

# One-Year Outcomes of Project Towards No Drug Abuse<sup>1</sup>

Steve Sussman, Ph.D.,<sup>2</sup> Clyde W. Dent, Ph.D., Alan W. Stacy, Ph.D., and Sande Craig

*Institute for Health Promotion and Disease Prevention Research and Department of Preventive Medicine,  
University of Southern California, Los Angeles, California 90033*

**Objectives.** This paper presents the 1-year outcomes evaluation of Project Towards No Drug Abuse (Project TND), a large-scale indicated drug abuse prevention program in southern California applied to continuation high school youth, who are at high risk for drug abuse.

**Methods.** The efficacy of a nine-lesson health motivation-social skills-decision-making curriculum was evaluated in a three-condition experimental design. Twenty-one schools were randomly assigned by block to one of three conditions—standard care (control), classroom program, and classroom program plus a semester-long school-as-community component. A pretest was followed by a 3-week-long drug abuse prevention program and then a posttest at 14 continuation high schools. The 7 standard care schools received only the pretest followed by the posttest (same time duration). Subjects were followed up 1 year later.

**Results.** Changes in use of cigarettes, alcohol, marijuana, and hard drugs were assessed in a pretest-1-year follow-up time interval. The follow-up rate was 67% (analysis  $n = 1,074$ ). Indicated preventive effects were found on alcohol and hard drug use. No differences were found across the two program conditions.

**Conclusions.** Project TND is the first program to demonstrate 1-year self-reported behavioral effects on alcohol use and hard drug use among older, high-risk youth by using a school-based, limited-session model.

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**Key Words:** drug abuse prevention.

at highest risk for drug abuse. It may be more cost-effective to target youth who are most likely to abuse drugs in the future than to target much larger numbers of youth, many of whom will not abuse drugs. It is not surprising that one of the most difficult but important tasks in the minds of many drug abuse prevention researchers and practitioners is to intervene with youth at highest risk for future drug abuse [1,2]. This task of indicated prevention was the primary goal of the present study.

## *Definitions of "High Risk"*

Applicability of substance abuse prevention programming to higher risk populations is contingent on how "high risk" is defined [3]. High risk could refer to a disadvantaged socioeconomic group, risk takers, youth at risk of dropping out of school, targets of some drug promotions (e.g., of cigarettes and alcohol), or other groups [2-5]. Another widely shared perspective of risk, particularly easy to conceptualize and operationalize in terms of social influence theory, is defined as "the percentage of users within a social environment" [5]. Using schools as the unit of study, schools differing in percentage of users reflect different levels of risk. The present definition focuses on a *direct environmental* definition of risk. This perspective differs from others that tend to focus on *indirect* measures of high risk for drug abuse (e.g., socioeconomic status).

## *Continuation High Schools*

The main goal of our project, Project Towards No Drug Abuse (Project TND), was to provide classroom-based programming to youth at continuation high schools, the alternative school system in California. Continuation versus comprehensive high schools form a natural demarcation of youth who are at relatively high or low risk for substance abuse. When students in California reach high school age, those youth who are unable to remain in the regular/comprehensive school system for functional reasons, including substance use, are transferred to a continuation high school. These

## INTRODUCTION

Not all high school students use drugs, and most students who use drugs do not abuse them in adulthood [1]. Perhaps prevention programs should focus on youth

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<sup>2</sup> To whom correspondence and reprint requests should be addressed at IPR-USC, 1540 Alcazar Street, CHP 209, Los Angeles, CA 90033. Fax: (213) 342-2601. E-mail: ssussma@hsc.usc.edu.

youth are at risk for dropout [6], but they have transferred to an alternative school to fulfill a California mandate that all youth receive part-time education until they are 18 years of age (California Educational Code Section 48400; established in 1919).

Drugs are used at a much higher level at continuation than at regular high school [6–9]. As measured at the pretest assessment used in the present study (1994/1995), data show that drug use in the past month was as follows: cigarettes, 57%; alcohol, 64%; marijuana, 55%; stimulants, 21%; hallucinogens, 13%; and all other drugs, 5–8%. On the other hand, among comprehensive high schools from overlapping school districts ( $n = 1,208$ ), use in the past month of these substances was as follows: cigarettes, 24%; alcohol, 36%; marijuana, 22%; stimulants, 2%; hallucinogens, 2%, and all other drugs, 1–3% [6–10]. Continuation high schools *as such* do not cause youth to continue to use drugs. On the contrary, these specialized schools may provide the additional personal attention needed to help youth correct deficiencies in life skills, increase bonding with social institutions, and otherwise help them to build resiliency to drug abuse [4]. However, continuation high school students also are exposed on a daily basis to numerous other students who use drugs, and attitudes favorable to drug use are likely to be shaped and supported by other youth in such an environment [6].

