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*Pediatrics* 2002;109;1088-1092
DOI: 10.1542/peds.109.6.1088

This information is current as of March 10, 2006

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Youth Access Interventions Do Not Affect Youth Smoking

Caroline M. Fichtenberg, MS, and Stanton A. Glantz, PhD

ABSTRACT. Objective. To determine the effectiveness of laws restricting youth access to cigarettes on prevalence of smoking among teens.

Methods. We conducted a systematic review of studies that reported changes in smoking associated with the presence of restrictions on the ability of teens to purchase cigarettes. We calculated the correlation between merchant compliance levels with youth access laws and prevalence (30-day and regular) prevalence of youth smoking, and between changes in compliance and prevalence associated with youth access interventions. We also conducted a random effects meta-analysis to determine the change in youth prevalence associated with youth access interventions from studies that included control communities.

Results. Based on data from 9 studies, there was no detectable relationship between the level of merchant compliance and 30-day (r = .116; n = 38 communities) or regular (r = .017) smoking prevalence. There was no evidence of a threshold effect. There was no evidence that an increase in compliance with youth access restrictions was associated with a decrease in 30-day (r = .294; n = 18 communities) or regular (r = .274) smoking prevalence. There was no significant difference in youth smoking in communities with youth access interventions compared with control communities; the pooled estimate of the effect of intervention on 30-day prevalence was −1.5% (95% confidence interval: −6.0% to +2.9%).

Conclusions. Given the limited resources available for tobacco control, as well as the expense of conducting youth access programs, tobacco control advocates should abandon this strategy and devote the limited resources that are available for tobacco control toward other interventions with proven effectiveness. Pediatrics 2002;109:1088–1092; smoking, tobacco, prevention.

ABBREVIATION. CI, confidence interval.

Virtually all smokers smoke their first cigarette as teenagers, so it would seem logical that making it more difficult for teenagers to obtain cigarettes would reduce the likelihood that a teen would become a smoker. The broad political appeal of this logic has led to the widespread enactment of so-called “youth access” laws, which make it illegal to sell cigarettes to teenagers. (In some cases, these laws also criminalize possession of tobacco by teenagers.) By August 2001, all 50 states and 1139 local governments had passed youth access laws (American Nonsmokers’ Rights Foundation database, personal communication, August 24, 2001). Youth access has also become a cornerstone of federal tobacco control policy, as reflected in the Synar Amendment to the 1992 Alcohol, Drug Abuse and Mental Health Administration Reorganization Act (Pub L No. 103-312) which required states to enact and enforce a minimum age-of-sale law of 18 years, as well as in the regulation of tobacco asserted by the Food and Drug Administration (and struck down by the Supreme Court). Youth access programs are also recommended by the US Centers for Disease Control and Prevention and the Institute of Medicine as an established component of comprehensive tobacco control programs.

Although several reviews have concluded that there is good evidence that these laws do make it difficult for teens to buy cigarettes, the evidence that these laws lead to decreases in teen smoking has remained inconclusive. We present a systematic review and meta-analysis to explore and quantify the effect of youth access programs on teenage smoking prevalence. We find that the evidence is convincing that youth access programs do not decrease youth smoking.

METHODS

Studies

We identified 14 studies of the effects of youth access laws on teen smoking from systematic searches of Medline for the years 1985 to 2001 using the search terms “youth access,” “sales,” “laws,” “adolescent,” and “smoking,” and from references in review articles and articles that were located. Because we were interested in estimating the association between youth access programs and youth smoking prevalence (as opposed to process variables such as whether or not youth perceive that they can buy cigarettes), we limited our analysis to 8 articles that either reported both compliance and prevalence data and/or reported prevalence in intervention and control communities (Table 1). We excluded 2 studies that reported neither compliance nor control data as well as 3 cross-sectional studies that were based on aggregate rather than individual data. We also excluded 1 study that reported effects on initiation, but not prevalence.

All 8 studies, which met our inclusion criteria, measured compliance with youth access laws by carrying out “sting operations,” which consisted of sending teens into stores to attempt to purchase cigarettes and recording whether the merchants were willing to sell. Percent compliance with the laws was calculated as the percent of stores visited which refused to sell to the underage youth. Teens of different ages, gender and ethnicity were used in these operations (Table 1). In small communities, researchers were able to test all the stores in the community, while in others they only visited a sample. The number of times each store was visited per survey period varied as well. In some cases the compliance tests were used as part of an enforcement protocol.

