The Impact on Tobacco Use of Branded Youth Anti-tobacco Activities and Family Communications about Tobacco

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Abstract In a randomized controlled trial, we evaluated the effect on tobacco use onset among middle school students of Family Communications (FC) activities designed to mobilize parental influences against tobacco use and Youth Anti-tobacco Activities (YAT) designed to market anti-tobacco norms to adolescents. We conducted a simple, two-condition experimental design in which 40 middle schools, with a prevalence of tobacco use at or above the Oregon median, received, by random assignment, either the intervention or no intervention. State, county, and local prevention coordinators around Oregon served as liaisons to schools. To generate interest, staff made presentations to these groups and distributed marketing packets at several conferences. Dependent variables were indices of smoking prevalence and use of smokeless tobacco (ST) in the prior month. Additionally, we created an intervention manual so that other communities could replicate this study. The findings suggest that efforts to influence parents to discourage their children's tobacco use and efforts to market an anti-tobacco perspective to teens are effective in preventing smoking. The impact of YAT is consistent with experimental and nonexperimental evaluations of media campaigns to influence young people not to smoke.

Keywords Tobacco · Outcomes assessment · Adolescents · Prevention · Family communications

This paper describes the results of a randomized controlled trial in which we evaluated family communications (FC) and youth anti-tobacco activities (YAT) for their effect on

the onset of tobacco use among students in middle schools. The study was motivated by a desire to strengthen two components of an earlier intervention, Project SixTeen (Biglan et al. 2000). That study found that a comprehensive tobacco prevention intervention had a small, but significant, effect in reducing adolescent smoking when compared with a school-based intervention alone. The comprehensive intervention had five components: (a) the PATH curriculum (Biglan et al. 1988), (b) media advocacy, (c) youth anti-tobacco activities (YAT), (d) family communications about tobacco use (FC), and (e) reduction of youth access to tobacco (Biglan et al. 1995a, 1996a, 2000). In the present study, we sought to improve on the intervention. In addition to our concern about the potential for weak results, as found previously, we wanted to develop an intervention that required less classroom time. Based on feedback from Project SixTeen (P-16), we wanted to reduce the burden of intervention implementation on teachers and make the program more attractive to school administrators.

Several studies have shown that parenting practices influence adolescent engagement in a range of problem behaviors, including tobacco use (Ary et al. 1999a, b; Biglan et al. 1995b; Metzler et al. 1994). Parenting practices that seem to influence youth problem behavior are (a) setting effective limits around those behaviors and situations in which they may occur, (b) monitoring children's associations with deviant peers, and (c) making and enforcing rules not to engage in problem behaviors (e.g., Biglan and Smolkowski 2002; Biglan et al. 1996b; Metzler et al. 1994). Preliminary evidence collected in P-16 indicated that the FC component reached parents and adolescents, and affected attitudes and norms about tobacco use (Biglan et al. 1996a, b). Since the FC component in P-16 consisted simply of an interview about tobacco use that the student did with a parent, we felt that a more intense

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intervention would increase the likelihood that parents would communicate their expectations about tobacco use. We hypothesized that a set of videos focused on communicating expectations, monitoring friendships, and setting limits and rewards for nonuse of tobacco, and assigned as homework to watch with a parent, would promote parent–child communication while requiring little class time.

We also thought that we could strengthen the anti-tobacco activities used in P-16 by decreasing positive social images of smokers and counteracting advertising by the tobacco industry, both mediators of tobacco use initiation (Chassin et al. 1981; Biglan 2004). In the years since P-16 ended, new research has shed light on the impact of marketing strategies, such as “branding” (i.e., associating a line of products with highly valued objectives), on youth smoking (Biglan 2004). In the past, most efforts to influence adolescents not to smoke have emphasized negative social consequences of smoking (Pechmann 2001; Worden 1999), harmful effects of smoking, or perfidy of the tobacco companies (Farrelly et al. 2003). However, careful review of the tobacco companies’ marketing strategies shows that they have succeeded in influencing adolescents to smoke by associating cigarette brands with outcomes highly valued by adolescents (e.g., popularity) and images teenagers perceive will lead to popularity (e.g., toughness, attractiveness; Biglan 2004; National Cancer Institute 2008). We therefore designed a branded campaign intended to make a positive association between not smoking and images or activities involving social acceptance. We associated the brand (f2b) with the phrase “freedom to breathe” and with non-smoking themes.

In addition to evaluating the impact of the present intervention on smoking initiation, we examined whether it prevented adolescents from becoming susceptible to smoking. Recent evidence indicates that it is appropriate to target adolescent susceptibility to begin smoking as well as actual smoking behavior. Pierce et al. (1996) showed that adolescents who do not definitely rule out future smoking are at significantly greater risk to initiate it. We recently replicated this finding; Forrester et al. (2007) found that adolescents in seventh and ninth grade not definitely ruling out future smoking were significantly more likely to be smoking 2 years later.

Methods

Design

With a simple two-condition experimental design, we randomized 40 middle schools with tobacco-use prevalence at or above the Oregon median either to receive or not receive the intervention. The intervention consisted of FC activities designed to mobilize parent influence against

tobacco and YAT activities designed to market anti-tobacco norms to teens. Because we did not have the resources and staff to assess and implement the intervention in all 40 schools at once, we recruited two successive cohorts of 20 schools—10 intervention and 10 control in each. Cohort 1 (C1) began in the 2001–02 school year and Cohort 2 (C2) the following year. During the first year of intervention implementation, our thinking about marketing of not using tobacco evolved and we decided to alter the YAT intervention in C2 to strengthen young people’s associations between not smoking and themes that are important to at-risk youth, such as popularity and excitement. The target outcome was smoking prevalence in eighth grade, but assessments began with sixth-grade students. See Fig. 1 for a description of the study design.

We selected schools high in smoking prevalence under the assumption that intervention effects would more likely show up in those schools. An earlier project had obtained tobacco-use data from Oregon eighth graders in many middle schools, providing an estimate of the schools’ tobacco-use prevalence once our sixth graders reached eighth grade. We gave highest priority to schools with tobacco-use prevalence above a median of 15% for Oregon eighth graders. However, recent work found school-level variation too little to justify such a procedure in future work (Smolkowski et al. 2006). While it would have been ideal to cross FC and YAT in a 2×2 design in schools, there was insufficient statistical power to do so and resources were unavailable to include additional schools.

