PREVENTIVE MEDICINE 24, 29–40 (1995)

The Television, School, and Family Smoking Prevention and Cessation Project

VIII. Student Outcomes and Mediating Variables[1]

BRIAN R. FLAY, D.PHIL.,*,[2] TODD Q. MILLER,*,[3] DONALD HEDEKER, PH.D.,* OHID SIDDIQUI,* CYNTHIA F. BRITTON,* BONNIE R. BRANNON,†,[4] C. ANDERSON JOHNSON, PH.D.,† WILLIAM B. HANSEN, PH.D.,†,[5] STEVE SUSSMAN, PH.D.,† AND CLYDE DENT, PH.D.†

Prevention Research Center, University of Illinois at Chicago, Chicago, Illinois 60607-3025; and †Institute for Prevention Research, University of Southern California, Los Angeles, California 90007

Background. **This paper presents the student outcomes of a large-scale, social-influences-based, school and media-based tobacco use prevention and cessation project in Southern California.**

Methods. **The study provided an experimental comparison of classroom delivery with television delivery and the combination of the two in a 2 × 2 plus 1 design. Schools were randomly assigned to conditions. Control groups included "treatment as usual" and an "attention control" with the same outcome expectancies as the treatment conditions. Students were surveyed twice in grade 7 and once in each of grades 8 and 9. The interventions occurred during grade 7.**

Results. **We observed significant effects on mediating variables such as knowledge, prevalence estimates, and coping effort. The knowledge and prevalence estimates effects decayed partially but remained significant up to a 2-year follow-up. The coping effort effect did not persist at follow-ups. There were significant main effects of both classroom training and TV programming on knowledge and prevalence estimates and significant interactions of classroom and TV programming on knowledge (negative), disapproval of parental smoking, and coping effort. There were no consistent program effects on refusal/self-efficacy, smoking intentions, or behavior.**

Conclusions. **Previous reports demonstrated successful development and pilot testing of program components and measures and high acceptance of the program by students and parents. The lack of behavioral effects may have been the result of imperfect program implementation or low base rates of intentions and behavior.**

[1] This research was funded by a grant from the National Institute of Drug Abuse, Grant 1-R01-DA03468 to Brian R. Flay, W. B. Hansen, and C. A. Johnson. The analyses reported were completed with the support of Grant 1-R01-DA 06307 to Brian R. Flay and Jean Richardson.

[2] To whom reprint requests should be addressed.

[3] Now at University of Texas at Galveston.

[4] Now at University of Washington, Seattle.

[5] Now at Bowman Gray School of Medicine, Wake Forest University.

INTRODUCTION

Smoking remains the leading preventable cause of mortality and morbidity in the United States (1). Though the number of adolescents who experiment with tobacco and go on to regular use declined in the early eighties, experimentation has not decreased in recent years. Thus, preventing the onset of tobacco use among adolescents remains a major public health objective. The purpose of this paper is to report the results of a large-scale smoking prevention and cessation trial, the Television, School, and Family Project (TVSFP), on student smoking and mediating variables.

School-based social influences programs have been effective in delaying the onset of smoking in adolescents, at least for 1 or 2 years (2–4). However, the few studies that have reported long-term results have been less encouraging (5,6). Mass media may be an effective means to alter smoking behavior, especially when combined with printed materials and interpersonal communication opportunities (7,8). There are many ways school- and media-based interventions can complement and enhance the effectiveness of each other (9). Worden *et al.* (10) and Flynn *et al.* (11) tested the effects of mass media (television and radio) messages that complemented a school-based social influences curriculum and found that they improved program effects. Flay *et al.* (12) reported less encouraging results from the only previous study to investigate the effectiveness of combined school- and media-based programming for both prevention of student smoking and smoking cessation by parents, but methodological weaknesses limited interpretation of results.

We designed the TVSFP study to test the independent and combined effects of a classroom curriculum and television programming for social resistance skills training, smoking prevention, and smoking cessation (13). The TVSFP program encouraged discussion (a) among students in the classroom, (b) between students

0091-7435/95 $6.00

and parents for both prevention and cessation in the home through a media component, and (c) among adults in the home and workplace for cessation.

Treatment effects observed in many previous studies may have been attention or Hawthorne effects (2). Only a few previous smoking prevention studies (14,15) have included attention-control groups. For these studies, we still do not know whether attention control and experimental groups responded to their programs with equal expectations of program success. Thus, differential attention by the treatment and attention control group remains a threat to the interpretation of results from these studies. This study improves on previous research by including an attention control group with outcome expectancies that were equivalent to the treatment conditions (16), but which was not expected to change smoking behavior (17). It was thought that a health-information-based program would increase students' tobacco and health knowledge without increasing their social-resistance skills or subsequently decreasing their smoking, thus providing an effective control for expectancy effects. The study also included a "treatment as usual" control condition.

Both current theory and the results of previous prevention interventions guided development of the TVSFP curriculum (13), which contains seven key elements: (a) correction of norms (particularly misperceptions of the prevalence of tobacco use among adolescents and in the general population); (b) awareness of peer influences to smoke; (c) development of peer resistance skills; (d) awareness of family influences to use tobacco; (e) development of media influence resistance skills; (f) instruction on the social and physiological consequences of smoking; and (g) development of decision-making skills. We devoted significant effort to formative evaluation of the television segments and pilot-testing of program components and measures (16,18,19), the integrity of program delivery (20), and program acceptability (21). The current paper reports on TVSFP program effects on youth smoking outcomes and hypothesized mediators at the immediate post-test and 1- and 2-year follow-ups.

METHODS

Subjects

The initial study sample consisted of students in seventh grade in 340 classrooms within 35 Los Angeles and 12 San Diego schools within six different school districts in January 1986. The project pretested 7,351 students and 6,695 (91%) indicated their gender, race, and smoking status. Students present at the pretest later completed an immediate postintervention questionnaire (April 1986), a 1-year follow-up questionnaire (April 1987), and a 2-year follow-up (April 1988). The pretested students were 49.1% male and 50.9% female. They were 35.5% Hispanic, 33.3% White, 13.9% African–American, and 17.3% Other. By the 2-year follow-up, 3,155 (47%) of the original sample was present. At that time, 47.6% were male, 36.1% were Hispanic, 34.8% were White, 10.3% were African–American and 18.6% were Other. Greater attrition was observed in Los Angeles than San Diego, and African–Americans and students with lower school grades were more likely to drop out. Differential attrition by experimental/control conditions did not occur.

Procedure

Figure 1 summarizes the theoretical study design. The central study design investigates the relative effectiveness of (a) a social-resistance classroom curriculum, (b) a media (television) intervention, (c) a health-information-based attention-control curriculum, (d) a social-resistance classroom curriculum combined with a mass-media intervention, and (e) a no-treatment control group. The three study factors are classroom curriculum, media intervention, and site (San Diego or Los Angeles). No schools in the San Diego area received the televised intervention. One purpose of the San Diego site was to eliminate possible contamination of the controls by inadvertent exposure to the televised intervention.