Smoking prevalence was assessed in school-based surveys in
<table>
<thead>
<tr>
<th>Reference</th>
<th>Intervention</th>
<th>Control Study Type</th>
<th>N (Communities)</th>
<th>Compliance Tests</th>
<th>Merchant Compliance</th>
<th>Smoking Survey</th>
<th>30-Day Smoking Prevalence(%)</th>
<th>Regular Smoking Prevalence(%)</th>
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<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Time to FU (Months)</td>
<td>Intervention (%)</td>
<td>Control (%)</td>
<td>I-C‡ (FU-BL)</td>
<td>BL FU FU-BL</td>
</tr>
<tr>
<td>199919§</td>
<td>Altman, Community intervention with community and retailer education (no enforcement)</td>
<td>Y Co 2</td>
<td>13- to 17-year-old boys and girls</td>
<td>34</td>
<td>100</td>
<td>25 100 75 36 61 25 50</td>
<td>Grades 7-9</td>
<td>29-31</td>
</tr>
<tr>
<td>199813</td>
<td>Forster, Comprehensive community intervention, enactment of laws, variety of enforcement strategies</td>
<td>Y CS 7- to 15-year-old girls</td>
<td>36</td>
<td>97</td>
<td>63 97 34 59 91 32 1</td>
<td>Grades 8,9,10</td>
<td>36 700/735†</td>
<td>700/735†</td>
</tr>
<tr>
<td>1997</td>
<td>Rigotti, Retailer education and enforcement (warnings and fines)</td>
<td>Y CS 16-year-old girls</td>
<td>24</td>
<td>82</td>
<td>35 82 47 28 45 17 30</td>
<td>Grades 9-12</td>
<td>24 3566/7350</td>
<td>3566/7350</td>
</tr>
<tr>
<td>199219§</td>
<td>DiFranza, Enforcement (fines)</td>
<td>N CS 10- to 16-year-old girls</td>
<td>11-19</td>
<td>84</td>
<td>67</td>
<td>12- to 17-year-olds</td>
<td>22 500/663</td>
<td>6 26</td>
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<tr>
<td>200110</td>
<td>Cummings, Enforcement</td>
<td>N CS 15- to 17-year-old</td>
<td>24</td>
<td>77</td>
<td>21 77 56</td>
<td>Grade 9</td>
<td>4055/7471#</td>
<td>76 10</td>
</tr>
<tr>
<td>199816§</td>
<td>Staff, Retailer education and community awareness (no enforcement)</td>
<td>Y CS NR</td>
<td>12- to 17-year-olds</td>
<td>7</td>
<td>3818/3438</td>
<td>2338/2660</td>
<td>20 21</td>
<td>1 22 22 1 1</td>
</tr>
<tr>
<td>199814</td>
<td>Baggott, Enforcement (fines) active enforcement (including fines, licensing, possession fines)</td>
<td>Y CS ≤13 years old</td>
<td>1</td>
<td>100</td>
<td>100</td>
<td>14- to 15-year-olds</td>
<td>1 117/68</td>
<td>107/95</td>
</tr>
<tr>
<td>199110</td>
<td>Jason, Enforcement (fines)</td>
<td>N CS 12- to 13-year-olds</td>
<td>5-19</td>
<td>100</td>
<td>30 98 68</td>
<td>14- to 15-year-olds</td>
<td>1 117/68</td>
<td>107/95</td>
</tr>
</tbody>
</table>

CS indicates cross-sectional study; Co, prospective cohort study; BL, baseline; FU, follow-up; I, intervention; C, control; NR, not reported in the article.
*30 day smoking includes weekday smoking and self-reported "smokers" as well as 30 day smoking.
†Regular smoking includes daily smoking, frequent smoking (>20/past 30 days) and self-reported "regular smokers."
‡I-C = (FU-BL干预) - (FU-BL控制)
§Averaged prevalence data from different grades and gender.
¶Excluded youth ≥18 years old from prevalence data.
#For all 12 communities combined.
**Maximum compliance reached in the during the study.
all studies. We pooled the various reported smoking measures into 2 groups: “30-day smoking” which includes smoking at least once during the past 30 days,9,11,13,15,16 as well as smoking at least once a week14 and self-reported “smokers”12; “regular smoking” includes daily smokers,9,13 frequent smokers (at least 20/30 past days)15 and self-reported “regular smokers.”10 The ages of the teens surveyed varied from study to study, with a range of 12 to 17 years old. When a study reported separate measurement for different age groups, we used the arithmetic average of the separate effects.11,12,16 We excluded from the analysis data for teens 18 years and older because they would not be affected by the youth access laws.9,12