We evaluated intervention impact in two ways. Through an anonymous survey of all students present at assessment, we assessed the change in tobacco-use prevalence from sixth to eighth grade. This provided an overall estimate of the longer-term impact of the intervention among all students in the school. We also recruited a random sample of families in each school to provide non-anonymous and more detailed data on intervention impact and family interactions relevant to the FC component. This paper reports the results from the in-school assessments.

For districts without sixth grade in middle school, we assessed elementary school sixth graders and considered them in the same school, as they were in middle school by the time we targeted eighth-graders. In C2, one control school accidentally (randomly) obtained intervention materials; we subsequently reclassified it as an intervention school.

School Recruitment

We created a marketing packet to introduce the study in each district and, to facilitate school contact, reached state, county, and local prevention coordinators. Our School Relations Team presented to them and distributed marketing packets at several prevention conferences. The team also traveled to approximately 70 districts to explain the study and determine district interest.

Fig. 1 CONSORT diagram

Participants

We conducted 12,308 student surveys (6,276 at T₁, 6,032 at T₂; 6,903 in C1, 5,405 in C2). We collected two surveys, so the 6,903 C1 surveys include 3,496 at T₁ and 3,407 at T₂, with most of the surveys completed by the same students. At baseline, 68% were Caucasian; 11% Hispanic; 6% Native American; 2% African American; 2% Asian; and 1% Pacific Islander. Sixth grade had 50% female; 97% age 11–12; 62% with mostly As or Bs. Eighth grade had 51% female, 30% age 13, 66% 14, and 66% with mostly As or Bs. Table 1 presents prevalence or means and standard deviations, along with sample sizes, for all measures by cohort and condition.

Measures

Outcome measures Key dependent variables were indices of smoking prevalence for both males and females, and use of smokeless tobacco (ST) for males only, in the prior month. We based prevalence on two measures: number of

days smoked in past month and number of cigarettes per day in past month. To derive a prevalence estimate, we coded as smokers those who endorsed smoking on either of these two items. Our index of ST-use prevalence was based on one item (number of days the student used ST).

Other variables of interest were alcohol and marijuana use. We hypothesized that, since alcohol and tobacco have similar advertising methods, and the two substances often are used together, our intervention might affect use of both. However, marijuana is illicit and our intervention would not affect its use. We assessed students' self-reported alcohol and marijuana use. In each case, our measure was students' self-reported use in the past 30 days.

Mediator and moderator variables As noted, there is evidence that adolescents who do not definitely rule out smoking initiation are susceptible to starting. We assessed susceptibility to smoke with three items: (a) "Do you think you will try a cigarette soon?" (b) "If one of your best friends were to offer you a cigarette, would you smoke it?"

Table 1 One-month prevalence or means and standard deviations for all measures by condition in all schools and in cohort 2 schools

	All schools		Cohort 2	
	Control	Intervention	Control	Intervention
Cigarette use, 8th grade				
Prevalence	12.3	12.4	13.5	11.1
<i>N</i>	2,676	3,168	1,115	1,351
Smokeless tobacco use, 8th grade males				
Prevalence	5.5	5.7	6.7	6.8
<i>N</i>	1,286	1,535	537	645
Alcohol use, 8th grade				
Prevalence	33.1	29.2	32.3	28.6
<i>N</i>	2,570	3,039	1,016	1,236
Marijuana use, 8th grade				
Prevalence	12.4	11.9	12.9	11.3
<i>N</i>	2,528	2,968	980	1,186
Susceptible, 6th grade nonsmokers				
Prevalence	21.7	24.8	22.8	23.3
<i>N</i>	2,150	2,215	1,258	1,401
Smoker image, 6th grade (range: 1 to 4)				
Mean	1.75	1.78	1.80	1.81
(SD)	(0.59)	(0.61)	(0.59)	(0.60)
<i>N</i>	2,876	3,219	1,252	1,392
Tobacco rules, 6th grade (range: 1 to 4)				
Mean	2.83	2.81	2.77	2.84
(SD)	(1.31)	(1.34)	(1.32)	(1.32)
<i>N</i>	2,716	2,917	1,250	1,378
Praise for not smoking, 6th grade (range: 1 to 5)				
Mean	3.65	3.62	3.62	3.67
(SD)	(1.49)	(1.54)	(1.52)	(1.52)
<i>N</i>	2,676	2,831	1,253	1,372
Views on cigarette ads, 6th grade (range: 1 to 4)				
Mean	2.22	2.16	2.26	2.21
(SD)	(1.09)	(1.04)	(1.11)	(1.02)
<i>N</i>	2,007	2,006	1,138	1,228

and (c) “At any time during the next year, do you think you will smoke a cigarette?” Each item offered four choices: definitely not, probably not, probably would, or definitely would. We classified them as not susceptible if they responded *definitely not* to all three items; otherwise, we classified them as susceptible. The descriptive information (Table 1) and analysis of susceptibility did not include smokers in sixth grade (T_1), nor did we include students who failed to reporting smoking status in sixth grade.

We asked students to report on their home rules about tobacco use: “Is there a rule in your house against using tobacco?” Students responded to one of four choices: (a) there is no rule; (b) there is a rule, but it is not enforced; (c) there is a rule and it is sometimes enforced; or (d) there is a rule and it is strictly enforced.

To assess smoker image, students were directed to “please tell us what comes to mind when you think of someone who smokes.” Students then rated the following

adjectives on a four-point scale: popular, unattractive, sexy, unhealthy, fun, adult, exciting, loser, unpopular, relaxed, uptight, and tough. The ratings were averaged, after reversing some items such as unattractive and unpopular, to provide a score where one represented a poor image of smokers and four represented the best image of smokers.

Students reported the praise they expected to receive for tobacco abstinence by answering this question: “How likely is it that your parents would encourage or praise you for not using tobacco?” Student responded on a five-point scale, from not at all likely to very likely.

