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A RAND NOTE

Baseline Nonresponse in Project ALERT: Does It Matter?

Robert M. Bell, Cyndie Gareleck, Phyllis L. Ellickson

April 1990

RAND

The research described in this report was supported by a Grant from the Conrad N. Hilton Foundation.

Published by The RAND Corporation
1700 Main Street, P.O. Box 2138, Santa Monica, CA 90406-2138

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N-2933-CHF

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PREFACE

This Note assesses the impact of nonresponse in baseline data collection on the ability to analyze treatment effects in Project ALERT (Adolescent Experiences in Resistance Training), a multisite, multiyear test of a smoking and drug prevention program for adolescents. The project was supported by the Conrad N. Hilton Foundation.

The Note should interest researchers who wish to evaluate the quality of the Project ALERT database, as well as those who plan to undertake similar research on school-based populations. The design and implementation of Project ALERT is described in Ellickson et al. (1988). Results from the program's first 15 months appear in Ellickson and Bell (1990).

SUMMARY

This Note evaluates the impact of baseline nonresponse in Project ALERT (Adolescent Experiences in Resistance Training), a longitudinal experiment that tests the effectiveness of a school-based drug prevention program for seventh- and eighth-grade students in 20 treatment and 10 control schools in California and Oregon. Of the intended sample,16 percent failed to participate in some or all of the baseline data collection. The main reasons were: parents' refusal to allow their children to participate in any data collection, students' refusal to participate in some or all of the data collection, and absenteeism from school on survey dates. Students who missed the baseline survey will be dropped from the main analysis of treatment effects in Project ALERT.

This analysis addresses four questions:

1. How many students were lost because of baseline nonresponse?
2. How did each group of nonrespondents—parent refusals, student refusals, and absentees—differ from respondents?
3. Overall, how did nonresponse change the sample?
4. Did nonresponse introduce differences among the treatment groups?

The last two questions bear directly on the validity of the results on treatment effectiveness.

Earlier prevention studies have found that each nonresponse group includes disproportionately more students who have used substances or who were at high risk to do so. To the extent that nonrespondents differ from a random sample of the original population, the losses alter the sample available for analysis and limit the experiment's external validity. Furthermore, if nonresponse differs by treatment, it can threaten internal validity (whether an experiment provides unbiased information about treatment effects for the sample that remains).

METHODS

To evaluate the program's effectiveness, we asked students to fill out questionnaires about their drug use and related topics and to provide samples of saliva. To reduce the impact of baseline nonresponse, we chose passive informed consent, which requires a parent to return a (non)consent form only if permission is denied. We also recaptured absentees in 18 schools by using makeup data collection sessions.

We used several data sources in this analysis to compare respondents with each type of nonrespondent on various institutional, demographic, and personal characteristics. Because survey data were unavailable for students whose parents refused consent, we turned to two other data sources, student master lists and school records. The samples for these special data collections included all the nonrespondents and a stratified random sample of 50 respondents from each school.

RESULTS

Some 8.5 percent of parents denied consent, accounting for the majority of baseline nonresponse. Slightly less than 1 percent of students refused to fill out a survey or provide a saliva sample. Approximately 2 percent responded to the survey but refused to provide saliva, and 3 percent were absent. Miscellaneous other nonresponse raised the total to 16 percent of the baseline population.

Each type of nonresponse related significantly to school, but only student refusals related to district. Student refusals were also influenced by what might be termed a "bandwagon" effect: Once one or more students had refused part of the data collection, other students in the same class were likely to refuse too.

Baseline nonrespondents because of absence and student refusals included more students at high risk for future substance use. Both groups had much lower grades and many more absences than respondents did. Postbaseline survey data also showed significantly higher use of marijuana and cigarettes by both groups.

Compared to respondents, students whose parents refused for them tended to be at similar or less risk for future substance use. This finding differs dramatically from what others have found for refusals of active parent consent. Students in the parent refusal group in Project ALERT had higher average grades than respondents did, although the differences were not significant. Numbers of absences were almost identical to those for respondents. In each case, the results differed significantly from those for each of the other nonresponse groups.

Despite substantial differences in respondents from absentee and student refusal groups, baseline nonresponse altered sample characteristics only slightly. For example, we estimate that nonresponse increased the mean math grade point average by only 0.02 grade points—from 2.39 in the baseline population to 2.41 among respondents. The reason is that total nonresponse was low, and the largest nonresponse group—parent refusals—tended to resemble respondents. That is, parent refusals tended to dampen the impact of other nonresponse rather than to aggravate it.

Parents and students knew the treatment assignments before baseline, but that knowledge does not seem to have affected who responded. Although each type of nonresponse occurred most in the control condition, none of the individual differences achieved statistical significance. More important, we found no evidence that treatment affected who failed to respond. Thus, no evidence exists that baseline nonresponse altered the balance among the treatment groups.

