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A review of 25 long-term adolescent tobacco and other drug use prevention program evaluations

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Abstract

Background. Although the initial effectiveness of psychosocial strategies programming in preventing smoking and other drug abuse among adolescents has been well established through literature reviews and meta-analyses, much less evidence exists for the long-term follow-up success of these interventions. The primary goal of this paper, therefore, is to summarize the effectiveness of published program evaluation studies that have followed adolescents across the transitional period between junior high and high school for a period of at least 2 years.

Methods. Studies for inclusion in this review were accessed primarily through a computerized search of Medline, Healthstar, and PsychINFO databases. Intervention studies that met five core criteria were retained for review. Two authors independently abstracted data on study characteristics, methodology, and program outcomes.

Results. Search results yielded 25 studies suitable for examination. The majority of these studies reported significant program effects for long-term smoking, alcohol, and marijuana outcomes, while indicating a fairly consistent magnitude of program effects.

Conclusions. This review provides long-term empirical evidence of the effectiveness of social influences programs in preventing or reducing substance use for up to 15 years after completion of programming. However, this conclusion is still somewhat tenuous given the lack of significant program effects reported in several studies and the great variability that existed in the level of internal and external validity across all studies.

Keywords: Long-term; Prevention; Intervention; Tobacco; Smoking; Alcohol; Marijuana; Drug; Adolescents

Introduction

Despite recent national data indicating small and sporadic declines in adolescent drug use over the past decade [1], tobacco, alcohol, and marijuana are still the most widely abused substances by both younger and older American teenagers. In 2001, the proportions of 8th, 10th, and 12th graders who reported use of cigarettes in the past 30 days were 12.2, 21.3, and 29.5%, respectively. At the same time, alcohol and marijuana use remained extremely widespread with respective 30-day prevalence rates of 21.5, 39.0, and 49.8% for alcohol use and 9.2, 19.8, and 22.4% for marijuana use.

The period of peak risk for the onset of drug problems begins in adolescence, a period spanning the ages 10 to 20. During the early adolescent years, individuals experience major biological, cognitive, social, and emotional changes that influence behavioral choices, such as experimentation with health-compromising substances including tobacco and alcohol, the gateway drugs that increase the likelihood of progression to more advanced forms of drug use [2]. For a significant number of individuals, such risk-taking behavior may lead to the formation of more enduring health behavior patterns including a lifetime development of drug dependence [3]. Moreover, those who initiate drug use at an earlier age are at greater risk for later drug abuse [4]. Thus, preventing or delaying the onset of drug use in early adolescence is of critical importance.

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Over the past few decades, wellness advocates [5] stimulated considerable drug prevention research efforts that have resulted in a variety of educational programs that typically address the primary prevention of tobacco, alcohol, and marijuana use among adolescents through school-based programs. The most recent and promising prevention approaches are based on the psychosocial influences that promote drug use initiation. The two major psychosocial approaches that have been adopted by schools are the social influences approach [6] and the more comprehensive personal and social competence enhancement (Life Skills Training) strategy [7]. Social influences programs are designed to increase the awareness of the social influences promoting drug use, alter norms regarding the prevalence and acceptability of drug use, and build drug resistance skills. Personal and social competence enhancement programs incorporate aspects of the social influence approach and also include general self-management and social competence skills.

Literature reviews [8–10] and meta-analyses [11–13] of these social influences programs have indicated short-term (under 24 months) reductions in the rate of initiation of tobacco use generally ranging from 30 to 50% or more in students exposed to social influences programs compared to control students. To a lesser extent, positive short-term results for alcohol and marijuana use have also been reported [12,13], with reductions typically ranging from 15 to 30% or more for alcohol and marijuana use. Although the initial effectiveness of psychosocial strategies programming in preventing—or at least delaying—smoking and other drug abuse among adolescents has been demonstrated, much less evidence exists for the long-term follow-up success of these substance use interventions. In fact, the current and prevalent folk wisdom—even among drug prevention scientists—is that initial program effects will necessarily start to decay and disappear altogether once programming has concluded. This notion was generated in the absence of a systematic review of existing empirical studies that have assessed program effects for a minimum 24-month follow-up period.

To date, there has been no specific review of the long-term effectiveness of tobacco, alcohol, and other drug use prevention programming among adolescents. The goal of this paper, therefore, is to summarize empirical evidence on the effectiveness of school- and community-based prevention programs by critically reviewing all published long-term evaluation studies that have followed adolescents (across the transitional period between junior high and high school) for a period of at least 2 years. Information from the individual studies is examined to obtain an overview of research designs and methodologies of the prevention trials that have been implemented and to summarize the effectiveness of these evaluations in terms of percentage of reductions in drug use in adolescence. Also of interest is whether the preventive effects, found in studies that demonstrated strong initial results, are maintained beyond 2 years. Implications of the findings are then considered for future prevention programming activity.

