

The Influence of a Family Program on Adolescent Tobacco and Alcohol Use

ABSTRACT

Objectives. This study examined a family-directed program's effectiveness in preventing adolescent tobacco and alcohol use in a general population.

Methods. Adolescents aged 12 to 14 years and their families were identified by random-digit dialing throughout the contiguous United States. After providing baseline data by telephone interviews, they were randomly allocated to receive or not receive a family-directed program featuring mailed booklets and telephone contacts by health educators. Follow-up telephone interviews were conducted 3 and 12 months after program completion.

Results. The findings suggested that smoking onset was reduced by 16.4% at 1 year, with a 25.0% reduction for non-Hispanic Whites but no statistically significant program effect for other races/ethnicities. There were no statistically significant program effects for smokeless tobacco or alcohol use onset.

Conclusions. The family-directed program was associated with reduced smoking onset for non-Hispanic Whites, suggesting that it is worthy of further application, development, and evaluation. (*Am J Public Health.* 2001;91:604–610)

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Adolescent use of tobacco and alcohol is prevalent in the United States.¹ In 1997, 43.0% of 9th to 12th graders reported use of a tobacco product in the past 30 days, and 20.0% of 12- to 17-year-olds reported use of alcohol in the past 30 days.¹ In this article, we evaluate a family-directed program's influence on the onset of adolescent tobacco and alcohol use in the general population.

The ubiquity of US school-based drug education programs suggests the need to justify additional prevention approaches. Although research suggests that some types of school-based programs influence adolescent drug use, the effects of these programs are smaller than desired and usually short-lived,^{2–5} and the national prevalence of the behaviors remains unacceptably high.¹ Moreover, the program favored by most schools appears to have no effect on adolescent drug use.⁶ Thus, additional programs designed to lower the prevalence of adolescent substance use should be identified.

One potentially attractive approach—involving families of adolescents—is particularly promising, because family characteristics are related to adolescent drug use, and families have strong and persistent influences on their children.⁷ That potential rarely has been adequately addressed by evaluation research.

Only 7 family interventions directed at general populations, in which the family intervention could be separated from other intervention modalities and in which adolescent drug behavior effects were assessed, have been evaluated for their effectiveness in preventing adolescent substance use. As is often the case for new areas of program development and evaluation, most of the studies' research designs precluded satisfactory conclusions about effects. One had no baseline measures and no adequate comparison group.⁸ Two evaluated a parent component as part of a more comprehensive program without a comparison group of parents not offered the program.^{9,10}

Two compared adolescents whose parents were or were not offered a program without random assignment of parents to conditions.^{11,12}

The remaining 2 evaluations used randomized experimental designs. In an evaluation of multiple-session workshops for parents and their children, no effects for adolescent alcohol and tobacco use were detected; however, fewer than 6% of the eligible families began the program, compromising the interpretation of program effects.¹³ Another evaluation randomly allocated 33 public schools to 1 of 3 conditions: (1) a 5-session family-directed program, (2) a 7-session family-directed program, and (3) a control condition. Adolescents whose parents were selected to go to central locations for the program used less tobacco, alcohol, and other drugs (R. L. Spoth, PhD, C. Redmond, PhD, C. Shin, PhD, unpublished data, 1999).^{14,15} Family participation was higher than in other programs implemented independent of school curricula, and the research design used was strongest for inferring effects.

Collectively, these studies suggest the need for further evaluation of family programs with randomized research designs. The study with the most rigorous methods found substantial behavioral effects, suggesting particular promise for further consideration of family-directed programs (R. L. Spoth, PhD, C. Redmond, PhD, C. Shin, PhD, unpublished data, 1999).^{14,15}

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Family Matters

We assumed that general populations must be involved for population prevalence to be influenced. Two additional considerations were fundamental to program design: favorable participation rates and implementation independent of schools. Family programs directed toward general populations typically attract only 3% to 35% of eligible families if they are not directly linked to school curricula.^{12,13,16,17} Thus, the program had to be capable of completion at times and places convenient to families. Also, even though family programs directly related to school curricula typically have higher participation rates than family programs less involved with schools,^{9,10,18,19} most US schools are unlikely to add family programs to their curricula.

