Longitudinal Effects of the Midwestern Prevention Project on Regular and Experimental Smoking in Adolescents

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The purpose of this study was to compare longitudinal smoking prevention program effects estimated on a population-based cohort sample of sixth- and seventh-grade students (average n per year = 4,664) using different schools as units of analysis (middle/junior high school as the school of origin or high school as the endpoint school of intervention). Fifty schools in 15 school districts were demographically matched and assigned to either a school and community-based program for prevention of cigarette, alcohol, and marijuana use, or a health education as usual control group. Smoking was measured by questionnaires administered to the students. An expired air (CO) measure of smoking was also administered to increase accuracy of self-reports. Program effects were estimated with regression analyses, controlling for school-level socioeconomic status, racial/ethnic make-up, urbanicity, and grade. Using school of origin as the unit of analysis, program effects showed 1 year net reductions of −8, −6, and −5% in prevalence rates of smoking in the last month, last week, and last 24 hr; 2-year program effects showed similar net reductions of −6, −5, and −3% (P's < 0.10-0.001). Analyses with endpoint school as the unit showed slightly weaker effects at the 2-year follow-up, the year during which 64% of students had moved to a junior high or high school. The findings are discussed in terms of the potential contamination of experimental groups in longitudinal studies from school consolidations, family mobility, and feeder patterns to high schools, and the different smoking environments represented by middle, junior high, and high schools. © 1989 Academic Press, Inc.

INTRODUCTION

Almost two decades of research in school-based smoking prevention with adolescents have shown that programs focused on counteracting social influences to smoke can delay smoking onset (1–5). With few exceptions, however, smoking prevention programs have not significantly affected prevalence rates of experimental and regular smoking over the long term (2, 6, 7). There are at least two possible reasons why the effects of these programs have been limited. The first relates to research methodology. Most studies have been small-scale Phase 3 studies conducted under efficacy trial conditions, i.e., conducted by and under the

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control of the researchers (8). In these studies, a school-based program may have been too brief in duration or too limited in scope to produce long-term changes in smoking. Study samples (typically panels of students tracked over time) may have been too small—either by design or through attrition—to control for the multiple demographic characteristics expected to affect smoking behavior, including race/ethnicity, age/grade, gender, socioeconomic status, urbanicity and size of the school, and social norms for smoking in the school (9). In addition, effects may have decayed if research support and monitoring were removed after completion of the active intervention phase of the study. The second possibility is that the smoking environment for adolescents changed over time. If during the course of a study, adolescents moved from a school in which the prevention program was implemented, to a new school with no program or to a school with older students who are smoking more, the change in environment could moderate or weaken initial program effectiveness (10).

The pattern of results from these studies has led to expanded tests of the capacity of social influence programs to produce population-wide changes in adolescent smoking in large-scale Phase 4 and Phase 5 studies conducted under effectiveness trial conditions, i.e., with implementation subject to local controls or constraints (11). Several methodological improvements have been introduced into these studies to facilitate the evaluation of program effects on whole populations. One is the use of cross-sectional cohort samples as a cost-effective alternative to assessing an entire study population, or as a complement to assessing small panel samples. Another is the use of school as the unit of analysis, particularly when school has been used as the unit of experimental assignment.

Using the school as the unit of analysis is valid from both a theoretical and an analytical perspective. From a theoretical perspective, social influence programs are expected to operate by changing social normative expectations for smoking, and subsequently, social norms for smoking (1-3). For adolescents, social normative expectations and social norms are typically set by the school environment (9). At least one recent study, for example, has shown that the school is a major social environment for smoking and for acquiring drugs (12). Other studies consistently have shown that the single largest predictor of adolescent smoking is smoking by peers, most of whom are assumed to influence adolescents in and through the school (12, 13). In addition, most smoking prevention programs are conducted, or at least initiated, in the school as part of an educational curriculum. From an analytical perspective, using the school as the unit of analysis matches the level of assignment to the level of inference, and reduces Type I error rates that might be generated from large intraclass correlations (non-independence) of smoking behavior among students in the same classes or schools (14-18).

Analysis of program effects in longitudinal smoking prevention studies becomes problematic when adolescents move from the school of assignment (school of origin) to another school (16, 19-21). Kim’s research on implementing drug prevention programs based on school district feeder patterns suggests that most of student movement across time can be attributed to normal school feeder patterns from middle/junior high to high school (22). Thus, assigning schools to experimental conditions based on the largest school at the end of the feeder pattern
(endpoint school) or the school district as the unit may prevent contamination of experimental groups during the course of a longitudinal school-based study and enable analysis of program implementation at a district administrative level of inference. However, assignment based on school feeder patterns does not address the question of which school is the appropriate unit for analysis of program effects if each school that an adolescent moves to represents a different smoking environment. It also does not eliminate the potential for experimental group contamination from school consolidations, family mobility, or student busing within or across districts that may be mandated as part of a desegregation order. These contamination problems can affect evaluation of program effects in a panel sample, but can be intensified in a cross-sectional cohort sample that does not rely on the tracking of individuals to confirm experimental group assignment, or that includes individuals with varying degrees of program exposure in the same experimental group (e.g., a sample from program schools that includes students who received the program, students who were absent or left all or part of the program, and new students who entered program schools but never received the program).