Methods

Studies for inclusion in this review were accessed primarily through a computerized search of Medline (1966–October 2002), Healthstar (1975–October 2002), and PsychINFO (1887–October 2002) databases, using the keywords "tobacco," "smoking," "drug," "prevention," and/or "intervention." The search was restricted to English-language articles. Further relevant published literature was identified from the reference lists of papers detected by this literature search; previous reviews and meta-analyses of tobacco and drug abuse prevention programming; Web sites of the U.S. Department of Health and Human Services [14] and the Centers for Disease Control and Prevention [15]; and contact with first authors to request updated information on unpublished work or research in progress. For many of the studies, multiple published articles were available and examined as a set.

This review included intervention evaluation studies that met all of the following five core criteria: (a) intervention populations included subjects under the age of 21 at baseline; (b) at least one of the interventions or modalities evaluated was school- or community-based; (c) at least one of the outcomes assessed in the study included tobacco incidence or prevalence use rates; (d) studies consisted of at least a quasi-experimental design, which includes a program group comparison to a control group; and (e) evaluations provided at least a 2-year follow-up on subjects between the ages of 12–15 and 16–19, the transitional period between junior high and high school.

As presented in Table 1, the search results yielded 25 studies that were suitable for examination [16–59]. Two of the authors independently abstracted data from each of the studies on the following information: (a) study identification: investigator(s), publication information, project name, years data collected; (b) methods: study design, unit of assignment to condition, social mixing, evaluation measures, biochemical validation; (c) programming characteristics: program contents, modality of programming, teacher in-service, number of program sessions and boosters; (d) recruitment: grade of baseline sample, "through-study" age range, percentage female, percentage white, percentage enrolled in study of those approached, number of schools at baseline, number of subjects at baseline; (e) retention: tracking of subjects, length of follow-up, percentage followed at last wave of collection, number of schools at follow-up, number of subjects at follow-up; (f) description of analysis: assessment of baseline equivalence, confounders, attrition, implementation fidelity and student exposure to programming, unit of analysis, statistical tests; and (g) outcomes for tobacco and other drug use (initial and long-term effects). If necessary, discrepancies in data abstraction were jointly

Table 1
Study identification

Investigators	Key articles and texts where results reported/ additional articles reviewed	Project name/site	Years data collected
Abernathy [16]	Can J Public Health, 1992	Peer Assisted Learning (PAL), Calgary, Canada	1988–1991
Aveyard [17]	Prev Med, 2001; Aveyard et al.; BMJ, 1999[41]	Transtheoretical Model (TTM), West Midlands, England	1997–1999
Bergamaschi [18]	Subst Use Misuse, 2000	Leave Us Clean, Romagna, Italy	1993–1997
Botvin [19]	JAMA, 1995; Botvin et al., J Consult Clin Psychol, 1990[42]	Life Skills Training Program, NY	1985–1991
Cuijpers [20]	Addiction, 2002	Healthy School and Drugs Project, The Netherlands	1990's
Del Greco [21]	Adolescence, 1986	Assertiveness Training, Amherst, NY	1976–1980
Elder [22]	Am J Public Health, 1993; Eckhardt et al., Am J Health Promot, 1997[43]	Project SHOUT, San Diego, CA	1988–1992
Ellickson [23]	Am J Public Health, 1993; Ellickson & Bell, Science, 1990[44]	Project ALERT, CA and OR	1984–1989
Flay [24]	Am J Public Health, 1989; Flay et al., J Behav Med, 1985[45]	Waterloo Smoking Prevention Project, Waterloo, Canada	1979–1985
Flynn [25]	Am J Public Health, 1994; Flynn et al., Am J Public Health, 1992[46]	Mass Media and School Intervention, VT, NY, and MT	1985–1991
Hansen [26]	Health Educ Q, 1988	Tobacco and Alcohol Prevention Program (TAPP), Los Angeles, CA	1981–1984
Klepp [27]	Prev Med, 1993; Tell et al., Prev Med, 1984[47]	Oslo Youth Study, Oslo, Norway	1979–1989
Murray [28]	J Behav Med, 1989; Murray et al., J Behav Med, 1988[48]; Murray et al., J Behav Med, 1987[49]	Peer-taught Smoking Prevention, MN	1979–1986
Pentz [29]	Under Review, 2003; Pentz et al., Prev Med, 1989[50]	Midwestern Prevention Project (MPP), Kansas City, KS and MO	1984–1990
Perry [30]	Am J Public Health, 1992; Perry et al., Health Educ Res: Theory Prac, 1989[51]	Class of 1989 Study/Minnesota Heart Health Program, ND and MN	1983–1989
Peterson [31]	J Natl Cancer Inst, 2000	Hutchinson Smoking Prevention Project, WA	1984–1999
Shean [32]	Aust J Public Health, 1994; Armstrong et al., Med J Aust, 1990[52]	University of Minnesota (social consequences curriculum), Western Australia	1981–1988
Shope [33]	J Drug Educ., 1998; Shope et al., J Drug Educ., 1996[53]	Michigan Model for Comprehensive School Health Education, MI	1988–1993
St. Pierre [34]	Am J Community Psychol, 1992	Stay SMART, across United States	1988–1990
Sussman [35]	Sage Publications (Book), 1995; Dent et al., J Consult Clin Psychol, 1995[54]	Project Towards No Tobacco Use, Southern CA	1989–1992
Taylor [36]	Prev Sci, 2000; Hansen & Graham, Prev Med, 1991[55]	Adolescent Alcohol Prevention Trial (AAPT), Los Angeles and Orange Counties, CA	1987–1992
Telch [37]	J Behav Med, 1982; McAlister et al., Am J Public Health, 1980[56]	Project CLASP, CA	1977–1980
Vartiainen [38]	Am J Public Health, 1998; Vartiainen et al., Am J Public Health, 1990[57]; Vartiainen et al., Prev Med, 1986[58]	North Karelia Youth Project, North Karelia, Finland	1978–1993
Walter [39]	J Natl Caner Inst, 1989; Walter et al., Am J Prev Med, 1986[59]	Know Your Body, New York, NY	1979–1985
Winkleby [40]	Prev Med, 1993	Stanford Five-City Project, Northern CA	1979–1990