#### *Drug Abuse Prevention among High-Risk Youth*

Social influences are major precipitants of adolescent drug use experimentation, and almost all *successful* school-based or school plus community-based drug use prevention programs have been designed to counteract social influences [11,12]. Still, drug abuse prevention researchers have not been decisive regarding whether social influence prevention program effects differ as a function of risk of target group. Some researchers have not found effects of school-based [13] or community [5] programs to vary as a function of risk, and some research suggests that social influence prevention programs may even be more effective among higher risk students [14]. The subjects in these studies were defined as high risk by definitions such as minority status [14], friend and family drug use [5], or pretest regular drug use (use in past 30 days) [5,13]. Also, the subjects in these studies were young adolescents from general populations. They were not at immediate potential for dropping out of school, and a very small percentage of the subject cohort used drugs regularly.

Other researchers have found that social influence prevention programming may be less effective for those at relatively highest risk for substance abuse [1,11,15,16]. More research has been needed with higher risk populations to better assess the potential applicability of social influences prevention programming.

Several studies have found preventive effects on drug use among relatively high-risk youth [2,5,11,17–24]. These studies share in common some focus on teaching values confrontation/clarification, general social skills, and decision-making skills. These types of lessons may help youth bond to new groups, but do not directly counteract peer pressure to use drugs. Unfortunately, most of these studies have been quasi-experimental and have involved a small sample size [2], generally have not included a detailed evaluation component [18,20,24], generally focus on middle school youth [5,12,21,24], and involve either delivery of daily lessons over a semester [2,17] or analogous intensive programming such as 2-week camp-out-type survival training [11,23] or community-wide programming [5,21,22]. Intensive, prolonged program delivery is impractical at many schools.

#### *Continuation High School Curriculum Development*

Before the present intervention study was initiated, several curriculum development studies were conducted to derive program components suitable for continuation high school students. These earlier studies are summarized in detail elsewhere [6]. Generally, pretest/posttest two-group experimental designs were used in the curriculum development studies [6].

The curriculum development studies in Project TND indicated that social influence activities are not likely to produce preventive effects in relatively high-risk youth (e.g., assertion refusal training, normative restructuring), aside from instruction in social skills that enhance bonding to others (e.g. self-control, effective listening, effective communication) and improve decision-making [6]. Motivation activities that were derived from classical direction-energy, stages of change, motivational interviewing, and recovery-oriented literatures [25–28] appeared to be more personally relevant to these youths. In particular, students preferred three motivation-type activities (attitudinal perspective taking, stereotyping, and health as a value) over other activities often used with lower risk groups [6,12]. For example, in the stereotyping activity, these students report that they believe traditional high school peers view continuation high school youth as more deviant than they really are. They learn that they can rebel against pressure to give into a self-fulfilling prophecy by not using drugs. It was considered essential to develop a curriculum that, at minimum, this high-risk population would accept. Otherwise, it was anticipated that the prevention material would be ignored.

The final Project TND curriculum consists of nine sessions and provides a health motivation–social skills–decision-making approach to drug abuse prevention [6]. The first trio of sessions motivates students to listen to subsequent prohealth programming and provides them

with effective listening skills (listening, stereotyping, and drug use myths and denial sessions). The second trio of sessions instructs students in chemical dependency issues and alternative coping skills (stages of chemical dependency, talk show on consequences of drug abuse, and stress-coping sessions). Finally, the third trio of sessions encourages making non-drug-use choices (self-control skills, taking a moderate perspective, and decision-making and commitment sessions).

### *School-as-Community Component*

The school-as-community component was derived from several theories that suggest that preventive effects can be obtained through encouraging students to engage in more healthful interconnections with others at the school and beyond its borders [29]. The school-as-community component included the implementation of weekly Associated Student Body Core Group (ASB) meetings for 6 months, implementation of at least six events per school (e.g., job training, sports participation, drug-free parties, and drug awareness week), and distribution of a continuation community newsletter across condition schools [29]. Banners were provided ("Check it out. Drug-free event.") to help schools maintain inclusion of a drug-related focus at each event. The hypothesis here was that provision of drug abuse material outside of the classroom, involving use of extraclassroom activities that make a limited movement outward to the nearby community, would enhance the effectiveness of classroom-based programming. In other words, the assumption is that utilizing more environments for drug abuse prevention efforts will extend the spatial and temporal range of anti-drug-use impact, increasing the effectiveness of programming [29]. The weekly ASB meetings involved approximately 6% of the student body of each school, and the events involved an average of 20% of the student body [29]. All schools implemented at least six events, and school personnel reports in a separate validation study confirmed that more activities were conducted in the school-as-community plus classroom condition than in the other two conditions [29].

### *The Present Study*

It is important to evaluate intervention programs that are practical (i.e., likely to be implemented) and tailored for unique populations (i.e., most likely to counteract personally relevant reasons for engaging in an unhealthy behavior) [6]. Project TND involved a three-group experimental comparison of a nine-lesson classroom program or classroom plus a school-as-community (limited community component) program against a standard care control condition, tailored for a relatively large sample of high-risk, continuation high school

youth in southern California. This paper presents the 1-year follow-up outcomes evaluation.