Assignment to conditions. Within each of two counties (Los Angeles and San Diego) we assigned entire

SITE:	LOS ANGELES[a]			SAN DIEGO[b]
TELEVISION:	Television	No Television		No Television
CLASSROOM:				
Social Resistance Classroom curriculum	SR + TV	SR-Only (LA)		SR-Only (SD)
No classroom curriculum	TV-Only	Attention Control Placebo	No-Treatment Control (LA)	No-Treatment Control (SD)

[a] Seven schools per condition.
[b] Six schools per condition.

FIG. 1. Television, School, and Family Project research design.

schools to conditions (22) using a randomized multiattribute blocking approach developed by Graham *et al.* (23). At the pretest, there were no significant differences in smoking rates across any of the seven conditions; 40.4% stated that they had tried a cigarette at least once and 4.5% stated that they had smoked at least once in the past week. More males (43%) than females (38%) had smoked cigarettes. Rates of experimentation with cigarettes also varied by ethnicity (24): 33% for African–Americans, 38% for Others, and 45% for Whites and Hispanics. At the 2-year follow-up, 58.3% had smoked at least once in their lifetime, an increase of 18% from pretest, and 10.8% had smoked in the past week, an increase of 6.3% from pretest.

Questionnaire administration. Trained data collectors provided standard instructions and administered the questionnaires to entire classrooms. Expired air samples were collected under bogus pipeline conditions to increase the validity of self-reports of cigarette use (2).

Measures

The project included measures of the variables in Fig. 2. We derived many of the items and scales from Project SMART measures (25,26), and they are of known and acceptable reliability (27). Several additional items were developed to tap specific material provided in the curriculum and constructs such as expectancies and coping effort (28). Below, we present the major immediate outcome variables, most of which we assume mediate program effects on behavior (29). Petraitis (18) lists the individual questionnaire items used to assess each construct. We report internal consistency and test–retest (waves B to C and waves C to D) stabilities where appropriate. The length of time between measures and the circumstances of an intervention may account for relatively low stability values.

Tobacco and health knowledge. We used seven questionnaire items to assess student tobacco and health knowledge. Although some tobacco and health information was presented in the treatment conditions, these items sampled the broader set of material that was presented in the health-information-based classroom curriculum. A student's score on the tobacco and health knowledge scale is the number of items the student answered correctly. Test–retest stability was 0.30 between waves B and C and 0.37 between waves C and D.

Social influences/resistance skills knowledge. Eight questionnaire items assessed student's knowledge of the social resistance curriculum. These items sampled the material presented in the social-resistance-based classroom curriculum. We expected that a social-influences- and resistance-skills-based prevention program would (a) correct perceptions of prevalence of tobacco use among adolescents in the general population, (b) increase awareness of peer, adult, and media influences to smoke; and (c) teach effective resistance skills against peer and media influences (2,13). As with the health knowledge items, a student's social resistance knowledge scale was the number of items the student answered correctly. Test–retest stability was 0.26 between waves B and C and 0.28 between waves C and D.

Refusal self-efficacy. Two items assessed student's confidence in refusing to smoke cigarettes. The refusal self-efficacy scale score is the sum of the ratings for these two items (internal consistency = 0.33 at wave A to 0.46 at wave D; test–retest stability was 0.33 between waves B and C and 0.35 between waves C and D).

Coping effort. The classroom social resistance curriculum attempted to encourage students to increase their efforts not to smoke. Lazarus and Folkman (30) distinguish coping from mere adaptation as a mobili-

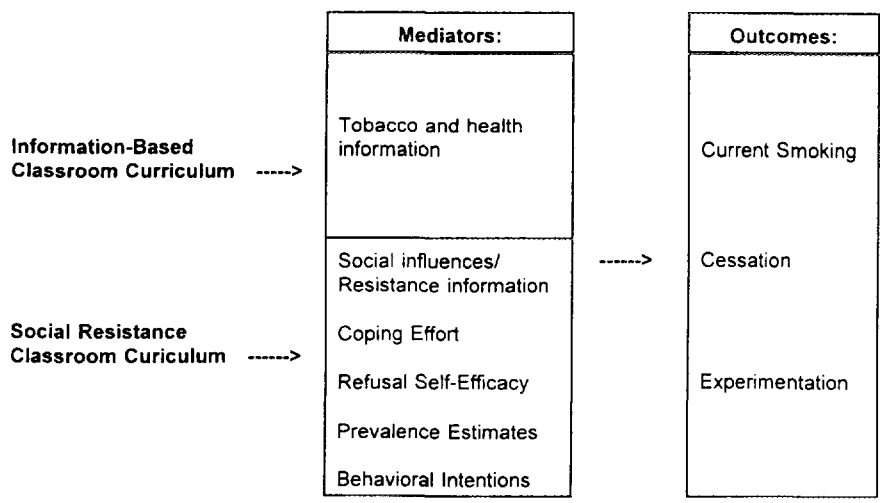

FIG. 2. Presumed relations between intended mediators of TVSFP program effects.

zation of effort. The effort a student exerts to resist smoking cigarettes may be a major determinant of success. Three items assessed students' efforts to resist trying cigarettes. A student's coping effort consists of the sum of the student's ratings for the three items (28). This scale had high internal consistency that remained the same across waves (0.934 ± 0.002) and moderate test–retest stability (0.35 between waves B and C and 0.41 between waves C and D).

Intentions. Ajzen and Fishbein (31) suggest that intentions are the most proximal predictor of actual behavior. Two items were summed to assess intentions to use cigarettes in the future (internal consistency = 0.78 at wave A increasing to 0.85 at wave D; test–retest stability was 0.48 between waves B and C and 0.50 between waves C and D).

Estimates of the prevalence of student and adult smoking. Several studies have shown that estimates of the prevalence of cigarette smoking in adolescents is predictive of cigarette use (32–36). We asked students to estimate (a) the number of seventh-grade students who have ever smoked in their lifetime and (b) the number of students who smoke weekly. These two items were used to form a student prevalence estimate scale (internal consistency = 0.77 at wave A, 0.75 at wave C, and 0.73 at wave D; test–retest stability was 0.36 to 0.39 for ever smoked estimates and 0.33 to 0.37 for weekly smoking estimates). In addition, a single question asked students what percentage of adults smoke regularly (test–retest stability 0.30 to 0.38).

Approval of parental smoking. A single item assessed the extent to which students approved of their parents' smoking. We expected that the television program would increase students' disapproval of their parents' smoking (test–retest stability was 0.31 between waves B and C and 0.27 between waves C and D).

Outcome Variables

Tobacco use. The ultimate goal of the TVSFP social resistance curriculum was to decrease smoking rates among students who were already smoking and to prevent experimentation by students who were not smoking. Six questionnaire items assessed cigarette use including (a) three questions regarding lifetime use, (b) one item regarding use in the past week, and (c) two items regarding use in the last day. However, base rates of more than occasional use were very low in the seventh grade. Therefore, we only compare users with nonusers for (a) use in past week (test–retest stability was 0.26 between waves B and C and 0.31 between waves C and D) and (b) ever used in lifetime (test–retest stability was 0.71 between waves B and C and 0.72 between waves C and D).

