Supplemental Material

Appendix 1.Age interaction analysis.

Because the primary analyses focused on long-term effects of laws targeting youth behavior, and the sample comprised both young adults and individuals well beyond the age of majority, it is possible that the policy associations documented in Tables 2 and 3 may vary with age. For example, effects may fade with time if policies serve to delay, rather than prevent smoking. Likewise, because age is highly correlated with length of time smoked, policy effects on the likelihood of quitting or becoming a heavy smoker may vary.

Policy exposure is completely determined by state and year of birth, which is why the inclusion of these as unordered categorical variables is key for estimation of the policy effect. However, when an age x policy interaction is introduced, a strong possibility for confounding exists, because the state and birth year variables account for policy exposure, but not for the product of age and policy. Hence, age-state and age-birth year interactions must also be introduced into the model for reasons directly analogous to those for including the state and birth year main effects.

Results of analyses that include age-policy interactions and these additional covarates are presented in 3 steps in Tables S1 through S3. In each table, “Model 1” is identical to models presented in the bottom half of main body Table 3, which presented the results of policy count analyses using the four-selected policies from single-policy results, as well as the count of all nine policies, and included demographic and policy covariates. “Model 2” in Tables S1 through S3 demonstrates the effect of including the additional age-by-state and age-by-birth year interactions, and show that the inclusion of these additional covariates results in similar estimates and only modest increases in uncertainty. Finally, Model 3 in each Table incorporates the age-by-policy interaction. In no case was the interaction between age and policy count significant.

Appendix II. The effects of cross-state migration on exposure misclassification and coefficient estimation.

Introduction

The primary analyses examined smoking outcomes for adults aged 18 to 34 responding to the Current Population Survey Tobacco Use Supplement (CPS-TUS) and their association with state policy environments encountered prior to age 18. Because we only have information on state of residence at the time of survey, we relied on the approximation that each subject lived in the same state prior to age 18 as they did when they completed the CPS-TUS survey. The surveys were conducted during the period 1999 through 2007, whereas subjects turned 18 during the years 1990 through 2003. Hence, the period between the subjects' 18th birthday and the time they responded to the survey varied from zero to seventeen years. Error is introduced into policy exposure estimation because a number of subjects would have changed state of residence during this period. However, this error is mitigated by the fact that a change in state of residence does not necessarily result in an incorrectly imputed policy: Some participants, by chance, move to states with similar policies. We used data on migration patterns from a general population longitudinal survey collected during the relevant time period to examine this effect empirically, and to estimate the misclassification rate when state of exposure is approximated as state of residence at time of survey. Because of concerns about attrition in the general population panel, we also compared migration rates from that sample with those from the 2000 U.S. Census.

Finally, we calculated the expected misclassification rate as a function of migration rate under the assumption of random migration with regard to state policy (see Supplemental Discussion).

Supplemental Methods

We utilized data from the Panel Study of Income Dynamics (PSID) to investigate migration patterns. The PSID is a nationally representative, longitudinal study of U.S. residents that studies a variety of socio-demographic, economic, and other topics. It is administered every two years, includes information about state of residence from 1968 to the present (1). This sample gives us the best currently available information with which to compare true exposure with imputed exposure from state of residence later in life. A limitation of using the PSID for this assessment is that—as a longitudinal cohort study—it is vulnerable to sample attrition. However, the PSID has high follow-up rate (96-98% per wave), and attrition in is not completely cumulative because participants may be re-contacted after missing a wave. PSID data was obtained from the Institute for Social Research.(1) Analyses were conducted using survey procedures in SAS version 9.2, incorporating design variables and 2005 longitudinal weights.