## METHOD

### *School Selection and Experimental Design*

A total of 29 school districts from a five-county region of southern California were recruited for participation in a procedure approximating random selection [9]. Each of those cooperating districts contained 1 continuation high school. Twenty-one continuation high schools were selected from that pool for participation in the present study by eliminating schools with atypical student enrollment size (fewer than 50 or more than 500 students). Selected schools were blocked by estimates of drug use prevalence (based on preliminary student and staff interviews at each school), ethnic composition of the school and community, student enrollment, and standardized achievement test scores (based on public data) and were randomly assigned by block to one of three experimental conditions—control (standard care,  $n = 7$ ), classroom program ( $n = 7$ ), and classroom program plus school-as-community component ( $n = 7$ ).

A school-wide pretest survey was conducted at each of the 21 schools. In the program condition schools ( $n = 14$ ), the pretest survey was followed immediately by school-wide implementation of a nine-session classroom-based drug abuse prevention program, which consisted of three 50-min sessions per week (Tuesday through Thursday) for 3 consecutive weeks. Start dates for classroom curriculum delivery (and pretest survey dates) at individual schools were disbursed throughout the 1994/1995 school year (October through May). A total of nine project staff health educators were assigned to instruct at program schools such that each one taught at an approximately equal number of classrooms and schools across the two program conditions. Health educators were all trained by the project manager. Each health educator was instructed in each session and practiced and observed each session being instructed once, before teaching the session. Approximately 2½ h of training was completed for each session.

Seven of the 14 program condition schools also conducted school-as-community component activities during the same semester as their classroom program delivery. The school-as-community component was implemented by a volunteer school staff member under project-created guidelines [29].

One month after the pretest survey was collected, a second survey (immediate posttest) was administered at all 21 schools. These data were used primarily for program implementation evaluation purposes and are not analyzed further here. Previously published summaries of those data showed implementation of both program components was successful [29,30]. Activities and lessons were delivered as planned, the students

attended approximately two-thirds of the drug abuse prevention sessions with no decreased attendance over time, staff and student process evaluations were positive, and, as expected, program content learning differed by program and control schools [29,30]. A 1-year follow-up data collection effort was completed an average 13.5 months after the pretest assessment (SD = 1.7 months) and serves as the outcome end point for the present analysis.

### Subjects

While an attempt was made to collect data from all students at the 21 study schools (and provide classroom instruction for all students in the 14 program schools), project staff were administratively provided access to 2,863 students or 75% of the students on the enrollment rosters. Lack of access was due to students who were in independent study or were completing final credits and did not participate in a classroom situation (36% of those for whom access was not achieved); students who were not taking the core classes within which Project TND programming was to be delivered (32%); availability of enough health educators and/or data collectors to serve the school (13%); timing problems such as enrollment or scheduling changes (10%); or classes that were dropped due to an excessive overlap of students from a previous class period (9%).

Pretest data were collected from approximately 70% ( $n = 2,001$ ) of those for whom access had been achieved. Reasons data were not collected from potential subjects included chronic absenteeism (no in-school contact after 3 weeks of daily attempts, 48% of those not surveyed for whom access had been achieved), those contacted but absent on survey days (including make-ups, 44%), and those who declined survey participation (parental or student decline, 8%). Of the pretested students, 1,587 (79%) provided parental consent allowing a resurvey of the students in the future (see data collection section below).

At 1-year follow-up, an attempt was made to reach consented students who had completed the pretest survey ( $n = 1,587$ ). The homes of 76% of the targeted sample were reached at the 1-year follow-up. However, 6% of the students were not available for interview after repeated attempts, and 3% of the youth or their parents refused to continue participation. Project data collectors successfully resurveyed 1,074 (67%) of the target follow-up sample, and those subjects compose the primary analysis sample. Table 1 presents the frequencies in each category of access, survey, and follow-up for the sample.

Subjects varied from 14 to 19 years of age; 93% of this sample was 16 to 18 years of age (mean age 16.7 years, SD = 0.8). The sample was 62% male, 37% white, 46% Latino, 4% Asian American, 8% African American,

**TABLE 1**

Number of Students Lost Due to Access Restrictions and Lost from Follow-up	
<b>(A) Administrative access restrictions</b>	
Total on rolls	3,813
Nonclass situation	-342
Non-core classes	-302
Project staff shortage	-124
Schedule changes	-95
Redundant classes	-87
Total access	2,863
<b>(B) Lost at baseline</b>	
Total access	2,863
No contact	-414
Absent at testing	-379
Refusal	-69
Total measured	2,001
<b>(C) Available for follow-up</b>	
Total baseline	2,001
Anonymous	-423
Total targeted	1,578
<b>(D) Lost at follow-up</b>	
Total targeted	1,578
No contact	-374
Unavailable	-89
Refusal	-41
Total measured	1,074

3% Native American, and 2% other. Only 1.2% of the sample reported a preference for a language other than English, 46% lived with both parents, and approximately 60% of the youth's parents completed high school. Modal occupations among fathers were skilled or semiskilled laborers (42%) and minor professionals or small business owners (34%). Modal occupations among mothers were minor professionals or small business owners (31%) and unskilled laborers or houseworkers (25%).