Interventions ranged in intensity from simple enactment of laws13 to retailer and community education11,16 to education combined with active enforcement via compliance testing, warnings, fines, and suspension of tobacco selling licenses.9,12-14

Analysis

To test whether the level of merchant compliance with youth access laws is related to teen smoking, we computed the Pearson product moment correlation between teen smoking prevalence (both 30-day and regular use) and percent merchant compliance at both baseline and follow-up, in intervention and control communities. Whenever possible, we used results for individual communities rather than pooled results.15 For 30-day smoking, the 8 studies included prevalence and compliance data at baseline and follow-up for 15 intervention and 3 control communities, and either baseline or follow-up for 2 intervention communities, for a total of 38 data points (Table 1, Fig 1A). For regular smoking, the studies included baseline and follow-up data for 15 intervention and 2 control communities, for a total of 34 data points (Table 1).

We used the reported merchant compliance and the 2 measures of teen prevalence to compute absolute changes from baseline to follow-up for those variables, in both intervention and control communities. To assess the impact of active enforcement efforts, we correlated the changes in compliance with the changes in 30-day as well as regular use prevalence. For 30-day prevalence, there were 15 intervention and 3 control communities that reported baseline and follow-up measures of compliance and prevalence (Table 1; Fig 1B). For regular smoking, there were 14 intervention and 2 control communities (Table 1).

We computed the difference between the intervention and control groups for the baseline to follow-up changes in 30-day prevalence in the9,11,13,14,16 controlled studies that reported longitudinal smoking data. We used these differences to calculate a pooled estimate of the effects of youth access programs on changes in 30-day smoking prevalence, according to a random effects meta-analysis model12 and treating the community as the unit of analysis. We did not pool the data on regular use because there were only 2 controlled studies that reported the necessary data (1 of which reported a 2.9% increase [P = .08] in the prevalence change between intervention and control communities9 and the other of which reported a 4.9% decrease [95% confidence interval: [CI]: −9.0; −0.7]).13 Four of 5 studies that we pooled reported compliance data, and in all 4 cases compliance rates exceeded 82% in the intervention communities.

Only 1 of the studies13 reported an estimate of the standard error necessary for meta-analysis. To be consistent, we used the following procedure to estimate the standard errors for all of the studies: 1) we computed the variance of the point estimates at baseline and follow-up in intervention and control using the standard formula for the variance of a proportion, \( \hat{p}(1-\hat{p})/n \); 2) we then estimated the variance of the difference between baseline to follow-up changes in intervention and control by computing a weighted average of the variances of the 4 point estimates, weighing by sample size; and finally 3) we estimated the standard error of the differences with

\[
SE = \sqrt{\text{var} \times \left( \frac{1}{n_1} + \frac{1}{n_2} + \frac{1}{n_3} + \frac{1}{n_4} \right)}
\]

where \( n \) is the sample size.14 This method generated a value for the standard error one third as large as that reported in the study that estimated this parameter. Therefore, it is likely we are underestimating the standard errors and biasing our analysis toward finding a statistically significant effect.

RESULTS

Relationship Between Compliance and Smoking

Thirty-day youth smoking rates as a function of percent compliance with youth access laws at various times in 20 communities are plotted in Fig 1A. There is no statistically significant relationship between merchant compliance and 30-day (\( r = .116; P = .486 \); Fig 1A) or regular (\( r = .017; P = .926 \); Table 1) teen smoking prevalence. These analyses had 80% power to detect correlations of \( \pm 0.44 \) and \( \pm 0.46 \), respectively. There is no visible evidence of a threshold effect after compliance reached a certain level (eg, 90%).9,10,12 Baseline to follow-up changes in 30-day smoking and merchant compliance in 18 communities are plotted in Fig 1B. There is no evidence that an increase in compliance is associated with a decrease in 30-day (\( r = .294; P = .237; \text{Fig } 1B \)) or regular (\( r = .274; P = .287 \)) prevalence. These analyses had 80% power to detect correlations of \( \pm 0.62 \) and \( \pm 0.64 \), respectively. Although none of these correlations are statistically significant, it is interesting to note that their sign indicates a positive association between increased compliance and increased smoking prevalence, which is opposite of the desired effect of these laws.

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Effect of Interventions

There were no statistically significant differences in youth smoking in communities with youth access interventions compared with control communities with no such efforts (Table 1); the pooled estimate of the difference in baseline to follow-up changes in 30-day prevalence between interventions and control communities was \( -1.5\% \) (95% CI: \(-6.0\%\) to \(+2.9\%\)).