The steps taken to reduce baseline nonresponse in Project ALERT—passive informed consent, careful explanation of data privacy, and makeup sessions for absentees—proved successful. They held the combined level of baseline nonresponse to 16 percent. More important, these steps sharply limited differences between the baseline population and baseline sample and avoided substantial differences among the treatment groups.

ACKNOWLEDGMENTS

We are grateful to the Conrad N. Hilton Foundation for its commitment to drug prevention and its support of this research. Many people contributed to the effort of collecting data for this Note. In particular, we thank Peggie Thomas, Jennifer Hawes-Dawson, the project's field coordinators in each site, and the office workers in the 30 schools. Paul Honig performed much of the data management. Allan Abrahamse provided a perceptive review that helped clarify our thinking and, we hope, the final version of the document.

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I. BASELINE NONRESPONSE IN PROJECT ALERT

INTRODUCTION

In an ideal world, all subjects selected for a research project would actually participate. Inevitably, however, some potential subjects fail to respond. This Note evaluates the impact of baseline nonresponse in Project ALERT (Adolescent Experiences in Resistance Training)—a multisite, longitudinal experiment designed to evaluate a school-based drug prevention program.[1] The program, targeted at seventh- and eighth-grade students, seeks to prevent or delay drug use onset and the transition to regular use or abuse.

Students who missed the baseline survey will be dropped from the main analysis of treatment effects in Project ALERT. That analysis requires baseline substance-use data to assign students to analysis subgroups based on exposure to the three target substances: cigarettes, alcohol, and marijuana (Ellickson and Bell, 1990). The main reasons for baseline nonresponse were: parents' refusal to allow their children to participate in any data collection, students' refusal to participate in some or all of the data collection, and absenteeism from school on survey dates.

Our analysis addresses four questions:

1. How many students failed to respond at baseline?
2. How did each group of nonrespondents—parent refusals, student refusals, and absentees—differ from respondents?
3. Overall, how did nonresponse change the sample?
4. Did nonresponse introduce differences among the treatment groups?

The last two questions bear directly on the validity of the results on treatment effectiveness. If nonresponse systematically eliminated from the experiment certain types of students (for example, those most likely to use drugs), this would alter the population to which the results applied. Similarly, if nonresponse created substantial

[1]This Note does not address problems associated with attrition after baseline or other sources of missing data (see Ellickson et al., 1988).

imbalance among the treatment groups, it could undermine the results. Using various data sources to compare baseline nonrespondents with baseline respondents, we examine these two potential threats to the experiment. The analysis also provides evidence about the impact of steps we took to reduce nonresponse.

OVERVIEW OF PROJECT ALERT

Project ALERT took place in 30 diverse schools in eight California and Oregon districts. The schools are located in rural, suburban, and urban communities with a variety of socioeconomic and racial compositions. Nine schools had minority populations of 50 percent or more. This is important because most previous evaluations have failed to include schools with substantial minority populations (Botvin and Wills, 1985).

During the 1984–1985 academic year, eight weekly lessons were taught to all seventh-grade students in 20 of the 30 schools. In 10 schools, assigned at random, an outside health educator alone presented the curriculum, denoted the Health Educator Curriculum (HEC); in the other 10 treatment schools, two local high school students assisted the health educator in four of the eight sessions, denoted the Teen Leader Curriculum (TLC). The 10 other schools, which received no special curriculum, served as the experiment's control group.

To evaluate the program's effectiveness, we asked students to fill out questionnaires about their drug use and related topics and to provide specimens of saliva. Students were told that tobacco and marijuana use can be detected in saliva and that the specimens would be tested. At the beginning of each data collection session, students received the option to refuse to participate in either or both types of data collection.

Baseline data collection was conducted the week before curriculum sessions began in a district. Program implementation occurred in two phases, each contained within either the fall or spring semester. At 12 schools in four districts, baseline data collection occurred during the first two weeks of October 1984. At the 18 schools in the other four districts, baseline data collection began near the end of February 1985. By April 1988, a total of six data collection waves were completed. Ellickson et al. (1988) more fully describe the curriculum, experimental design, and data collection.

For analyses of the effectiveness of the Project ALERT curriculum, we have identified what we term the experiment's *baseline population* of 7566 students. The baseline population includes all seventh-grade students who were enrolled in mainstream classes in a project school at baseline and who remained in the same school through week 5 of the seventh-grade curriculum (for the core of the curriculum).

POTENTIAL IMPACT OF BASELINE NONRESPONSE ON EXPERIMENTAL RESULTS

The most obvious effect of baseline nonresponse is to reduce the sample size available for analysis. Some prevention studies have lost about half their potential students. Such losses substantially increase the standard errors of treatment effect estimates and limit the opportunity to observe statistical significance. However, losses of that magnitude cause a much more important problem: They threaten an experiment's validity.

External Validity

The value of any study depends on whether the results apply to a population of interest. *External validity* refers to the extent that the results of an experiment apply beyond the particular sample.