### Measures

**Drug use.** The primary type of measure of interest in this study was current drug use, at pretest and again 1 year later. To access current drug use behavior at each time point, subjects were asked "How many times in the last month have you used" each of several different drug categories. Questions were directed to frequency of use of cigarettes, alcohol, marijuana, cocaine (crack), hallucinogens (LSD, acid, mushrooms), stimulants (ice, speed, amphetamines), inhalants (rush, nitrous), and other drugs (depressants, PCP, steroids, heroin, etc.). For the purposes of analysis, the responses to the last five drug categories (cocaine through other drugs) were summed to form a hard drug use index ( $\alpha = 0.82$ ). Responses were given on 11-point rating scales, next to the drug category label. Response choices

included 0, and other response choices were provided increasing in intervals of 10 (e.g., 1–10 times, 11–20 times) with a last (11th) category being 91–100+ times. The reliability and predictive validity of this format have been previously established [9,31].

*Other pretest measures.* A total of 31 pretest measures were used to assess the retained sample generalizability (to the full pretest sample) and pretest condition comparability. These measures included the four self-reported drug use measures (cigarette, alcohol, marijuana, and hard drug use), and 12 demographic and 15 psychosocial variables. Demographic variables included age, gender, ethnicity, living situation (living with both parents or not), a four-item acculturation index (coefficient  $\alpha = 0.89$ ) [32], and a four-item socioeconomic status index (mother and father's education and occupation; coefficient  $\alpha = 0.68$ ) [9,32]. Ethnicity was binary coded as six groups: white, Latino, African American, Asian American, Native American, and others.

Psychosocial measures included friends' drug use (8-item index; coefficient  $\alpha = 0.83$ ) [28,33], prevalence estimates of use (4-item index; coefficient  $\alpha = 0.83$ ) [33], peer approval of use (3-item index; coefficient  $\alpha = 0.79$ ) [28,33], family conflict (3-item index,  $\alpha = 0.56$ ) [9,32], perceived stress (3-binary-item index;  $\alpha = 0.68$ ) [9,32], sensation seeking (11-item index;  $\alpha = 0.88$ ) [10,32], morality of drug use (2-item index;  $r = 0.59$ ) [34], violence victimization (4-item index;  $\alpha = 0.77$ ) [32], baseline program outcome expectancies (4-item index;  $\alpha = 0.84$ ) [35], assertiveness (5-item index;  $\alpha = 0.50$ ) [28], health as a value (3-item index;  $\alpha = 0.62$ ) [36], membership in a gang or tagging crew (2 separate yes or no items) [32], depression (20-item index;  $\alpha = 0.84$ ) [32], and drug use myths (17-item index;  $\alpha = 0.67$ ) [28]. Analyses were redone for items composing the three indices with  $\alpha < 0.60$ ; however, the results were identical.

### Data Collection

Prior to the pretest survey administration, all students in the accessible classes were asked to have their parents sign and return an Internal Review Board (IRB)-approved consent form providing written permission or refusal for participation in the program testing. For all students who did not return a signed form, attempts were made by project staff to contact the parent by telephone to describe the study and obtain verbal permission or refusal. Students for whom parental response could not be obtained after at least three attempts were surveyed anonymously at pretest, as approved by the local IRB and school officials, but were not targeted for long-term follow-up.

The pretest data collection involved the collection of paper-and-pencil questionnaires and breath samples

analyzed for carbon monoxide (CO) content. Data collection was conducted solely by project staff who were not responsible for instruction of that particular set of students. Students were informed that participation in any measurement was an entirely voluntary activity. Names were not placed directly on the questionnaires; instead, code numbers were used. Confidentiality of the data was ensured by maintaining restricted access to the master link list of names and identification numbers assigned to subjects at the time of testing. Anonymously surveyed students had the identification numbers erased from their questionnaires. Pretest measures were collected during single classroom sessions during regular school hours.

One-year follow-up surveys were administered in several different ways. If a targeted student was still enrolled at the continuation high school (23% of those surveyed), project staff (previously unknown to the student) went to the school and surveyed them there using a paper-and-pencil questionnaire. The majority of follow-up students (77%) was surveyed by telephone using an interview format. Project staff (previously unknown to the student) contacted the subjects by telephone, read the questionnaire items to them, and recorded their responses on a survey form. Survey items and response categories were identical to the in-school questionnaire format and subject responses generally consisted of innocuous words such as numbers, letters, agree–disagree, or true–false.

All collection efforts were stopped after 4 months of attempting to follow-up subjects from a given school, utilizing classroom, mail-out (only 3 of 377 surveys mailed to the home address were returned), and telephone tracking protocols (utilizing significant other's name and referral and telephone directory look-up information). The mean number of follow-up days was 25.8 (SD = 32.9 days). The 1-year follow-up survey was provided to 67% ( $n = 1,074$ ) of the targeted sample, which is comparable with those rates obtained with traditional school samples at 1-year follow-up, as documented in a review by Hansen et al. [37].