Only a few studies have attempted to analyze smoking prevention program effects on smoking using school as the primary unit of analysis (7, 11, 23–25). For the most part, these studies have focused on evaluating effects on onset rates of smoking, in small panels of adolescents, using school of origin as the unit of analysis. Research has not evaluated longitudinal program effects on prevalence rates of experimental and regular smoking, in large population-based samples, using different units of analysis to represent movement of adolescents from one school to another over time.

The purpose of the present study was to evaluate the first 2-year effects of a Phase 4 adolescent smoking and drug prevention trial conducted under continuous efficacy conditions of research monitoring and support, and effectiveness conditions of local community support and implementation (26). The intervention is a comprehensive, continuous school and community program based on counteracting social influences to smoke and use other drugs. For this study, longitudinal program effects were evaluated on both experimental and regular levels of smoking in adolescents as they moved from middle or junior high to high school. Effects were evaluated on combined panel and cohort samples of adolescents from all of the schools in a large metropolitan area. School of origin and endpoint school were compared as units of analysis of program effect at baseline, 1 year, and 2 years, with analyses adjusted for school-level socioeconomic status, race/ethnicity, grade, and urbanicity. Differences in program effect from using each unit were explored as possible indicators of change in school smoking environment.

METHODS

Design

The Midwestern Prevention Project (MPP) is a longitudinal school- and community-based trial for prevention of drug use in adolescents, with a major emphasis on preventing cigarette smoking, alcohol use, and marijuana use (26).
MPP is implemented in the communities that constitute the Standard Statistical Metropolitan Areas (SMSA) of Kansas City (population base = 1,500,000) and Indianapolis (population base = 1,200,000, according to the 1980 U.S. Census (27)]. The overall research design within each SMSA is a $2 \times 2$ factorial design, varying intervention condition (prevention program or delayed program control with health education as usual) and grade of initial intervention (six or seven, depending on which represents the transition year to middle or junior high school). The middle or junior high school is the unit of assignment for initial intervention, with intervention continuing from middle/junior high through high school. In the 1984–1985 school year, the MPP was initiated in 50 public middle/junior high schools and 15 communities in the Kansas City SMSA, and in the 1987–1988 year, in 57 public and private middle/junior high schools, and 11 communities in the Indianapolis SMSA. This study focuses on the effects of intervention on smoking 2 years after intervention was initiated in Kansas City, when almost two-thirds of adolescents and 40% of schools had changed from the middle/junior high school of assignment to the next larger junior high or high school.

The MPP measurement design in Kansas City consists of annual assessments on several different samples of youth. For measuring longitudinal effects of the intervention on the entire population, two of these samples are merged: a panel consisting of all students from the 1984–1985 entering grade cohort in eight schools, who are tracked individually over time ($n = 1,607$), and a 25% sample of students from the 1984–1985 entering grade cohort in the remaining 42 schools, who are cross-sectionally and randomly sampled by classroom each year ($n = 3,771$ in 1984–1985). Program effects on the panel have been reported elsewhere (24), as have 1-year effects on all schools (26).

**Subjects**

Subjects were students in the entering sixth- or seventh-grade cohort in 1984–1985 who received parental consent for participation in annual assessments of smoking, drug use, and related behaviors. Of an average of 4,664 students or 33% of the entire entering grade cohort solicited for assessment each year (100% of students solicited in the eight panel schools plus 25% of students solicited in the remaining 42 schools), 94% received parental consent for participation. Because classroom was used as the unit of sampling and sizes of classrooms, grades, and schools differed in each year of the study, the resulting sample sizes were 5,378 at baseline, 5,119 at 1 year, and 3,875 at 2-year follow-up. At baseline, the sample was 50.9% male, 77.9% white; 19.3% black, and 2.8% other; 21.1% of the sample was in sixth grade (middle) schools and 78.9% in seventh grade (junior high) schools, with a mean age of 12.8 years (range = 12–14 years). At 2-year follow-up, the sample was 50.9% male, 77.9% white, 19.3% black, and 2.8% other; 30.4% of

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1 The higher $n$'s at baseline and 1-year follow-up were due to oversampling from 8 of the 42 cohort sample middle/junior high schools before students moved to high school. Part of another MPP study, the oversampling is included here in order to present results on all students sampled at each wave.
the sample was in eighth grade and 69.6% in ninth grade, with a mean age of 16.0 years (range = 14–18 years); 63.6% of the sample was enrolled in schools different from the schools of origin, and 43.1% (22 at 2-year follow-up) of the schools had changed through feeder patterns to high school \( (n = 17) \), closings \( (n = 4) \), or openings \( (n = 1) \).