Nonresponse can reduce external validity. When nonrespondents differ from a random sample of the baseline population, the losses change the sample available for analysis. If the baseline population (the sample before nonresponse) was carefully chosen to match the target population for an intervention, then the change would reduce the external validity. The size of that change depends on two factors: the overall nonresponse rate and the amount of difference between respondents and all nonrespondents. If either factor can be kept small, then the impact of nonresponse will also remain small.

On the other hand, small or moderate changes to the baseline sample do not necessarily imply a loss of external validity. External validity rests on the characteristics of the final sample, which depend mainly on the initial selection of subjects. If a study includes only upper-class suburban schools, one should have little confidence that the results will also apply to students in other environments. Project ALERT was not designed as a probability sample of all seventh-grade students in the United States, or in California and Oregon; no sample of eight districts could adequately serve that purpose.

Instead, our goal was to include a variety of schools whose students represent the diversity of the U.S. population.

Losses from substance-use prevention experiments are often thought to include disproportionately more students at high risk for substance use (Biglan and Ary, 1985). In that case, experiments would provide less information about the part of the population most needing help.

Past studies have indicated that students whose parents refuse to provide *active* informed consent differ substantially from those whose parents do. Active consent requires that a parent return a signed consent form permitting the student to participate in the experiment. If the form is not signed and returned, the student must be excluded from data collection. Active consent typically causes a loss of 40 to 50 percent of all potential students (Josephson and Rosen, 1978; Severson and Ary, 1983), including relatively more blacks, Asian Americans, low achievers, and children with less well-educated parents (Kearney et al., 1983; Lueptow et al., 1977; Severson and Ary, 1983; Thompson, 1984).

To avoid the high nonresponse rate typical of active consent, many researchers have turned to *passive* informed consent, which requires a parent to return a (non)consent form only if permission is denied (Biglan et al., 1987; Murray et al., 1987). If the form is not returned, the student can participate in data collection.[2] However, little is known about how passive consent affects a sample's characteristics. We used passive consent in Project ALERT.[3] Hence, this study provides new information about whether the two consent procedures yield similar results.

Some students who refuse to fill out the questionnaire or to provide saliva might do so to conceal whether they use drugs. If that happened, we would expect refusals to

[2]The danger of passive consent is that parents who do nothing (consent passively) may not understand the consequences of their inaction. If many of those parents do not want their children to participate in data collection, passive consent would not provide true informed consent. To learn whether passive consent would fail in that respect, Ellickson and Hawes (1989) conducted a pilot study as part of Project ALERT's development of data collection procedures. That study indicated that passive consent, combined with diligent efforts to reach and inform parents, can provide adequate informed consent for data collection of this type. Severson and Biglan (1989) reached the same conclusion.

[3]To maximize the chances that parents would receive and read the informed consent material, we provided three separate deliveries: an initial package via first-class mail, a reminder postcard one week later, and a repeat of the initial package taken home by students three weeks after the initial mailing (Ellickson, 1989).

lower the reported use rates. However, students might refuse for other reasons not relating clearly to drug use. For example, some students in Project ALERT refused to provide saliva, complaining that it was "gross." If that reason accounted for all saliva refusals, those students might differ from respondents only in their degree of assertiveness.

Although absences tend to occur at random times, the absentees on any given day certainly do not represent the population of all students. Several studies have found that frequent absences predict subsequent drug use; hence, absentees would presumably include a greater percentage of substance users than would respondents.

Internal Validity

Internal validity means that an experiment provides unbiased information about treatment effects for the sample that remains. This requires that the sets of respondents forming the treatment groups resemble each other. Differences could arise in either of two ways: if nonresponse rates differ among treatments, or if the type of students who fail to respond differs among treatments.[4]

Because parents and students knew the treatment assignments at the time of informed consent and baseline data collection, treatment could have affected the frequency of nonresponse.[5] Parents of students in the treatment groups might refuse less often because their children were receiving something valuable in return, or more often because some did not want their children to receive the curriculum. To avoid the latter occurrence, the informed consent material emphasized that the schools mandated participation in the curriculum and that refusing consent applied only to data collection. How treatment might affect which type of student fails to respond is less clear. Although there is less reason to expect these problems than ones affecting external validity, our analysis investigates them as well.

[4]An even larger threat to internal validity would occur if an entire school were lost from the experiment after completion of the treatment assignment. This did not happen in Project ALERT.

[5]Necessary preparations for providing the curriculum in treatment schools precluded concealing the assignments from parents or students.

DISTRIBUTION OF BASELINE NONRESPONSE

We took three steps to reduce baseline nonresponse in Project ALERT. First, we used passive parent consent. Second, in the 18 schools where the program was implemented in the spring semester, we conducted makeup sessions for students who missed school on the day of baseline data collection scheduled for their class. Third, to reduce the motivation for students to refuse (or to lie), we told the students that no one at their school or home would see any of their answers and explained our procedures for ensuring this.