Telephone surveys were used because research with groups including high-risk youth has shown that a telephone approach is a successful method of reaching subjects who may not be accessible through other means [38]. Telephone surveys yield much higher rates of completion than mail surveys, and most research shows that they lead to negligible differences in responses compared with face-to-face interviews [39]. A recent, carefully controlled study on the validity of self-reports of drug use among youth found that telephone interviews yielded drug use rates comparable with those obtained through face-to-face interviews for white but not for African-American youth [40]. In fact, for cocaine use, rates obtained among whites were significantly higher and presumably more valid in the telephone

interviews than in the face-to-face interviews, whereas for African-Americans rates obtained from telephone interviews were fairly consistently biased downward. The only study we could find on the validity of drug measures in telephone interviews among Hispanic youth reported no differences in reports of tobacco use in telephone interviews compared with face-to-face interviews, in a sample of 349 teenage Latinas [41]. Although there does not appear to be any single method of assessing self-reported drug use that can be considered the most valid across all ethnic groups, the outcome evaluation in the present study examines possible interactions with ethnicity in program effects to rule out possible ethnic effects. In addition to meeting some criteria for validity, telephone interviews are far less costly than face-to-face interviews [39], and they were the only feasible means of contacting many of the youth in the present project.

## ANALYSIS AND RESULTS

### *Attrition*

The retained sample size for analysis was 1,074. The analysis sample was restricted from the full pretest measured sample by (1) failure to obtain parental response (consent) for longitudinal measurement and (2) loss of subjects to 1-year follow-up (study attrition). The equivalence of the consented pretest sample to the full sample measured at pretest has been documented elsewhere [32]. To assess the potential sample bias introduced by the combination of the two selection processes (consent and attrition), a comparison was done of the analysis sample means on pretest measures (those measured at pretest and 1-year follow-up) with those of the full measured pretest sample using a series of single sample *t* tests. There were no statistically significant value differences on any assessed variable (a total of 31 items or indices) between those subjects measured at both occasions (pretest and 1-year follow-up) and all those measured at pretest. One may conclude, therefore, that the analysis sample approximated a random subsample of pretest students; they were no less likely to use drugs nor did they differ from the full sample on any of the psychosocial correlates of drug use. This pattern of results indicated good external validity for the primary results [42]. Because there is no drug use or psychosocial correlates information on those students not measured at pretest, the extent of this generalizability is limited to a population with pretest measurement access restrictions like those experienced in this study (i.e., similar administrative access, absentee, and refusal mechanisms).

### *Condition Comparability*

Using random assignment [42], the expected values of the measurements should be equal across conditions,

and any observed differences between conditions (at pretest and/or 1-year follow-up) may be evaluated against sampling fluctuations expected under the null hypothesis (no condition effects). Also, probability statements regarding the truth of that hypothesis (i.e., *P* values) at follow-up should completely incorporate any pretest (measured or unmeasured) chance differences. However, systematic influences differentially affecting subjects across conditions, such as may be plausibly argued to occur due to lack of parent response (consent) and/or continued student participation (attrition) in reaction to the program condition requirements/experiences, may systematically bias the outcome *P* values evaluating treatment effects, essentially by undermining the random assignment and rendering the expected values (under the null hypothesis) of the condition groups nonequivalent to each other (on measured and/or unmeasured factors). One source of evidence that such a bias has not occurred is to determine the condition comparability at pretest. If one can reasonably conclude that the differences in these measures across groups show a pattern consistent with random assignment (i.e., have the same expected value; are not different from each other within sampling error), then one may infer that the internal validity of the experiment afforded by randomization is not jeopardized [42]. As a check on the pretest condition comparability, means on each of the 31 pretest measures were compared across conditions, using the retained analysis sample and a series of one-way analysis of variance tests. No statistical evidence (statistically significant differences) that would indicate that the condition groups systematically varied on any of the pretest measures beyond random error was found, indicating successful randomization to conditions and good internal validity of the experiment, despite analysis sample restrictions.

### *Assessment of Program Effects on Drug Use*

The use of pretest measurement of outcome measures and random assignment to groups suggests that an analysis of covariance model (ANCOVA), with pretest use as a covariate and condition as the grouping factor, is an appropriate procedure to test differences between groups at follow-up. This model constitutes a "trend" analysis in that differences between groups at 1-year follow-up are considered in light of the pretest means of each group, and these differences are equivalent to the expected differences between pretest and 1-year follow-up means for each condition group.

In this study, a sample of 1,074 students was randomly assigned to condition. Students were assigned as a unit (their school). This means of assignment has two statistical implications. First, the probability space (the degrees of freedom) on which outcome *P* values

should be based is limited to the number of schools assigned ( $n = 21$ ), not the number of subjects ( $n = 1,074$ ). Second, because respondents in clustered units (schools) may have similar responses (errors may be nonindependent), the estimates of sampling error may be biased downward, resulting in inflated  $P$  values and increasing the likelihood of null rejection over the stated value. The common index of this statistical non-independence is the intraclass correlation, and others have shown that self-reports of drug use in school samples exhibit intraclass correlations large enough to affect the computed  $P$  value if ignored [42]. In order to properly compute the  $P$  values associated with statistical tests of differences between groups, analytic models need to incorporate these two features of clustered random assignment [42]. The SAS data analysis package provides a procedure, PROC MIXED, that properly handles clustered data in the context of an ANCOVA. Technically, this procedure estimates parameters of mixed-model regressions, consisting of "fixed" (i.e., condition), "random" (i.e., schools), and intraclass correlation variance components. The procedure is easily parameterized to estimate and test experimental ANCOVA models, including specific condition group contrasts.

