative Database, 1991–2008.

smokers were contrasted with light (≤ 10 cigarettes per calendar day) and former smokers. This threshold was based on the “Heaviness of Smoking Index,” a short test for nicotine dependence that counts smoking more than 10 cigarettes a day as a symptom of dependence.²⁷

Youth Access to Tobacco Policies as Independent Variables

Smoking outcomes were predicted from 9 youth access policy areas that states variously implemented just prior to and after passage of the Synar Amendment. We developed binary scores for each policy that were derived from an ordinal policy coding system proposed by Alciati et al.⁵ and maintained by the NCI State Cancer Legislative database. Policy data were obtained from the ImpacTeen Web site, which maintains a database of various tobacco control policies.²⁸ The following contrasts were tested:

1. Signage Requirements: Provision requiring the posting of a warning sign about minimum purchase age and penalizing retailers who fail to post such a sign versus no requirement.
2. Vending Machine Restrictions: Bans or location restrictions on cigarette vending machines versus no bans or restrictions.
3. Inspection Requirements: Provisions for random inspections of retailers to ensure compliance with youth access laws versus no requirements.
4. Graduated Penalties: Provisions that establish a series of increasingly steeper penalties to retailers who violate any youth access laws versus nongraduated penalties for some or all youth access laws, or delays of penalties beyond 3 years.
5. Identification Requirements: Provisions that require retailers to request identification from individuals appearing to be aged 21 or younger versus no provision.
6. Repackaging restrictions: Provisions designed to restrict the sale of single cigarettes, or inexpensive small packages, by requiring that cigarettes be sold in a sealed package that meets federal labeling requirements versus no provision.
7. Statewide Enforcement Authority: Provision establishing a statewide enforcement authority for tobacco sales versus no provision,

or provision only for a non-statewide authority.

8. Free Distribution Restrictions: Bans on free samples of tobacco products with minimal exceptions, versus no provision or location restrictions only.
9. Clerk Intervention Requirements: Provisions mandating that tobacco purchases take place through store clerks versus no policy.

Analytic Plan

Our analytical approach was an extension of a classical “differences-in-differences” method that compares 2 groups at 2 different time points, and for which one of the groups underwent an environmental and policy change or treatment while the other served as a control.^{29,30} We used logistic regression to extend the 2-by-2 analysis to multiple groups at multiple time points. In our case, the groups corresponded to states, and time points to birth years, because these variables determined the policy to which a person was exposed. Hence, we included these variables in all models as fixed effect, unordered categorical covariates. State effects account for all time-invariant characteristics of each state, and birth year effects account for all state-invariant characteristics of each birth year, allowing for estimation of the exposure effect.³¹

In addition to state and birth year fixed effects, we also included observed individual demographic covariates: respondent-reported race/ethnicity was coded as non-Hispanic White, non-Hispanic Black, Hispanic and Other; the latter category was chosen to avoid small cell sizes and excessive degrees of freedom. Age was incorporated as a continuous measure. Survey wave was included as a categorical variable. Gender was included as a covariate in primary analyses; gender-stratified analyses were also conducted. In second-stage models, 3 state policy covariates were included: tobacco excise taxes, an index of clean indoor air laws, and inflation-adjusted state tobacco funding per capita. Excise taxes were obtained from historical data³² and adjusted for inflation based on the annual Consumer Price Index. Clean indoor air laws were coded using a scoring system developed by Chiqui et al.³³; both clean indoor air and state tobacco control spending data were obtained from the ImpacTeen Web site.²⁸

Parameter estimates and standard errors were calculated using the “surveylogistic” procedure in SAS version 9.2 (SAS Institute, Cary, NC) using the state as the clustering unit.^{34,35} This approach accounts for intracorrelation of observations within clusters (states) in estimating standard errors, which is the primary consideration in standard error estimation when applying the differences-in-differences approach.^{31,35} Sampling error resulting from lower levels of clustering is expected to be small provided that the sample size from within a state is small relative to the full state population.^{36,37} Unweighted data were used for analyses; this is appropriate providing that policy effects are not substantially heterogeneous with respect to selection probability.³⁸

RESULTS

Table 1 presents the characteristics of the combined TUS-CPS samples, which, after applying exclusion criteria, comprised 105 519 participants with complete data. Altogether 34.4% reported ever having smoked 100 cigarettes, and 24.4% reported current smoking at the time of the survey. Current heavy smoking, defined as smoking more than 11 cigarettes per day, was endorsed by 44.4% of ever smokers (37 390 participants).