Of the 50 public middle and junior high schools in the Kansas City SMSA in 1984–1985, 48 were assessed at baseline (1 of the 50 schools was a seventh-grade school only and thus was not measured until 1-year follow-up, and 1 school closed and fed into another school in the baseline year). At 1-year follow-up, 44 schools were assessed (4 schools had closed or consolidated, and 2 schools missed data collection due to scheduling conflicts), 42 of which merged with baseline schools for analysis of the first-year effects of the program. At 2-year follow-up, 51 schools were assessed (1 new school opened), 47 of which merged with baseline schools for analysis of the second-year effects of the program.

**Intervention**

The MPP program will expand over a 6-year period to include school programs, a parent organization program, community organization, health policy change promotion, and mass media programming. The first four components are introduced to schools and communities at the average rate of one per year, while mass media programming is used in all years. The program is based on social learning and problem behavior theories of behavior change and developmental transition \((28, 29)\), transactional and systems theories of environmental change \((30)\), and communication theories as they relate to the use of school, parents, mass media, and community organizations as channels for program delivery in whole communities \((31)\). Behavior change is promoted through direct training of adolescents in skills to resist social pressures to use drugs and to counteract peer, adult, mass media, and other environmental drug use modeling influences, and through indirect training of adults who are expected to influence adolescents—including parents, teachers, and community leaders—in communication, support, and prevention practice skills. Environmental change is promoted through community support of drug prevention activities, including awards and recognition of program participants and implementors, and through school and community policies aimed at institutionalizing drug prevention programming in schools and restricting drug use. The objectives of the MPP are to reduce the incidence and prevalence of use of ‘gateway’ drug use (tobacco, alcohol, and marijuana use) in adolescence and risk for use of other drugs later in life, and to promote social norms for non-drug use.

In Kansas City, the local name for the MPP is Project STAR (Students Taught Awareness and Resistance). From Fall 1984 through the Fall 1985 1-year follow-up assessment, STAR consisted of:

- a 10-session school program for resisting and counteracting drug use influences, and prevention practice homework activities with parents;
- 31 news clips, commercials, talk shows, press conferences, and articles covering baseline drug use and STAR goals in Kansas City, introduction of each
program component, skills demonstration, and public recognition of participating students and implementors.

From Fall 1985 through Fall 1986 2-year follow-up assessment, STAR continued with:

- a 5-session booster school program for maintaining resistance skills practice, counteracting influences anticipated in the transition to high school, and homework activities with parents;
- a minimum of three planning meetings and one educational seminar per year, a parent organization program for training parents in parent–child communication and prevention practice support skills, and organizing to change school policies about institutionalizing drug prevention curricula and restricting drug use in and around schools;
- 39 mass media events and programs.

The school and booster programs were delivered in regular health, science, or social studies classes by teachers, using modeling, role-playing, group discussion, and peer facilitation methods of instruction. Through 2-year follow-up, a total of 378 teachers were trained for school and booster program implementation by MPP health educators, Project STAR program staff, and experienced STAR teachers in an initial 3-day workshop and annual 1-day refresher workshops. The parent program was conducted in each school as a series of planning meetings chaired by the principal and attended by 2–4 parent representatives and 2 STAR program students; and an annual Parent Skills Night for all parents of STAR program students. Through 2-year follow-up, a total of 55 principals, 198 parent representatives, and 88 student representatives were trained for parent program implementation by MPP health educators and STAR program staff in annual 1-day workshops. Mass media programming was conducted as a series of news clips, commercials, and programs throughout each year. The Midwestern Prevention Project research staff, STAR program staff, and the Communications Department of Marion Laboratories provided consultation about scripts and program content, as well as a prevention overview meeting for the nine major television station managers in the Kansas City area.

Program implementation was assessed through periodic phone calls and meetings of STAR program staff with principals; observations of program sessions and content analyses of media events by an MPP project archivist; program implementation evaluations completed by teachers (self-report surveys) and parent group members (phone survey interviews); and a consensus rating of overall program implementation quality developed by three STAR program staff members. Summary implementation scores indicated that the program was implemented as planned and that implementation was unrelated to baseline rates of smoking in schools.