Prediction of level of current use (use in the past 30 days) of cigarette smoking, alcohol use, marijuana use, or hard drug use (this last variable consisting of the sum of five illicit drug categories) from pretest use level, condition, schools nested within condition, method of 1-year follow-up collection, and the interaction between pretest level of use and condition was completed using the PROC MIXED procedure [42]. Aside from calculating a overall condition effect ( $df = 2, 18$ ), two *a priori* orthogonal condition mean contrasts were calculated: the classroom-only program intervention condition versus the standard care control condition and the school-as-community plus classroom condition versus the classroom-only condition (each with  $df = 1, 18$ ). Mean comparisons were evaluated at  $P < 0.05$ , one-tailed, since directional hypotheses were stated (drug use at

the 1-year follow-up should be less in the school-as-community plus classroom condition than in the classroom-only condition which, in turn, should be less than the standard care control condition).

If a statistical interaction between pretest level of use and condition was found for a particular outcome measure, a second set of simple main effect analyses predicting 1-year outcomes from pretest use level (and measurement method) for each condition was completed. The slope and intercepts from these models were then used to plot the interactions between pretest use level and condition.

A tabular presentation of the outcomes data is shown in Table 2. The models for cigarettes, alcohol, and marijuana failed to indicate a significant main effect for condition. The model for hard drug use did indicate a significant main effect ( $F = 3.85, P < 0.04$ ). Those who received the intervention showed nearly half the monthly drug use frequency at follow-up compared with those in the control group. The *a priori* contrast of classroom-only condition versus control was significant ( $t = 1.78, P < 0.05$ ), whereas the contrast between the two program conditions was not ( $t = -0.08, P = 0.93$ ). The intraclass correlations estimated in these models were between 0.013 and 0.019, which agree quite well with those estimated by Murray and Hannan [43] for drug use measures using school-based subjects in this age range. The resulting underestimation in true standard errors (and proportional overestimation in  $P$  values) by not including these terms would have been between 75 and 77% for the cluster (school) sizes observed in this study [42].

Two drug use measures showed a significant interaction between pretest use level and condition: alcohol ( $F = 3.71, P = 0.02$ ) and hard drug use ( $F = 9.74, P < 0.01$ ). Separate regression lines by condition are plotted for those measures in Fig. 1 over the pretest range of 0 (i.e., nonuse) to 30 (i.e., daily) times per month. For alcohol, there appears to be no effect of the program among pretest nonusers and lower levels of use, with the regression intercept values all roughly equal (average value = 11.64, standard error of the

**TABLE 2**

One-year Outcomes of Project TND—Mean Levels of Use in the Past 30 Days by Condition

Drug type	Adjusted means at follow-up			Condition effect		Interaction effect	
	Control	Class	SAC	$F(2, 18)$	$P$	$F(2, 1,049)$	$P$
Cigarettes	30.71	34.53	33.08	0.16	0.85	0.45	0.64
Alcohol	8.61	8.15	7.16	1.11	0.34	3.71	0.02
Marijuana	11.21	12.31	13.02	0.07	0.92	0.28	0.75
Hard drugs	5.03	2.74	2.87	3.85	0.04	9.74	0.00

*Note:* Means adjusted for baseline use level, the interaction between condition and baseline level, and method of collection at follow-up; Control, standard care control; Class, classroom-only program condition; SAC, classroom plus school-as-community condition.