Control Variables

Demographic variables. We examined relations between treatment conditions after statistically controlling for gender, father's occupational status, and ethnic status (African–American, Hispanic, White, and Other).

Intentions to smoke in the future. We expected that the program would have its largest effects on students who at pretest were uncertain about their intentions to use cigarettes. Therefore, we summed the two items that asked students about their intentions to use cigarettes. We grouped students into three groups: (a) those who stated on both items that they definitely did not intend to smoke (40.0%), (b) those who indicated they intended to smoke or didn't know whether they would smoke in the future (24.2%), and (c) those who stated they probably would not smoke (35.3%).

History of smoking. We considered that students who had smoked at least once in the past might have reactions to the program different from those of students who had never smoked. For this reason, we used prior history of smoking as a control variable.

Statistical Analysis

Analysis strategy. The general strategy was to test for any differences across the five conditions in Los Angeles[6] and then attempt to replicate our results in San Diego.[7] In the presence of a significant condition-related effect, we made a priori comparisons across treatment conditions to test specific research hypotheses. In particular, for the Los Angeles data with five conditions, unless otherwise noted, we performed four a priori comparisons: (1) attention control placebo vs no–treatment control; (2) TV vs no TV; (3) SR classroom curriculum vs no SR; and (4) an interaction between TV and the classroom curriculum. These a priori comparisons were only interpreted in the presence of a significant condition effect which considered all four comparisons jointly.[8] For the San Diego data, we sim-

[6] An exception was the test for the effects of the attention–placebo program on tobacco health knowledge. We expected tobacco health knowledge to improve only in the attention–placebo condition, so we compared the attention–placebo condition with the other four Los Angeles conditions combined.

[7] The design of the study was an incomplete three factor design. That is, some cells for the three factors (Site, Los Angeles versus San Diego; treatment, control, attention control, social resistance curriculum, Television curriculum) are missing. When treatment factor combinations are completely missing, as in this design, the test statistics for factorial effects from statistical computer package routines may be biased (37).

[8] If the first comparison (attention vs no-treatment control) was ever statistically significant, we would then have assessed comparisons (2)–(4) by performing two sets of post hoc tests (three tests in each): the first treating the attention control group as the sole control group, the second treating the no-treatment group as the sole control group. This situation never occurred.

ply compared the two conditions (treatment and control). Finally, we addressed several additional statistical problems: (a) differential effectiveness of treatment for different subpopulations (e.g., smokers versus nonsmokers), (b) random assignment at the school level when inferences are to be made at the individual level of change, (c) problems encountered with categorical and nonnormally distributed data, and (d) redundancy in information across different groups and (e) adjusting for experimentwise error rates.

Testing for differential effectiveness of treatments. Our population was heterogenous in terms of social and environmental influences for smoking and previous experience. Therefore, we tested for interactions between our control variables and the assignment to treatment conditions. Significant interactions would suggest that treatment conditions had different effects for different subpopulations. Treatment interactions were tested for statistical significance by comparing the model including interactions (interaction model) to the model excluding them (main effects model) using the likelihood ratio (LR) test. Results for the interaction model were reported only when the LR test was significant; otherwise, the reported results were based on the main effects model.

Inferences of individual change when random assignment is at the school level. Units of assignment and treatment determine the appropriate statistical model for evaluating an intervention (38–40). The primary problem with individual level analyses that are not corrected for school level variation is that standard errors for the individual level analyses may be attenuated, although in a study that was similar to the current study, Dwyer *et al.* (41) found that this was not a serious problem[9]. Recently, statistical analysis of multilevel data (students nested within classrooms nested within schools) in unbalanced designs (the numbers of students within classrooms and the numbers of classrooms within schools is not constant) has been developed under a variety of names including random-effects models (42,43), random-coefficients models (44), variance component models (45); hierarchical linear models (46), multilevel models (47), two-stage models (48), and mixed models (49). For continuous variables, our method is to model individual outcomes as three-level data (students within classrooms within schools) with differences between schools and differences between classrooms treated as random effects and differences between treatments as fixed effects (50). The advantages of the approach are that the researcher can enter covariates at the individual student level and avoid problems of ecological inferences at the group level when the interest is at the individual level of analysis. For approximately normally distributed quasicontinuous variables (information scale, social resistance scale, and the prevalence estimates scales), we analyzed the effects of the treatment conditions using the ML3 (51) multilevel analysis program for unbalanced data that uses iterative generalized least squares estimation. To assess the significance of condition-related effects in these analyses, we performed likelihood-ratio χ^2 tests. The significance of specific model parameters was determined by comparing the z-scores associated with each parameter (parameter estimate divided by its estimated standard error) to a standard normal distribution frequency table (52).

We used change scores from the pretest to each of the post-tests as the dependent variables. Change scores have the advantage of creating easily interpretable results and clearly indicating the direction of individual change (53). By using only two waves of data for each analysis, we decreased problems associated with missing data and subject attrition.

Models for categorical and nonnormally distributed data. Scales for intentions, coping effort, and refusal/self-efficacy were bimodal, with fewer individuals responding in the middle categories. The change scores for these variables were polymodal and not amenable to analysis as continuous normally distributed outcomes. Therefore, we divided the sample into three groups on these variables (change in a positive direction, change in a negative direction, and no change). We used recently developed random-effects models for ordinal outcome data (54,55), with the three-point change scores as the outcome variable. Since, at present, this procedure has only been developed for two-level data (56), our strategy was to fit both the two-level models of students within schools (treating schools as a random effect) and students within classrooms (treating classrooms as a random effect). For the reported results involving condition effects, it was always the case that once the condition-related effects were included in the model, the program was not able to estimate a nonzero school variance term for the students within schools model. We then based our results either on the students within classrooms model if the class variance was estimated as being nonzero, or on the ordinary student-level analysis if neither two-level model was able to estimate any residual effect of the clustering of students (accounting for the school condition effects).

Redundancy in measures. Our measures could be so highly correlated that different measures could essentially account for the same variance in other vari-

[9] Intraclass correlations for the data presented here were similar to those reported by Dwyer *et al.* (41). Intraschool correlations ranged between zero and 0.03 (3%) while intraclass correlations ranged between zero and 0.05 (5%). Both generally decreased across time and were greater for knowledge scales and coping effort (0–5%) than for intentions and parental smoking rates (0–4%), prevalence estimates, refusal self-efficacy, and behaviors (0–2%).

ables. Therefore, we examined correlations between all of our outcome variables to ensure that each measure was unique. The *r* between change in adult and student prevalence estimates was on the average 0.44 across all seven treatment conditions. Correlations between all other scales and outcome measures were less than 0.22 across all conditions. Thus, most of our measures contain statistically unique variance.

Attrition. In each case, we compare the pretest means/percentages for subjects with complete data at two waves (the pretest and one post-test) with the means/percentages for subjects with data only at pretest. We report significant differences when they occur. Koepke (57) and Dent (58) report more detailed attrition analyses.