* Marion Laboratories provides direct support of STAR program implementation through funding and indirect support through its corporate resources for communication, production, and dissemination.
Measures

Measures included a self-report survey and a biochemical measure of smoking administered by trained MPP data collectors, and school archival records of feeder patterns of students through middle, junior high, and high schools. Assessments were conducted in Fall of each year, approximately 1.5 months prior to the initiation of programming.

The survey was modified from a prior drug prevention study [Project SMART (32, 33)] and included a battery of items measuring smoking and other drug use, demographic characteristics, and social psychological factors related to drug use. Three smoking items were used in this study as dependent variables: smoking in the last month, last week, and last day. Items were ordinally scaled, with six or eight response categories per item representing an increasing amount of smoking. Prevalence rates were derived from rescaling amount items to dichotomous categories (0 = no use, 1 = any use in the last month, last week, last day). Internal consistency of items (Cronbach α) averages 0.85; 3-week test–retest reliability of individual items (Pearson product moment correlation r) ranges from 0.41 to 0.90 (24, 32). In addition, four demographic items were used as covariates to control for differences in smoking that might be attributed to demographic differences among schools: urbanicity (coded as 1 = urban, 2 = suburban/urban, 3 = suburban, 4 = rural, using 1980 U.S. census designations for community size and density (27), with urbanicity and density/square mile r = 0.40, P < 0.001); race/ethnicity (coded as percentage white); socioeconomic status (SES, coded as percentage of fathers in professional occupations, based on 1980 U.S. census designations for occupations and on other recent research using father’s occupation as a major socioeconomic status indicator for adolescent smoking (7, 27); and grade (0 = six, 1 = seven at baseline). Three measures were used to determine experimental group status of students: primary school of origin (if more than one school was attended in 1984–1985, the school the student listed first), current school coded by data collectors, and archival records of school feeder patterns.3

Immediately prior to self-report survey administration, a sample of expired air was obtained from students seated at their desks, and the concentration of carbon monoxide (CO) was estimated, using the MiniCO indicator.4 The instrument was calibrated in each school prior to use on students, to adjust for ambient air levels of CO. Each student was instructed to inhale deeply, hold breath for 10 sec, and exhale through a straw attached to the indicator, with the subsequent CO reading immediately recorded by the data collector. Because the CO data were collected under field conditions with little control for actual length of time students held

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3 In the panel sample, for which experimental group status is determined through tracking of individual students’ records, 97.1% of students in program groups and 95.5% in control groups at 2-year follow-up could be correctly classified by self-reported school of origin alone. Self-reported participation in the program is not used to determine experimental group status in the MPP because of its questionable validity: Both 95.7% of panel sample students in program groups and 79.6% in control groups reported having received the program.

their breath, or for class interruptions of the procedure, the CO measure was used primarily to increase the accuracy of self-reported smoking rather than as an outcome measure (34).

School archival records of student feeder patterns were used to determine the percentage of students from original program schools that were expected to move to other schools. Feeder patterns were coded as 0 if a control school of origin was also the endpoint school at 2-year follow-up or if all control school students were expected to feed into the same school; 0.5 if the students from original program or original control schools were expected to feed into an endpoint school with mixed program and control students; or 1 if a program school of origin was also the endpoint school at 2-year follow-up (or if all program school students were expected to feed into the same school).

Data Analysis

An unconditional regression model was used to estimate program effects on smoking prevalence rates. The model was specified in the form of a regression estimated by least squares (23). The proportion of monthly, weekly, and daily cigarette smokers were the dependent variables. Analyses were conducted varying type of school as the unit (school of origin vs. endpoint school at 2-year follow-up). Regression models were estimated first on cross-sectional data at baseline and 1- and 2-year follow-up, and then longitudinally from baseline to 1- and 2-year follow-up. All analyses were replicated using group and grade, and then with group, grade, SES, race/ethnicity, and urbanicity. Analyses were also replicated using a conditional model that adjusted for baseline use as well as demographic variables, and on logits of the proportions, to normalize smoking prevalence rates.

RESULTS

Preliminary Analyses

Analyses were first conducted on panel and cohort sample schools separately, to test whether the schools differed in demographic or smoking characteristics and whether the main program effect was in the same direction at each wave of measurement. The two samples did not differ on any variable at baseline. The program effect was in the same direction for the two samples at each wave, although, as expected from a sample with repeated measurement on the same individuals and more reliable tracking of experimental group assignment, the size of the intervention parameter estimate was somewhat larger for panel schools. Panel and cohort sample schools were subsequently pooled for population-based analyses.