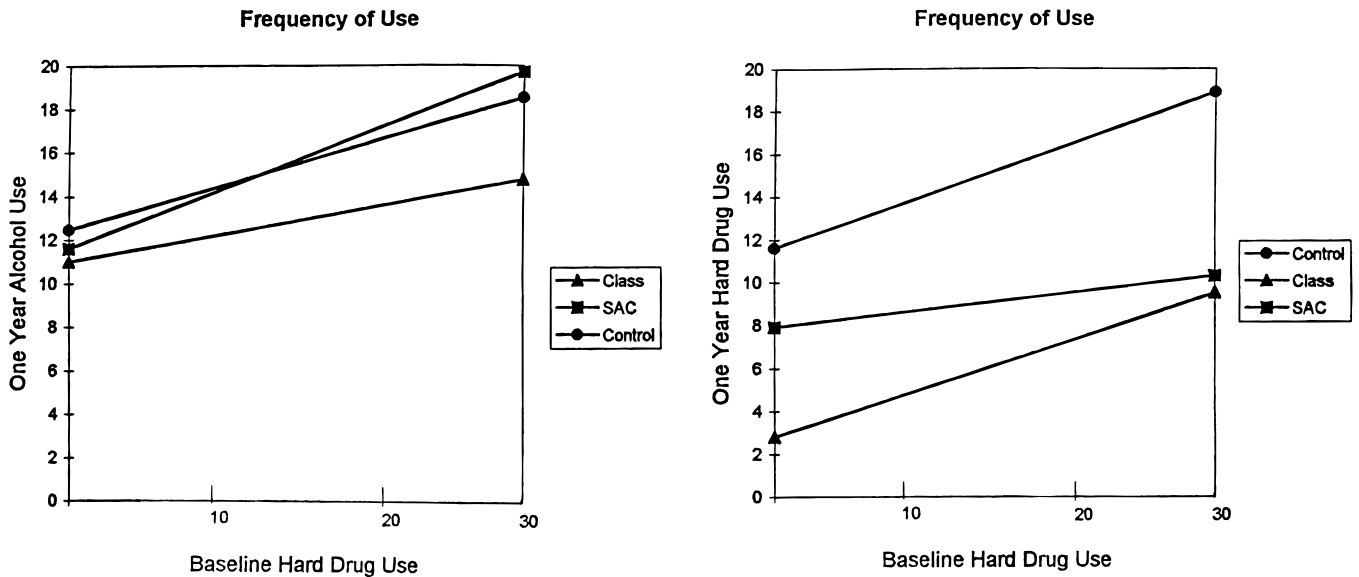


FIG. 1. Treatment condition by baseline use level interactions.

alcohol intercepts = 2.33). At higher pretest alcohol use levels, the classroom-only condition subjects did exhibit lower alcohol use at 1-year follow-up compared with the control condition subjects at comparable pretest use levels (at pretest use level=30,  $t = 7.42$ ,  $P < 0.01$ ). The contrast between the two program conditions among subjects with higher levels of pretest use is not significant in the expected direction ( $t = -2.30$ , NS, one-tailed).

For hard drug use, the pretest nonusers did show a benefit from program exposure, with control group intercept = 11.60 (SE = 2.84), the classroom-only condition intercept at 2.78 (SE = 1.15), and the school-as-community plus classroom intercept = 7.91 (SE = 2.10). At these low levels of baseline use, the contrast between classroom-only and control is significant ( $t = 4.42$ ,  $P < 0.01$ ); the contrast between the two program conditions is not significant in the expected direction ( $t = -2.33$ , NS, one-tailed). The classroom-only condition subjects maintain this advantage in lower 1-year follow-up use levels across the range of use, whereas the school-as-community plus classroom condition only gains an advantage among intermediate and higher level pretest users.

## DISCUSSION

The main goal of this project was to use an empirical program development process to create and implement effective classroom-based programming to youth at continuation high schools, the alternative school system in California whose students are at relatively high risk for drug abuse. This target group previously had not been a focus of classroom-based, theoretically derived drug abuse prevention programs or their evaluation.

Before the development of the present project, it was unclear what program would be most suitable to this population, whether these high-risk students could be tracked over time, or whether they would regularly attend a prevention program. Moreover, it was unclear if any short-term program would have beneficial effects.

Several findings from Project TND directly address these questions as well as other prevention issues. First, the prevalence of drug use among continuation high school students was much higher than among students from regular high schools, confirming the high-risk status of continuation school students. Thus, continuation high schools are a context in which prevention efforts are needed, and they serve as a natural laboratory in which high-risk youth can be studied longitudinally. Regarding program acceptability, typical social influence activities such as refusal assertion training [6] were not preferred by continuation high school students. General social skills training and decision-making components of social influences programming could be adapted to them, but the inclusion of additional program components was necessary in the present project. These components included sessions on coping skills, recovery movement ideas, and motivation activities [6,28,44].

Even though this was a high-risk population, students could be successfully followed up. It was possible to reach 76% of the students originally surveyed at pretest at least once at 1-year follow-up, and the 1-year follow-up questionnaire completion rate was 67%, which resulted in a rate of pretest–1-year follow-up completion (of those consented) that is equivalent to youth from regular schools. It was also found that those surveyed at the one-year follow-up had very similar

characteristics to the entire pretest sample. It is possible to consent, collect data, and track youth from this high-risk sample.

As reported elsewhere [30], continuation high school students attended approximately two-thirds of their drug abuse prevention lessons with no decreased attendance over time. These students showed learning of TND material relative to the standard care condition and reported high interest and belief in the material [30]. These findings reveal that continuation high school students are amenable to prevention programming, despite their high levels of drug use and problematic academic standing.

Of course, the most important concern of the present research is the effect of the program on drug use. At 1-year follow-up, the program led to a significant reduction in hard drug use and reduction in alcohol use among pretest users, but no reductions in marijuana or tobacco use. The program was based on a health motivation–social skills–decision-making model [6]. This model assumes that students need to be motivated to want to resist temptations to use drugs, taught skills that help them cope with factors that foster drug use, and provided health-behavior decision-making strategies that tie motivations to not use drugs to specific skills. The theoretical basis of this program has much in common the work of Eggert and colleagues regarding the importance of social skills and decision-making [2] and the work of Leventhal and others on motivation [26,44]. However, the content of the curriculum was refocused to apply specifically to older, high-risk youth.