Youth Access Policies

Figure 1 plots the number of states that had adopted each of the youth access policies by year, using the coding previously described. The period selected for our analyses, 1990 through 2003, bracket most of the policy change associated with the Synar Amendment (Figure 1). Individual states strengthened some policies during the 1980s, particularly signage requirements and vending machine restrictions, but most major changes took place during the period on which we focus.

Single Policy Analyses

We predicted the 3 smoking outcomes from each of the 9 policies. Each policy variable was analyzed separately while adjusting for demographic covariates, state, and birth year fixed effects, and survey wave as described. Estimates of policy effects were made for the full sample, for men alone, and for women alone. These results are reported in Table 2. In the

TABLE 1—Smoking Prevalence Among Individuals Born in the United States Between 1973–1986: US Current Population Survey Tobacco Use Supplement, 1998–2007

	No. (%)	Ever Smoking (≥ 100 Lifetime Cigarettes), % (SE)	Current Smoking, % (SE)	Current Heavy Smoking Among Ever Smokers (n = 35 745), % (SE)
Gender				
Women	58 244 (55.2)	32.2 (1.3)	22.3 (1.1)	42.1 (1.4)
Men	47 275 (44.8)	37.2 (1.0)	26.9 (0.9)	46.9 (1.5)
Race/ethnicity				
White	72 267 (68.5)	39.9 (0.7)	28.0 (0.7)	47.4 (1.1)
Black	15 857 (15.0)	20.5 (1.0)	16.7 (0.9)	40.7 (1.4)
Hispanic	10 445 (9.9)	20.5 (1.0)	14.0 (1.3)	25.1 (2.4)
Other	6950 (6.6)	30.1 (2.3)	22.3 (1.8)	37.1 (2.0)
Birth year^a				
1973–1979	66 781 (63.3)	36.2 (1.1)	24.6 (0.9)	43.7 (1.5)
1980–1986	38 738 (36.7)	31.3 (1.3)	24.0 (1.1)	45.8 (1.3)
Age,^b y				
18–24	55 218 (52.3)	31.7 (1.2)	25.0 (1.1)	49.0 (1.3)
25–29	39 521 (37.5)	36.4 (1.1)	25.3 (1.0)	43.1 (1.5)
30–34	10 780 (10.2)	34.7 (1.3)	21.6 (1.0)	40.5 (1.5)
Total	105 519 (100.0)	34.4 (1.1)	24.4 (1.0)	44.4 (1.4)

^aBirth year categories are for descriptive purposes only. Birth year was included in regression analyses as an unordered categorical variable.

^bAge categories are for descriptive purposes only. Age was included in regression analyses as a continuous variable.

full sample, only 2 of the 9 policies were associated with statistically significant ($P < .05$) reductions in any outcome prevalence. Vending machine restrictions appeared to reduce both current smoking (odds ratio [OR] = 0.93; 95% confidence interval [CI] = 0.87, 0.99) and current heavy smoking among ever smokers (OR = 0.91; 95% CI = 0.84, 0.99). Identification requirements were associated with reductions in prevalence of ever smoking (OR = 0.94; 95% CI = 0.88, 0.997) and current smoking (OR = 0.98; 95% CI = 0.87, 0.99).

Because some studies have suggested that underage women may have less trouble purchasing cigarettes than young men^{11,32,39–42} we also conducted gender-stratified analyses. Among women, both vending machine restrictions and ID requirements exhibited stronger associations than in the full sample. In addition, for the women-only analyses, repackaging restrictions were associated with statistically significant reduction in prevalence of current smoking (OR = 0.93; 95% CI = 0.87, 0.99); restrictions on free-sample distribution were associated with a significant reduction in ever smoking prevalence (OR = 0.88; 95% CI =

0.77, 0.996), and the presence of a statewide enforcement agency was associated with a reduction in prevalence of current heavy smoking among ever smokers (OR = 0.87; 95% CI = 0.77, 0.98). By contrast, for analyses limited to men, only 1 policy showed significant association with any outcome: sign-posting requirements were associated with a reduction in current heavy smoking prevalence among ever smokers (OR = 0.84; 95% CI = 0.72, 0.98).