The overall nonresponse rate was 16 percent of the baseline population—divided into six types (see Table 1). Some 8.5 percent of parents denied consent, accounting for the majority of baseline nonresponse. Slightly more than 3 percent were absent on the day of data collection and the makeup day (in spring schools). Less than 1 percent of students refused to fill out a survey or provide a saliva sample; approximately 2 percent responded to the survey but refused to provide saliva. Because we have baseline surveys for students who refused saliva only, those students will be included in the experiment's evaluation of treatment effects. We count them as nonrespondents in this analysis because we wanted to compare them with complete respondents and we could not always analyze survey refusals separately (see Sec. II).

The fifth group—parent consent not solicited—consists mainly of students who enrolled in a Project ALERT school within a week or two before baseline, too late to

Table 1

DISTRIBUTION ACROSS NONRESPONSE CATEGORIES IN THE BASELINE POPULATION

		Percentage of	
Nonresponse Category	Number	Baseline Pop.	Nonresponse
Parent refusals	642	8.5	52.9
Student refusal of survey	58	.8	4.8
Student refusal, saliva only	171	2.3	14.1
Student absent	246	3.3	20.3
Parent consent not solicited	79	1.0	6.5
Miscellaneous nonresponse	17	.2	1.4
Total nonresponse	1213	16.1	100.0
Total response	6353	83.9	(a)
Total baseline population	7566	100.0	(a)

[a]Not applicable.

allow their parents adequate time to give informed consent. The last group—miscellaneous nonresponse—includes students who left the classroom for reasons that did not relate to refusal. Because these groups are small and would not be expected to differ greatly from respondents, we have not included their members in the nonresponse analysis.

The steps taken to reduce baseline nonresponse worked very well. The rate of parent refusals was a fraction of that in some studies that used active consent. The low absentee rate results from the makeup sessions, which reduced the absentee rate at spring baseline from 8.3 percent before makeups to 2.5 percent. At fall baseline, the absentee rate was 5 percent without makeups. Student refusals of the experiment's main data source—the survey—were negligible.

II. DATA SOURCES AND DATA COLLECTION METHODS

Several data sources allowed us to compare respondents with each type of nonrespondent on institutional, demographic, and personal characteristics. We used sample accounting data to relate nonresponse to district, school, class, and treatment. Postbaseline survey data provided substance use information for many of the student refusals, absentees, and respondents.

Because survey data were unavailable for students whose parents refused consent, we turned to two other data sources: (1) our student master lists, which contained gender and race/ethnicity data for both respondents and nonrespondents; and (2) school records, from which we could abstract academic grades and absences for both groups. The school records data provided the best available indicators of substance use.

SAMPLE FOR DEMOGRAPHIC AND ACADEMIC COMPARISONS

To compare nonrespondents and respondents with respect to grades, absenteeism, race/ethnicity, and gender, we constructed special data sets—one for grades and absences, the other for race and gender. We initially selected 2771 students for inclusion in both analyses. This group included a stratified random sample of 50 respondents from each school (if available) and all the nonrespondents in each of three categories—parent refusals, student refusals, and absentees. Fifty respondents per school far exceeded the number in any nonresponse category at most schools; sampling more respondents would have increased data collection/entry costs without substantially improving precision. To improve correspondence between the sample and the population of respondents from each school, we selected stratified random samples from each school.[1] For fall schools, the stratifying variable was a questionnaire item eliciting information about the student's usual grades. Because responses to this item were not available in time for selection of the spring sample, we stratified by data collection session.

[1]This sampling procedure is described by Chromy (1979). Subject to randomization, the procedure provides the closest possible match of the distribution of the stratifying variable in the sample to its distribution in the population.

This initial sample contained 1484 respondents and 1287 nonrespondents. Because we needed to select this sample before identifying the baseline population, it included many students we had attempted to survey but who were ineligible for our baseline population. These include eighth graders, special education students, students who transferred within five weeks of baseline, and some students who appeared on enrollment lists but were not enrolled at baseline. Deleting these students required special procedures for each data set.

GENDER AND RACE/ETHNICITY DATA

We abstracted the gender and race/ethnicity data from printed school master lists, aggregating different district codes to a consistent set of ethnic categories: white, black, Hispanic, Pacific Islander/Asian, and American Indian. We deleted five sensitive cases—instances where only one or two students fell into a school's particular nonresponse category. If these cases had remained in the data set, individual students might have been readily identifiable even without access to the link between name and identification number. We also identified and deleted 167 students who were not part of the baseline population—eighth graders, special education students, and transfer students.[2] Finally, we could not find 18 cases in the master lists. The gender and race/ethnicity analysis data set contained 2581 cases (see Table 2).

GRADES AND ABSENCE DATA

From school records, we collected data on fall semester absences and final fall semester grades in seventh-grade English and math. We used fall semester records to allow completion of data collection before the 1985 summer break. Where available, unexcused and excused absences were counted separately. However, because the participating schools use different standards for defining unexcused absences, we report results for total absences only.

Most schools did not want to provide grades and absence information by name, particularly for students whose parents had refused consent for participation in the study. Consequently, we developed a special data collection form to keep student data

[2]Because some ineligible students were not identified until after creation of this data set, it includes some students who are not in the baseline population.