To solicit views on cigarette advertisements, we asked students to rate whether they agreed or disagreed with the following statement on a four-point Likert scale: “Cigarette companies deliberately advertise and promote cigarettes to encourage youth under 18 years of age to smoke.” Lower scores represented agreement; higher scores represented disagreement.

Table 2 Youth anti-tobacco activities by cohort

	Cohort 1	Cohort 2
Types of activities	Poster and mural contests Health fairs After-school clubs Scavenger hunts School carnivals School assemblies “Hackademy” Awards Community festival booths	Roller skating Rock climbing Bowling School carnivals Snowboarding/skiing Disc golf tournaments Skateboard competitions School assemblies Amusement parks Community festival booths
# Activities (mean) per community	78 (7.8)	75 (7.5)
# Kids reached by publicity (estimate)	11,000	8,500
# Kids attending ≥ 1 activity	5,300	4,000
# At-risk youth (%) attending ≥ 1 activity	1,900 (36%)	1,600 (40%)

Intervention

FC and YAT were the two major components of the intervention. Based on other successful anti-tobacco and marijuana media campaigns (Palmgreen et al. 2002; Pechmann and Reibling 2000), we targeted each component specifically to high sensation-seeking youth, using an iterative development process following Worden and colleagues (1994). We led structured focus testing separately for sixth graders and their parents, including more parents and teens whose parents or siblings smoked or chewed.

Family communications had six elements: (a) parental introductory letter, (b) distribution of videos and homework via sixth-grade classes, (c) individual incentives to return assignments, (d) classroom incentives for 80% or better assignment return, (e) family incentives (\$300) via lottery in each community, and (f) parent newsletters mailed after the distribution of the videos.

We developed the videos using a “stealth model”—the format and content were created to attract sixth graders, while providing an adult-aimed segment on positive parent–child tobacco communication. The videos and corresponding homework targeted three areas of tobacco-related education: (a) tobacco health consequences, (b) social influences to use, and (c) media influences to use. Each program had four segments (stories) within a teen-led television show, *Tobacco Free TV* (TFTV). Two provided entertaining education about a tobacco-related domain and one showed youth partaking in organized anti-tobacco activities. The pivotal segment was devoted to teaching parents specific behavioral skills targeting our proposed mediators (e.g., rule-setting). The first video presented basic communication skills (e.g., involving a child in discussions, sharing experiences, listening), and subsequent videos gave instruction in stating expectations for not using tobacco, creating rules about not using tobacco, and

collaborating with a child to define consequences and rewards based on rules adherence.

Video 1: *Focus on Health* taught parents and children how to talk about tobacco use and discuss expectations about not using tobacco; described health effects of tobacco use and ETS; discussed people affected by its use; and highlighted youth in action against tobacco use.

Video 2: *Focus on Friends* showed how to discuss tobacco-use expectations, monitor children’s activities with friends, and set time limits with friends who use; showed how few teens use tobacco and how many disapprove of it; and highlighted kids trying to limit tobacco access.

Video 3: *Focus on Media* taught families how to discuss tobacco use (expectations, limit-setting, rewards for nonuse); analyzed tobacco ads and promos; showed social undesirability of using; and highlighted teen advocates for tobacco prevention, education, and public policies.

Staff distributed videos and homework to sixth graders in intervention schools, requiring students to watch them with a parent/guardian. Classes received rewards when at least 50% of students returned assignments in a week. A card with questions about the program and space for comments was included for adults to complete and return after watching videos. Completed, returned postcards placed families into the drawing. FC was consistent in each cohort.

To reinforce co-viewing of videos and family engagement in communication behaviors, we developed homework for each video. We used assignment returns and completed postcards as measures of FC-protocol adherence.

Youth anti-tobacco activities We developed YAT based on successful activities from previous research (Biglan et al.

2000). Focus groups helped choose a name and logo, selecting *f2b* (Freedom to Breathe); a graphic artist developed the logo. Focus groups provided ideas for giveaways and prizes with imprints of the logo and as incentives for participation in YAT. For C1, we focused on anti-smoking themes and tobacco harm. We stressed recruiting community leaders, service groups, and businesses to help fund and coordinate activities, hoping to build local support post intervention.

We wrote a manual showing how to stage each event; identify and obtain resources; develop time lines; advertise, promote, and organize activities; and report after-event follow-up. The manual provided guidelines for mobilizing community organizations and leaders for intervention support. Most events occurred within a 9-month period, starting immediately after baseline. Research staff coordinated and led activities in intervention communities in each cohort. Table 2 contains information about the number of activities and students reached in C1.

Based on our experience during C1 and feedback from our intervention coordinators in the field, we decided to modify the YAT component for C2 schools. We limited efforts to engage community leaders and focused instead on youth directly, allowing us better access and the ability to engage youth in our efforts. Our review of tobacco companies' marketing techniques (Biglan 2004) convinced us that emphasizing the harmful effects of smoking would have a limited impact on many at-risk adolescents, since it did not speak to their most pressing needs for social acceptance and excitement, and made adult efforts to restrict adolescent behavior salient—a circumstance likely to motivate rebellion among some youth. Thus, in C2, we emphasized fun, excitement, and social acceptance, associating each with *f2b*. The campaign did communicate some information about negative social and health effects of smoking to C2, but much more strongly associated *f2b* with fun social activities and branded gifts and prizes. For example, we limited discussion of tobacco-related issues, gave away *f2b* paraphernalia at events in general (not as rewards), used positive affirmations for engaging in healthful activities, and limited discouragement of negative behaviors. We polled students to create a menu of exciting activities and allowed students to vote for their preferred activities. The variety of activities was greater and several multicomunity activities allowed youth to interact with others (e.g., beach clean up).

For C2 we also increased efforts to identify and recruit youth at highest risk of starting tobacco use. In partnership with health teachers, principals, and school counselors, we obtained access to youth with problem behaviors. We increased the number of high sensation-seeking activities (e.g., skiing, roller skating, rock climbing) to appeal to these at-risk youth. Table 2 contains information about the number of activities and students reached in C2.

