

Pretest and Treatment Effects in an Elementary School-Based Alcohol Misuse Prevention Program

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Forty-nine schools ($N = 5,680$ fifth and sixth grade students) were assigned to pretest/treatment, pretest/no treatment, no pretest/treatment, and no pretest/no treatment conditions in the context of an alcohol misuse prevention study. At the first posttest, five months after the pretest and two months after the intervention, the effects of the pretest and of the intervention were examined. The analyses showed that failure to correct for the design effect due to clustering within schools resulted in the overestimation of the significance of treatment and pretest effects. After correction for the design effect, a significant treatment effect in the hypothesized direction was found with respect to students' awareness of the content of the curriculum. As hypothesized, significant treatment effects on the alcohol use and misuse measures had not yet developed but are expected to occur at subsequent posttest occasions. Significant pretest effects were found for indices measuring trouble with peers resulting from students' alcohol use, students' internal health locus of control, and their perceptions of adults as a locus of control for their health. Two of the three pretest effects were in the direction that would be hypothesized if the pretest were providing the same impetus as the intervention. Implications of these findings for school-based substance abuse prevention programs are discussed.

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INTRODUCTION

In efforts to prevent alcohol or other substance abuse by adolescents, attention has been focused increasingly on a social skills approach. This approach has as its theoretical basis the social learning theory of Bandura,¹ Festinger's cognitive dissonance model² and McGuire's communication persuasion model of attitude change.³ The approach postulates that in order to prevent the adoption of an undesirable behavior, the subject must first be "inoculated" by exposure to the negative argument or model, followed by a strong counterargument and extensive opportunity to practice the social skills involved in resisting social pressures to adopt the undesirable behavior. Application of this approach to the prevention of adolescent smoking was begun by Evans et al.^{4,5} and extended by several other investigators. A review of these studies was given by Flay.⁶ The social skills approach to prevention was applied to alcohol misuse in an early study by McAlister et al.⁷ and more recently in studies by Botvin et al.,^{8,9} Duryea,^{10,11} and Duryea et al.¹²

Only in the study conducted by Duryea^{10,11} and his colleagues¹² was the pretest condition experimentally manipulated in order to examine the main effect of pretesting and the pretest by treatment interaction on outcome data. As initially explained by Solomon,¹³ and later expanded by Campbell¹⁴ and Campbell and Stanley,¹⁵ testing for the pretest main effect is especially important in social-psychological studies in which pretesting may have an effect of a magnitude similar to the experimental treatment. For example, students taking an achievement or knowledge test for the second time usually do better. Testing for the pretest by treatment interaction is especially important when the pretest might increase subjects' sensitivity or responsiveness to the treatment and thus produce a larger effect when combined with the treatment than would be produced by the treatment or the pretest alone. Duryea,^{10,11} for example, found a significant effect of pretest on students' alcohol knowledge scores and a significant pretest by treatment interaction on students' skills in refuting pro-drinking and driving arguments. Duryea explained both of these as effects due to "practice." Students had become more familiar with the questionnaire and with the hypothetical alcohol vignettes, which were a component of the pretest, posttest, and treatment.

A main effect of pretest and a pretest by treatment interaction may also result from factors other than simple familiarity with the measurement instrument. A pretest may serve to increase students' awareness of what answers are socially approved. For example, individuals typically appear more adjusted upon retaking personality tests.¹⁵ Campbell and Stanley¹⁵ have suggested that after students take a pretest, they may discuss their answers with their friends, form hypotheses concerning the disguised nature of the test, or decide how to present themselves more "acceptably" on the second test. In a pretest that measures alcohol use and misuse, students' answers to future administrations of the same questionnaire could be influenced if students believe that the adults who administer the questionnaire disapprove of drinking among young people. These changes would be in the same direction as those expected for the treatment outcomes.

An additional consideration is the matter of reactivity.¹⁵ The mere administration of a pretest questionnaire may lead to true change in the attitude or the behavior that is being measured. For example, asking students about use of substances may act as a channel through which negative aspects of drinking are reinforced. This in turn may influence their substance use attitudes and perhaps serve as a deterrent to substance

use behavior. In addition, when the pretest questionnaire is administered and followed by a series of substance abuse prevention sessions in the classroom, there is a possibility that the students will respond differently to these sessions than they would have if they had experienced the classroom sessions without the pretest.