Table 2

REASONS FOR DELETION OF STUDENTS FROM SAMPLES FOR THE NONRESPONSE ANALYSES

Category	Type: Gender and Race/Ethnicity	Type: Grades and Absences
Deletions		
Sensitive cases	5	5
Identified as ineligible[a]	167	87
School with many eighth graders	0	118
Not in records	18	22
Total deletions	190	232
Initial sample	2771	2771
Total available for analysis	2581	2539

[a]Not in baseline population.

confidential. Each page contained three columns, with student names in the far left column. Next to each name was a removable label where the grades and absence data were to be entered. The labels identified whether the student responded at baseline and, if not, the appropriate nonresponse category. After the data had been copied from school records onto the labels, the labels were moved in random order to the far right column of the page. This procedure severed the linkage between student name and grades/absences data before the forms reached RAND. Hence, we could not link data between the demographic and academic data sets.

As before, we deleted the five sensitive cases from the grades and absences data set. We also deleted 22 students with data on neither fall semester grades nor absences.

Identifying ineligible students was considerably more difficult because we could not use names to link grades/absences data to the project's sample accounting data. We worried particularly about students whose names appeared on enrollment lists even though they were not really enrolled at baseline. When the grades/absences data were collected, these students were often classified as absent (instead of ineligible). If school records showed these students to have many absences and failing grades, that would distort our analysis of absentees. Written comments on labels enabled us to delete 87 apparently ineligible students from 13 schools, and we determined that there were none in 6 more schools. Other information indicated that there were few or no ineligible students in 10 of the other 11 schools. Because the school where eighth graders participated in data collection included some 50 ineligible students whom we could not identify, we deleted all 118 students in that school from the grades and absences analysis. The resulting grades and absences analysis file contained 2539 students (see Table 2).[3]

[3]Comparing grades and absences results restricted to the 19 schools where we were confident we had deleted all ineligible students with results in 29 schools indicated little bias of the type we had anticipated. Thus, we present results for the 29 schools.

III. RESULTS

In this section, we relate baseline nonresponse rates to various factors. We begin by exploring institutional factors, such as district, school, and treatment. Later, we consider personal characteristics that relate to nonresponse. Except for the comparison among treatment groups, these results apply to the investigation of external validity—whether nonrespondents differed from respondents. We defer to Sec. IV discussion of how much nonresponse altered the baseline sample.

INSTITUTIONAL FACTORS

All the data presented in this subsection come from the main data collection files for Project ALERT instead of from the special data sets described in Sec. II. Thus, these results refer to the complete baseline population of 7566 students, less 79 cases where parent consent was not solicited and 17 miscellaneous cases of nonresponse (see Table 1). Because the main Project ALERT data sets distinguish student refusals of the survey from student refusals of saliva only, we consider four nonresponse categories: parent refusals, student refusals of survey, student refusals of saliva only, and absentees.

District and School

Nonresponse relates significantly to school but, for the most part, not to district. Rates for each type of nonresponse differ significantly among the 30 schools ($P < .001$ for each type of nonresponse, with or without controlling for district). Analysis of rates of student saliva refusals for the 30 schools, with school as the unit of analysis, shows significant differences among the districts ($P = .02$), with the highest rates in districts with the most minority students. However, analyses of other nonresponse rates show no significant differences among the eight districts (each $P > .15$).

Several factors probably contribute to the differences in nonresponse rates among schools. As we shall see below, homogeneity of student and parent characteristics within schools explains some of the differences. In addition, short-term absentee rates certainly relate to illnesses that pass through schools. Finally, decisions within the same school to refuse may depend on each other.

Student refusals were clearly influenced by what might be termed a "bandwagon" effect; once one or more students had refused part of the data collection, it became easier for other students in the same class to refuse too. The data collectors felt that they observed this phenomenon, and the data support it. Between-classroom variation in the number of student refusals was 2.7 times what would be expected if all refusals were independent within schools (true for both survey and saliva refusals). For example, 5 classrooms (out of 293), with 6 to 11 refusals each, accounted for 40 of the 171 saliva-only refusals. If refusals occurred independently, that *any* classroom would have had 6 or more is unlikely. Perhaps some clustering of refusals is explained by correlation of personal characteristics, but the main reason is probably that refusing becomes more attractive when one sees others doing so.

Treatment

Treatment does not seem to have affected who responded at baseline. The treatments differed moderately in nonresponse rates, but the differences appeared to result because each school was assigned to a single treatment. Although each type of nonresponse was highest in the control condition, none of the individual differences achieved statistical significance based on school as the unit of analysis (see Table 3). Statistical significance arises only when comparing the overall nonresponse rate in the control group with that in the other two conditions combined (P = .025). Given the many possible comparisons, that finding should be interpreted with caution. More important,

Table 3

PERCENTAGE IN EACH NONRESPONSE CATEGORY, BY TREATMENT GROUP

	Treatment Condition			
Nonresponse Category	TLC	HEC	Control	P-Value
Parent refusal	8.3	7.4	10.2	.20
Student refusal of survey	.5	.8	1.0	.62
Student refusal, saliva only	1.4	2.5	3.0	.16
Student absent	2.7	3.3	4.0	.25
Total nonresponse	12.9	14.0	18.2	.08

NOTE: P-values test the hypothesis that a particular type of nonresponse was unrelated to treatment. Analysis of variance was used to compare school nonresponse rates (weighted by school size) across treatments.

we found no evidence that treatment affected who failed to respond. Analyses of race, gender, English and math grades, and absences showed that after controlling for school, treatment did not relate significantly to which types of students responded (P = .16, .89, .96, .50, and .08, respectively). In other words, nonrespondents differed from respondents, but appeared to do so in the same way in each treatment group.