Analysis Methods

We assessed intervention effects, the primary outcomes of smoking prevalence and susceptibility, and secondary outcomes of ST, alcohol, and marijuana use. Due to intervention improvements made between cohorts, we expected larger effects for C2 schools. We tested each outcome for intervention effects among (a) all schools, which included schools in C1 and C2 combined, and (b) separately for C2 schools only. We also analyzed the data to test these condition effects with two different approaches, as they have complementary strengths.

We used a nested time $\times$ condition analysis to test differences between conditions on outcome changes from sixth (T_1) to eighth grade (T_2) (Murray 1998). This analysis included all data—whether or not the student was present at each time point—to estimate differences between assessments times and between conditions. The time by condition analysis accounts for autocorrelation among assessments and intraclass correlation associated with multiple students nested within same schools. This approach also assumes that “all groups and members respond to the intervention in the same way” (Murray 1998, pp. 182–184). As we cannot ensure the data meet this assumption, we also analyzed the data with a different approach.

We conducted a mixed-model analysis of covariance (ANCOVA) with sixth-grade scores as covariates for eighth-grade outcomes. This analysis contrasted residualized outcome scores between intervention and control conditions for students nested in schools. The mixed-model ANCOVA “avoids altogether assumptions about the pattern of the within-member covariance matrices” (Murray 1998, p. 190) incorporated into the time by condition analysis. The simple adjustment is also “less complex than the net difference in the repeated measures approach” (Murray 1998, p. 190). A disadvantage of this model, however, is that it includes only cases with data from both time points. We use ANCOVA as our primary method of analysis, as it generally offers greater power to detect differences between conditions (Janega et al. 2004a; Venter et al. 2002), but not always (Janega et al. 2004b). Due to the complementary strengths and limitations of each model, however, we also report on whether the time by condition analysis confirmed the findings for our primary outcomes. In addition, to aid in the planning of similar group-randomized trials, we reported intraclass correlation (ICC) values from the mixed-model ANCOVA for each measure.

Model estimates We fit models to our data with SAS PROC MIXED 9 (SAS Institute 2002) using the restricted maximum likelihood method (REML) recommended generally for multilevel models (Hox 2002; Verbeke and Molenberghs 2000). From each, we estimated fixed effects

and variance components. Variance parameter estimates were not constrained to values at or above zero, and MIXED provided negative variance estimates for some models. Negative variances often result from computational limitations for estimates very near zero and do not represent problems with the models or analysis procedures (Kreft and de Leeuw 1998; Singer and Willett 2003). Indeed, forcing non-negative estimates can lead to depressed Type I error rates and reduced statistical power (Murray et al. 1996; 1998). Maximum likelihood estimation for the time by condition analysis also allows the use of all available data. Such an analysis gives unbiased results even in the face of substantial attrition, provided any missing data were missing at random (Laird 1988; Schafer and Graham 2002). In the present study, we did not believe that attrition or other missing data represented a meaningful departure from the missing at random assumption, meaning that missing data were not likely to depend on unobserved determinants of the outcome of interest (Little and Rubin 2002).

The estimated models assume independent and normally distributed observations. We addressed the first, more important assumption (van Belle 2002) using multilevel statistical models. Regression and ANOVA have also been found to be robust to violations of normality (Fitzmaurice et al. 2004; Gibbons et al. 1993). Several studies have found non-normal data to lead to few problems in various multilevel modeling scenarios (Donner and Klar 1996; Hannan and Murray 1996; Maas and Hox 2004a, b; Murray et al. 2006a and b).

Multiple tests, Type I and II errors, and effect sizes In large community randomized trials, the tradeoff between Type I and II error rates represents a delicate balance. The cost of a false conclusion that intervention affects smoking or susceptibility (Type I) is problematic. Yet with high costs to conduct community randomized trials, Type II errors can also raise substantial problems, as they would obscure the value of an effective preventive intervention, misleading those hoping to curb tobacco use. To balance these concerns, we adopted a studywide Type I error rate of 0.10 for tests of effects on prevalence or susceptibility for schools in all cohorts and for those only in C2. With four primary comparisons, we then employed an α -level of 0.025 for each test, ensuring a 10% chance of 1 error among all four tests and a 90% chance of no errors.

The overall pattern of results reduces the likelihood of a Type I error. Type I errors represent a low-likelihood pattern of data due to random variation, not systematic effects due to intervention. A consistent pattern of results across measures and those expected from theory and prior research provide additional support for the intervention. That is, we expect condition effects for smoking and susceptibility but not necessarily for measures such as

marijuana use. We also expect larger effects for those who had a positive—rather than negative—image of smokers. The overall pattern of effects is thus another criterion for judging whether differences attributed to experimental condition were not generated from random sampling variation.

To ease effects interpretation, we computed an effect size for each fixed effect estimate. Given the limited N —especially with C2 alone—we did not want to overlook substantial effects approaching traditional levels of statistical significance. We thus focused more on effect sizes than on significance level when examining secondary hypotheses (e.g., moderator effects).

Unlike standard regression software, PROC MIXED provides no standardized beta estimates. This effect-size formula transforms the t -value and degrees of freedom from a statistical test into a correlation coefficient (Hunter and Schmidt 2004; Rosenthal et al. 2000; Rosenthal and Rubin 2003):

$$r = \frac{t}{\sqrt{t^2 + df}}$$

In simple models with one outcome and one predictor, this provides an effect-size estimate interpreted just like the usual Pearson correlation coefficient. When squared, it gives the proportion of overlapping variance or variance explained and equals a usual R^2 statistic. In standard regression models with multiple predictors or covariates, the r -value computed from this formula equals the partial correlation coefficient, which illustrates the magnitude of the relationship between two variables controlling for all other variables in the regression equation.