Preliminary analyses were also conducted on the 48 schools that merged or did not merge between baseline and each follow-up. With one exception, there were

There was a higher proportion of white students in the 42 schools that merged between baseline and 1 year (76.8%) compared with the 6 schools that did not merge (43.7%). The difference was due primarily to 4 of the nonmerged schools that closed or consolidated as part of a desegregation order.
no baseline differences in smoking prevalence rates or demographic characteristics between the 42 merged and 6 nonmerged schools at 1 year, or the 47 merged and 1 nonmerged schools at 2-years, controlling for grade level. Analyses of program effect were subsequently based on all schools with data for baseline and 1 year \((n = 42)\), and between baseline and 2 years \((n = 47)\).

**Equivalence of Experimental Groups**

Tests for differences between program and control schools were conducted on demographic characteristics and smoking prevalence rates of program and control schools at baseline, adjusting for grade. Results are shown in Table 1. There were no group differences on any demographic or smoking variable and no group differences in the proportion of students expected to transfer to different schools by 2-year follow-up \((63.6\%)\), or expected to be enrolled in mixed feeder schools by 2-year follow-up \((12.3\%)\). On the basis of the original panel students who were tracked individually, 3.1% had no assessment at any follow-up after baseline, 16.0% were lost by 1-year follow-up, and 21.8% were lost by 2-year follow-up. Attrition rates and smoking among students lost to follow-up did not differ among panel program and control schools, although consistent with other smoking prevention research, smokers in both groups were more likely overall to be absent for one or both follow-ups after baseline, compared with nonsmokers \((7)\).

**Program Effects on Smoking Prevalence Rates, 1 and 2 Years after Program Initiation**

The intervention parameter estimates for each variable at baseline and 1-year follow-up (cross-sectional program effect) and the differences in intervention parameter estimates between each wave (longitudinal program effect) are presented in Table 2. The estimates for baseline and 2-year follow-up are presented in Table 3. The parameter estimates are shown with both school of origin and endpoint
### Table 2
Regression Analyses of 1-Year Program Effects on Monthly, Weekly, and Daily Smoking, Varying School as the Unit of Analysis

<table>
<thead>
<tr>
<th>Smoking prevalence rate</th>
<th>School of origin (n = 42)</th>
<th>Endpoint school (n = 39)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Baseline</td>
<td>1 Year</td>
</tr>
<tr>
<td>% Smoking in last month</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cross-sectional</td>
<td></td>
<td></td>
</tr>
<tr>
<td>program effect</td>
<td>0.0005 (0.0254)</td>
<td>-0.0756 (0.0239)***</td>
</tr>
<tr>
<td>Longitudinal program effect</td>
<td>-0.0761 (0.0251)***</td>
<td></td>
</tr>
<tr>
<td>F value</td>
<td>9.16***</td>
<td></td>
</tr>
<tr>
<td>% Smoking in last week</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cross-sectional program effect</td>
<td>0.0043 (0.0185)</td>
<td>-0.0516 (0.0206)**</td>
</tr>
<tr>
<td>Longitudinal program effect</td>
<td>0.0559 (0.0213)***</td>
<td></td>
</tr>
<tr>
<td>F value</td>
<td>6.91**</td>
<td></td>
</tr>
<tr>
<td>% Smoking in last day</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cross-sectional program effect</td>
<td>0.0098 (0.0013)</td>
<td>-0.0338 (0.0163)*</td>
</tr>
<tr>
<td>Longitudinal program effect</td>
<td>-0.0436 (0.0185)*</td>
<td></td>
</tr>
<tr>
<td>F value</td>
<td>5.53*</td>
<td></td>
</tr>
</tbody>
</table>

**Note.** Last month use is based on any smoking in the last 30 days; last week use is based on any smoking in the last 7 days; last day use is based on five or more cigarettes smoked in the last week. Analyses are adjusted at the school level for grade (0 = six or 1 = seven at baseline), race (% white students), SES (% fathers in professional occupations), and urbanicity (1 = urban, 2 = urban/suburban, 3 = suburban, 4 = rural community). 

† P < 0.10; *P < 0.05; **P < 0.01; ***P < 0.005, based on one-tailed tests.

Program effects were significant for all variables at each wave and across waves using the school of origin, with the exception of the 2-year effect on daily smoking, which was marginally significant. Using the endpoint school, program effects were significant on all variables at 1 year, significant for monthly smoking, and marginally significant for weekly and daily smoking. At all waves, the race/ethnicity and SES covariates were significantly related to smoking, with primarily white schools and low SES schools showing higher prevalence rates of smoking (P's < 0.10–0.001). Grade was significantly and positively related to smoking only at baseline (P's < 0.05–0.01). Urbanicity was not related to smoking at any wave.