RESULTS

Mediating Variables

Tobacco and health knowledge. Table 1 shows the means and standard deviations for the tobacco and health knowledge scale across all waves and conditions. In terms of condition-related comparisons, we contrasted the attention control group to all other groups for the Los Angeles data. As expected, tobacco and health knowledge was significantly higher in the attention control group than that in any of the other conditions in Los Angeles at the immediate post-test ($N = 2133$[10], $\chi^2 = 42.3$; $df = 1$; $P < 0.00001$), 1–year follow-up ($N = 1765$; $\chi^2 = 25.9$; $df = 1$; $P < 0.00001$) and 2-year follow-up ($N = 1246$; $\chi^2 = 14.5$; $df = 1$; $P < 0.00014$). There was a significant interaction between smoking history and the attention control group at the immediate post-test ($N = 2115$; $\chi^2 = 7.4$; $df = 1$; $P < 0.007$), 1-year follow-up ($N = 1749$; $\chi^2 = 3.1$; $df = 1$; $P < 0.08$) and 2-year follow-up ($N = 1236$; $\chi^2 = 6.7$; $df = 1$; $P < 0.01$), such that students without a previous history of smoking learned more than students with a history of smoking. However, both smokers and nonsmokers in the attention control condition learned more than students in any of the other conditions.

Unexpectedly, tobacco and health knowledge also increased in the social resistance program in San Diego at the immediate post-test ($N = 1978$; $\chi^2 = 30.5$; $df = 1$; $P < 0.00001$). However, this effect was not statistically significant at the 1- and 2-year follow-ups. At the immediate post-test only, there also was a significant interaction between treatment condition and gender ($N = 1978$; $\chi^2 = 48.4$; $df = 1$; $P < 0.00001$), such that females gained more knowledge in the treatment group than males.

[10] The numbers of cases presented in the text are often different from those shown in the tables. Numbers in the text are the numbers of cases available for the particular statistical test being reported, usually only those subjects with data at both of the waves included in the particular analysis.

TABLE 1
Means and Standard Deviations for Tobacco and Health Knowledge Scale

Conditions	Pretest	Immediate post-test	1-Year follow-up	2-Year follow-up
		Los Angeles		
Information-based control				
Mean	2.12	4.03	3.76	3.97
Standard	1.31	1.72	1.64	1.63
N	613	626	525	425
Control group				
Mean	2.09	2.21	2.57	2.76
Standard	1.18	1.32	1.52	1.49
N	512	536	424	311
Social resistance				
Mean	2.10	2.79	2.87	3.10
Standard	1.30	1.46	1.52	1.54
N	460	499	421	360
TV				
Mean	2.06	2.40	2.58	2.91
Standard	1.26	1.42	1.42	1.45
N	535	530	486	313
TV + social resistance				
Mean	1.94	2.69	2.80	3.07
Standard	1.28	1.31	1.47	1.59
N	457	484	393	245
		San Diego		
Control group				
Mean	2.26	2.57	3.66	3.59
Standard	1.31	1.37	1.54	1.54
N	1,484	1,192	1,149	902
Social resistance				
Mean	2.23	3.16	3.40	3.56
Standard	1.26	1.45	1.51	1.58
N	1,048	966	854	713

Social influences/resistance skills knowledge. Table 2 shows the means and standard deviations for the social influences/resistance skills knowledge scale across all waves and conditions.

There were significant differences in the social influences and resistance skills knowledge scale between conditions in Los Angeles at the immediate post-test ($N = 2236$; $\chi^2 = 71.9$; $df = 4$; $P < 0.00001$), 1-year follow-up ($N = 1848$; $\chi^2 = 32.2$; $df = 4$; $P < 0.00001$) and 2-year follow-up ($N = 1295$; $\chi^2 = 19.0$; $df = 4$; $P < 0.0008$). At the immediate post-test, the a priori comparisons indicated significant positive main effects of the social resistance ($z = 14.04$, $P < 0.00001$) and television ($z = 2.16$, $P < 0.031$) conditions, as well as a significant negative interaction between the television and social resistance conditions ($z = -4.05$, $P < 0.00006$). As can be seen from Table 2, the presence of the negative interaction indicates that the combined television and social resistance condition did not im-

TABLE 2
Means and Standard Deviations for Estimates of Social Resistance Scale

Conditions	Pretest	Immediate post-test	1-Year follow-up	2-Year follow-up
		Los Angeles		
Information-based control				
Mean	2.77	2.70	2.98	3.14
Standard	1.48	1.47	1.62	1.59
N	636	634	541	429
Control group				
Mean	2.68	2.48	2.80	3.05
Standard	1.43	1.47	1.45	1.61
N	526	549	432	319
Social resistance				
Mean	2.44	3.94	3.49	3.45
Standard	1.44	1.85	1.73	1.72
N	484	513	435	363
TV				
Mean	2.47	2.54	2.97	3.10
Standard	1.45	1.55	1.57	1.58
N	558	547	490	315
TV + social resistance				
Mean	2.44	3.50	3.29	3.44
Standard	1.41	1.78	1.64	1.74
N	487	497	398	248
		San Diego		
Control group				
Mean	2.76	2.82	3.33	3.53
Standard	1.49	1.50	1.58	1.60
N	1,514	1,205	1,160	913
Social resistance				
Mean	2.58	4.12	3.90	3.87
Standard	1.41	1.82	1.71	1.71
N	1,075	972	870	718

prove as much, relative to the control conditions, as the social resistance condition. The significant positive main effect of the social resistance condition was also present at both the 1- ($z = 6.70$, $P < 0.00001$) and 2-year ($z = 4.06$, $P < 0.00005$) follow-ups, while the negative interaction between the television and social resistance conditions remained significant only at the 1-year follow-up ($z = -2.05$, $P < .041$). Similar to the results for Los Angeles, there was increased social resistance knowledge in the social resistance condition in San Diego at the immediate post-test ($N = 2032$; $\chi^2 = 37.4$; $df = 1$; $P < 0.00001$), 1-year follow-up ($N = 1894$; $\chi^2 = 15.6$; $df = 1$; $P < 0.00008$) and 2-year follow-up ($N = 1523$; $\chi^2 = 6.7$; $df = 1$; $P < 0.01$). In Los Angeles, there was a significant interaction between the television condition and history of smoking at the 1-year follow-up ($N = 1831$; $\chi^2 = 11.4$; $df = 4$; $P = 0.025$), such that students who had smoked at least once in their lifetime learned less in the television condition than smokers and nonsmokers in the control conditions ($z = -2.70$, $P < 0.007$).

Estimates of prevalence of student smoking. Table 3 presents the means and standard deviations for the estimates of prevalence of student smoking across all waves and conditions. There were significant differences in the prevalence estimates between conditions in Los Angeles at the immediate post-test ($N = 2026$; $\chi^2 = 43.2$; $df = 4$; $P < 0.00001$) and 2-year follow-up ($N = 1079$; $\chi^2 = 13.2$; $df = 4$; $P < 0.0003$). The specific condition comparisons at the immediate post-test revealed lower prevalence estimates for the social resistance ($z = -8.83$, $P < 0.00001$) and television ($z = -2.75$, $P < 0.006$) conditions. At the 2-year follow-up, the main effect of the social resistance condition remained ($z = -3.42$, $P < 0.0007$); however, the presence of a significant interaction between the television and social resistance conditions ($z = 1.99$, $P < 0.05$) indicated that the lower prevalence estimate of the social resistance condition was increased in the television plus social resistance condition. The prevalence estimates were lower in the social resistance condition in San Diego at the immediate post-test ($N = 1857$; $\chi^2 = 29.9$; $df = 1$; $P < 0.00001$) and 1-year follow-up ($N = 1732$; $\chi^2 = 4.9$; $df = 1$; $P < 0.028$).