Instead of strengthening the effect of the treatment, pretest effects may take the opposite direction. It is possible that asking students about alcohol could stimulate their curiosity about or their interest in alcohol, resulting in subsequent increased experimentation with alcohol among those pretested. Impetus for this hypothesis stems from the fact that treatment groups have occasionally in the past exhibited higher levels of experimental alcohol use than control groups.¹⁶ In fact, concern over the possibility of encouraging alcohol and drug use by asking about it has led some investigators to exclude substance abuse behavior questions from their pretests.⁷

The purpose of the study reported in this article was to compare the treatment, pretest, and treatment by pretest interaction effects on reports of alcohol use, alcohol misuse, and measures of awareness of curriculum content, susceptibility to peer pressure, self-esteem, and health locus of control in the context of a large-scale elementary school-based, alcohol misuse prevention project. The data reported here are based on the first posttest, which occurred five months after the pretest and two months after the intervention.

METHOD

Study Design and Subjects

A total of 213 fifth and sixth grade classrooms in 49 schools from six southeastern Michigan communities with populations ranging from 10,000 to 100,000 were recruited in 1984 to participate in a longitudinal alcohol misuse prevention study. Half of the schools within each district were randomly assigned to the pretest condition and half to the no pretest condition. Within each of these two conditions about one third of the schools were assigned to either the treatment plus booster, treatment only, or control conditions. The school building, rather than the classroom or the individual student, was used as the unit of assignment to these six conditions in order to guard against any behavioral contagion effect of the prevention program. The "booster dose" of the treatment was scheduled to take place between the first and second posttests. Further details on the longitudinal design, methods, implementation, and quality control procedures of the study are presented elsewhere.¹⁷ Results of pretest to posttest one longitudinal analyses on the subset of the sample who were pretested (approximately half of the sample considered in the current paper) are also available elsewhere.¹⁷

The analyses reported in this article were based on the 4,911 students, 86.46% of those eligible when schools were assigned to conditions, who had consented to participate and who were present at the first posttest. Because the booster sessions had not occurred as of the first posttest, the data for the treatment and treatment plus booster groups were pooled in the current analyses and the study design is thus reduced to a Solomon Four-Group Design.¹³ Table 1 shows the number of schools, classrooms, and students assigned to the four conditions of interest at the first posttest: pretest/treatment, pretest/no treatment, no pretest/treatment, and no pretest/no treatment.

Table 1. Study Design and Numbers of Schools, Classrooms, and Students Assigned to Each Condition

	Pretest		No Pretest		Total
	Treatment	Control	Treatment	Control	
Schools ^a	16	9.5	16	7.5	49
Classrooms	70	38	70	35	213
Students	1,960	953	1,872	895	5,680

^aIn one school district, there were three consolidated upper elementary schools which each contained approximately 14 fifth and sixth grade classrooms. In these schools, half of the classrooms were randomly assigned to the pretest condition and half were randomly assigned to the no pretest condition.

Approximately 74% of the students who were scheduled to receive the intervention were present for all four curriculum sessions, 95% missed no more than one of the four sessions, and 99% missed no more than two of the four sessions. Because the curriculum material for each session was reviewed and reinforced in the following session and because such a high percentage of students were present for a majority of the sessions, all of the students in the treatment condition were included in the analyses of the treatment effects. Approximately 4% of the students who were assigned to the pretest condition did not actually experience the pretest (i.e., they were absent). These latter cases have been excluded from the analyses.

Intervention Procedures

The Alcohol Misuse Prevention study (AMPS) curriculum guide was developed by the study staff¹⁸ specifically for this project and was implemented in a standardized manner by specially trained project staff teachers. The standardization of the curriculum implementation was supported by analyses of posttest one data which showed that students did not differ on the dependent variables as a consequence of experiencing the curriculum with a particular teacher. Based on social learning theory, the focus of the AMPS curriculum was on teaching the acquisition of social skills to resist pressures to misuse alcohol. The curriculum goals included increasing students' awareness of the short-term effects of alcohol, the risks of alcohol misuse, and the situations or social pressures to misuse alcohol that students might encounter in the near future. Students were then introduced to skills with which to deal with such pressures. Thus, the AMPS curriculum was designed to "inoculate" the students, preparing them to cope effectively with future peer and other social pressures to misuse alcohol.

The AMPS curriculum was designed to actively involve students and to offer positive reinforcement for their efforts. The first year's curriculum was presented in four sessions, each 45 minutes long. Each session was previewed, taught, and then summarized. Previous sessions were reviewed. Audiovisual materials, student activity sheets, and handouts were designed to maintain interest. The sessions provided multiple opportunities for students to develop and practice their new skills in role-playing saying "No" to peer pressure. In the fourth session, students were videotaped refusing the offer of a drink in hypothetical situations, and then viewed the videotape of their

successful refusals. Further details concerning the curriculum sessions are available elsewhere.^{17,18}

Measurement Procedures

Eleven indices were developed and served as the dependent variables in the current analyses. These 11 indices were designed to measure awareness of curriculum content, frequency of alcohol use, misuse of alcohol (alcohol overindulgence and problems with peers and adults experienced as a result of alcohol use), susceptibility to peer pressure, self-esteem, and health locus of control. The items for the indices were generated from a review of the literature and three pilot studies. The items used in these indices are listed in Appendix A, along with the original item sources and the alpha coefficients resulting from the pretest index construction. The pretest indices were the baseline measures from which change was to be assessed, and the posttest indices were constructed in an identical fashion. Separate alpha coefficients for each of the four experimental conditions and for the total sample at posttest one were similar to or slightly larger than those for the pretest data.