Figures 1 and 2 show the changes in smoking prevalence rates among baseline, 1 year, and 2 years, adjusting for all covariates. On the basis of the school of origin (Fig. 1), by 2-year follow-up, students in program schools had increased 9.1, 9.1, and 6.8% in monthly, weekly, and daily smoking, respectively; students in control schools had increased 15.4, 13.8, and 9.8%, respectively. On the basis of endpoint school analyses, from baseline to 2-year follow-up, program schools had increased 8.6, 8.8, and 6.5% in monthly, weekly, and daily smoking respectively; mixed schools had increased 8.8, 9.0, and 5.4%; and control schools had increased 15.0, 12.6, and 9.3%. Thus, using either unit of analysis, the rate of increase in control schools was approximately 1.5 times the rate of increase in program
TABLE 3
REGRESSION ANALYSES OF 2-YEAR PROGRAM EFFECTS ON MONTHLY, WEEKLY, AND DAILY SMOKING, VARYING SCHOOL AS THE UNIT OF ANALYSIS

<table>
<thead>
<tr>
<th>Smoking prevalence rate</th>
<th>Unit of analysis</th>
<th>School of origin (n = 47)</th>
<th>Endpoint school (n = 41)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Baseline</td>
<td>2 years</td>
</tr>
<tr>
<td>% Smoking in last month</td>
<td>% Smoking in last month</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cross-sectional program effect</td>
<td></td>
<td>0.0172 (0.0239)</td>
<td>-0.0450 (0.2560)*</td>
</tr>
<tr>
<td>Longitudinal program effect</td>
<td></td>
<td>-0.0622 (0.0293)*</td>
<td>4.50*</td>
</tr>
<tr>
<td>F value</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% Smoking in last week</td>
<td>% Smoking in last week</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cross-sectional program effect</td>
<td></td>
<td>0.0088 (0.0165)</td>
<td>-0.0860 (0.0227)*</td>
</tr>
<tr>
<td>Longitudinal program effect</td>
<td></td>
<td>-0.0948 (0.0486)*</td>
<td>3.80*</td>
</tr>
<tr>
<td>F value</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% Smoking in last day</td>
<td>% Smoking in last day</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cross-sectional program effect</td>
<td></td>
<td>0.0138 (0.0109)</td>
<td>-0.0167 (0.0195)</td>
</tr>
<tr>
<td>Longitudinal program effect</td>
<td></td>
<td>-0.0305 (0.0206)*</td>
<td>2.19*</td>
</tr>
<tr>
<td>F value</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note. Last month use is based on any smoking in the last 30 days; last week use is based on any smoking in the last 7 days; last day use is based on five or more cigarettes smoked in the last week. Analyses are adjusted at the school level for grade (0 = six or 1 = seven at baseline), race (% white students), SES (% fathers in professional occupations), and urbanicity (1 = urban, 2 = urban/suburban, 3 = suburban, 4 = rural community). * P < 0.10; ** P < 0.01; *** P < 0.05, based on one-tailed tests.

schools. Lower smoking in the small group of mixed schools in the endpoint analyses at each wave may be due to sampling error.

Expired air (CO) samples were analyzed at 2-year follow-up, when students were in eighth or ninth grade. Mean CO across all schools was 1.78 ppm, well below the 8 ppm suggested as a standard cut-off for measuring adolescent smoking (34). Across all schools and all levels of self-reported smoking in the last week, the Pearson product moment correlation between CO and self-report was r = 0.26 (P < 0.0001). The separate correlations for experimental groups were r = 0.30 (P < 0.0001) for program schools and r = 0.24 for control schools (P < 0.0001). There were no group differences on Z-score transformations of the correlations.

Differences in Program Effects from Units of Analysis

Because the analyses using school of origin and endpoint school represent different categorizations of the same data, a comparison of the magnitude of the program effect (intervention parameter estimate at follow-up minus the intervention parameter estimate at baseline) using each unit of analysis may be more appropriate than tests of statistical significance between the two parameter esti-

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6 Pechacek et al. (34) have reported a slightly higher correlation of $R = 0.39$ for some age groups under more controlled field conditions for CO data collection.
FIG. 1. Program effects on prevalence rates of monthly, weekly, and daily smoking in adolescents (% of smokers), adjusted for grade, socioeconomic status, race/ethnicity, and urbanicity. School of origin in 1984–1985 was used as the aggregate unit of analysis from baseline through 2-year follow-up (n = 48 middle/junior high schools at baseline, n = 42 at 1 year, and n = 47 at 2 years).

FIG. 2. Program effects on prevalence rates of monthly, weekly, and daily smoking in adolescents (% of smokers), adjusted for grade, socioeconomic status, race/ethnicity, and urbanicity. Endpoint school in 1986–1987 was used as the aggregate unit of analysis from baseline through 2-year follow-up (n = 41 junior high/high schools at baseline, n = 39 at 1 year, and n = 41 at 2 years).
effect using the school of origin analyses. At 2-year follow-up, the difference was positive, indicating a slightly larger program effect using the endpoint school.