reviewed until consensus was reached. When data were not reported in the key articles, additional information was obtained from the original authors or publications relevant to the intervention evaluation.

It should be noted that all studies had a school-based programming component except the St. Pierre et al. study [34] which was implemented in a community setting only. This review was thought to be inclusive and therefore retained this community-based study because it implemented programming that was adapted from a school-based curriculum and was the only study of its kind that provided a long-term follow-up assessment of adolescents between the ages of 12–15 and 16–19. Notwithstanding the inclusion of this study, the results of this review are essentially based on interventions that provided at least one programming component in the school setting.

Results

Methodological designs

Tables 2 and 3 present detailed information on the methodological designs of the 25 studies reviewed. The majority of the prevention intervention studies utilized a quasi-experimental design; 14 were quasi-experimental and 11 were

Table 2
Methodological design

Investigators	Study design	Unit of assignment to condition	Social mixing during postintervention
Abernathy [16]	Experimental—two condition: program, control	School	NR
Aveyard [17]	Experimental—two condition: program, control	School	NR
Bergamaschi [18]	Quasi-experimental—two condition: campaign, noncampaign	Individual	NR
Botvin [19]	Experimental—three condition: workshop training, videotape training, control	School	NR
Cuijpers [20]	Quasi-experimental—two condition: program, control	School	NR
Del Greco [21]	Quasi-experimental—three condition: assertiveness training + innovative smoking education, innovative smoking education only, control	Classroom	NR
Elder [22]	Experimental—four condition: continued intervention, lapsed intervention, delayed intervention, continued control	Individual	"Minimal because home-based treatment"
Ellickson [23]	Experimental—three condition: adult-led, older teen-led, control	School	NR
Flay [24]	Experimental—two condition: program, control	School	NR
Flynn [25]	Quasi-experimental—two condition: media (community-wide) and school intervention, school intervention only	Community	NR
Hansen [26]	Quasi-experimental—two condition: program, comparison	School	NR
Klepp [27]	Quasi-experimental—two condition: program, control	School	NR
Murray [28]	Quasi-experimental—five condition: adult-led video, peer-assisted skills practice, peer-assisted video, adult-led long term consequences, comparison	School	NR
Pentz [29]	Quasi-experimental—two condition: intervention community, delayed intervention control community	School	NR
Perry [30]	Quasi-experimental—two condition: intervention community, control community	Community	NR
Peterson [31]	Experimental—two condition: program, control	School district	No
Shean [32]	Experimental—three condition: teacher-led, peer-led, control	School	NR
Shope [33]	Quasi-experimental—two condition: program, comparison	Classroom	NR
St. Pierre [34]	Quasi-experimental—three condition: program, program with 2-year booster, control	Boys & Girls Club	NR
Sussman [35]	Experimental—five condition: normative social influence, informational social influence, physical consequences, combined, control	School	NR
Taylor [36]	Experimental—four condition: combined normative education and information plus resistance training, normative education and information, resistance training and information, control	School	NR
Telch [37]	Quasi-experimental—two condition: older peer-led, control	School	NR
Vartiainen [38]	Quasi-experimental—three condition: health educator-led, teacher-led, control	School	NR
Walter [39]	Experimental—two condition: program, control	School	NR
Winkleby [40]	Quasi-experimental—two condition: intervention community, control community	Individual	NR