Family Matters, a family-directed program to prevent adolescent tobacco and alcohol use, involves successively mailing 4 booklets to families and conducting telephone discussions with health educators after each mailing. To prevent onset of use as necessary to reduce population prevalence, Family Matters was designed for families with adolescents aged 12 to 14, because at these ages, many have not yet used the substances but are beginning to make decisions about use. Involving those ages also optimizes variance in onset, which is necessary for the statistical power to detect program effects.

Family Matters involves families with both drug-using and drug-abstaining adolescents. Two weeks after family members read a booklet and carry out activities intended to reinforce its content, a health educator contacts a parent (usually the mother) by telephone to encourage participation by all family members, answer questions, and record information. The adolescent is reached through family members and is never contacted directly by the health educator. A new booklet is mailed when the health educator determines that the prior booklet has been completed. When the fourth booklet is completed, or when a family decides to end participation, mailings and telephone calls end. The estimated cost for replicating the program, omitting research expenses and other costs not required for dissemination, is \$140.42 per eligible participant.²⁰

As with many multiple-component interventions, the model that guided booklet content was informed by several social and behavioral science theories. For booklet 1, we assumed that some families require motivation to participate and become engaged. From value expectancy theory, we reasoned that families will be motivated if they expect negative consequences from adolescent substance use.²¹⁻²⁴ Among other activities, adult family members

are asked to identify and discuss possible consequences to the family if the adolescent used tobacco or alcohol. From the Health Belief Model, we reasoned that families will be motivated if they feel susceptible to their child's substance use.²⁵ Adult family members are informed that children from all types of families use tobacco and alcohol and that their child may do so. From social learning theory, we reasoned that families will be motivated if they feel they can exert control over their child's behavior.²⁶ Adult family members are told about family influences on children and, for illustration of their own influence, are encouraged to discuss their similarities with the adolescent.

Booklet 2 focuses on general family characteristics known or believed to influence adolescent drug use that are not specific to drug behavior, such as supervision, support, communication skills, attachment, time spent together, educational achievement, conflict reduction, and how well adolescence is understood.⁷ These characteristics are central to theories of socialization,^{27,28} social control,^{29,30} social development,³¹ and family interaction.³² Among other things, adults are asked to list behaviors that reflect normal changes in adolescents, to practice communication skills with their child, and to plan special times to be together.

Booklet 3 is concerned with tobacco and alcohol-specific variables, largely encompassed by social learning theory,²⁶ that originate in the family and predict adolescent drug use.³³ Adults are asked to list things they do that might inadvertently encourage their child's use of tobacco or alcohol, to identify rules that might influence their child's use, and to consider ways to monitor use. Adult family members and the adolescent then meet to agree on rules and sanctions related to adolescent tobacco or alcohol use.

Booklet 4, based largely on social inoculation theory,³⁴ considers variables that originate largely outside the family that may influence adolescent use.³⁵ The adult and adolescent consider what the adolescent can do to resist peer and media pressures for use. In one activity, the parent and adolescent practice responses to a friend's offering alcohol and tobacco, and in another, they watch favorite television shows to discuss these shows' tobacco- and alcohol-related messages.