An index reflecting awareness of curriculum content was created for each student by summing the number of curriculum items which were answered correctly. The possible range of scores was 0-17. A frequency of alcohol use index was created using each student's highest frequency response reported for any of three substances: beer, wine, or liquor. Responses and associated codes (in parentheses) to the individual frequency of alcohol use items, and therefore to the index, were: I didn't drink any or I've only had a taste (0); a few times a year (1); about once a month (2); about once a week (3) three or four days a week (4) and every day (5).

Three alcohol misuse indices were created by summing the alcohol misuse items which dealt with overindulgence in alcohol, alcohol use resulting in trouble with peers, and alcohol use resulting in trouble with adults. Because the frequency of alcohol misuse was low in this age group, responses to each item were recoded to none (0) and at least once in the past year (1), prior to creating the indices. These misuse indices were constructed on an a priori, face validity basis rather than from factor analytic findings. As shown in Appendix A, internal consistency (alpha) reliability coefficients for these three indices ranged from 0.21 to 0.66.

Prior to index construction for the susceptibility to peer pressure, self-esteem, and health locus of control items, the pretest data were divided into two random subsets and a factor analysis of these items was conducted for each subset. The factor analyses resulted in six factors which were highly replicable across the two solutions (congruence coefficients¹⁹ ranged from 0.86 to 0.99 for the six factors). Six indices were then constructed: susceptibility to peer pressure; family, peer and school adjustment (from the self-esteem items); and adult (external) and internal health locus of control. The possible ranges for these six indices were 7-28, 0-8, 0-5, 0-4, 0-7, and 0-6, respectively. The alpha coefficients for these indices ranged from 0.50 to 0.78 and are given in Appendix A for the individual indices. The details of these factor analytic procedures are available elsewhere.²⁰

The questionnaire was administered to classrooms of students by trained project staff. The regular classroom teachers were asked to position themselves in the classroom during the testing session so as not to inhibit or bias the students' responses.

Project staff introduced the questionnaire as a survey about health activities and attitudes rather than calling attention to its substance use aspects. This was done in order to minimize any influence on the students' responses. In addition, project staff stressed the voluntary nature of participation and assured students that their answers would remain confidential. Additional details concerning questionnaire administration and quality control procedures are available elsewhere.¹⁷

In conjunction with the pilot testing of the questionnaire, the validity of students' self-reports of alcohol use and misuse was experimentally evaluated with a variation of the "bogus pipeline" technique.^{21,22} Subjects in the pilot study were asked to provide saliva samples and were informed that a biochemical test would be used to provide an independent check of the accuracy of their self-reports of alcohol use or misuse. In fact, however, the biochemical test was bogus. The alternative to the null hypothesis was that the students who experienced such a procedure would report their use or misuse of alcohol more "accurately" because they believed that verification would occur. In the pilot study, however, the group of students who experienced the bogus pipeline procedure did not report alcohol use or misuse at significantly different rates than the control group, even at very liberal alpha levels.²³ No support for the alternative hypothesis could be claimed and the self-reports were consequently judged to be equally valid for both groups. Other independently conducted and recently published studies are in agreement with this finding.²⁴⁻²⁷ The bogus pipeline procedure was consequently not used in the main study.

Hypotheses and Analysis Procedures

Of interest were the main effect of the treatment, the main effect of the pretest, and the interaction effect of the treatment and the pretest on the 11 indices measuring students' awareness of curriculum content, alcohol use and misuse, susceptibility to peer pressure, self-esteem, and adult and internal health loci of control. In terms of treatment effects, it was hypothesized that at the first posttest occasion, the treatment group students would exhibit a higher level of curriculum awareness, internal health locus of control, and self-esteem, and a lower level of susceptibility to peer pressure than the control group students. In view of the preventive nature of the intervention, no treatment effects on the alcohol use or misuse indices were hypothesized to occur at the first posttest; rather these effects were hypothesized to occur at later posttest occasions when the prevalence of alcohol use and misuse among the control group students reached the point that differences between the treatment and control groups could be detected statistically.