It is not certain why behavioral effects were not achieved on cigarettes and marijuana. It is likely, however, that youth perceive these drugs as relatively safe in the short run compared with heavy alcohol use and hard drug use [45,46]. Marijuana use also may be considered safe in the long run, because of lack of consistent media and social–cultural messages about the negative effects of marijuana [46]. Perhaps more information on the immediate and progressive consequences of use of both of these substances is needed. Another possible explanation regarding the failure to achieve effects on cigarette use is that this is a highly addictive behavior, and many of the students smoked cigarettes daily [34]. Previous research on tobacco use prevention among adolescents has found positive results primarily for early ages and less habitual smokers [9]. Possibly, tobacco use cessation program concepts need to be incorporated into the curriculum to help these youth learn how to cope with withdrawal symptoms. Previous positive findings on marijuana use prevention also has been restricted primarily to younger age, less high-risk youth [46].

The most consistent program effects from the present project were obtained for hard drug use. Hard drug use

may be more amenable to intervention in this population because of several characteristics of this behavior. First, hard drug use (and higher levels of alcohol use) is popularly regarded as immediately dangerous, leading to overdose-related sickness or deaths. Because of potential negative immediate effects, hard drug use may be more amenable to change. Second, there probably are more numerous and consistent messages in the media against use of hard drugs, buttressing prevention efforts. Finally, hard drugs may not be as pervasive or as readily available as the other drugs studied, and hard drugs probably require more effort to obtain. Such effort undoubtedly includes more conscious or deliberate steps in the drug acquisition process, possibly allowing time for “second thoughts” or other anti-drug cognitions that could emanate from sessions taught in the TND program. Future research should try to pinpoint which of these possibilities is the most tenable.

The present project also demonstrated that a school-as-community component can be implemented successfully, because each of the target schools carried out this component as designed [29]. However, this component did not appear to add much effect to that of a classroom-only program. One should not minimize the importance of substance abuse prevention programming outside of the classroom. Nonschool prevention modalities have a rich and important history in substance abuse prevention research [5]. While the school-as-community component in the present study appeared to be successfully implemented [29], perhaps too few students participated in events to reach a “threshold” of involvement needed to produce an incremental effect above that of the classroom program. Possibly, school-as-community program efforts need to involve a larger percentage of the school community than in the present study, or more numerous community components outside the school are needed, to be effective.

Conversely, schools are the one context in which youth are a “captive audience” to provision of educational material. Also, youth spend 25% of their waking lives at school, so the impact of school on youth’s behavior should not be underestimated. Furthermore, the school context provides a rich research history on which to build new studies, and the implementation and effectiveness of programming may be most easily assessed within a school system [9]. For these reasons, continued development of substance abuse prevention programming for new school contexts should be encouraged.

Intensive curriculum development was necessary in this continuation school population to make the program acceptable to students [6]. Without at least minimal acceptability, it is doubtful that a program would have shown any effectiveness. Also, it seems clear from the present results that when curriculum development

involves both theoretical derivation and student feedback, a curriculum can be successful even in a high-risk population given a relatively brief intervention. We suggest that this practice of intensive curriculum development can be applied to research with other high-risk populations. Otherwise, interventions may have components that are ineffective or possibly even counterproductive, if they have been derived from populations with fundamental differences.

The present results probably are most generalizable to high school students enrolled in continuation or alternative schools. The present findings did not show relevant gender or ethnic effects, which might have suggested limits of generalizability. If students from other, nonproject continuation schools are enrolled for reasons similar to those of the students in the present study, there is no reason to believe that project TND would not have similar effects in such schools. The present findings also may be applicable to regular high school students who exhibit higher than average levels of drug use or low levels of academic performance. As mentioned previously, the generalizability of this study is restricted further by some selection criteria that are often inevitable in student populations. The most prominent selection bias limiting external validity is chronic absenteeism. As in other prevention programs, the results are most generalizable to students who are not chronically absent.

There also are some possible limitations regarding the internal validity of this study. Perhaps the greatest threat to internal validity involves the fact that posttests were assessed on the basis of telephone interviews, and it is possible that demand characteristics led to more underreporting in the experimental than in the control conditions. Although previous research suggests such interviews are valid [39], it is possible that some underreporting of drug use occurred in the program conditions. On the other hand, if underreporting was a serious problem in only the program conditions, then these conditions should have also shown significantly lower levels of marijuana and cigarette use compared with the control condition. Because marijuana use is frowned upon more than is alcohol use among parents and the larger society [47], it should have shown at least the same decrease in use as alcohol if phone interviews indeed led to underreporting due to the demand characteristics of this procedure. Overall, the possible invalidity of phone interviews does not provide a consistent explanation for the obtained pattern of findings.

Despite obtaining promising results, future studies in our research program will attempt to discover how effects can be obtained for marijuana and cigarette use and how they can be strengthened for alcohol use. Nonetheless, the achievement of consistently strong effects on hard drug use and effects on alcohol use among

pretest users, in a high-risk population of youth, is an important finding. Continuation school students are quite amenable to a certain type of limited-length prevention programming, and drug use behaviors can be influenced in these students.

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