Communication is at the core of Family Matters. The booklets and the health educators communicate with adult family members, adult family members are encouraged to communicate among themselves, and communication between adult family members and the adolescent is the mechanism through which behavioral effects on the adolescent would occur. Many theories of communication are reflected

in the program.³⁶⁻³⁸ Elsewhere, we describe family communication about adolescent tobacco and alcohol use in our program.³⁹ The program materials and implementation characteristics are presented in more detail at our Web site (<http://www.sph.unc.edu/familymatters/index.htm>) and elsewhere.^{20,40}

Methods

Design, Sample, and Data Collection

Twelve- to 14-year-olds living throughout the contiguous United States with their families were entered into a randomized experimental design. To identify families with 12- to 14-year-olds, 64811 telephone numbers selected to be representative of all telephone numbers in the contiguous states were generated by random-digit dialing. Of those numbers, 2395 (3.7%) were estimated to be in a household with an eligible parent-adolescent pair. When more than 1 age-eligible adolescent lived in the household, the second adolescent was randomly omitted from the study. Of these parent-adolescent pairs, 1326 (55.4%) completed baseline telephone interviews averaging 15 minutes from June 1996 through February 1997. A parent provided verbal consent for his or her own and his or her child's participation. The mother or mother surrogate was the parent interviewed for 96.0% of the pairs.

Of the 1326 parent-adolescent pairs, 9 subsequently asked to be withdrawn from the study, and 1 was lost to follow-up because of administrative error, leaving 1316 baseline pairs for the study. As baseline interviews were completed, parent-adolescent pairs were matched by date and time of completion and then allocated randomly either to receive Family Matters or to serve as control subjects. Within a month after the baseline interview, the treatment group parents were mailed booklet 1; the first booklets were sent in July 1996, and the final parent-adolescent pair completed the program in September 1997.

Of the 2395 families eligible for the baseline interview, half (1198) were eligible for the program. Of those eligible for both, 549 (45.8%) began the program, and 407 (34.0%) completed it.

The 1316 parent-adolescent pairs were called for a first follow-up interview 3 months after the treatment group participants completed the program, terminated the program, or were declared unable to begin or continue the program. The baseline pairs were similarly called for a second follow-up interview 12 months after the treatment group completed the program. The first follow-up interviews were conducted from November 1997 through

April 1998. The mean, range, and median days between baseline and the first follow-up were, respectively, 324.9 (SD=89.6), 147 to 621, and 312. The second follow-up interviews were conducted from August 1997 through January 1999. The mean, range, and median days between baseline and the second follow-up were, respectively, 606.1 (SD=93.3), 401 to 923, and 596. Of the 1316 baseline pairs, 1135 (86.2%) of the adolescents completed either the first follow-up or the second follow-up, and 1014 (77.1%) completed both follow-up interviews.

The control group was contacted only for data collection. Interviewers and health educators were different people, and their interaction was minimized. Interviewers and health educators were blinded from study findings until all data had been collected.

Measures

Adolescent smoking was determined by the question "How much have you ever smoked cigarettes in your life?" Response categories were "none at all, not even a puff;" "1 or 2 puffs but not a whole cigarette," "1 to 5 whole cigarettes," "6 to 20 whole cigarettes," and "more than 20 whole cigarettes." Categories were collapsed to never used ("none at all, not even a puff") and had used (all other categories).

Drinking was determined by the question "How much alcohol have you ever had in your life?" Response categories were "none at all, not even a sip," "1 or 2 sips but not a whole drink," "3 or more sips but never a whole drink at one time," "1 to 5 whole drinks," "6 to 20 whole drinks," and "more than 20 whole drinks." The categories were collapsed to never used ("none at all, not even a sip") and had used (all other categories). We instructed adolescents that "alcohol" refers to beer, wine, wine coolers, and liquor. A "drink" was defined as a glass of wine, a can of beer, a wine cooler, a shot glass of liquor, or a mixed drink. We told adolescents not to include alcohol they might have had at church but to include alcohol they might have had with their family.

Adolescent use of smokeless tobacco was determined by the question "Have you ever tried chewing tobacco (such as Redman, Levi Garrett, or Beechnut) or snuff (such as Skoal, Skoal Bandits, or Copenhagen)?" Response categories were "yes" and "no."