Our analyses of the CPS-TUS used responses gathered between 1999-2007, with the majority of responses coming from the 2003 and 2006-07 releases (see main text, Table 1). Hence, we chose the 2005 administration of the PSID, situated between the two modal CPS-TUS waves, as our source of migration data. As was done in our primary analyses, we restricted migration analyses to respondents born between 1973 and 1986. While all policy exposure prior to age 18 is relevant, we focus on state-of-residence for each respondent during the year in which they turned 17. This is appropriate as age 17 corresponds to the age of peak incidence for smoking initiation (2). These criteria resulted in a total of 3,329 subjects for analysis. Exposure to each of the youth access to tobacco (YATT) policies were determined for each subject using

(a) actual state of residence at age 17 (true exposure), and (b) state of residence during the 2005 survey wave (proxy exposure). These analyses used the same dichotomous policy scores that were used in our primary analyses. For each YATT policy type, we calculated the proportion of subjects whose true exposure matched their proxy exposure, which allows us to estimate the degree to which the adolescent policy exposure is correctly predicted using the subject's state of residence in adulthood.

The overall attrition rate for PSID subsample born between 1973 and 1986 and who participated in the survey at age 17 was 21.6%. Given that attrition may be related to migration, we examined whether migration within the PSID sample is representative of migration within the general population by comparing migration in the PSID sample with migration data from the U.S. Census. Specifically, we calculated the proportion of the PSID sample that changed state of residence between the years 1996 and 2001 and compared this estimate with the 5-year cross-state migration rate from the 2000 U.S. Census. Data from the 2000 U.S. Census data was analyzed using a 5% extract in the online data-analysis system (http://usa.ipums.org/usa/sda/) for the Integrated Public Use Microdata System maintained by the Minnesota Center for Population Research (3).

Supplemental Results

Based on PSID data, we estimated the that 14.0% (95% CI: 12.0%, 14.0%) of the population migrated across state borders between the ages of 17 and the age at which they responded to the 2005 wave of the PSID. Although 86.0% of respondents were non-movers, the proxy exposure score matched the true exposure score for 92.5% to 96.0% of respondents for all policies (Table S4), indicating that, for a given YATT policy type, 6.5% to 10.0% of the population moved to a state that had a similar YATT policy in place when they were age 17 as their state of origin. In other words, for a majority of cross-state movers, migration did not result in an incorrectly imputed exposure score. Table S4 also enumerates the percentage of the population moving from less restrictive to more restrictive states after age 17, and the percentage moving from more to less restrictive states—designated as false positive and false negative classifications, respectively. Overall, the use of state of residence in adulthood as a proxy for state of residence at age 17 appears to exhibit little bias toward positive or negative misclassification.

It is possible that the PSID sample is biased with respect to rate of cross-state migration because of the associations between attrition and migration. Subjects who migrate may be more difficult to locate for follow-up. On the other hand, factors such as low socio-economic status may be associciated with both attrition and a lower likelihood of follow-up. To examine whether cross-state migration may be over- or under-estimated from PSID data, we compared 5-year cross-state migration rates in the PSID to those estimated from the 2000 U.S. census. Specifically, we compared the number of subjects moving between states in the 1996 and 2001 PSID survey waves with the number of persons retrospectively reporting living in a different state than they did 5-years prior in the 2000 U.S. Census. Additionally, we focused on the 1973-1986 birth year cohort, as these were the subjects of interest for the primary analyses. The PSID move-rate for this cohort and time period was 18.1% (95% CI: 16.1-20.1%). This compares with a census estimate of 12.7% (SE=12.6-12.8%). Hence, after attrition and post-hoc sample weighting, the PSID appears to over-estimate migration relative to the U.S. census. The implications of cross-state migration, and possible over or under-estimation in the PSID sample are explicated further in the Supplemental Discussion.

Supplemental Discussion

Our primary analyses rely on the approximation that participants' state of residence when responding to the CPS-TUS survey is the same as their state of residence at age 17, which we presume to be the most relevant year for YATT policy exposure. While this approximation constitutes an obvious limitation of our estimates, PSID estimates suggest that that a large majority of people (86.0%) remain in the same state between the ages of 17 and age at survey response (18 to 34).