PERSONAL CHARACTERISTICS

The analyses in this subsection use the special data sets created for assessing nonresponse. Thus, these analyses do not distinguish student refusals of the survey from refusals of saliva only.

Race/Ethnicity

Race/ethnicity relates strongly to nonresponse category (see Table 4). Parent refusal rates were much higher for Asian students than for the other groups—13 percent, compared with 8 to 9 percent. Asian students were least likely to be absent, compensating somewhat for their high rate of parent refusals. Rates of student refusals (survey combined with saliva only) were highest for the Hispanic students. Overall, baseline nonresponse was highest for Hispanic students and lowest for whites. All the relationships except those for absences achieved high statistical significance (first column of P-values).

Table 4

PERCENTAGE IN EACH NONRESPONSE CATEGORY, BY RACE/ETHNICITY

	Race/Ethnicity				P-Value	
Nonresponse Category	White	Black	Hispanic	Asian	Pearson	M-H
Parent refusal	7.9	8.8	9.1	13.1	.002	.16
Student refusal[a]	2.3	3.7	6.7	2.8	<.001	.002
Student absent	3.6	3.6	4.0	1.6	.08	.33
Total nonresponse	13.8	16.1	19.8	17.5	<.001	.18

NOTE: P-values test the hypothesis that a particular type of nonresponse was unrelated to race. The first set are based on simple Pearson's chi-squares for two-way contingency tables. The second set are adjusted for school by use of the general association Mantel-Haenszel test, which uses only within-school information about the relationship of nonresponse to race. American Indians are not included because there were too few to provide reliable estimates of nonresponse rates.

[a]Combines refusals of survey and refusals of saliva only.

Because both race/ethnicity and nonresponse rates differ greatly among schools, we tested for relationships between race and nonresponse *within* schools. The Mantel-Haenszel test, which uses only within-school information, achieved statistical significance only for student refusals (Mantel and Haenszel, 1959). Thus, we generally could not isolate a unique effect of race/ethnicity.

Gender

Girls appeared more often than boys in each nonresponse group (see Table 5). The largest difference occurred for parent refusals, where girls were exempted from data collection one-third more often than boys. That difference and the greater rate overall were significant at $P < .001$.

Absenteeism

Students who were absent frequently during fall semester were more likely to refuse the survey and/or saliva than students with few absences (see Table 6). Not surprisingly, those students were much more likely to be absent on the day of baseline data collection.[1] However, number of absences did not relate to the rate of parent refusal.

Table 5

PERCENTAGE IN EACH NONRESPONSE CATEGORY, BY GENDER

	Gender		
Nonresponse Category	Female	Male	P-Value
Parent refusal	10.0	7.4	<.001
Student refusal[a]	3.5	2.7	.04
Student absent	3.7	3.2	.16
Total nonresponse	17.2	13.3	<.001

NOTE: P-values test the hypothesis that a particular type of nonresponse was unrelated to gender. They are based on Pearson's chi-squares for two-way contingency tables.
[a]Combines refusals of survey and refusals of saliva only.

[1]For the fall sample, there is some artificial correlation resulting from the inclusion of absences around baseline data collection in the count of fall semester absences. However, the relationship was almost as strong for students in the sample of spring schools.

Table 6

PERCENTAGE IN EACH NONRESPONSE CATEGORY, BY NUMBER OF FALL SEMESTER ABSENCES

	Number of Absences				
Nonresponse Category	0–2	>2–5	>5–10	>10	P-Value
Parent refusal	9.3	7.9	8.1	9.1	.46
Student refusal[a]	2.6	3.0	2.8	4.3	.02
Absent	1.3	3.4	4.6	10.2	<.001
Total nonresponse	13.2	14.3	15.5	23.6	<.001

NOTE: P-values test the hypothesis that a particular type of nonresponse was unrelated to the number of absences. They are based on Pearson's chi-squares for two-way contingency tables.

[a]Combines refusals of survey and refusals of saliva only.

Grades

Fall semester seventh-grade English and math grades also related to nonresponse status, each showing a similar pattern (see Fig. 1). As one might expect, students who were absent during data collection had very low grades in both English and math—one-half grade point lower than respondents. To a smaller extent, the same holds true for student refusals (0.23 grade points lower than respondents). Questionnaire data collected from baseline absentees and student refusals at wave 2 (three months after baseline) support those in the figure. For each of these two nonresponse groups, the proportion who reported that their own grades were usually C or lower was 52 to 53 percent, compared with 35 percent for baseline respondents.