Measures also were obtained for 5 background variables because of possible relevance to program engagement and adolescent substance use. Adolescent race/ethnicity and adolescent age at the time of the baseline interview were measured by questions in the adolescent interview. Four categories (non-Hispanic White, non-Hispanic Black, Hispanic, and other) were collapsed to non-Hispanic

White or other to obtain a sufficient sample size to test for interactions with treatment. Age was left continuous. Adolescent sex (female or male) was measured during the screen for study eligibility. Number of parents in the home (2 parents or 1 parent) and mother's education (collapsed to college graduate, some college, and high school or less) were measured with parent interview items.

Analyses

Adolescents classified at baseline as nonusers of cigarettes, smokeless tobacco, or alcohol were used in the analyses of smoking, smokeless tobacco, and drinking onset, respectively. Onset was determined to have occurred if an adolescent reported during either of the follow-up interviews that he or she had at least taken a puff of a cigarette, tried smokeless tobacco, or taken a sip of alcohol.

Generalized estimating equation methods, used for all analyses of program effects, allowed inclusion of the adolescents who completed either 1 or both of the follow-up interviews, thereby enhancing power and generalization to the population. Our conclusions on program effects were the same for respondents who provided data at 1 or both of the follow-ups. Because positive program effects were hypothesized, we used the 1-tailed test ($\alpha = .05$) and lower-bound confidence intervals (CIs). Background variables were controlled for in all analyses and, when indicated, used in assessments for interactions.

Sample Assessment

The distribution of baseline respondents by state of residence correlated 0.96 with the distribution of 12- to 14-year-olds in the 1990 US census,⁴¹ suggesting that our sample had realistic geographic representation. The respondents generally were similar to families with children aged 12 to 14 years in the 1990 US census with respect to adolescent age and sex, but non-Hispanic Whites, 2-parent households, and college-educated mothers were somewhat overrepresented.

Two other national studies of adolescents that asked about drug use were conducted at about the same time as ours: the National Longitudinal Study of Health (Add Health)⁴² and Monitoring the Future (MTF).⁴³ We limited comparisons to similar age groups across studies: eighth graders for MTF and 12- to 14-year-olds for Add Health. Fewer tobacco users were in our sample than in MTF and in Add Health—34.5% of our baseline sample had smoked cigarettes vs about 46.0% in comparable MTF and Add Health samples. More had used alcohol in our baseline sample (68.5%)

than in MTF (54.5%); comparable data for alcohol use from Add Health were unavailable because of incompatible questions. Given the many differences between these studies in question wording, sample design, and data collection procedures, it is impossible to know whether the differences in substance use were due to those factors or to inaccurate responses. Tobacco use may have been underreported in our study because the data were collected by telephone.⁴⁴

We compared treatment and control groups on baseline adolescent age, sex, race/ethnicity, number of parents in the home, mother's education, and adolescent use of tobacco and alcohol. The only statistically significant difference ($\chi^2 = 5.08, P < .05$) was that fewer non-Hispanic Whites were in the treatment group (70.6%) than in the control group (76.1%). In all analyses of treatment effects, we controlled for all background variables to further enhance baseline similarity of the treatment and control groups.

To assess attrition bias after baseline, we compared respondents who did and did not complete follow-up interviews. As found in other longitudinal studies, respondents lost to follow-up were more likely to be baseline users than were those not lost to follow-up.⁴⁵ In addition, adolescents other than non-Hispanic Whites, in single-parent homes, and with mothers having the least education were more likely to be lost to follow-up. However, the sample we used in our analyses for effects was very similar to the overall sample (completed and lost combined) because a high percentage of baseline respondents (86.2%) participated at follow-up. Analyses comparing respondents who completed 1 or both of the follow-up interviews had similar findings except that the two groups did not differ on baseline cigarette and alcohol use.