A specific hypothesis was not made concerning the direction of a treatment effect for the adult health locus of control index. The adult health locus of control items concerned students' perceptions that the doctor, dentist, teacher, parent, or "other adults" were responsible for decisions regarding their health. As such, this index could be considered the obverse of the internal health locus of control index, and thus lower posttest scores might be expected among treatment group students than among control group students. On the other hand, if students held the perception that the project staff teachers were providers of valuable health information, this perception might serve to maintain the students' adult health locus of control orientation to a

greater extent than that of the control group students, in which case higher posttest scores might be expected among the treatment group students.

Commensurate with past research on pretest effects, it was hypothesized that a "practice" effect would be observed on the awareness of curriculum content index, with pretested students showing higher posttest curriculum scores (more "right" answers) on the average than nonpretested students. Also, as suggested by earlier research on personality tests, any pretest effects on the intrapersonal constructs of self-esteem, health locus of control and susceptibility to peer pressure, should be in the direction of more adjustment, i.e., pretested students should have higher scores on self-esteem and internal health locus of control indices and lower scores on the adult health locus of control and susceptibility to peer pressure indices than nonpretested students. Specific hypotheses with regard to the direction of pretest by treatment interaction effects were not made with respect to these curriculum content and intrapersonal measures.

As suggested in the introduction, the main effect of pretest on the alcohol use and misuse variables could take one of two directions. First, it could lead to higher mean scores among the pretested students than among the nonpretested students if the pretest had stimulated students' curiosity about or interest in alcohol. Or, second, it could lead to lower mean scores among the pretested students than among the nonpretested students if the pretest had either stimulated students to give socially desirable answers or had acted as an intervention in its own right and reduced or prevented alcohol use and misuse behavior. Therefore, no specific directional pretest or pretest by treatment hypotheses were made with respect to the alcohol use and misuse indices.

In the current experimental design, the unit of randomization was the school rather than the individual student. The substance use and misuse behavior of students within schools tends to be correlated and cannot be considered independent. As pointed out by McAlister et al.²⁸ and Moskowitz et al.,²⁹ if students are used as the unit of analysis when larger units are used for assignment, the statistical assumption of independence of the error term is violated. Suggested analytic strategies for compensating for this violation of assumptions have included specifying the grouping factor (e.g., school) in an hierarchical analysis of variance or using group means as the unit of analysis.³⁰ Another applicable technique is suggested by the survey sampling literature³¹ and has been used in the current study to adjust for clustering in the design.

The sampling approach employed in this analysis first assumes that the data represent a random sample of clusters, in this case schools, from a larger universe of clusters. Then, new sampling variances for the statistics of interest are calculated which take the clustering of the design into account. To the extent that students within a school resemble each other, as measured by intraclass correlations on the dependent variables, the new sampling variances will be larger than those which ignore the clustering of the design. In the sampling literature, the ratio of the sampling variance which takes into account the complexity of the design, to the sampling variance based on a simple random sample is known as the design effect. Thus, the design effect indicates the degree to which sampling variances based on simple random sampling will need to be inflated to account for the clustering of the design. In the current analyses, the new sampling variances for the effects of treatment, pretest, and the treatment by pretest interaction were computed through a jackknife estimation procedure using the Repeated Replication Sampling Error Analysis program within the OSIRIS IV software

package.³² For comparison purposes, both the results based on treatment by pretest condition 2-way analyses of variance which used the student as the unit of analysis and those based on the sampling approach were conducted.

RESULTS

The results of the 2-way analyses of variance both before and after the adjustment for the design effect due to clustering are summarized in Table 2 for each of the 11 indices. As shown in that table, prior to adjustment for the design effect, significant treatment effects in the hypothesized direction were noted for the awareness of curriculum content index ($p < 0.001$) and the frequency of alcohol use index ($p < 0.05$). A significant treatment effect was also observed on the adult health locus of control index ($p < 0.001$), for which no directional hypothesis had been made. As shown in Table 3, the significant treatment effects were due to a higher mean among the treatment group subjects ($\bar{X} = 10.07$) than among the control group subjects ($\bar{X} = 8.53$) on the awareness of curriculum content index, and a lower mean among the treatment group subjects ($\bar{X} = 0.43$) than among the control group subjects ($\bar{X} = 0.50$) on the frequency of alcohol use index. The mean for the treatment group students was also lower than the control group mean on the adult health locus of control index ($\bar{X} = 1.24$ vs. $\bar{X} = 1.42$, respectively).