TABLE 3
Means and Standard Deviations for Estimates of Prevalence of Student Smoking

Conditions follow-up	Pretest	Immediate post-test	1-Year follow-up	2-Year follow-up
		Los Angeles		
Information-based control				
Mean	16.03	16.54	18.66	19.68
Standard	7.48	7.07	6.65	6.41
N	584	616	499	400
Control group				
Mean	16.41	16.12	18.36	20.04
Standard	7.63	7.75	7.19	6.70
N	521	532	391	263
Social resistance				
Mean	15.37	11.29	16.92	18.35
Standard	7.61	6.11	6.64	6.96
N	443	504	403	321
TV				
Mean	16.42	15.24	17.90	20.30
Standard	7.46	7.58	7.18	6.44
N	497	537	450	287
TV + social resistance				
Mean	17.11	11.96	17.77	19.39
Standard	8.11	6.80	7.10	6.85
N	438	486	381	222
		San Diego		
Control group				
Mean	17.05	17.28	18.60	18.36
Standard	6.79	6.79	6.17	6.04
N	1436	1182	1140	819
Social resistance				
Mean	17.16	12.17	17.17	18.01
Standard	7.10	6.34	6.41	6.15
N	1034	960	815	650

Estimates of prevalence of adult smoking. Table 4 presents the means and standard deviations for the estimates of prevalence of adult smoking across all waves and conditions. There were significant differences in adult prevalence estimates between conditions in Los Angeles at the immediate post-test (N = 2026; χ^2 = 29.7; df = 4; $P < 0.00001$), and the specific condition comparisons revealed lower prevalence estimates for social resistance ($z = -6.06$, $P < 0.00001$) and television ($z = -2.56$, $P < 0.011$) conditions. Similarly, the estimates were lower in the social resistance condition in San Diego at the immediate post-test (N = 1861; χ^2 = 36.2; df = 1; $P < 0.00001$), 1-year follow-up (N = 1746; χ^2 = 18.1; df = 1; $P < 0.00002$), but only marginally significant at the 2-year follow-up (N = 1310; χ^2 = 2.93; df = 1; $P < 0.09$).

Refusal/self-efficacy. Table 5 presents the means and standard deviations for the estimates of the refusal self-efficacy scale across all waves and conditions. In Los Angeles, there were no significant condition-related effects at any of the waves. In San Diego, refusal/self-efficacy was unexpectedly improved in the control condition, relative to the social resistance condition, at the immediate post-test (N = 2245; χ^2 = 8.6; df = 1; $P < 0.004$), but not at subsequent waves.

TABLE 4
Means and Standard Deviations for Adult Smoking Prevalence Estimates

Conditions	Pretest	Immediate post-test	1-Year follow-up	2-Year follow-up
		Los Angeles		
Information-based control				
Mean	6.71	6.66	6.81	6.88
Standard	3.03	2.89	2.67	2.56
N	576	604	489	392
Control group				
Mean	6.68	6.29	6.63	7.20
Standard	3.24	3.16	2.97	2.58
N	507	530	386	259
Social resistance				
Mean	6.51	4.20	6.41	6.59
Standard	3.24	2.69	2.61	2.67
N	434	493	398	315
TV				
Mean	6.59	5.92	6.77	7.21
Standard	3.12	3.07	2.89	2.53
N	485	528	447	283
TV + social resistance				
Mean	7.00	5.14	6.72	6.98
Standard	3.21	2.87	2.76	2.78
N	427	474	374	216
		San Diego		
Control group				
Mean	6.70	6.75	6.78	6.61
Standard	2.73	2.60	2.43	2.45
N	1414	1165	1134	809
Social resistance				
Mean	6.93	4.99	6.26	6.38
Standard	2.83	2.46	2.54	2.56
N	1016	954	806	644

TABLE 5
Means and Standard Deviations for Refusal/Self-Efficacy Scale

Conditions	Pretest	Immediate post-test	1-Year follow-up	2-Year follow-up
		Los Angeles		
Information-based control				
Mean	6.80	6.65	7.21	7.83
Standard	1.98	2.08	1.97	1.64
N	816	727	631	472
Control group				
Mean	6.65	6.68	7.12	7.47
Standard	2.23	2.19	2.04	1.91
N	753	638	514	356
Social resistance				
Mean	6.83	6.57	6.99	7.70
Standard	2.08	2.08	2.08	1.71
N	711	634	506	409
TV				
Mean	6.68	6.53	7.07	7.68
Standard	2.16	2.22	2.02	1.84
N	774	637	548	352
TV + social resistance				
Mean	7.00	6.43	7.07	7.65
Standard	2.14	2.22	2.05	1.71
N	680	593	467	294
		San Diego		
Control group				
Mean	6.89	7.00	7.40	7.80
Standard	1.98	1.97	1.81	1.64
N	1633	1247	1231	1038
Social resistance				
Mean	6.66	6.45	7.10	7.53
Standard	2.04	2.03	1.91	1.88
N	1224	1012	944	805

Coping effort. Figure 3a presents the results for the coping effort scale. For Los Angeles, while there was a marginally significant overall condition effect at the immediate post-test (N = 3165; χ^2 = 8.2; df = 4; $P < 0.085$) and the 1-year follow-up (N = 2591; χ^2 = 6.9; df = 4; $P < 0.14$), the specific condition comparisons indicated a consistent significant interaction between the television and social resistance conditions at both immediate post-test ($z = -2.61$, $P < 0.009$) and the 1-year follow-up ($z = -2.35$, $P < 0.019$). This interaction was a result of improved coping in the television condition but not in the television plus social resistance condition. In San Diego, there were no significant condition-related effects at any of the waves.

Disapproval of parental smoking. Figure 3b presents the results for disapproval of parental smoking.