Results

Attrition Analysis

Participant attrition, also called experimental mortality, poses a threat to both external and internal validity of a study (Barry 2005; Graham and Donaldson 1993; Shadish et al. 2002). We expected no attrition among schools and none of the schools dropped from the study. We also did not expect substantial attrition among students, nor did we expect student attrition to differ by treatment condition. To test for attrition, we conducted an analysis for our two primary outcomes, smoking prevalence and smoking susceptibility. The analysis examined the effects of condition, attrition status, and their interaction on pretest levels of prevalence and susceptibility within a mixed-model ANOVA. Students were nested within schools, and the interaction between condition and attrition status represented a cross-level interaction. Below, we report interactions first and then conditional effects (Jaccard and Turrisi 2003). As

Table 3 Estimates from mixed model ANCOVA and time $\times$ condition analyses of smoking prevalence and susceptibility

Effect	Parameter	Prevalence			
		All schools		Cohort 2	
		ANCOVA ^a	$T \times C^b$	ANCOVA	$T \times C$
Fixed	Intercept (<i>I</i>)	0.1141*** (0.0100)	0.0372*** (0.0089)	0.1258*** (0.0121)	0.0268 (0.0141)
	Condition (<i>C</i>)	−0.0116 (0.0137)	−0.0069 (0.0123)	−0.0419* (0.0162)	0.0192 (0.0189)
	Time (<i>T</i>)		0.0919*** (0.0097)		0.1124*** (0.0142)
	$T \times C$		0.0076 (0.0133)		−0.0418**** (0.0187)
	Pretest Cov.	0.3290*** (0.0322)		0.3680*** (0.0568)	
Random	School (<i>C</i>)	0.0007	0.0006	0.0001	0.0008
	$T \times S$ (<i>C</i>)		0.0004		0.0003
	Time		0.0106		0.0094
	Residual	0.0979	0.0601	0.0947	0.0573

Entries show parameter estimates and, for fixed effects, standard errors in parentheses. Random effect parameters School (*C*) and $T \times S$ (*C*) represent school within condition and time by school within condition variances. The random effect Time within student was estimated with a compound-symmetric covariance structure; the table reports the off-diagonal element, while diagonal elements are the sum of the off-diagonal element and the residual (see Murray 1998, p. 299).

* $p < 0.025$; ** $p < 0.01$; *** $p < 0.001$; **** $p < 0.05$

^aThe mixed-model ANCOVA used 38 *df* for all schools and 18 *df* for Cohort 2 schools in tests of condition.

^bThe nested time $\times$ condition analysis used 37 *df* for all schools and 17 *df* for Cohort 2 schools in tests of condition.

with all analyses, we conducted the attrition analysis once for all schools and again for schools in C2.

Prevalence We found no attrition-by-condition interaction for prevalence for all schools or for C2 schools. For schools in both cohorts, students without posttest information reported higher levels of prevalence than students with posttest data, 0.027, $t = 3.76$, $df = 6,194$, $p = 0.0002$. Students did not, however, differ by condition at pretest. Among C2 schools, students without posttest data also reported higher levels of prevalence than students with posttest data, 0.029, $t = 2.90$, $df = 2,740$, $p = 0.0037$. For C2 schools, students did not differ by condition at pretest.

Susceptibility Unlike the analysis of prevalence, there was a significant interaction between condition and attrition status for susceptibility. Across both cohorts, the statistical model estimated pretest susceptibility means of 0.19 for control students and 0.19 for treatment students among those with posttest data and 0.15 for control students and 0.23 for treatment students among those without posttest data. Students without posttest data in the intervention condition reported greater susceptibility than students with posttest data in the intervention condition and students without posttest data in the control condition (condition $\times$ attrition interaction, 0.078, $t = 3.07$, $df = 35$, $p = 0.0041$), and students without posttest data in the control condition were less susceptible than those with posttest data, -0.037 , $t = -2.02$, $df = 3,924$, $p = 0.0436$. Susceptibility at pretest did not differ by condition for those with complete data.

The pattern of results was replicated with C2 schools. For students with posttest data, the model estimated susceptibility

means of 0.20 for the control condition and 0.18 for intervention, and among students without posttest data, 0.15 for control and 0.23 for intervention. We obtained a statistically significant condition by attrition effect, 0.095, $t = 2.93$, $df = 17$, $p = 0.0093$. We also found an attrition effect among controls, -0.047 , $t = -1.98$, $df = 2,442$, $p = 0.0478$ and did not find a condition effect among students with complete data.

Intervention Effects

Table 3 presents the results for the analysis of smoking prevalence and susceptibility. This table shows the results of eight models: the mixed-model ANCOVA and nested time by condition analysis for all schools and for C2 schools and for both dependent variables—smoking prevalence and susceptibility. For the mixed-model ANCOVA analysis, the condition parameter represents the test of condition effects. For the nested time by condition analysis, in contrast, the time by condition ($T \times C$) parameter provides the test of condition effects. Within nested time by condition analysis models, the condition and time parameters are considered *conditional effects* (Jaccard and Turrisi 2003): condition gives the test of condition at pretest, and time provides a test of change from pretest to posttest among control schools. Thus, Table 3 shows that smoking prevalence and susceptibility differences by condition at pretest were not statistically significant. Although not presented, tests of cohort differences at pretest were also not statistically significant.

Smoking prevalence We found no overall intervention effect on smoking prevalence across both cohorts with

Susceptibility

All schools		Cohort 2	
ANCOVA	$T \times C$	ANCOVA	$T \times C$
0.4324*** (0.0231)	0.1773*** (0.0212)	0.4046*** (0.0260)	0.1767*** (0.0205)
−0.0496 (0.0307)	0.0344 (0.0295)	−0.0805* (0.0328)	0.0215 (0.0275)
	0.3812*** (0.0247)		0.3220*** (0.0253)
	−0.0718**** (0.0339)		−0.0977** (0.0335)
0.5303*** (0.0339)		0.5237*** (0.0429)	
0.0020	0.0025	−0.0003	0.0010
	0.0025		0.0003
	0.0717		0.0621
0.4009	0.2071	0.3922	0.1912

either the mixed-model ANCOVA, $p=0.4010$ or the time by condition analysis ($p=0.5716$). These results correspond to the first and second columns of estimates in Table 3. The intraclass correlation (ICC) for smoking prevalence among all schools was 0.007 from the mixed-model ANCOVA.