We also tested for statistical interactions between the treatment condition and the 5 background variables with attrition as the dependent variable. The one statistically significant interaction (Wald $\chi^2 = 7.66, P = .006$) suggested that the treatment-control difference in participation for non-Hispanic Whites (treatment = 83.1%, control = 94.4%) was larger than for others (treatment = 76.6%, control = 81.0%). A Bonferroni-Holm adjustment ($\alpha = .05$) suggested that the finding may have been due to chance because of the multiplicity of interactions evaluated.⁴⁶ We controlled for race/ethnicity and the other background variables in all program effects analyses. We also conducted analyses to account for the correlation between treatment and control groups that was introduced by our matching for date of baseline interview. This consideration of design effect was accomplished by repeating the generalized estimating equation analysis with the matched

pairs as a cluster variable. The same general conclusions about program effects were derived.

Results

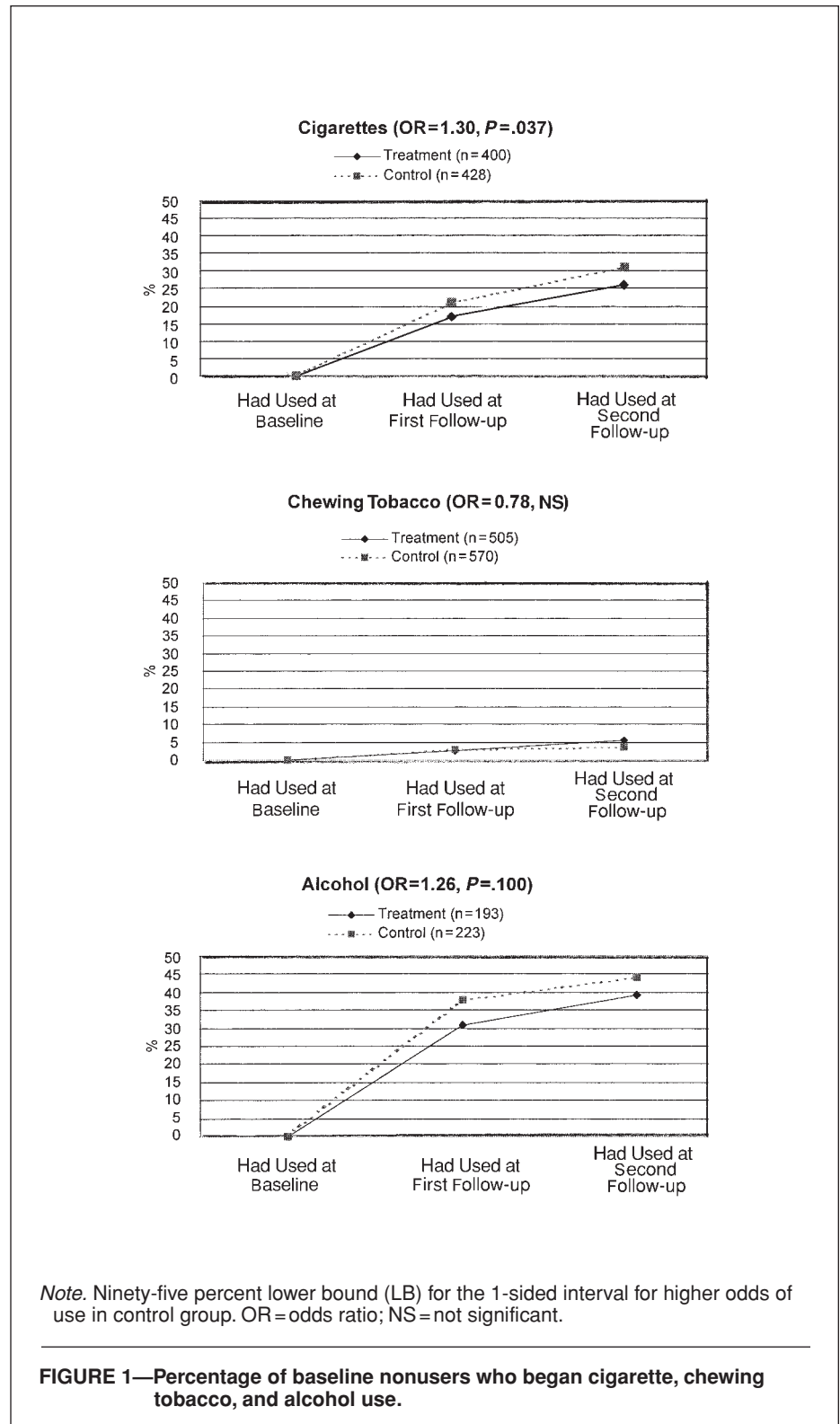
Figure 1 shows the treatment-control comparisons of cigarette, smokeless tobacco, and alcohol use onset for adolescents who had never used the substance at baseline. For cigarette smoking, a statistically significant difference in onset between treatment and control groups suggests that the program decreased smoking onset (odds ratio [OR]=1.30, $P=.037$, lower-bound CI=1.02). When adjusted for the design effect, the odds ratio of 1.30 was reduced to 1.27 ($P=.059$, lower-bound CI=0.99). No statistically significant program effect was seen for onset of smokeless tobacco or alcohol use. Tests for treatment and time of follow-up interactions indicated that findings did not differ at the first and second follow-ups.