The PSID analysis of population migration rates limited by the fact that, as with any panel survey, attrition may contribute to various types of bias with respect to migration estimates. Even though PSID attrition is not cumulative (i.e., efforts are made to participants who were lost to follow-up and subjects may participate in non-contiguous waves [4]), we estimate that 21.6% of those who participated in the survey at age 17 did not participate in the 2005 wave. Sample attrition may contribute to an under-representation of movers in the PSID, and hence and under-estimate of migration rates. Furthermore, factors such as low socioeconomic status are associated with PSID attrition, and socially disadvantaged movers, while likely to undergo short-term migration, tend to return to their earlier place of residence (5, 6). In other words, socioeconomic factors associated with attrition are also associated with local and/or short-term migration, and this could lead to an over-estimation of cross-state migration.

The U.S. Census may constitute the gold standard for migration analysis, however, the information available from the Census is limited to analysis of whether subjects changed states during the five years prior to the Census. Comparison of this move rate with the corresponding rates in the PSID suggest that the PSID may actually over-estimate cross-state migration (12.7% Census estimate vs. 18.1% PSID estimate). Hence we are limited to the conclusion that "most" people remain in the same state between ages 17 and other points in young adulthood. However, our estimates of accuracy of policy-exposure imputation from PSID data indicate that the accuracy rate (92.5% to 96.0%) is much higher than the proportion of non-movers (86.0%). This is because there is a chance that movers may move to a state with a similar policy as that which they emigrated from. Hence, individuals who are correctly classified with regard to policy can be divided into three sub-populations: (a.) the portion of the population who lived in the same state as adolescents as they did at time of survey (b.) those who migrate from a state with a given policy to another state that had the same policy in place when the individual was age 17, and (c.) those who migrate from states without a given policy in place to another state without that policy in place during the individual's adolescence. If random migration is assumed, and the probability of moving across state borders is designated P_{move} , and the portion of the population living under a policy is designated as P_1, it follows that the probability of not moving is $(1- P_{move})$ and the portion of the population not effected by a policy is $P_0 =(1-P_1)$. The probability of moving from a state with a given policy in place to a state with a similar policy is $P_1^2P_{move}$, and the probability of moving from a state without a given policy to another state without that policy is $P_0^2(1 - P_{move})$. The sum of these three terms is:

$$P_{match}= (1- P_{move}) + P_{move}(P_1^2 + P_0^2). \qquad (Equation\ 1)$$

Given the constraint that P_1 and P_0 must sum to one, the sum of squares term is always 0.5 or higher. Hence, the proportion of subject correctly classified, under the assumption of random migration is the proportion of non-movers, plus at least half of the movers.

The above equation is approximate because move probabilities may correlate with state of origin simply because of large differences in state populations and migration rates. Although this would create deviation from estimates of imputation accuracy, the overall picture would remain the same: As long as the majority of individuals do not cross state borders, a larger majority will have their policy exposure correctly imputed. Perhaps a more important question is the degree to which misclassification influences the observed association in relation to the true association. Providing misclassification is uncorrelated with the outcome variable, it is expected to bias the association measure toward the null hypothesis; i.e., true effects are expected to be larger than estimates (7). The relation between relative risk and misclassification rate in the case of policy exposure has been worked out elsewhere (8). Making the simplifying approximation that misclassification rates are symmetrical (i.e., false positives are as common as false negatives), it can be shown that:

$$RR^{obs} = \frac{P_1^{obs}}{P_0^{obs}} = \frac{P_{match}RR^{true} + (1 - P_{match})}{P_{match} + (1 - P_{match})RR^{true}} \qquad (Equation\ 2)$$

Where RR^{obs} is the measured risk ratio in relation to the risk ratio expected in the absence of misclassification, RR^{true}. P_1^{obs} and P_0^{obs} are the observed outcome prevalences in states with and without a given policy, respectively, and the accuracy of classification is P_{match}. The relationship between observed and misclassification-adjusted odds ratios is similar, and can be calculated by adjusting risk ratios for baseline prevalence of the outcome. Based on the prior work noted above, we calculated the expected odds ratios (as opposed to risk ratios), adjusted for misclassification, as a function of observed odds ratios at two different outcome baseline prevalence--20% and 50%--and at three different misclassification rates: 5%, 10% and 15%. Results of these calculations are shown in Figure S1. The Figure demonstrates that migration will generally result in a slightly reduced observed association (ORs closer to 1). For example, an observed OR of 0.90 (typical of some of the significant single-policy ORs in Table 2) corresponds to an expected misclassification-adjusted OR of 0.89 at a 5% misclassification rate, and 0.87 at a 10% misclassification rate, and 0.86 at a 15% misclassification rate; the effect of baseline prevalence on the difference between observed and true ORs is practically negligible.