Multipolicy Models

Some of the policies analyzed in Table 2 exhibited significant associations with adult smoking, primarily for women. However, interpretation of those results is limited by 2 factors. First, the policies are correlated with each other. Second, it is impossible to rule out small effects for policies that did not attain nominal statistical significance. Prompted by these limitations, 2 sets of analyses were conducted to test aggregate-policy associations. In the first set, we used the results from single-policy analyses to select those with the strongest evidence of effectiveness, and then

tabulated a count of these policies in place for each state and birth year combination. Policies were selected if they exhibited a negative coefficient of association for each outcome in the full-sample analysis and were significantly associated with any outcome in either the full sample or gender-stratified analyses. This resulted in 4 policies: vending machine restrictions, identification requirements, repackaging restrictions, and the presence of a statewide enforcement agency. The predictor variable was the count of these 4 policies based on state and birth year. The second approach was to use the count of all 9 policies, regardless of the results of the single policy analysis. By expressing these variables as counts, these models assume independent, homogenous contribution of each policy to smoking behavior.

Table 3 summarizes the results of the multiple policy analyses. The top half of the Table lists results for the models using the same set of covariates described before, and the bottom half includes models that adjust for potential policy confounders including state tobacco excise tax, clean indoor air policies, and state funding for spending on tobacco control at the time of observation. The 4-policy count was associated with significant or near-significant reductions in prevalence of ever smoking, current smoking, and current heavy smoking among ever smokers in the full sample, but stratification by gender indicated that the associations were only significant for women. The 9-policy count exhibited similar results, but associations with current heavy smoking among ever smokers did not meet the statistical significance threshold. After adjustment for additional policy covariates (Table 3), both counts were significantly, or near-significantly associated with reductions in all 3 outcome prevalences for women, but not men. To test whether policy associations diminish with age, interactions between age and policy were incorporated into each model. In no case was the interaction significant (all interaction $P > .09$; data available as a supplement to the online version of this article at <http://www.ajph.org>).

Finally, to test whether states with high baseline smoking prevalence were more or less likely to adopt restrictive youth access policies, we calculated the correlation coefficient between the prevalence of “current smoking” in 1999 and the policy count variable in 2006.

TABLE 2—Adjusted Associations Between 9 Youth Access Policies and Smoking Status: US Current Population Survey Tobacco Use Supplement, 1998–2007

	Ever Smoking (≥ 100 Lifetime Cigarettes; n = 105 519), OR (95% CI)	Current Smoking (n = 105 519), OR (95% CI)	Current Heavy Smoking Among Ever Smokers (n = 35 745), OR (95% CI)
Full Sample			
Signage requirements	1.05 (0.97, 1.15)	1.01 (0.91, 1.13)	0.88 (0.76, 1.02)
Vending machine restrictions	0.95 (0.89, 1.01)	0.93 (0.87, 0.99)	0.91 (0.84, 0.99)
Inspection requirements	1.01 (0.96, 1.06)	1.00 (0.94, 1.06)	0.98 (0.90, 1.08)
Graduated penalties	1.01 (0.95, 1.07)	0.99 (0.91, 1.07)	0.99 (0.88, 1.12)
Identification requirements	0.94 (0.88, 0.997)	0.93 (0.87, 0.99)	0.97 (0.88, 1.08)
Repackaging restrictions	0.95 (0.89, 1.01)	0.94 (0.87, 1.01)	0.94 (0.85, 1.04)
Statewide enforcement authority	0.98 (0.92, 1.03)	0.97 (0.90, 1.04)	0.94 (0.86, 1.04)
Free distribution restrictions	0.95 (0.87, 1.03)	1.02 (0.96, 1.09)	1.07 (0.95, 1.20)
Clerk intervention requirements	0.99 (0.93, 1.07)	1.00 (0.86, 1.15)	1.01 (0.82, 1.24)
Women			
Signage requirements	1.07 (0.95, 1.20)	1.00 (0.87, 1.15)	0.92 (0.73, 1.15)
Vending machine restrictions	0.91 (0.84, 0.98)	0.89 (0.82, 0.96)	0.86 (0.77, 0.97)
Inspection requirements	0.99 (0.94, 1.05)	0.98 (0.92, 1.05)	0.96 (0.85, 1.09)
Graduated penalties	1.01 (0.94, 1.09)	1.01 (0.94, 1.07)	0.99 (0.86, 1.13)
Identification requirements	0.91 (0.84, 0.99)	0.91 (0.84, 0.98)	0.92 (0.79, 1.06)
Repackaging restrictions	0.93 (0.86, 1.01)	0.93 (0.87, 0.99)	0.89 (0.76, 1.05)
Statewide enforcement authority	0.97 (0.91, 1.04)	0.97 (0.89, 1.05)	0.87 (0.77, 0.98)
Free distribution restrictions	0.88 (0.77, 0.996)	0.96 (0.87, 1.06)	1.09 (0.91, 1.30)
Clerk intervention requirements	0.99 (0.91, 1.09)	0.99 (0.86, 1.14)	1.08 (0.86, 1.36)
Men			
Signage requirements	1.04 (0.96, 1.11)	1.03 (0.91, 1.16)	0.84 (0.72, 0.98)
Vending machine restrictions	0.99 (0.92, 1.06)	0.96 (0.88, 1.05)	0.97 (0.88, 1.07)
Inspection requirements	1.03 (0.97, 1.10)	1.02 (0.95, 1.10)	1.01 (0.91, 1.12)
Graduated penalties	1.00 (0.93, 1.08)	0.97 (0.86, 1.10)	1.01 (0.87, 1.17)
Identification requirements	0.96 (0.89, 1.03)	0.95 (0.88, 1.03)	1.04 (0.93, 1.16)
Repackaging restrictions	0.96 (0.88, 1.05)	0.95 (0.84, 1.06)	0.99 (0.89, 1.12)
Statewide enforcement authority	0.98 (0.91, 1.06)	0.97 (0.89, 1.06)	1.03 (0.92, 1.16)
Free distribution restrictions	1.03 (0.96, 1.11)	1.10 (0.997, 1.21)	1.05 (0.94, 1.17)
Clerk intervention requirements	1.00 (0.91, 1.10)	1.02 (0.85, 1.22)	0.94 (0.75, 1.19)