Prior to adjustment for the design effect, statistically significant effects of the pretest occurred in the analyses of the overindulgence ($p < 0.05$), trouble with peers ($p < 0.001$), peer adjustment ($p < 0.05$), adult health locus of control ($p < 0.001$), and internal health locus of control ($p < 0.001$) indices. All of the significant pretest effects were in the direction hypothesized except for the adult health locus of control index, for which there was no directional hypothesis. As shown in Table 3, the significant pretest effect in the case of the overindulgence index was due to a higher mean score among the nonpretested students ($\bar{X} = 0.47$) than among the pretested students ($\bar{X} = 0.40$). The same was true of the significant effects observed on the trouble with peers and adult health locus of control indices (for trouble with peers, non-pretested $\bar{X} = 0.15$ vs. pretested $\bar{X} = 0.10$; for adult health locus of control, nonpretested $\bar{X} = 1.42$ vs. pretested $\bar{X} = 1.17$). The significant pretest effects in the case of the peer adjustment and internal health locus of control indices were due to a higher means among the pretested students than among the nonpretested students. In the case of the peer adjustment index, the mean score for pretested students was 3.25 compared to 3.16 for nonpretested students. The means for the pretested and nonpretested students on the internal health locus of control index were 5.44 and 5.29, respectively.

Prior to adjustment for the design effect, a significant pretest by treatment interaction effect was found only in the analysis of the school adjustment index ($p < 0.01$). The data in Table 3 show that this interaction resulted from the treatment group students scoring slightly higher than the control group students in the pretest condition, while the opposite was the case for the no pretest condition. This interaction was not supportive of the hypothesized treatment effect nor of the pretest effect trend observed on the other variables.

To the extent that students within schools resembled one another with respect to the dependent variables (i.e., homogeneity within schools), the unadjusted F ratios in Table 2 are overestimates of the magnitude of these effects. In the current analyses,

Table 2. Summary of Treatment by Pretest Condition Analyses of Variance of the Posttest Index Scores

Index	Source of Variation	df	MS	F	Adjusted F^a
Awareness of curriculum content	Treatment	1	2366.43	334.35 ^d	62.54 ^d
	Pretest	1	1.96	0.28	0.04
	Interaction	1	2.36	0.33	0.07
	Residual	4,620	7.08	—	—
Frequency of alcohol use	Treatment	1	3.60	5.38 ^b	1.37
	Pretest	1	0.21	0.32	0.09
	Interaction	1	1.17	1.75	0.44
	Residual	4,406	0.67	—	—
Alcohol misuse: Overindulgence	Treatment	1	1.07	1.57	0.28
	Pretest	1	4.42	6.48 ^b	1.59
	Interaction	1	0.16	0.23	0.04
	Residual	4,575	0.68	—	—
Alcohol misuse: Trouble with peers	Treatment	1	0.09	0.39	0.10
	Pretest	1	3.05	12.92 ^d	4.32 ^b
	Interaction	1	0.10	0.41	0.10
	Residual	4,576	0.24	—	—
Alcohol misuse: Trouble with adults	Treatment	1	0.06	1.32	0.71
	Pretest	1	0.06	1.39	0.68
	Interaction	1	0.12	2.63	1.44
	Residual	4,568	0.04	—	—
Susceptibility to peer pressure	Treatment	1	15.07	1.06	0.56
	Pretest	1	26.25	1.84	0.87
	Interaction	1	2.83	0.20	0.10
	Residual	4,637	14.28	—	—
Family adjustment	Treatment	1	0.54	0.22	0.11
	Pretest	1	9.01	3.64	1.99
	Interaction	1	0.24	0.10	0.05
	Residual	4,584	2.47	—	—
Peer adjustment	Treatment	1	2.83	1.56	0.71
	Pretest	1	8.65	4.76 ^b	2.57
	Interaction	1	2.17	1.19	0.52
	Residual	4,578	1.82	—	—
School adjustment	Treatment	1	2.10	1.65	0.40
	Pretest	1	2.41	1.89	0.39
	Interaction	1	11.54	9.06 ^c	2.39
	Residual	4,587	1.27	—	—
Adult health locus of control	Treatment	1	31.68	12.77 ^d	2.14
	Pretest	1	66.89	26.97 ^d	4.28 ^b
	Interaction	1	8.10	3.26	0.52
	Residual	4,567	2.48	—	—
Internal health locus of control	Treatment	1	2.44	2.41	1.10
	Pretest	1	26.32	26.00 ^d	7.81 ^c
	Interaction	1	1.96	1.94	0.85
	Residual	4,570	1.01	—	—

^a F -ratio resulting after adjustment for design clustering

^b $p < 0.05$

^c $p < 0.01$

^d $p < 0.001$

22 Table 3. Means and Standard Deviations by Treatment and Pretest Conditions on Indices Showing Significant Treatment, Pretest, or Interaction Effects