A more serious concern is whether or not misclassification as a result of migration is correlated with risk for smoking, and under what conditions this might produce a bias away from the null hypothesis. This question has also been addressed in prior work (8). In general, differences in risk for smoking between movers and non-movers are not sufficient to produce an inflated estimate as the differential risk terms from movers in one direction (with respect to policy) will tend to cancel those from movers in the opposite direction. We only expect bias away from the null hypothesis if individuals migrating from permissive states to restrictive states have—on average—risk for smoking that is very different from those who move from restrictive states to permissive states.

In summary, we estimate that subjects are classified with 94-95% accuracy when state of residence at time of survey is used as a proxy for state of residence at age 17. While there are some issues with using the PSID sample as the basis for misclassification estimates, comparison of migration rates between the PSID and the Census suggest that the PSID may actually over-estimate cross-state mobility. We expect this migration to contribute to random error in effect size estimates, which results in slight under-estimates of the true policy associations. Differences in smoking prevalence between movers and non-movers would bias the estimates further toward the null hypothesis. Only in cases of high migration rates and a strong dependence of smoking risk on direction of migration (with respect to state policy) would we expect a bias in the opposite direction (i.e., a false positive association between policy and smoking).

Table S1: Adjusted associations between counts of selected four and all nine youth access policies and smoking outcomes for the full sample, examining interaction of policy count with age[1]

	Ever-Smoking (100+ Lifetime Cigarettes N =105,519)		Current Smoking (N=105,519)		Current heavy smoking Among Ever-Smokers (N=35,745)	
Model	β	SE	β	SE	β	SE
FULL SAMPLE						
<u>Selected four policies</u>						
Model 1[2]						
Selected four policies	**0.974**	**0.956 - 0.993**	**0.968**	**0.950 - 0.987**	**0.968**	**0.937 - 1.000**
Age	0.991	0.953 - 1.031	0.982	0.941 - 1.025	1.018	0.954 - 1.086
Model 2[3]						
Selected four policies	**0.970**	**0.946 - 0.995**	**0.970**	**0.946 - 0.995**	0.969	0.936 - 1.004
Age	1.264	1.051 - 1.519	1.208	0.991 - 1.472	0.823	0.552 - 1.227
Model 3[4]						
Selected four policies	**0.971**	**0.947 - 0.996**	**0.972**	**0.948 - 0.997**	0.970	0.937 - 1.005
Age	1.259	1.043 - 1.519	1.196	0.977 - 1.464	0.820	0.549 - 1.222
Selected four policies*age	1.002	0.996 - 1.008	1.005	0.999 - 1.011	1.002	0.996 - 1.008
P for Interaction		*0.46*		*0.10*		*0.50*
<u>All nine policies</u>						
Model 1[2]						
All nine policies	0.990	0.979 - 1.002	**0.987**	**0.976 – 1.000**	0.984	0.964 - 1.005
Age	0.991	0.953 - 1.031	0.982	0.941 - 1.025	1.017	0.953 - 1.085
Model 2[3]						
All nine policies	0.987	0.974 - 1.001	0.987	**0.973 – 0.999**	0.987	0.966 - 1.009
Age	1.264	1.049 - 1.522	1.208	0.989 - 1.475	0.821	0.551 - 1.225
Model 3[4]						
All nine policies	0.987	0.974 - 1.001	0.987	**0.973 – 0.999**	0.987	0.966 - 1.009
Age	1.265	1.050 - 1.524	1.204	0.982 - 1.477	0.821	0.551 - 1.225
All nine policies*age	1.000	0.998 - 1.002	1.001	0.997 - 1.005	1.000	0.996 - 1.004
P for Interaction		*0.81*		*0.64*		*0.94*

Notes: [1] Age centered at age 25 years.
[2]Adjusted for demographic variables, survey wave, state excise tax, clean indoor air policy, and tobacco control funding per capita at time of survey.
[3]Model further includes interactions of age with year of birth and state of residence.
[4]Model further includes interaction of age with count of all nine policies.