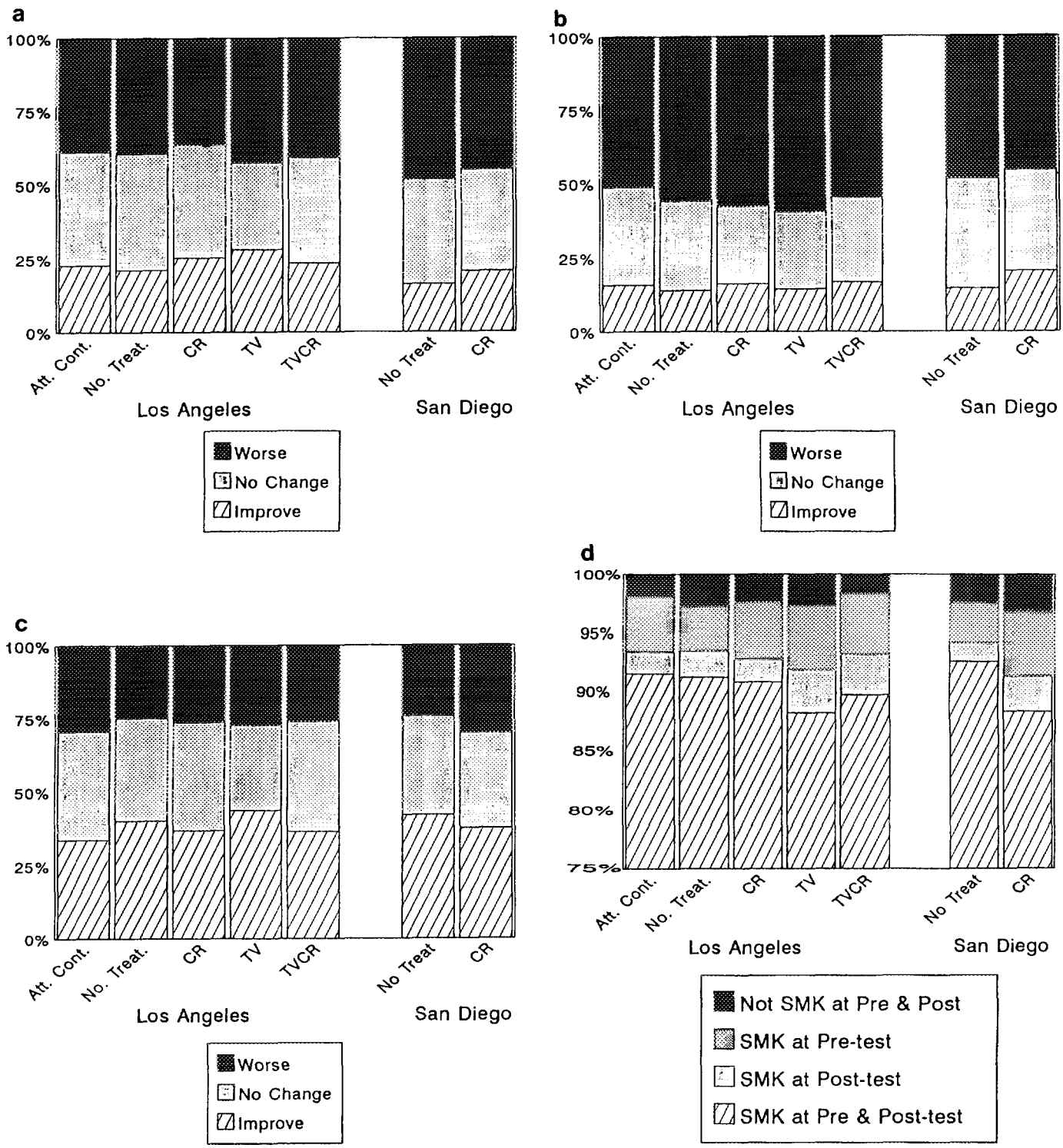

FIG. 3. Changes in categorical variables from pretest to immediate post-test by treatment condition. (a) Coping effort; (b) disapproval of parental smoking; (c) intentions to smoke; (d) current smoking behavior.

For Los Angeles, while there was a marginally significant overall condition effect at the immediate post-test ($N = 3815$; $\chi^2 = 8.9$; $df = 4$; $P < 0.064$), the specific condition comparisons indicated a significant interaction between the television and social resistance conditions at the immediate post-test ($z = -2.10$, $P < 0.036$). As Figure 3b indicates, this interaction was a result of greater improvement in the television plus

social resistance condition. In San Diego there was more positive change in the social resistance condition ($N = 2875$; $\chi^2 = 9.0$; $df = 1$; $P < 0.003$).

Outcomes

Intentions. Figure 3c presents the results for the intentions scale. There were no significant condition-related differences at any wave in Los Angeles or San Diego.

Current smoking and cessation. Figure 3d presents the results for current smoking and cessation. We used logistic regression to predict current smoking immediate post-test. For these analyses, only current smoking at the pretest and strong intentions to smoke in the future were predictors of current smoking at the post-test. No treatments were significant predictors of smoking at any of the post-tests at any waves for current smoking in Los Angeles or San Diego. There were no interactions between treatment conditions and either prior smoking or intentions, suggesting that the treatment was not more or less effective for different subgroups.

Attrition Analysis

There were no substantial pretest differences between the sample with complete data at each pair of waves (pretest with each subsequent wave, respectively) and the sample of subjects with data only at the pretest on the prevalence, adult prevalence, refusal skills, social resistance knowledge, and information-based knowledge scales. The greatest mean differences amounted to less than 0.17 on the information-based knowledge scale, 0.38 on the social resistance knowledge scale, 0.08 on refusal skills, 0.22 on adult prevalence, and 0.73 on student prevalence. For the categorical mediator variables, we considered attrition patterns on the immediate post-test since the main statistically significant findings were observed at this timepoint. Attrition rates were never greater than 11%. In addition, the pretest differences between the students with pretest data only and both pretest and post-test data were small (mean pretest differences of less than half a point for all variables).

DISCUSSION

We were successful in pilot-testing the program components (19), executing the design (22), and developing reliable measures (18). Unfortunately, the television programming was poorly executed (13) and there was significant variability in the integrity of classroom program delivery (20). Despite these partial failures, we found strong program acceptance by students, parents, and teachers (21). In addition, we found strong effects on important mediating variables (consequences and social resistance knowledge, prevalence estimates, and coping effort). The knowledge and prevalence estimates effects decayed partially but remained statistically significant up to the 2-year follow-up. The coping effort effects did not persist at follow-ups. There were no significant effects for smoking intentions or behavior, while for refusal skills/self-efficacy, the only significant result was in the opposite direction, though this was observed only at the post-test and only in the San Diego sample.

The decay of effects over time is not unusual, but it deserves further comment. It was cognitive effects (knowledge and prevalence estimates) that persisted partially for up to 2 years. Skills-related effects (coping effort) were smaller and decayed more quickly. We did not observe significant behavior-related effects, but if we had, they probably would have decayed most quickly. In general, it seems that those effects most related to behavior are most difficult both to obtain and to maintain, while those effects most distal from behavior are the easiest to obtain and to maintain for longer periods. Nonetheless, all effects appear to decay over time if not reviewed, reinforced, or used. Thus, truly effective preventive interventions will need to include a long-term commitment to repetition, reinforcement, and practice.

The lack of effects on intentions and behavior in this study might be due to the poor execution of the television programming or the variability in classroom curriculum delivery. However, the strong and fairly persistent effects on mediating variables suggest that we should also look for other reasons for the lack of behavioral effects. One possibility is the low rates of adoption of smoking in the control groups. For example, although over 40% of students had tried smoking by grade 7, only about 4.5, 9 and 10.8% reported smoking during the previous week in grades 7, 8, and 9, respectively. In addition, at pretest, over two-thirds of the students reported that they would not smoke in the future (45% definitely never and 21% probably would not). These rates (initial intentions and subsequent behavior) are more conservative than those reported in most previous studies. The resulting "floor effects" minimize the chances of finding program effects on intentions and behavior. These intentions and behavior data might also suggest that interventions designed to prevent substantial use either (a) are no longer necessary or (b) must continue until later grades. However, it is also possible that high subject attrition among those youth at higher risk may have attenuated any beneficial effects among those subjects for whom the program could have its greatest impact.