In contrast, students whose parents refused for them had higher grades than did respondents, although the differences were not statistically significant (P = .08 for English grades). Grades for the parent refusal group were significantly higher that those in either of the other two nonresponse groups. Because parent refusals retain that status at later waves, we could not use wave 2 survey data to confirm the finding of higher grades for that group.

The relatively high grades for students in the parent refusal group—the majority of nonrespondents—dampened the overall difference between nonrespondents and respondents. The differences were 0.11 and 0.14 for English and math grades, respectively (P < .05 and P < .01).

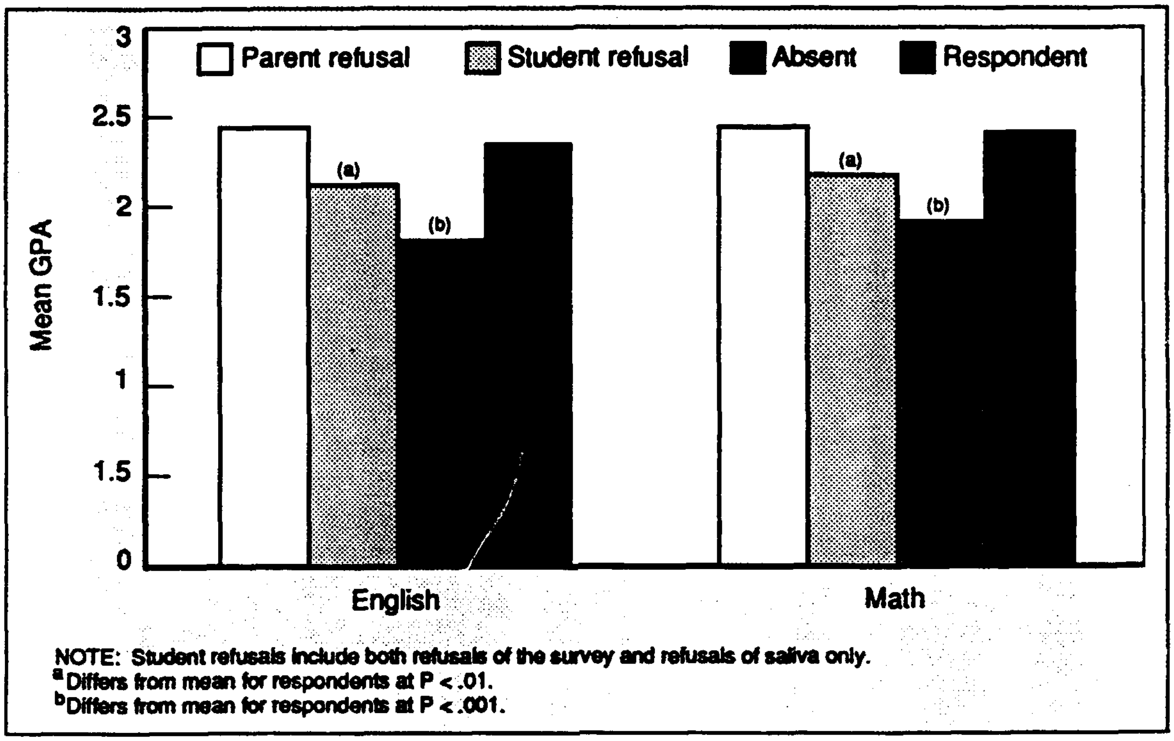

Fig. 1—Mean English and math grades, by nonresponse category

SUBSTANCE USE

Survey data from wave 2 tend to show higher substance use by baseline student refusals and baseline absentees than by baseline respondents (see Fig. 2).[2] Current cigarette and marijuana use (in the past month) was significantly higher for those categories of baseline nonrespondents than for baseline respondents. The same pattern occurred for alcohol use, but the differences were not statistically significant. No substance use information is available for parent refusals.