Note: CI = confidence interval; OR = odds ratio.

The correlation coefficient was small (-0.02) and nonsignificant ($P = .87$).

DISCUSSION

Our findings suggest that restricted access to tobacco during adolescence is associated with reduced smoking prevalence in adulthood, but the associations are observed only among women. No single policy exhibited large associations, but various policies may have had small, additive effects. Significant associations with prevalence of both ever smoking and

current smoking were observed. Additionally, in analyses limited to lifetime smokers, policies were independently associated with lower prevalence of current heavy smoking.

Although individual policy associations were small, the aggregate effect of multiple policies could be substantial. For example, the multi-policy models (Table 3) suggested that each of the 4 most consistently associated policies could decrease odds for ever smoking by 3.6% for women, or that all 4 combined could lower odds by nearly 14%. Similarly, the cumulative effect of the 4 policies would be a 29%

reduction in odds for current heavy smoking among ever smokers. Taken together, these findings offer evidence that youth access policies may promote lower rates of smoking initiation among women, in addition to possibly leading to lighter smoking or quitting.

The 4 policies that exhibited the strongest results when analyzed singularly were vending machine restrictions, identification requirements, repackaging restrictions, and restrictions on free-sample distribution. A mandated statewide enforcement agency was associated with reduced current heavy smoking among ever smokers. Because individual associations were small, we cannot rule out small effects for other policies that were not detected because of power limitations. Accordingly, we predicted smoking from the count of all 9 policies. These results were also consistent with the interpretation that youth access policies exhibit small, additive effects on smoking prevalences, but only for women. Although we cannot rule out the possibility of minor effects for men, the differences in effect sizes between women and men were striking.

Any interpretation of the gender differences in the policy associations is necessarily speculative given our current knowledge. However, given that the Synar policies were intended to intensify the enforcement of nominal purchase ages, a plausible interpretation is that, absent uniform enforcement protocols, girls might have an easier time obtaining age-restricted goods than boys. For example, in a South Australian study, girls younger than 16 years had more success than boys when attempting to illicitly purchase cigarettes.⁴³ In the United States, at least 5 studies—largely conducted prior to the full implementation of Synar amendment policies—exhibited similar findings.^{11,39–42} Parallel gender differences have been reported for field studies of underage alcohol purchase.^{44–46} We found no field studies of underage alcohol or tobacco purchases that reported higher success rates for boys. Hence, policies that promote more uniform enforcement of nominal restrictions, such as the Synar policies, might have larger effects on girls' access to tobacco than on boys' access.