Multiple regression analyses were conducted to determine whether interactions occurred between treatment condition and each of the 5 background variables as they related to onset of substance use. The one statistically significant interaction is shown in Figure 2. A positive program effect was found for smoking onset for non-Hispanic Whites but not for other racial/ethnic group members. A Bonferroni-Holm adjustment ($\alpha=.05$) suggested that this finding likely was not due to chance.⁴⁶ What might appear to be a negative program effect for persons other than non-Hispanic Whites was not statistically significant. After adjustment for the design effect, program effects also were seen for only non-Hispanic Whites (data not shown).

We conducted analyses parallel to those described above, but our focus was on movement to more frequent use (smoking or drinking 6 or more days out of the past 30 days) by adolescents who did not use at that level at baseline. Baseline nonusers and infrequent users were combined, and the particular cut-point for frequency of use was chosen to allow sufficient numbers of cases in cells for analysis. There were too few smokeless tobacco users for parallel analyses. No statistically significant program effects were seen for frequency of use.

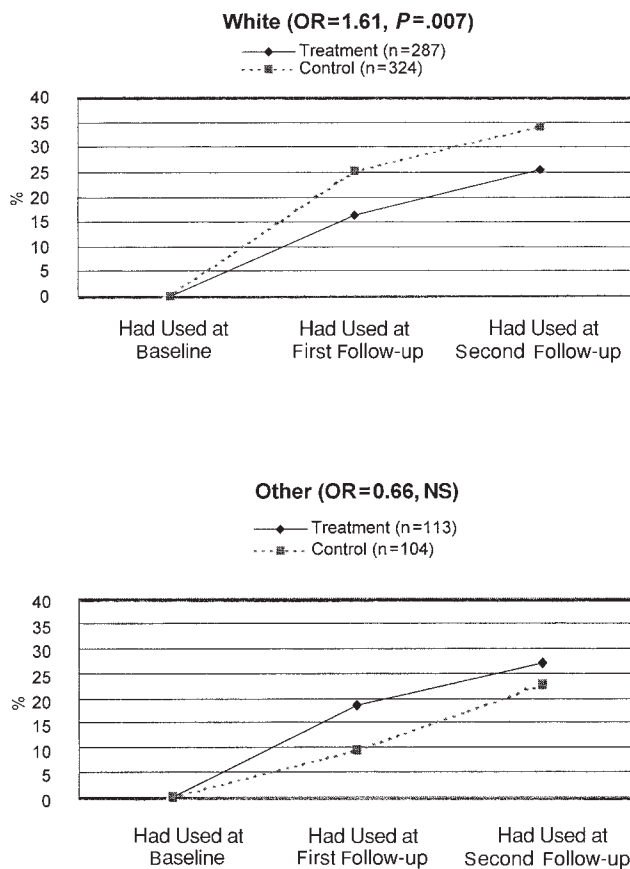
Discussion

Families that received the Family Matters intervention had 16.4% fewer adolescent smoking initiators at the second follow-up at 1 year than did families that did not receive the program. Because the program effect was moder-