Effects of Change in School Environment

One method of estimating the moderating effects of school environment on program effectiveness is to test whether program effects weakened substantially after students moved from one school to another. The majority of students who changed schools did so between the 1- and 2-year follow-up (only two schools changed between baseline and 1 year compared with 18 between 1 and 2 years). Thus, a weakened program effect might be suggested if the sign of the change in program effect size between 1- and 2-year follow-up (calculated as the intervention parameter estimate at 2 years minus the intervention parameter estimate at 1 year) was positive. Using either unit of analysis, the change in size of program effect was positive, but not significant ($P's > 0.20$). $T$ tests conducted on the change in program effect size yielded from each unit of analysis were also not significant, although the positive change in size of program effect for endpoint school analyses was almost twice the change in size of effect for school of origin analyses (intervention parameter estimate differences between 1 and 2 years were 0.0350 for monthly smoking, 0.0210 for weekly smoking, and 0.0140 for daily smoking compared with 0.0184, 0.0100, and 0.0090 respectively). However, because mostly older students moved on to high school, grade differences are confounded with changing schools; thus, these results should be regarded with caution.

Another method to investigate the effects of change in school environment is to compare schools that did or did not change by 2-year follow-up, within each grade cohort. The sixth-grade cohort yielded cells too small in size for valid school comparisons (only 3 sixth-grade schools had changed by 2-year follow-up, compared with 14 that stayed the same). However, the seventh-grade cohort had 17 schools that changed from a junior high to high school by 2 year follow-up and 11 junior high schools that stayed the same. Comparisons of these schools at 2-year follow-up, using endpoint school as the unit of analysis, showed that overall, schools that changed had slightly higher smoking prevalence rates, but the group differences were not significant.

Tests of Confounds

Additional analyses were conducted to eliminate the possibility that the slightly weaker program effects obtained in the endpoint school analyses were due to grade differences, mixed condition schools, or inappropriate assumptions about change or distribution in smoking prevalence rates in the subject population. To test for a grade difference effect, regression analyses were conducted on each grade cohort separately, with socioeconomic status, race/ethnicity, and urbanicity entered as covariates, program entered as the independent variable, and varying school as the unit of analysis. Although the grade stratification produced most cells with numbers of schools too low to achieve conventional levels of statistical significance ($n's$ ranging from 5 to 18 per cell), the size of the intervention parameter estimates yielded by each unit of analysis was consistent with the major
findings. To test for the confounding effects of mixed schools, analyses were also conducted on all grades, but eliminating the five middle/junior high schools that fed into three mixed high schools at 2-year follow-up. The results were the same as the major findings. Finally, to test the possibility that unit of analysis differences might be dependent on the statistical model or scaling of the smoking variables, the major analyses were replicated with a conditional rather than an unconditional model, adjusting for baseline smoking in addition to the other covariates, and with logits rather than proportions as the dependent variables. The conditional model assumes that any differences between experimental groups at baseline regress over time. Logits adjust for nonnormal distributions of smoking rates. Results using the conditional model yielded findings similar to the unconditional model, except that the size of the program effect was smaller. Results of analyses on both models, using logits, also showed a similar pattern.  

DISCUSSION

Results of the MPP from baseline through the first 2 years appear to be significantly reducing experimental and regular smoking in adolescents. The findings are robust, regardless of covariates, units, and statistical models used in analysis. The findings do suggest, however, that program effects are somewhat diluted by 2-year follow-up, using the endpoint school as the unit of analysis.

The finding of a slightly weaker program effect using the endpoint school as the unit of analysis suggests that school environment may moderate the effects of a smoking prevention program. The school environment effect does not appear to be a function of grade, mixed experimental groups, or type of analysis model used. However, an alternative explanation is that weaker program effects were due to the event of the school transition rather than the new smoking environment posed by the endpoint school. Although not significant, the decrease in the size of the program effect between 1- and 2-year follow-up, particularly for endpoint school analyses, offers some support for this hypothesis. Each assessment was administered early in the Fall semester, which, for students in a new school at 2-year follow-up, was 1-2 months after transition to that school. The relatively brief period of exposure to the new school’s smoking environment raises the question of whether the transition rather than the environment may have produced the decrease in program effect size. There is research to suggest that a school transition may represent a significant life event for adolescents that predicts smoking behavior (10). Future research might explore this possibility by comparing students who transfer to schools with similar vs higher rates of smoking than the school of origin.