	Pretest			No pretest			Total		
	<i>n</i>	\bar{X}	SD	<i>n</i>	\bar{X}	SD	<i>n</i>	\bar{X}	SD
Awareness of curriculum content									
Treatment	1,534	10.08	2.71	1,624	10.07	2.65	3,158	10.07	2.68
Control	708	8.59	2.65	758	8.48	2.60	1,466	8.53	2.62
Total	2,242	9.61	2.77	2,382	9.56	2.74	4,624	9.58	2.75
Frequency of alcohol use									
Treatment	1,478	0.44	0.80	1,530	0.43	0.78	3,008	0.43	0.79
Control	676	0.46	0.81	726	0.52	0.93	1,402	0.50	0.87
Total	2,154	0.45	0.80	2,256	0.46	0.83	4,410	0.45	0.82
Alcohol misuse: Overindulgence									
Treatment	1,518	0.39	0.78	1,604	0.46	0.84	3,122	0.43	0.81
Control	701	0.44	0.82	756	0.48	0.89	1,457	0.46	0.86
Total	2,219	0.40	0.79	2,360	0.47	0.86	4,579	0.44	0.83
Alcohol misuse: Trouble with peers									
Treatment	1,521	0.09	0.42	1,601	0.15	0.55	3,122	0.12	0.49
Control	701	0.11	0.42	757	0.15	0.51	1,458	0.13	0.47
Total	2,222	0.10	0.42	2,358	0.15	0.54	4,580	0.12	0.49
Peer adjustment									
Treatment	1,527	3.25	1.34	1,616	3.13	1.36	3,143	3.19	1.35
Control	711	3.25	1.35	728	3.23	1.33	1,439	3.24	1.34
Total	2,238	3.25	1.34	2,344	3.16	1.35	4,582	3.21	1.35

Table 3. Means and Standard Deviations by Treatment and Pretest Conditions on Indices Showing Significant Treatment, Pretest, or Interaction Effects (*continued*)

	Pretest			No pretest			Total		
	<i>n</i>	\bar{X}	SD	<i>n</i>	\bar{X}	SD	<i>n</i>	\bar{X}	SD
School adjustment									
Treatment	1,528	3.06	1.11	1,624	2.95	1.14	3,152	3.01	1.13
Control	711	3.00	1.15	728	3.10	1.10	1,439	3.05	1.13
Total	2,239	3.04	1.13	2,352	3.00	1.13	4,591	3.02	1.13
Adult health locus of control									
Treatment	1,526	1.15	1.53	1,622	1.33	1.56	3,148	1.24	1.55
Control	697	1.23	1.56	726	1.60	1.70	1,423	1.42	1.64
Total	2,223	1.17	1.54	2,348	1.42	1.61	4,571	1.30	1.58
Internal health locus of control									
Treatment	1,526	5.41	0.98	1,622	5.29	1.08	3,148	5.35	1.03
Control	698	5.51	0.85	728	5.30	1.04	1,426	5.40	0.96
Total	2,224	5.44	0.94	2,350	5.29	1.07	4,574	5.37	1.01

the degree of similarity of students' responses within schools, as measured by intraclass correlations on the dependent variables, ranged from 0.00 to 0.12 with a median of 0.02. Although these correlations are small by conventional standards, they resulted in design effects of 1.83 to 6.30 with a median of 3.63 for the treatment, pretest, and pretest by treatment interaction effects of interest. This translated into inflation factors of 1.83 to 6.30 for the residual mean square values shown in Table 2. The new F ratios which resulted after adjustment for the degree of homogeneity within schools are shown in Table 2 in the column following the standard F ratios. After adjustment for the design effect, only the awareness of curriculum content index continued to show a statistically significant treatment effect ($p < 0.001$). Statistically significant pretest effects remained for the trouble with peers ($p < 0.05$), adult health locus of control ($p < 0.05$) and internal health locus of control ($p < 0.01$) indices. No significant pretest by treatment interaction was found subsequent to adjustment for the design effect.

DISCUSSION

The comparison of the unadjusted results to those adjusted for the design effect due to clustering in this study showed that two main effects of treatment that would have been declared significant in the unadjusted analyses (frequency of alcohol use and adult health locus of control) did not reach significance subsequent to adjustment. Two main effects of pretesting (overindulgence and peer adjustment) and one pretest by treatment interaction effect (school adjustment) were similarly only significant prior to adjustment for the design effect. These results emphasize the importance of correcting for within-cluster homogeneity in clustered designs such as those commonly employed in schools, hospitals, and other institutional settings.

After adjustment for the design effect, the analysis with respect to students' awareness of the content of the curriculum at the first posttest occasion showed a significant treatment effect in the hypothesized direction and did not show a significant pretest effect nor pretest by treatment interaction effect. Thus, it can be concluded that experiencing the prevention curriculum resulted in a significant increase in curriculum awareness, and that this increase was not due to pretesting nor an interaction of the pretest with the curriculum.