ated by race/ethnicity, non-Hispanic White families that received the program had 25.0% fewer smoking initiators than did families of other race/ethnicity. These reductions translate into effect sizes of 0.15 for the total sample and 0.25 for non-Hispanic Whites. These effects compare favorably with the 0.18 effect size found

for tobacco use in the most effective school-based curricula evaluated with randomized experimental designs.⁴⁷ The effects for smoking were larger than those at 6 and 18 months reported by Spoth et al. (R. L. Spoth, PhD, C. Shin, PhD, unpublished data, 1999), and it is noteworthy that they found even larger pro-



Note. Ninety-five percent lower bound (LB) for the 1-sided interval for higher odds of use in control. OR=odds ratio; NS=not significant.

FIGURE 2—Percentage of cigarette nonusers at baseline who began use, by race/ethnicity (non-Hispanic Whites and others).

gram effects for smoking, alcohol, and marijuana use 3 and 4 years after their program.

The observed difference between treatment and control trends for alcohol use initiation, although not statistically significant, was similar to that for cigarettes (Figure 1). Alcohol use initiation was 11.1% lower for the program group than for the control group at the second follow-up, less than the 16.4% observed for smoking onset but still substantial. Our alcohol use onset sample was substantially smaller than our smoking onset sample, because there were fewer nonusers of alcohol than nonusers of cigarettes at baseline. We would have required nearly 3 times more nonusers of alcohol at baseline, and the odds ratio of 1.26 we found for alcohol use, to have obtained significance at the $P=.05$ level (1-tailed, power=.80). Given the actual

number of baseline nonusers of alcohol, however, we must conclude that there is no evidence of program influence for adolescent alcohol use. Larger samples than ours would be needed for assessment of program effects on smokeless tobacco use, because too few in our sample reported that behavior.

Why were effects observed for smoking but not for smokeless tobacco use or drinking? So few adolescents reported smokeless tobacco use that we consider our sample size inadequate for assessing program effects for that behavior; therefore, we offer no speculation regarding lack of program effects for smokeless tobacco use onset. There are reasons, in addition to the sample size possibility presented earlier, that the program might have reduced smoking but not alcohol use onset. Perhaps the considerable negative pub-

licity and social stigma associated with tobacco use, compared with that accorded to alcohol use, caused families to take adolescent smoking more seriously than adolescent drinking, as reflected in how they treated the respective behaviors when delivering the program to their children. This and other speculations remain topics for future research.

Why were program effects for smoking onset found for non-Hispanic Whites but not for the other racial/ethnic group? In our data at baseline, there were 3 times as many non-Hispanic White nonusers as there were nonusers in the other group, and we would have needed approximately 3 times more nonusers in the other group to have obtained significance at the $P=.05$ level (1-tailed, power=.80) for the observed odds ratio of 0.66. Given the actual number of respondents we had, however, we conclude that there is no evidence of program influence on smoking onset by persons other than non-Hispanic Whites, and we offer no speculation about the potential for negative effects in this population.

We cannot explain why the program appears to have influenced smoking initiation for non-Hispanic Whites but not for the other racial/ethnic group. Our strategy was to develop a program that would be equally acceptable to all segments of the population, and this goal was reflected in the racial/ethnic neutral appearance of the booklets and in the content presentation. Perhaps non-Hispanic Whites are accustomed to neutral presentations, while others find them less appealing. To minimize program cost, race/ethnicity was not used as a criterion for assigning health educators to families; this, too, might have caused the difference in effectiveness. Perhaps programs need to be tailored for different racial/ethnic groups or made more attractive to all groups.⁴⁸ Future studies are required to address such questions.