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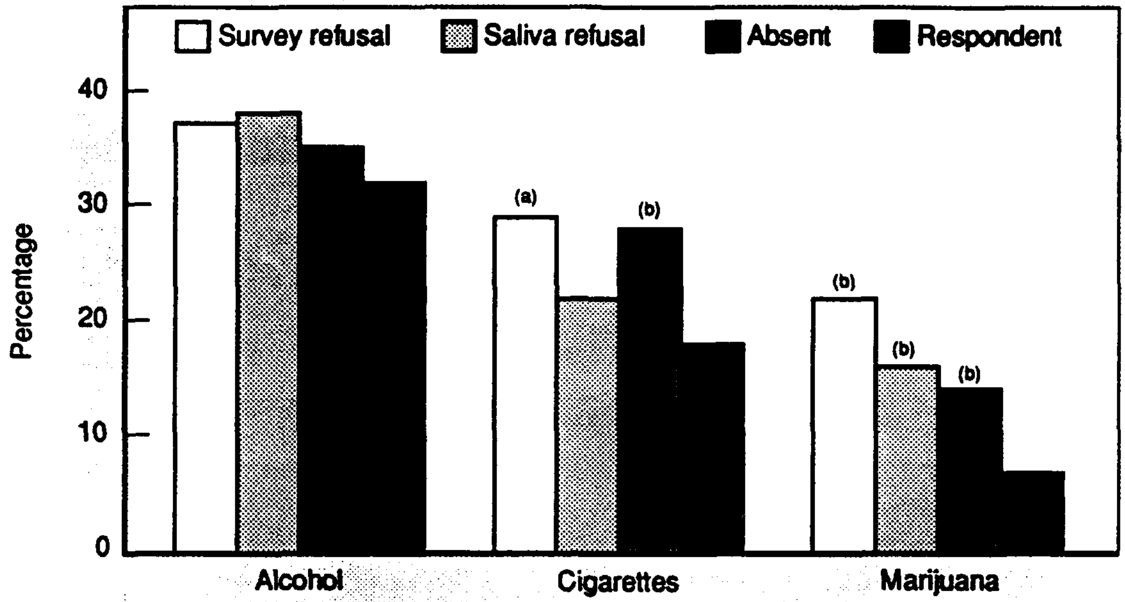
[2]If students who missed both the first two waves were especially likely to use substances, Fig. 2 would understate the difference between baseline respondents and these nonrespondent groups.



NOTE: Parent refusals do not appear because no substance-use data were collected for them.
[a]Differs from percentage for respondents at $P < .05$.
[b]Differs from percentage for respondents at $P < .001$.

Fig. 2—Percentage of current substance users at wave 2, by nonresponse category

IV. CONCLUSIONS

1. Nonrespondents and respondents differed substantially. Our analyses demonstrate that baseline nonrespondents in Project ALERT differed from respondents in many ways. We found significant differences on most variables we investigated, often surfacing for each source of nonresponse. We observed substantial differences in nonresponse rates among schools, among racial/ethnic groups, and between boys and girls. Student refusals and absentees clearly differed from respondents in ways the literature suggests. These groups exhibited much lower grades, more absences, and higher rates of cigarette and marijuana use.

2. Nonresponse had little impact on the sample. Despite the often dramatic differences noted above, baseline nonresponse altered sample characteristics only slightly. For example, we estimate that nonresponse reduced the percentage of nonwhite students by only 0.9 percent—from 29.2 in the baseline population to 28.3 among respondents. Similarly, the percentage of girls dropped by only 1.1 percent and mean math grade point average rose by just 0.02 grade points. If one treats student refusals of saliva as respondents, the gaps become even smaller. Clearly, whether the experiment covered a population of interest (external validity) depends much more critically on the initial selection of 30 schools than on baseline nonresponse. That the experiment retained a highly diverse sample of schools and students means the results will apply in a wide variety of environments.

Two facts explain the small impact of nonresponse on sample characteristics. First, baseline nonrespondents comprised less than one-sixth of the baseline population. If nonresponse had occurred more frequently, the potential damage would have increased. Second, parent refusals tended to resemble respondents and, consequently, to dampen or even counter the impact of student refusals and absentees.

We expect that nonresponse would have had a much larger impact if we had used active parent consent. The literature suggests that parent refusals from an active consent procedure would have looked something like our student refusals and absentees. If parent refusals had also inflated the nonresponse rate near 50 percent or higher, it would have seriously damaged the experiment's external validity. That parent refusals resulting from active consent differ from those resulting from passive consent is not surprising.

Refusals to return active consent include not only parents who do not want their children to participate, but, apparently, many who are less involved in their children's education.

Providing makeup surveys to spring semester absentees also helped reduce the size of differences between the baseline population and the sample of baseline respondents. Obviously, this step reduced the number of nonrespondents. Just as important, spring semester students who attended makeup sessions at baseline reported wave 2 substance use, grades, and absences similar to those reported by students who remained absent.

3. Internal validity was maintained. The need to assign each school to a single treatment condition contributed to differences among treatments in nonresponse. Because nonresponse rates differed substantially by school, cross-treatment differences showed up despite random assignment of schools. Similarly, some differences remained across treatments in the characteristics of nonrespondents. If it had been desirable to assign treatments to individual students (or classes) within schools, these differences could have been reduced.

Given that we needed to assign schools to treatments, internal validity was good. Little evidence exists that treatment affected either the rates of nonresponse or who responded. Thus, the small differences among treatments appear to be random instead of systematic. Standard statistical methods will be used in analysis of treatment effects to adjust for differences caused by baseline nonresponse and other random factors.

4. Steps to reduce nonresponse succeeded. The steps taken to reduce baseline nonresponse in Project ALERT—passive informed consent, careful explanation of data privacy, and makeup sessions—proved successful. They held the combined level of baseline nonresponse to 16 percent. More important, these steps sharply limited differences between the baseline population and baseline sample and avoided substantial differences among the treatment groups.

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