DISCUSSION

Youth access interventions are not associated with consistent positive effects on youth smoking prevalence. Furthermore, there is no evidence that increased compliance is associated with decreased prevalence.

One reason why these policies may not affect youth smoking although they do affect the ease with which children can purchase cigarettes is that approximately half of teen smokers use commercial sources as their usual source of cigarettes; the rest obtain cigarettes from parents, friends, and strangers, or steal them.\(^{25-34}\) As teens find it harder to buy cigarettes they may simply shift to these other sources. Forster et al.\(^{13}\) found that the percentage of teen smokers reporting commercial sources for their most recent cigarette decreased from 22.9% to 16.9% in communities where youth access laws were more aggressively enforced, while it increased in the control communities. Likewise, Cummings et al.\(^{15}\) found that the percentage of 30-day smokers who reported relying on commercial sources for their usual source of cigarettes, decreased by 52% (in relative terms) in intervention communities. Hinds et al.\(^{18}\) reported an increase of 75% (in relative terms) in the proportion of smokers using friends as sources for cigarettes after implementation of a youth access program. In an analysis of the national data of the Youth Risk Behavior Surveys, Jones et al.\(^{35}\) found that, from 1995 to 1999, the percentage of teen smokers relying on store purchases decreased from 38.7% to 23.5% while having someone else buy cigarettes for them increased from 15.8% to 29.9%. Although some tobacco control advocates have argued for attempting to restrict access to these “social” sources,\(^{35}\) doing so with a high level of effectiveness is a practical impossibility and could reinforce the tobacco industry’s efforts to present tobacco control advocates as unreasonable and extremist. It would also shift the focus of tobacco control efforts further away from the tobacco industry and its marketing practices.

The proponents of youth access have argued that the reason some studies have not found that youth access laws were associated with declines in youth smoking is that the level of merchant compliance did not reach a threshold necessary to effectively curb purchases and consequently smoking.\(^{9,13,36}\) We found no evidence of such a threshold (Fig 1A). Moreover, all 4 controlled studies in our meta-analysis (Table 1) which reported compliance achieved better than 82% compliance, yet failed to produce any consistent decrease in smoking.

Evidence from population-based studies is also mixed. Using data from a 1994 survey, Chaloupka and Pacula\(^{37}\) estimated that aggressive statewide enactment and enforcement of youth access laws as required by the Synar Amendment was associated with an 18% decrease in youth smoking prevalence. Based on 1996 data, Luke et al.\(^{20}\) found an association between strength of state level youth access policies and declines in 30-day prevalence (\( r = -0.36; \) \( P = .04\) ). However, this study did not control for the presence of other policies that are known to affect youth smoking, such as clean air laws and media campaigns. Chaloupka and Pacula\(^{38}\) report that youth access policies significantly decrease smoking rates among black youths but have no significant effects on white teens. Gruber,\(^{39}\) in an analysis of data from several surveys, found a significant negative effect of youth access in only 1 of the 8 subgroups he analyzed. Two other econometric analyses of the effects of youth access as well as other tobacco control policies related to youth smoking found no evidence of effects of youth access on smoking.\(^{21,40}\)

One limitation of our analysis is the relatively small number of controlled studies that have been conducted evaluating the effects of youth access interventions on teen smoking prevalence (Table 1). Although this is of concern, the consistency with the results showing no relationship between compliance and prevalence (Fig 1) increases the confidence one can have in this conclusion. It is also important to note that major resources have already been devoted to the youth access policy in the absence of positive evidence of effectiveness.

Some have argued that youth access programs should be part of a comprehensive tobacco control program, even absent of evidence of effectiveness in reducing teen smoking, because they are political popular and useful for coalition-building. This argument ignores the fact that youth access programs consume limited resources for tobacco control and have created an opportunity for the tobacco industry to build coalitions with local merchants; expanding the industry’s political base. In addition, youth access programs reinforce the tobacco industry’s central marketing message that kids should smoke because it will make them appear more “adult.”\(^{41}\)

In contrast to youth access, there are strategies that are known, on the basis of solid empirical evidence, to reduce teen smoking:\(^{6,8,42}\) taxes,\(^{8,19,21,38,43}\) smoke-free workplaces and homes,\(^{21,38,43-45}\) media campaigns,\(^{46-49}\) and education on the effects of second-hand smoke.\(^{50}\) Given the limited resources available for tobacco control, as well as the expense of conducting youth access programs,\(^{51}\) tobacco control advocates should start redirecting their energies and
funds away from youth access and toward other interventions that have proven effectiveness.

ACKNOWLEDGMENT
This work was supported by National Cancer Institute grant CA-61021.

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