Interpretation of our results should be tempered by several limitations. Despite using an analytical design that controls for many potential unobserved confounders, alternative

TABLE 3—Adjusted Associations Between Counts of Selected 4 and All 9 Youth Access Policies and Smoking Outcomes: US Current Population Survey Tobacco Use Supplement, 1998–2007

	Ever Smoking (≥ 100 Lifetime Cigarettes; n = 105 519), OR (95% CI)	Current Smoking (n = 105 519), OR (95% CI)	Current Heavy Smoking Among Ever Smokers (n = 35 745), OR (95% CI)
Model 1^a			
Full Sample			
Selected 4 policies	0.974 (0.956, 0.993)	0.968 (0.950, 0.987)	0.968 (0.937, 1.000)
All 9 policies	0.990 (0.979, 1.002)	0.987 (0.975, 1.000)	0.984 (0.964, 1.005)
Women			
Selected 4 policies	0.964 (0.942, 0.986)	0.959 (0.937, 0.981)	0.935 (0.894, 0.978)
All 9 policies	0.984 (0.968, 0.999)	0.981 (0.967, 0.996)	0.972 (0.942, 1.002)
Men			
Selected 4 policies	0.986 (0.961, 1.010)	0.977 (0.949, 1.006)	1.004 (0.967, 1.043)
All 9 policies	0.998 (0.985, 1.010)	0.994 (0.978, 1.010)	0.998 (0.973, 1.022)
Model 2^b			
Full Sample			
Selected 4 policies	0.975 (0.957, 0.993)	0.968 (0.950, 0.987)	0.967 (0.936, 0.999)
All 9 policies	0.990 (0.979, 1.002)	0.987 (0.974, 0.999)	0.983 (0.962, 1.004)
Women			
Selected 4 policies	0.964 (0.942, 0.987)	0.959 (0.937, 0.981)	0.935 (0.893, 0.978)
All 9 policies	0.984 (0.968, 0.999)	0.980 (0.966, 0.995)	0.970 (0.940, 1.001)
Men			
Selected 4 policies	0.986 (0.962, 1.010)	0.977 (0.950, 1.006)	1.004 (0.966, 1.043)
All 9 policies	0.997 (0.985, 1.010)	0.993 (0.977, 1.010)	0.997 (0.973, 1.021)

Note. CI = confidence interval; OR = odds ratio. Selected 4 policies are vending machine restrictions, identification requirements, packaging restrictions, and presence of a statewide enforcement agency.

^aAdjusted for demographic variables and survey wave (see Methods).

^bFurther adjusted for state excise tax, clean indoor air policy, and tobacco control funding per capita at time of survey.

explanations for reductions in smoking prevalence among those exposed to more restrictive policies cannot be exhaustively ruled out; potential unmeasured confounders include any factors that changed in the same states and at the same times as state youth access policies. On the other hand, we found no evidence for reverse causation (i.e., there was no correlation between baseline prevalence of smoking at the state level and the number of youth access policies subsequently adopted). Because we examined policy exposure at age 17 years, and some participants would have reached smoking milestones prior to this age, the protective effects of policy with ever smoking prevalence might be underestimated. Nonetheless, the choice of age 17 years is appropriate because most young people do not begin smoking regularly prior to that age.⁴⁷ Also, because the TUS-CPS provides a year but not the exact date

of birth, there is some random error introduced into the policy exposure estimation, in addition to that caused by the proxy use of current state of residence for state of residence during adolescence (see Methods and supplemental data). Random error would bias our findings toward underestimating the effects of policy exposure. Finally, voluntary retailer practices or local ordinances might place restrictions on youth access to tobacco beyond state policy; these effects would not be detected by our analyses. Limitations notwithstanding, the finding that youth access laws might result in substantial, long-term reductions in smoking prevalence, even if reductions are limited to women, is novel and significant. In addition to replication in other samples, future research on potential mediating mechanisms for youth access policies is warranted. For example, youth access policies might influence social

norms and other factors more proximal to youth smoking.

Analyses of long-term effects associated with major policy such as the Synar Amendment help inform evidence-based policy evaluation.⁴⁸ Although a number of studies have described the short-term influence of youth access measures on adolescents^{6,12,13,15} the results of this study address an area of unmet need by describing their long-term association with behaviors later in life and providing vital information for evaluating long-term benefits against costs. Policies aimed at affecting the health of adolescents and young adults might have effects that persist into older adulthood.^{20–22} Our analyses of the long-term outcomes associated with Synar Amendment policies provide evidence that maintaining or even strengthening youth access measures might be a worthwhile endeavor. ■

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Contributors

R. A. Gruzza designed and partially conducted the analysis, obtained the funding, completed the statistical analysis, and authored the article. A. D. Plunk conducted supporting analyses and contributed to writing of the article. P. R. Hipp and M. J. Krauss conducted analyses and contributed to writing of the article. P. Cavazos-Rehg, R. C. Brownson, and L. J. Bierut contributed to interpretation of results and writing of the article.

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Human Participant Protection

These analyses used deidentified data obtained from the US Census Bureau and the Minnesota Population Center, University of Minnesota. Study procedures were reviewed and approved by the Washington University Human Research Subjects Protection Office.

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