We did find a statistically significant effect for C2 with the ANCOVA, -0.042 , $t=-2.59$, $df=18$, $p=0.0187$, representing a 4.2% reduction in smoking prevalence in intervention schools after controlling for pretest smoking. A partial r of -0.52 indicates that condition accounts for 27% of school-level variance smoking prevalence. The time by condition analysis confirmed this finding, -0.042 , $t=-2.23$, $df=17$, $p=0.0394$. C2 exhibited an 11.2% increase in smoking prevalence in control schools and a 7.1% increase in intervention schools. The partial r associated with this effect for C2 was -0.48 . For details, see the third and fourth columns of estimates in Table 3. For C2 schools, the ICC from the mixed-model ANCOVA was less than 0.001.

We found a marginally significant interaction effect between cohort and condition in the ANCOVA, -0.054 , $t=-2.03$, $df=35$, $p=0.0498$ and a statistically significant interaction effect in the time by condition analysis, -0.062 , $t=-2.41$, $df=35$, $p=0.0216$.

Susceptibility In this analysis, we dropped those who smoked at T_1 and those missing data on lifetime smoking, as we could not determine their T_1 smoking status.¹ For all schools, we found no effect with the mixed-model ANCOVA, $p=0.1147$, but the time by condition analysis indicated a marginal reduction in susceptibility, -0.072 , $t=-2.12$, $df=37$, $p=0.0410$. For parameter estimates see the fifth and sixth columns in Table 3. We computed an ICC of 0.005 for all schools from the mixed-model ANCOVA.

¹ Lifetime smoking data were missing for 42 C2 cases from the mixed-model ANCOVA. In the nested time $\times$ condition analysis, we removed 1,961 surveys due to missing data, but only 84 of those had data at both pretest and posttest. We eliminated these cases since we included only nonsmokers.

C2 schools significantly differed by condition on susceptibility according to both the mixed-model ANCOVA, -0.080 , $t=-2.45$, $df=18$, $p=0.0245$, partial $r=-0.50$, and the time by condition analysis, -0.098 , $t=-2.92$, $df=17$, $p=0.0096$, partial $r=-0.58$ (see the last two columns of Table 3). The interaction between condition and cohort was not significant with the mixed-model ANCOVA, $p=0.0770$ or the time by condition analysis, $p=0.1331$, indicating the difference in effects between cohorts was not statistically significant. The mixed-model ANCOVA estimated a zero ICC for C2 schools.

Smokeless tobacco prevalence Due to the very low rates of female ST use, we examined ST use only for males. The intervention did not affect male ST use across both cohorts, $p=0.9349$ or in C2, $p=0.9058$. From the mixed-model ANCOVA, we estimated ICCs of 0.013 for all schools and 0.016 for C2 schools.

Other outcomes For the analysis of alcohol and marijuana use, pretest estimates were not available. These variables were analyzed with only the mixed-model ANCOVA; we entered smoking status as the pretest covariate. For eighth-grade alcohol use prevalence, differences between conditions across cohorts approached significance for all schools, -0.039 , $t=-2.028$, $df=38$, $p=0.0496$, $r=-0.31$ and for C2, -0.059 , $t=-1.99$, $df=18$, $p=0.0625$, and the mixed-model ANOVA estimated ICCs of 0.005 and 0.006, respectively. We found no effects for marijuana use across schools, $p=0.7222$ or in C2, $p=0.9289$, and found ICC estimates of 0.006 for all schools and 0.011 for C2 schools.

Analysis of Moderators and Mediators of Intervention Effects

A series of analyses clarified mechanisms by which the intervention affected smoking. Analyses of moderator variables examined whether effects differed for subgroups

of students and were conducted on five variables: (a) susceptibility to smoke, (b) images of smokers, (c) house rules about tobacco, (d) praise for not smoking, and (e) belief that cigarette ads promote smoking. Models for the moderator analyses included condition, the hypothesized moderator variable, and interaction of the moderator and treatment condition. For statistically significant moderators, we centered coding of each moderator variable at each level and reanalyzed data to determine the effect of condition at each response possibility (Jaccard and Turrisi 2003).

For moderation, we removed pretest smoking-prevalence covariates from analytical models. The estimates shown represent unadjusted smoking rates; that is, an estimate of -0.096 represents a decrease in prevalence of 9.6%. As few sixth graders smoked, the results for unadjusted models are nearly identical to prediction models including sixth-grade smoking as a covariate in each important respect, but unadjusted models are decidedly easier to interpret. Figure 2 shows the moderation effects for susceptibility, image of smokers, and tobacco rules in the home on the predicted eighth-grade smoking prevalence for all schools and for C2 schools. Table 4 reports observed smoking prevalence for each level of the T_1 moderator variables and by condition for all schools and for each cohort.

Mediation analyses examined whether the impact of the intervention on prevalence was mediated by its expected effect on variables. We performed mediation analyses on all variables except susceptibility. It was not possible to assess mediation for susceptibility because the highest level of the susceptibility variable was defined as those who were already smoking.

Susceptibility The moderator analysis for susceptibility found a statistically significant interaction between condition and susceptibility, -0.106 , $t=-3.56$, $df=38$, $p=0.0009$. Intervention effects were not statistically significant for nonsusceptible sixth graders, $p=0.5769$, but they were statistically significant among susceptible students, -0.096 , $t=-3.39$, $df=38$, $p=0.0017$. For control students across cohorts, susceptible students were significantly more likely to smoke in eighth grade than nonsusceptible students were, 0.197 , $t=9.31$, $df=2,438$, $p<0.0001$, $r=0.19$.

Similarly for C2 schools, we found a statistically significant interaction between condition and pretest susceptibility, -0.205 , $t=-5.47$, $df=18$, $p<0.0001$. The C2 intervention effects were not statistically significant for students who were nonsusceptible at pretest, -0.007 , $t=-0.43$, $df=18$, $p=0.6696$, but smoking rates differed significantly by condition for susceptible students, -0.212 , $t=-6.27$, $df=18$, $p<0.0001$. In C2, sixth-grade susceptibility also predicted eighth-grade smoking among control students, 0.276 , $t=10.07$, $df=1,416$, $p<0.0001$, $r=0.26$.