Another possibility is that at 2-year follow-up, the cross-sectional cohort sampling method drew most students with mixed degrees of program exposure in each experimental condition, than that at 1-year follow-up. Other possibilities are a

7 In other MPP studies, analyses were conducted using individual as the unit, controlling for gender, grade, and race/ethnicity. Gender was not significant as a covariate; grade and race/ethnicity showed the same pattern of results as reported in the current study (23, 24).
novelty effect of the program in the first year, random variability among schools, a differential response bias in reporting smoking each year, or regression of program effect toward the mean by 2-year follow-up. The use of continuous and varied programming in the MPP, the pattern of correlations between self-reported smoking and CO levels, and the consistency of program effects with both conditional and unconditional models of analysis mitigate these alternative interpretations of program effect changes and school as unit differences. Regardless, the design of the MPP requires continued annual refinement and monitoring of the intervention to identify, explain, and reverse any program effect decay, and to maintain program and research integrity (26).

Alternative explanations of the program effect on smoking should be considered in terms of whether the internal or external validity of the study was jeopardized. Major threats to the internal validity of this type of prevention include (a) baseline differences among program and control schools that could be hypothesized to influence smoking; (b) the potential for a measurement × treatment interaction in the direction of program school students underreporting cigarette use relative to control school students; (c) differential participation rates and differential participation rates by smoking status; (d) whether the program was implemented as intended; and (e) whether program or control groups were contaminated by other prevention programs (15, 17). Addressing each of these threats in turn, first, there were no significant differences among experimental groups at baseline on any variable, and use of these variables as covariates in longitudinal analyses did not affect program outcome. Second, measurement × treatment interaction was minimized by conducting each testing session approximately 1.5 months before and after an intervention component, with data collectors who were blind to experimental conditions and separate from teaching and STAR program staff. Also, the lack of a significant difference between experimental groups in the size of the CO/self-report correlations suggests that if underreporting occurred, it was equivalent for both program and control groups. Third, participation rates based on students who were solicited for participation at each wave did not differ among the two groups, and while attrition cannot be reliably calculated from successive cross-sectional samples, another study conducted on the panel sample in the MPP has shown no differential attrition rates and no differential attrition rates by smoking status of students in program compared with control schools (24). Fourth, implementation ratings developed from multiple independent data sources suggest that the experimental group differences obtained were due to the program as it was designed to be implemented. Finally, our school demographic profile data collected each year as part of an archival record of schools indicated that health education was routinely addressed in all schools, in either special or regular education classes, and that the only additional prevention program operating at the time of this study (ADEPT, a drug prevention program involving parents) was limited to program and control schools in one district and did not involve direct training of students.

Threats to external validity in this study include (a) whether students who were smokers or at high risk for smoking were not included or dropped out during the course of the study; and (b) whether a prevention program implemented in a
primarily white population area is generalizable to minority groups. First, since STAR is a population-based community program involving all students in all target grades and all schools in the Kansas City SMSA, and consent rates were extremely high, it is unlikely that smokers were left out of the study. In addition, although analysis of attrition of smokers across all schools in the panel sample has indicated that more smokers drop out of the study than nonsmokers over time, the percentage of smokers who drop out is less than half of the percentage of smokers constituting the baseline prevalence rate of smoking. Thus, a substantial proportion of smokers are being retained in the study. Second, the racial/ethnic distribution in this study is representative of the Kansas City SMSA, by U.S. 1980 Census and 1984 Census Projections (27). The SMSA is and has been primarily white. However, the covariates percentage of white students in each school and urbanicity of school did not have a significant effect on smoking and did not alter the size of the program effect in analyses, suggesting that the program may have the potential to exercise an impact on white and black students and on students in urban, suburban, and rural communities. Other research on community-based smoking prevention might focus on replicating this type of program in communities that vary more widely in racial/ethnic make-up.

The results of the MPP at 2-year follow-up in Kansas City communities represent the cumulative effects of the school program, the school booster program, the parent organization program, and mass media programming. Analyses have not yet been conducted to determine the relative effects of each program component on adolescent smoking. Although the time of implementation of each component can be evaluated for effects on smoking, the independent contribution of each component may be difficult to assess, on the basis of the theoretical underpinnings of this project which assume that each component interacts with the next. Upcoming analyses of component effects will attempt to estimate the impact of relative program exposure assuming a linear, additive program exposure effect. Without these analyses, it is tempting to compare results of the MPP with effects from school programs, controlling for years of follow-up, measurement of smoking, similar program content or procedure, and student demographics. Recent programs that meet these criteria and have published data include the multiple school program components of the Minnesota Youth Heart Health Program (25) and the Know Your Body Program (35). Although the MPP appears to have produced slightly greater effects on smoking than these studies, any conclusions would be premature and misleading given that the respective programs were conducted at different times and do not represent exact replications of the school program component for comparison. Future research might focus on blocking demographically equivalent and geographically separated schools for assignment to one or more program components, with reversed order of introduction of each component, in order to evaluate the independent effects of different program components.

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