Subsequent to adjustment for the design effect, there were no significant treatment effects, pretest effects, nor treatment by pretest interaction effects on the other dependent variables with the exception of pretest effects on the trouble with peers, adult health locus of control, and internal health locus of control indices. The first and last of these pretest effects were in the direction that had been hypothesized for the effects of the treatment. Students in the pretested group reported less trouble with peers as a result of alcohol use and a higher level of internal health locus of control than students in the non-pretested group. Pretested students also reported a lower perception of adults as responsible for their health than nonpretested students, although, as discussed in the Method section, there was no prior hypothesis concerning the effect that the treatment would have on this measure.

Because two of the pretest effects that were found were in the same direction as those expected for the treatment, one is tempted to suggest that the pretest may be serving as an additional component of the intervention. Experiencing the pretest may

have served as a channel through which awareness of the negative aspects of drinking, e.g., trouble with peers, was reinforced, and thus may have served as a deterrent to behavior. It is unclear, however, what the causal mechanism would be that would link experiencing the pretest to true change in the internal and adult health locus of control constructs. In addition, if the pretest were functioning as a component of the intervention, one would expect to observe significant pretest effects on the other constructs such as the overindulgence, trouble with adults, frequency of alcohol use, susceptibility to peer pressure, and self-esteem measures. These effects, however, were not found.

Another possible explanation of the observed pretest effects is that they represent a social desirability response bias, i.e., the experience of the pretest stimulated students to give socially acceptable answers. If this trend were operating, however, one would also expect to see large pretest effects on questions regarding frequency of alcohol use, overindulgence, and trouble with adults, or on the self-esteem and susceptibility to peer pressure indices. These were the topics for which socially acceptable answers were readily apparent. Yet, after adjustment for the design effect, there were no significant effects of the pretest on any of these other measures. In addition, it seems more likely that if such a self-report bias were to occur, it would occur in response to the intervention rather than to the pretest. Treatment effects, however, did not appear for any of the alcohol use and misuse or intrapersonal measures. Thus, the pattern of results suggest the observed pretest effects were probably not due to a social desirability bias. This conclusion is reinforced by the finding of several studies which support the validity of students' self-reports of substance use behavior.^{24-27,33-35}

A more likely explanation for the observed pretest effects on the trouble with peers and internal and adult health loci of control indices is that they represent "practice" effects. The questions in the health locus of control scale tended to be more complex in terms of wording than other items in the questionnaire. In addition, as suggested by the work of Dielman et al.,³⁶ younger children differ more among themselves in their health beliefs than do older children, indicating that health beliefs may be a difficult concept for younger children to grasp. During questionnaire administration, it was also noted that young students were confused by the trouble with peers items. Having had initial exposure to the health locus of control and trouble with peers questions during the pretest, it is possible that the pretested students were able to better understand these questions at the first posttest, thus altering their responses.

There was no support for the concern that the pretest may have stimulated students' use or misuse of alcohol. This finding is useful because a pretest-posttest design has the advantage of being more efficient (i.e., having smaller standard errors) than a post-only design and allows one the opportunity to investigate preexisting differences between experimental groups which may be present even when random assignment to experimental condition has been used.¹⁵ Based on the results of this study, the continued use of pretests for all students in school-based substance abuse prevention studies seems advisable.

These findings suggest that future school-based substance abuse prevention programs should, as a first step, control for the design effect due to clustering in the analysis of program evaluation data. More specifically, these findings suggest that interventions that include the goal of changing students' health locus of control orientation or problems experienced with peers as a result of alcohol misuse should control for the effects of pretesting in their experimental design. It should be noted that

while the pretest-posttest control group design or the posttest-only control group design control for the effects of pretest and allow valid treatment vs. control comparisons, these designs do not allow one to assess the magnitude of the pretest effect. The current results indicate that changes in reported trouble with peers or in health locus of control scores are due more to pretesting than to treatment effects, suggesting that future research must include measures of the magnitude of such pretest effects. Another implication of these findings is that longitudinal studies of the development of children's health locus of control would also need to attend to the possibility of a contaminating practice effect.

Overall, in this study the differences between the pretested and nonpretested group means were quite small (from approximately 0.1 SD to 0.15 SD), and the main effects of pretest were not a source of concern with respect to students' reports of alcohol use, alcohol overindulgence, problems with adults that are experienced as a result of alcohol use, susceptibility to peer pressure, self-esteem, or curriculum awareness scores. In addition, there were no significant treatment by pretest interaction effects that would confound the interpretation of treatment effects even in such designs as a pretest-posttest control group design or posttest-only control group design. Continued research into the nature of pretest effects in school-based substance abuse prevention programs is needed, and the results of this study should be replicated in the context of both different and similar treatment conditions.