Image of smokers As the intervention was based on promoting social acceptance and a positive image of nonsmokers, we might expect to find greater effects among those with an initial positive image of smokers. Across cohorts, sixth-grade smoker image was slightly but significantly associated with eighth-grade 30-day cigarette use, 0.042 , $t=4.64$, $df=3,641$, $p<0.0001$, $r=0.08$, controlling for sixth-grade use. However, the influence of sixth graders' image of smokers on their uptake of smoking was significantly less for those in the intervention condition than for those in control. The condition-by-smoker-image interaction, without pretest smoking in the model, was statistically significant for all schools, -0.038 , $t=-2.12$, $df=37$, $p=0.0411$, $r=-0.33$. There was no evidence that smoker image mediated intervention effects. Specifically, condition did not affect students' images of smokers.

In C2, sixth-grade smoker image was significantly associated with eighth-grade 30-day use, 0.040 , $t=2.93$, $df=1,479$, $p=0.0034$, $r=0.08$, controlling for sixth-grade use. Yet, the influence of sixth graders' smoker image on eighth-grade smoking was significantly less for those in intervention than for those in control. The condition by smoker-image interaction was statistically significant in C2 schools, -0.062 , $t=-2.26$, $df=17$, $p<0.0372$, $r=-0.48$. For each one-point increase in smoker image, 6.2% fewer intervention students smoked. Mediation, among C2 schools, was not supported.

Home tobacco rules

Across cohorts, sixth-grade reports on house rules were significantly related to eighth-grade 30-day cigarette use, controlling for sixth-grade use, -0.019 , $t=-4.87$, $df=3,406$, $p<0.0001$, $r=-0.08$, indicating a moderate influence of lax house rules on smoking uptake for all schools. Without controlling for pretest smoking, we found a marginally significant interaction between condition and house rules, 0.016 , $t=1.95$, $df=37$, $p=0.0584$, $r=0.31$. Condition effects were strongest for students without or with minimally enforced house rules.

In C2 schools, sixth-grade reports of home rules predicted eighth-grade smoking when controlling for sixth-grade smoking, -0.02 , $t=-3.43$, $df=1,472$, $p=0.0006$, $r=-0.09$. As in the analysis of all schools, house rules moderated intervention effect as the interaction between condition and house rules was statistically significant in C2, 0.029 , $t=2.39$, $df=17$, $p=0.0288$, $r=0.50$ (see Fig. 2).

We found no evidence that intervention effects were mediated by effects on house rules for the combined sample or C2. Condition did not predict changes in rules in the home.

Fig. 2 Predicted scores for condition by moderator interactions effects on 8th-grade smoking prevalence for all schools (*first column*) and Cohort 2 schools (*second column*)

Table 4 Smoking 1-month prevalence for levels of susceptibility, smoker image, and tobacco rules by condition in all schools and in cohort 2 schools

	All schools		Cohort 2	
	Control	Intervention	Control	Intervention
Susceptibility				
Not susceptible (<i>N</i>)	6.6 (1006)	7.0 (1094)	6.7 (523)	6.1 (673)
Susceptible (<i>N</i>)	26.4 (273)	20.9 (301)	31.7 (161)	17.4 (167)
Smoker image ^a				
Lowest (<i>N</i>)	7.8 (640)	8.5 (696)	8.3 (241)	8.2 (282)
Low (<i>N</i>)	13.4 (908)	11.6 (1075)	14.9 (356)	8.6 (443)
High (<i>N</i>)	18.1 (144)	17.0 (176)	19.7 (71)	12.8 (94)
Highest (<i>N</i>)	30.0 (20)	15.2 (33)	30.0 (10)	11.1 (9)
Tobacco rules				
No rule (<i>N</i>)	15.9 (453)	13.6 (543)	17.5 (200)	9.4 (235)
Not enforced (<i>N</i>)	18.7 (123)	8.9 (146)	26.3 (57)	13.4 (67)
Sometimes enforced (<i>N</i>)	15.0 (200)	14.1 (163)	16.5 (85)	13.2 (68)
Always enforced (<i>N</i>)	7.5 (854)	8.9 (973)	7.7 (337)	7.6 (449)

^a Smoker image was rounded to whole numbers in this table. The analysis used the 12-item average that included fractional values.

Praise for not smoking Although praise for not smoking in sixth grade predicted eighth-grade smoking, partial $r = -0.09$, $p < 0.001$, it neither moderated nor mediated intervention effects.

Cigarette ads promote youth smoking A sixth-grade belief that cigarette ads promoted smoking was associated with a slight reduction in smoking probability in eighth grade, partial $r = 0.07$, $p < 0.0001$. However, this variable did not moderate or mediate intervention effects.

Discussion

The results support the efficacy of family communications and youth anti-tobacco activities, when combined, for preventing smoking among early adolescents. Videotapes provided families with tobacco use education and techniques for discussing family norms about tobacco use. Our intent in using teen narrators who were older than the study participants was to reduce the sense that the activity involved parents telling their children what to do. Anti-tobacco activities (particularly in Cohort 2) associated fun, excitement, and social acceptance with a nonsmoking brand. Project design and intervention staff encouraged and facilitated parental involvement in both components.

Generally, the findings suggest that the combined approach of influencing parents to ask their teens not to use tobacco and marketing an anti-tobacco perspective to adolescents is an effective way to prevent smoking. The study does not allow us to determine the relative impact of the two components. However, the fact that most of the program impact emerged for Cohort 2 suggests that the nature of YAT activities in Cohort 2 was important. The impact of YAT is consistent with experimental and nonexperimental evaluations of media campaigns to influence youth not to smoke (Farrelly et al. 2002, 2005; Pechmann and Reibling 2000; Worden et al. 1988). Like those studies, YAT used marketing to influence teen smoking. However, it differed from those interventions in two ways. First, it involved direct contact with youth rather than via media. Perhaps more importantly, especially in Cohort 2, YAT associated not smoking with social acceptance and fun and with the f2b brand and deemphasized information about tobacco use harmfulness. This strategy was prompted by an extensive analysis of the way cigarettes are marketed to youth. Namely, they are associated with social acceptance and with symbols and images associated with social acceptance (Biglan 2004; NCI 2008). Given the nature of this component, we believe the results indicate it is possible and productive to market a nonsmoking lifestyle by

associating it with a brand that is, in turn, associated with symbols, images, and activities that adolescents highly value.