Table S2: Adjusted associations between counts of selected four and all nine youth access policies and smoking outcomes for the full sample, examining interaction of policy count with age[1]

	Ever-Smoking (100+ Lifetime Cigarettes N =105,519)		Current Smoking (N=105,519)		Current heavy smoking Among Ever-Smokers (N=35,745)	
Model	β	SE	β	SE	β	SE
FULL SAMPLE						
Selected four policies						
Model 1[2]						
Selected four policies	**0.964**	**0.942 - 0.986**	**0.959**	**0.937 - 0.981**	**0.935**	**0.894 - 0.978**
Age	1.021	0.969 - 1.077	0.993	0.942 - 1.047	0.982	0.896 - 1.077
Model 2[3]						
Selected four policies	**0.947**	**0.916 - 0.980**	**0.949**	**0.920 - 0.980**	**0.938**	**0.890 - 0.989**
Age	1.283	0.923 - 1.783	1.170	0.803 - 1.705	0.619	0.351 - 1.094
Model 3[4]						
Selected four policies	**0.947**	**0.916 - 0.980**	**0.950**	**0.921 - 0.981**	**0.941**	**0.892 - 0.992**
Age	1.281	0.920 - 1.785	1.163	0.795 - 1.701	0.611	0.347 - 1.077
Selected four policies*age	1.001	0.995 - 1.007	1.003	0.995 - 1.011	1.008	0.999 - 1.015
P for Interaction		*0.85*		*0.47*		*0.09*
All nine policies						
Model 1[2]						
All nine policies	**0.984**	**0.968 – 0.999**	**0.980**	**0.967 - 0.996**	0.972	0.942 - 1.002
Age	1.021	0.970 - 1.075	0.993	0.942 - 1.047	0.982	0.896 - 1.077
Model 2[3]						
All nine policies	**0.974**	**0.955 - 0.994**	**0.974**	**0.955 - 0.994**	0.973	0.940 - 1.008
Age	1.281	0.922 - 1.781	1.170	0.801 - 1.708	0.615	0.348 - 1.088
Model 3[4]						
All nine policies	**0.974**	**0.954 - 0.996**	**0.974**	**0.955 - 0.994**	0.974	0.941 - 1.009
Age	1.288	0.925 - 1.794	1.172	0.798 - 1.721	0.608	0.345 - 1.074
All nine policies*age	0.999	0.995 - 1.003	1.000	0.994 - 1.005	1.003	0.997 - 1.009
P for Interaction		*0.61*		*0.86*		*0.41*

Notes: [1] Age centered at age 25 years.
[2]Adjusted for demographic variables, survey wave, state excise tax, clean indoor air policy, and tobacco control funding per capita at time of survey.
[3]Model further includes interactions of age with year of birth and state of residence.
[4]Model further includes interaction of age with count of all nine policies.

Table S3: Adjusted associations between counts of selected four and all nine youth access policies and smoking outcomes for the full sample, examining interaction of policy count with age[1]