APPENDIX A

Items Forming the Eleven Indices and Their Original Sources*

Awareness of curriculum content^{Staff} (correct answer is given in parentheses)

1. Seeing adults drink alcohol makes young people want to drink too. (True)
2. Alcohol affects a person's ability to think. (True)
3. At least five of every six adults drink alcohol every week. (False)
4. If someone is healthy, alcohol does not affect that person's coordination. (False)
5. A can of beer has less alcohol than a shot of whiskey. (False)
6. Seeing beer and wine commercials pressures people to drink. (True)
7. When a man has four drinks in four hours, he is more affected by the alcohol than when he has two drinks in one hour. (False)
8. Just seeing young people drink beer or wine puts pressure on me to drink too. (True)
9. The offer of a drink of alcohol puts pressure on a person to drink. (True)
10. If you just ignore alcohol commercials you won't be influenced by them. (False)
11. Do you believe that people who drink too much alcohol only hurt themselves? (No)
12. Do you believe that young people who drink alcohol don't have to worry about getting hurt? (No)
13. Do you believe that alcohol commercials really show you what drinking will be like? (No)
14. Do you believe that alcohol commercials brainwash people to want to be like the people in the ads? (Yes)

15. Do you believe that you can say no to your friends without hurting their feelings? (Yes)
16. If your best friends want you to drink beer with them and you don't want to, do you have ways to say no? (Yes)
17. If someone offers you a drink of alcohol and you say no, can you make them take no for an answer? (Yes)

Alcohol use and misuse during the past 12 months^{37,38,Staff}

Frequency of alcohol use

1. How often have you had a "drink" of beer?
2. How often have you had a "drink" of wine?
3. How often have you had a "drink" of liquor?

Alcohol Misuse: Overindulgence (Alpha = 0.63)

1. How many times did you drink more than you planned to?
2. How many times did you feel sick to your stomach after drinking?
3. How many times did you get very drunk?

Alcohol misuse: Trouble with peers (Alpha = 0.66)

1. How many times did you get into trouble with your friends because of drinking?
2. How many times did you have a friend of the same sex complain because of your drinking?
3. How many times did you have a friend of the opposite sex complain because of your drinking?
4. How many times did you have someone you were dating complain about your drinking?

Alcohol misuse: Trouble with adults (Alpha = 0.21)

1. How many times did you get into trouble with your parents because of your drinking?
2. How many times did you get into trouble with teachers, school counselors, or the principal because of your drinking?
3. How many times did you get into trouble with the police because of your drinking?

Susceptibility to Peer Pressure^{37,39,Staff} (Alpha = 0.78)

1. If a friend offers you a drink of alcohol, would you drink it?
2. If a friend offers you a drink of alcohol, would you *want* to try it?
3. If you are at a party where your friends are drinking alcohol, would you feel left out if you are not drinking alcohol?
4. If a friend dares you to smoke a cigarette and your parents don't want you to smoke, would you smoke it?
5. If a friend dares you to tear a page out of a school library book, would you do it?
6. If your friends are going to the movies and you have to study for a test, would you go to the movies anyway?
7. If your best friend is skipping school, would you skip too?

Self-esteem⁴⁰

Family Adjustment (Alpha = 0.69)

1. Are you happy at home?
2. Are you pretty happy?

3. Do your parents understand you?
4. Do you and your parents have fun together?
5. Do you like the way you are?
6. Are you pretty sure of yourself?
7. Do you get a lot of attention at home?
8. If you have something to say, do you say it?

Peer adjustment (Alpha = 0.50)

1. Do kids pick on you?
2. Are most kids better liked than you are?
3. Do you often wish you were someone else?
4. Do you get upset easily if someone yells at you?
5. Do kids your age like you?

School adjustment (Alpha = 0.53)

1. Are you happy at school?
2. Do you like the teacher to call on you?
3. Are you proud of your school work?
4. Does your teacher make you feel bad?

Health Locus of Control⁴¹

Adult health locus of control (Alpha = 0.70)

1. Do you believe that other people must tell you what to do to stay healthy?
2. Do you believe that the teacher must tell you how to keep from having accidents at school?
3. Do you believe that other people must tell you what to do when you feel sick?
4. Do you believe that only a doctor or a nurse keeps you from getting sick?
5. Do you believe that your mother must tell you how to keep from getting sick?
6. Do you believe that you can only do what the doctor tells you to do about your health?
7. Do you believe that only the dentist can take care of your teeth?

Internal health locus of control (Alpha = 0.52)

1. Do you believe that you can do many things to fight illness?
2. Do you believe that when you are sick, you can do things to get better?
3. Do you believe that you can do things to keep from getting sick?
4. Do you believe that there are many things you can do to prevent accidents?
5. Do you believe that you can make many choices about your health?
6. Do you believe that there are things you can do to have healthy teeth?

*The reference numbers for the original sources of the items appear as superscripts after the appropriate headings. The word "Staff" refers to items that were newly created for the purpose of this study by the project staff.

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