The results align with recent experimental research on processes that establish brand functions. Grey and Barnes (1996) found that attitudes to labeled (but unviewed) videos were affected by whether the labels were associated indirectly with a tape with religious content or one with sexual content. Barnes-Holmes et al. (2000) found that college students' ratings of a soft drink's pleasantness depended upon whether the drink's label was associated with a nonsense syllable associated with either the word *cancer* or the word *holidays*. Although the drinks were the same in each condition, subjects preferred one labeled with the brand linked to the symbol associated with *holidays*. Similarly, Smeets and Barnes-Holmes (2003) found that 5-year-olds' preference for a soft drink was influenced by whether the label was associated with an arbitrary geometric shape that had been associated with a picture of a crying child or with a smiling cartoon character. Despite the fact that the liquid was the same in both conditions, children were significantly more likely to prefer the cup with the shape associated with the cartoon.

If these branded YAT activities truly made an impact by associating not smoking with social acceptance, it has implications for existing cognitive theories of adolescent substance use. In reviewing them, Petraitis et al. (1995) find the theories focus on establishing negative evaluations of substance use behaviors, but our results suggest it may be more important (and more feasible) to establish associations between *not* engaging in these behaviors and social acceptance. This does not imply the invalidity of such theories, but suggests that increasing positive attitudes toward nonuse and normative beliefs about nonuse may be more productive than instilling negative attitudes and normative beliefs about using.

The results also indicate that the intervention affected adolescent susceptibility to start smoking. Recent evidence indicates the value of targeting this outcome. Pierce et al. (1996) found that adolescents who do not definitely rule out the possibility of smoking are at significantly greater risk to initiate it. We recently replicated this; Forrester et al. (2007) found that adolescents in seventh and ninth grade who did not definitely rule out future smoking were significantly more likely to smoke 2 years later. Thus, interventions affecting susceptibility are likely valuable in preventing later smoking. This makes it possible to evaluate the impact of smoking prevention programs by assessing their effects on susceptibility. Although such programs must ultimately be evaluated in terms of their effects on smoking, testing their impact on susceptibility makes it possible to gauge their value more quickly than if the only dependent measure is actual smoking.

Forrester et al. (2007) also found that some predictors of susceptibility differ from those for actual smoking onset. In particular, parental monitoring predicted susceptibility, but not smoking. Since FC was designed to increase parental influence not to smoke, it is possible the effect was due to increased parental efforts to influence their adolescents not to smoke.

Our finding that the intervention had some impact on alcohol use may reflect a tendency for young people to generalize from not smoking to not using alcohol. That is, having altered their inclination to smoke, they may be less inclined to use alcohol. Perhaps the intervention changed participants' views about the need to use cigarettes or alcohol to achieve social acceptance. However, such an interpretation is tenuous, given the lack of an effect on ST use or marijuana use.

As noted, it is not possible to separate the effects of the FC and YAT activities. However, there is ample evidence that both parents and peers influence smoking (e.g., Biglan et al. 1995b). There may have been a synergistic relationship between the components. Each component targets a distinct aspect of the adolescent environment. Yet during the intervention, we became aware of a synergy between the two components, with parents and kids becoming involved in f2b activities due to watching the TFTV videos together. The combination of components appears to have increased positive affirmations about a nontobacco lifestyle from both parents and peers, and appears to be effective in reducing initiation of smoking, especially among those youth most at risk.

Impact of Attrition

The attrition effects for smoking prevalence indicate a potential threat to external validity but not to internal validity as the condition by attrition interaction was not statistically significant. The condition effects for susceptibility, however, pose a threat to internal validity, as attrition appears to have differed by condition, with fewer students who were high in susceptibility providing data at posttest within the intervention condition. The time by condition analyses, however, can reduce but not necessarily eliminate bias (Nich and Carroll 1997) as the procedure estimates the models with all available data. With the pretest measure (susceptibility in this case) in the model, the data become missing at random (MAR) rather than missing not at random (MNAR; Rubin 1976), and the analyses can reduce the bias caused by attrition (Graham and Donaldson 1993; Schafer and Graham 2002).

In addition, given that students with greater mobility may be more susceptible to cigarettes, and vice versa, it is important to keep in mind that the time by condition analyses also included all eighth-grade students regardless

of their participation in sixth grade. New students in eighth grade may partially counterbalance the potential bias due to differential attrition, although any test of this possibility would have been confounded with the condition effects.

Moreover, analyses of moderator effects show that the intervention had its strongest impact on students who, in sixth grade, were (a) susceptible to smoking, (b) had positive images of smokers, or (c) had families with lax rules about smoking. All of these findings are inconsistent with the notion that the overall results were simply due to differential attrition. These results are consistent with the view that the intervention had its impact through precisely the processes the intervention was created to counteract—susceptibility, positive images of smokers, and parental laxness about smoking. Moreover, the result were stronger for Cohort 2, as we had hypothesized. This network of findings increases confidence that the overall results were not due to differential attrition. Nonetheless, it is appropriate to view the results with caution.

Conclusion

It is noteworthy that the present intervention contained only two of the components included in the Project SixTeen program, yet had an equal or greater impact on smoking. It is impossible to precisely compare the outcomes of the two studies. Project SixTeen had five components (school-based curriculum, access reduction, anti-tobacco media, FC, and YAT). Moreover, its intervention was compared with a school-based curriculum control, whereas, the present study had a no-intervention control. Nonetheless, the results are encouraging in suggesting that significant tobacco prevention can be achieved without taking significant amounts of classroom time and without the extensive programming and resources employed in Project SixTeen. Although we cannot definitively say that branding a tobacco-free lifestyle with social acceptance is the essential ingredient in our intervention, the results point to the value of abandoning negatively framed messages about tobacco and other substance use and further evaluating the impact of positive social acceptance brands for motivating non-use of tobacco and other substances.

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