	Ever-Smoking (100+ Lifetime Cigarettes N =105,519)		Current Smoking (N=105,519)		Current heavy smoking Among Ever-Smokers (N=35,745)	
Model	β	SE	β	SE	β	SE
FULL SAMPLE						
Selected four policies						
Model 1[2]						
Selected four policies	0.986	0.961 - 1.010	0.977	0.949 - 1.006	1.004	0.967 - 1.043
Age	0.955	0.902 - 1.011	0.967	0.902 - 1.035	1.054	0.960 - 1.158
Model 2[3]						
Selected four policies	0.997	0.966 - 1.029	0.995	0.962 - 1.029	1.006	0.965 - 1.048
Age	1.230	0.880 - 1.720	1.244	0.914 - 1.692	1.123	0.620 - 2.034
Model 3[4]						
Selected four policies	0.998	0.967 - 1.030	0.998	0.965 - 1.032	1.005	0.964 - 1.047
Age	1.220	0.871 - 1.709	1.224	0.901 - 1.662	1.132	0.624 - 2.054
Selected four policies*age	1.004	0.996 - 1.012	1.007	0.997 - 1.017	0.996	0.984 - 1.008
P for Interaction		*0.35*		*0.11*		*0.49*
All nine policies						
Model 1[2]						
All nine policies	0.998	0.985 - 1.010	0.994	0.978 - 1.010	0.998	0.973 - 1.022
Age	0.954	0.901 - 1.010	0.967	0.902 - 1.035	1.054	0.962 - 1.156
Model 2[3]						
All nine policies	1.002	0.988 - 1.016	1.001	0.987 - 1.015	1.003	0.982 - 1.025
Age	1.229	0.879 - 1.718	1.242	0.915 - 1.687	1.123	0.619 - 2.038
Model 3[4]						
All nine policies	1.002	0.988 - 1.016	1.002	0.988 - 1.016	1.002	0.981 - 1.024
Age	1.226	0.875 - 1.718	1.231	0.903 - 1.678	1.133	0.622 - 2.064
All nine policies*age	1.001	0.997 - 1.005	1.002	0.996 - 1.008	0.998	0.990 - 1.006
P for Interaction		*0.77*		*0.39*		*0.56*

Notes: [1] Age centered at age 25 years.
[2]Adjusted for demographic variables, survey wave, state excise tax, clean indoor air policy, and tobacco control funding per capita at time of survey.
[3]Model further includes interactions of age with year of birth and state of residence.
[4]Model further includes interaction of age with count of all nine policies.

Table S4: Weighted Percentages of PSID respondents who would have been correctly classified on policy exposure given the approximation that state of residence at time of survey is equivalent to state of residence at age 17. False positive and False negative classifications are also tabulated.

	% Correctly Classified (95% CI)	% False Positive (95% CI)	% False Negative (95% CI)
Signage requirements	94.8 (93.6, 96.0)	2.2 (1.5, 3.0)	3.0 (2.0, 4.0)
Vending machine restrictions	94.8 (93.7, 96.0)	2.6 (1.6, 3.7)	2.6 (1.6, 3.4)
Inspection requirements	95.0 (94.1, 95.9)	2.7 (2.0, 3.4)	2.3 (1.7, 3.0)
Graduated penalties	93.9 (92.5, 95.2)	3.0 (2.1, 4.0)	3.1 (2.1, 4.1)
Identification requirements	94.4 (93.2, 95.7)	3.1 (2.1, 4.0)	2.5 (1.6, 3.3)
Repackaging restrictions	94.2 (92.6, 95.8)	2.6 (1.6, 3.7)	3.2 (2.1, 4.3)
Statewide enforcement authority	94.6 (93.6, 95.6)	3.0 (2.1, 3.8)	2.4 (1.9, 3.0)
Free distribution restrictions	94.7 (93.2, 96.2)	2.5 (1.6, 3.3)	2.8 (1.7, 3.9)
Clerk intervention requirements	94.5 (93.2, 95.8)	3.0 (2.0, 3.9)	2.5 (2.0, 3.1)

Supplemental Figure Legend:

Expected "true" or misclassification-adjusted odds ratios as a function of observed odds ratios under various misclassification rates and outcome baseline prevalences calculated using equations derived to estimate the effects of exposure misclassification due to migration on observed association between policy and outcome. Misclassification rates were 15% (- - - - - -), 10% (— — —), and 5% (———). Black lines represent 20% baseline prevalence and gray lines represent 50% baseline prevalence.

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