We found significant main effects of both classroom and television programming on the same set of mediating variables (knowledge about social influences/skills and prevalence estimates). Two interactions

were statistically significant in a positive direction (disapproval of parental smoking and coping effort). The observed multiple main effects and the two interactions, together with the reported improved acceptance (participation, satisfaction, and outcome expectancies) of television programming by those receiving the classroom program and vice versa (21), suggest that classroom and television programming can each reinforce, support, and improve the effects of the other. This is consistent with suggestions in the communications research (59) and community interventions (60) literatures.

There were few differences between the attention control and the no treatment control group. We did observe the expected differences in knowledge of smoking consequences and social resistance knowledge, but on no other variables. This finding demonstrates that interventions developed to control for attention and outcome expectancies do not produce unexpected positive or negative effects on other mediating variables. Future prevention studies will need to be explicit about whether they are testing their experimental interventions against attention controls, treatment as usual, or the best available treatment. Providing an attention-control intervention provides for cleaner interpretation of results. However, provision of anything less than an intervention of proven effectiveness will soon be unethical, assuming that we have fully investigated different intervention possibilities. Pressure to compare new interventions with the best available, plus the apparently decreasing levels of smoking by junior high school students, suggests that future studies also will need to plan for larger samples (to ensure statistical power) and longer follow-ups (to reduce the chances of floor effects). Still, the fact that changes in mediating variables were not followed by behavioral change suggests that we have more to learn about how prevention programs might work. Wide dissemination of current approaches may not be effective, or may induce Hawthorne or transient effects, unless a consistent picture of program mediation emerges.

REFERENCES

1. US DHHS. The Health Consequences of Smoking: 25 Years of Progress. A report of the Surgeon General, 1989; US DHHS, PHS, CDC, OSH. DHHS publication no. (CDC)89-8411.
2. Flay BR. Psychosocial approaches to smoking prevention: A review of findings. *Health Psychol* 1985; 4(5):449–488.
3. Flay BR. What we know about the social influences approach to smoking prevention? Review and recommendations. In: Bell C, Battjes R, Eds. Prevention Research: Deterring Drug Abuse among Children and Adolescents. Washington, DC: NIDA Research Monograph No. 63, 1985: 67–112.
4. Flay BR. Mass media and smoking cessation: A critical review. *Am J Public Health* 1987; 77(2):153–160.
5. Flay BR, Koepke D, Thomson SJ, Santi S, Best JA, Brown KS. Six-year follow-up of the first Waterloo smoking prevention trial. *Am J Public Health* 1989; 79(10):1371–1376.
6. Murray DM, Pirie P, Luepker RV, Pallonen, U. Five and six-year followup results from four seventh-grade smoking prevention strategies. *J Behav Med* 1989; 12:207–218.
7. Flay BR. Selling the Smokeless Society: Fifty-six Evaluated Mass Media Programs and Campaigns Worldwide. Washington, DC: American Public Health Association, 1987.
8. Flay BR. Social psychological approaches to smoking prevention: Review and recommendations. In: Ward W, Simonds S, Mullen P, Becker M, Eds. Advances in Health Promotion and Education. Greenwich, CT: JAI Press, 1987; 2:121–180.
9. Flay BR. Mass media linkages with school-based programs for drug abuse prevention. *J School Health* 1986; 56(9):402–406.
10. Worden JK, Flynn BS, Geller BM, Chen M, Shelton LG, Secker-Walker RH, Solomon DS, Solomon LJ, Couchey S, Costanza MC. Development of smoking prevention mass media program using diagnostic and formative research. *Prev Med* 1988; 17(5):510–530.
11. Flynn BS, Worden JK, Secker-Walker RH, Badger GJ, Geller BM, Costanza MC. Prevention of cigarette smoking through mass media and school programs. *Am J Public Health* 1992; 82(6):827–834.
12. Flay BR, Hansen WB, Johnson CA, Collins LM, Dent CW, Dwyer KM, Grossman L, Hockstein G, Rauch J, Sobol JL, Sobol DF, Sussman S, Ulene A. Implementation effectiveness of a social influences smoking prevention program using schools and television. *Health Educ Res* 1987; 2(4):385–400.
13. Flay BR, Brannon BR, Johnson CA, Hansen WB, Ulene AL, Whitney-Saltiel DA, Gleason LR, Sussman S, Gavin MD, Glowacz KM, Sobol DF, Spiegal DC. The television, school and family smoking cessation and prevention project. I. Theoretical basis and program development. *Prev Med* 1988; 17:585–607.
14. Schinke SP, Blythe BJ. Cognitive-behavioral prevention of children's smoking. *Child Behav Ther* 1981; 3(4):25–42.
15. Schinke SP, Gilchrist LD. Preventing substance abuse with children and adolescents. *J Consult Clin Psychol* 1985; 53:596–602.
16. Sussman S, Dent CW, Brannon BR, Glowacz K, Gleason LR, Ullery S, Hansen WB, Johnson CA, Flay BR. The television, school and family smoking prevention/cessation project. IV. Controlling for program success expectancies across experimental and control conditions. *Addict Behav* 1989; 14:601–610.
17. Thompson EL. Smoking education programs, 1960–1976. *Am J Public Health* 1978; 68:250–257.
18. Petraitis JP. The Television, School and Family Project: Reliabilities of the Student Questionnaire. University of Illinois: Prevention Research Center Technical Report 89-TVSFP-24, 1989.
19. Sussman S, Brannon BR, Flay BR, Gleason L, Senor S, Sobol DF, Hansen WB, Johnson CA. The television, school and family smoking prevention/cessation project. II. Formative evaluation of television segments by teenagers and parents—Implications for parental involvement in drug education. *Health Educ Res* 1986; 1(1):185–194.
20. Sobol DF, Rohrbach LA, Dent CW, Gleason L, Brannon BR, Johnson CA, Flay BR. The integrity of smoking prevention curriculum delivery. *Health Educ Res* 1989; 4(1):59–67.
21. Brannon BR, Dent CW, Flay BR, Smith G, Sussman S, Pentz MA, Johnson CA, Hansen WB. The television, school, and family project: V. The impact of curriculum delivery format on program acceptance. *Prev Med* 1989; 18(4):492–502.
22. Dent CW, Sussman SY, Flay BR. The use of archival data to select and assign schools in a drug prevention trial. *Eval Rev* 1993; 17:159–181.
23. Graham JW, Flay BR, Johnson CA, Hansen WB, Collins LM. Group comparability: A multiattribute utility approach to the use of random assignment with small numbers of aggregated units. *Eval Rev* 1984; 8(2):247–260.
24. Landrine H, Richardson JL, Klonoff EA, Flay B. Cultural diver-

sity in the predictors of adolescent cigarette smoking: The relative influence of peers. *J Behav Med* 1994; 17(3):331–346.