Our findings suggest that the program was effective for preventing the onset of smoking. It might have been more effective if more families selected for the treatment group had participated in and completed the program. Involving families has been a key obstacle to successful implementation of family-directed programs not closely tied to the adolescent's school homework assignments. Participation in our program, however, compared favorably with that in other programs, presumably because the demands on the family for participation were relatively low. The favorable participation rate may limit generalizability of our findings to those of research conducted by others. We consider factors that influenced participation in Family Matters, and participation in other family-directed programs, in more detail elsewhere.⁴⁰

Could the program be disseminated outside the context of our research? Although costs

for comparable programs are unavailable, the \$140.42 per eligible case required for implementing Family Matters does not appear excessive.²⁰ Moreover, implementation costs may be able to be reduced. The findings on program effects for alcohol use suggest that cost might be reduced by limiting the program to consideration of cigarette smoking, which may require fewer booklets and follow-ups by health educators. Moreover, we have yet to identify the factors that explain the program effects for smoking in our data, and eliminating content or processes that do not contribute to effects could reduce the number of booklets and health educator follow-ups required. Identification of such variables also could strengthen our conclusions about program effects. Additional analyses of our data, and evaluation of program variations that might be suggested by such analyses, are indicated.

The findings must be viewed in the context of several possible methodological limitations. First, not all families eligible for the study completed interviews at all data collection points. Attrition may have been increased by reliance on the telephone for data collection.⁴⁴ For example, the disdain for telemarketing by some might have reduced participation, and the disparity in telephone availability between lower- and higher-income households likely caused lower-income families to be underrepresented. Regardless of reason, attrition may limit the extent to which the findings can be generalized to the population and may have biased our findings on program effects.

Second, some families randomly chosen to receive the program did not participate, and others began the program but did not complete all parts. Our treatment group likely differed from the total population and from the control group, which could limit generalizability and internal validity.

Third, as in all other studies, our measures contained error. Of particular concern is that some adolescents were unlikely to reveal their tobacco use.^{49,50} This problem may have been magnified by reliance on the telephone for data collection. We informed participants about the legitimacy of the study and the importance of providing accurate information. Because parents often were home during the adolescent interviews, adolescents were asked whether a parent could hear their answers. When parents were in listening range, adolescents were asked to correct that situation, or the interview was rescheduled for a time when they could respond in private. Moreover, potentially sensitive questions were answered yes or no rather than by revealing a specific behavior out loud. We would have liked to include biochemical measures to supplement adolescent self-reports of tobacco and alcohol use,^{50,51} but that was precluded by telephone data collection.

Finally, there may have been a difference between treatment and control in the validity of self-reports. For example, knowledge that parents were in the program might have influenced adolescent reports. Our study design did not allow testing for this.

Conclusions

The findings suggest that Family Matters reduced smoking onset for non-Hispanic Whites. The effect sizes for the evaluated program compared favorably with those for school-based curricula designed to prevent adolescent drug use, which have benefited from more years of development and rigorous evaluation. No evidence indicated that the program influenced onset of adolescent smokeless tobacco or alcohol use. These findings suggest that Family Matters, and other family-directed programs, are worthy of further application, development, and evaluation. □

Contributors

All authors participated in the writing of the paper. K. E. Bauman, V. A. Foshee, and S. T. Ennett were responsible for creation and implementation of the program and for the research to evaluate the program. M. Pemberton was responsible for data management and analysis. K. A. Hicks was the project manager for program implementation and data collection. T. S. King and G. G. Koch were responsible for determining statistical applications.

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The School of Public Health Institutional Review Board for the Protection of Human Subjects at The University of North Carolina at Chapel Hill approved this research.

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