25. Graham JW, Johnson CA, Hansen WB, Flay BR, Gee K. Drug use prevention programs, gender, and ethnicity: Evaluation of three seventh grade Project SMART cohorts. *Prev Med* 1990; 19(3):305–313.
26. Hansen WB, Johnson CA, Flay BR, Graham JW, Sobel J. Affective and social influences approaches to the prevention of multiple substance abuse among seventh grade students: Results from Project SMART. *Prev Med* 1988; 17:135–154.
27. Graham JW, Flay BR, Johnson CA, Hansen WB, Grossman L, Sobel JL. Reliability of self-report measures of drug use in prevention research: Evaluation of the Project SMART questionnaire via the test-retest reliability matrix. *J Drug Educ* 1984; 14(2):175–193.
28. Sussman A., Brannon BR, Dent CW, Hansen WB, Johnson CS., Flay BR. Relations of coping effort, coping strategies, perceived stress, and cigarette smoking among adolescents. *Int J Addict* 1993; 28(7):599–612.
29. MacKinnon DP, Johnson CA, Pentz MA, Dwyer JH, Hansen WB, Flay BR, Wang EY. Mediating mechanisms in a school-based drug prevention program: First year effects of the Midwestern prevention project. *Health Psychol* 1991; 10(3):164–172.
30. Lazarus RS, Folkman S. Coping and adaptation. In: Gentry WD, Ed. Handbook of Behavioral Medicine. New York: Guilford, 1984.
31. Ajzen I, Fishbein M. The prediction of behavior from attitudinal and normative variables. *J Exper Soc Psychol* 1970; 6:466–487.
32. Chassin L, Presson CC, Sherman SJ, Corty E, Olshavsky, RW. Predicting the onset of cigarette smoking in adolescents: A longitudinal study. *J Appl Soc Psychol* 1984; 14:224–243.
33. Collins L, Sherman S, Mestel-Rauch J, Dent CW, Johnson CA, Hensen BW, Flay BR. Psychosocial predictors of young adolescent cigarette smoking: A sixteen-month, three-wave longitudinal study. *J Appl Soc Psychol* 1987; 17:554–573.
34. Conrad KM, Flay BR, Hill D. Why children start smoking cigarettes: Predictors of onset. *Br J Addict* 1992; 87:1711–1724.
35. Sherman SJ, Presson CC, Chassin L, Corty E, Olshasky R. The false consensus effect on estimates of smoking prevalence: Underlying mechanisms. *Personal Soc Psychol Bull* 1983; 9:197–207.
36. Sussman S, Dent CW, Mestel-Rauch J, Johnson CA, Hansen WB, Flay BR. Adolescent nonsmokers, triers and regular smokers' estimates of cigarette smoking prevalence: When do overestimations occur and by whom? *J Appl Soc Psychol* 1988; 18:537–551.
37. Milliken GA, Johnson DE. Analysis of messy data. Vol 1. Designed Experiments. Belmont, CA: Lifetime Learning Publications, 1984.
38. Koepke D, Flay BR. Levels of analysis. In: Braverman MT, Ed. The Evaluation of Health Promotion Programs. New directions for program evaluation, no. 44. San Francisco: Jossey–Bass, 1989.
39. McKinley SM, Stone EJ, Zucker DM. Research design and analysis issues. *Health Educ Q* 1989; 16:307–313.
40. Murray DM, Hannan PJ, Zucker DM. Analysis issues in school-based health promotion studies. *Health Educ Q* 1989; 16:315–320.
41. Dwyer JH, MacKinnon DP, Pentz MA, Flay BR, Hansen WB, Johnson CA, Wang EY. Estimating intervention effects in longitudinal studies: The Midwestern Prevention Project. *Am J Epidemiol* 1989; 130(4):713–724.
42. Laird NM, Ware JH. Random effect models for longitudinal data. *Biometrics* 1982; 40:961–971.
43. Gibbons RD, Waternaux C, Hedeker D, Davis JM. Random regression models: A comprehensive approach to the analysis of longitudinal psychiatric data. *Psychopharmacol Bull* 1988; 27:73–77.
44. Jennrich RI, Schluchter MD. Unbalanced repeated-measures models with structured covariance matrices. *Biometrics* 1986; 42:805–820.
45. Dempster AP, Rubin DB, Tsutakawa RK. Estimation in covariance components models. *J Am Statis Assoc* 1981; 76:341–353.
46. Bryk AS, Raudenbush SW. Hierarchical Linear Models: Applications and Data Analysis Methods. Newbury Park, CA: Sage Publications, 1992.
47. Goldstein H. Multilevel Models in Educational and Social Research. New York: Oxford Univ Press, 1987.
48. Bock RD. Measurement of human variation: A two stage model. In: Bock RD, Ed. Multilevel Analysis of Educational Data. San Diego: Academic Press, 1989.
49. Longford NT. A fast scoring algorithm for maximum likelihood estimation in unbalanced mixed models with nested random effects. *Biometrika* 1987; 74:817–827.
50. Hedeker D., Gibbons RD., Flay BR. Random-effects regression models for clustered data: With an example from smoking prevention research. *J Con Clin Psychol* 1994; 62(4):757–765.
51. Prosser R, Rasbash J, Goldstein H. ML3 Software for Three-Level Analysis, User's Guide for v.2. London, UK: Institute of Education, University of London, 1991.
52. Wald A, Tests of statistical hypotheses concerning several parameters when the number of observations is large. *Trans Am Math Soc* 1943; 54:426–482.
53. Rogosa D. Myths about longitudinal research. In: Schaie KW, Campbell RT, Meredith W, Rawlings SC, Eds. Methodological Issues in Aging Research. New York: Springer, 1988.
54. Jansen J. On the statistical analysis of ordinal data when extravariation is present. *Appl Stat* 1990; 39:75–84.
55. Hedeker D, Gibbons RD. A random-effects ordinal regression model for multilevel analysis. *Biometrics*, in press.
56. Hedeker D. MIXOR: A Fortran program for mixed-effects ordinal probit and logistic regression. University of Illinois: Prevention Research Center Technical Report, 1993.
57. Koepke D. The Television, School and Family Project: Attrition Analyses. University of Illinois: Prevention Research Center Technical Report 89-TVSFP-25, 1989.
58. Dent CW. Using SAS Linear Models to Assess and Correct for Attrition Bias. SAS User's Group International, Proceedings of the 13th annual conference, 1988.
59. Flay BR, Burton D. Effective mass communication campaigns for public health. In: Atkin C, Wallack L, Eds. Mass Communication for Public Health. Newberry Park, CA: Sage Publications, 1990.
60. Pentz MA, Dwyer JH, MacKinnon DP, Flay BR, Hansen WB, Wang EYI, Johnson CA. A multi-community trial for primary prevention of adolescent drug abuse: Effects on drug use prevalence. *JAMA* 1989; 261(22):3259–3266.

Received December 30, 1993
Revision requested February 14, 1994
Accepted April 20, 1994