Note. NR; not reported.

experimental. Of the 14 quasi-experimental evaluations, all used the nonequivalent group, pretest–posttest design, except 1 study (i.e., Bergamaschi et al. [18]) that chose the nonequivalent group, posttest only design. All 11 of the experimental studies employed a pretest–posttest, control group design, with the majority using more than one comparison treatment group [19,22,23,35,36]. For example, Taylor and colleagues selected a four-condition (group) design with randomization to normative education and information, resistance training and information, combined normative education and information plus resistance training, or control— enabling examination of the relative effectiveness of each treatment group. Almost half of the studies [16,19,23–25,27,29–32,36,38] included matched designs, in which pairs of schools or students were matched on important characteristics (e.g., pretest drug use scores or demographic variables) and then were randomly assigned to one of the treatment conditions. Other studies also used blocking procedures [23,28,35,38] to help ensure pretreatment equivalence by, for example, including at least one school from each district in each experimental condition.

The method of assignment to condition varied across the studies with almost two-thirds of the studies selecting school as the unit of assignment [16,17,19,20,23,24,26–29,32,35–39]. Classrooms [21,33], Boys & Girls Clubs of America (a youth-serving community organization) [34],

Table 3
Methodological design

Investigators	Smoking categories assessed (in treatment vs control group comparisons)	Biochemical validation
Abernathy [16]	Never vs any	NR
Aveyard [17]	Regular weekly (weighted average of one or more cigarettes per week and at least one cigarette per day)—yes/no	NR
Bergamaschi [18]	Non smoker—yes/no; experimenter—yes/no; occasional (within past month)—yes/no; current (daily)—yes/no	NR
Botvin [19]	Monthly—yes/no; weekly—yes/no; "heavy" cigarette smoking (a pack or more a day)—yes/no; tobacco and alcohol monthly—yes/no; tobacco and alcohol weekly—yes/no; tobacco and marijuana weekly—yes/no; tobacco, alcohol and marijuana monthly—yes/no; tobacco, alcohol and marijuana weekly—yes/no.	Expired air
Cuijpers [20]	Current use—yes/no; daily use—yes/no; mean No. cigarettes per week	NR
Del Greco [21]	Non smoker vs smoker (no definitions)	NR
Elder [22]	Past month (at least once)—yes/no	"Bogus pipeline" (in grades 7, 8, and 9 only)
Ellickson [23]	Lifetime—yes/no; past year—yes/no; monthly—yes/no; weekly—yes/no; daily—yes/no	Saliva
Flay [24]	Tried vs never; quit vs never; experiment vs never; regular vs never	Saliva thiocyanate
Flyna [25]	Weekly—yes/no; daily—yes/no; self-selected smoking	Saliva
Hansen [26]	30-day use—yes/no	Saliva (not all posttests)
Klepp [27]	Ever (never vs occasionally); weekly (not weekly vs at least weekly)	Serum thiocyanate
Murray [28]	Weekly—yes/no; daily—yes/no; light smoking (at least daily but no more than 10 cigarettes per day)—yes/no; heavy smoking (at least 25 cigarettes per day)—yes/no; number of cigarettes per week	Saliva thiocyanate and expired air
Pentz [29]	Last month—yes/no; last week—yes/no; heavy use (past 24 h)—yes/no	Expired air
Perry [30]	Weekly; smoking intensity (cigarettes smoked per week)	Saliva (in 1986 only)
Peterson [31]	Any; monthly; weekly; daily; smoking frequency; smoking acquisition stage; No. of cigarettes per day; cumulative smoking (over 100 cigarettes in lifetime)	Saliva cotinine
Shean [32]	Current smoker (smoked one or more cigarettes a day regularly for 6 months)—yes/no; non smoker (including ex-smoker)—yes/no	Saliva (at baseline only)
Shope [33]	Frequency of use: (0)never to (5) every day	NR
St. Pierre [34]	Never used or used more than a year ago vs used in last year; cigarette-related behavior scale (frequency and intention to use, 1–12)	NR
Sussman [35]	Ever tried cigarettes—yes/no; ever tried smokeless tobacco—yes/no; weekly cigarette use—yes/no; weekly smokeless tobacco use—yes/no	Saliva or expired air
Taylor [36]	Recent alcohol use index; lifetime alcohol use; lifetime drunkenness; recent cigarette use index; lifetime cigarette use	No
Telch [37]	Weekly—yes/no; monthly—yes/no	Expired air
Vartiainen [38]	Any—yes/no; monthly—yes/no; weekly—yes/no; daily—yes/no	Serum thiocyanate (at baseline, 1980, and 1981 only)
Walter [39]	Smoking initiation—yes/no (as determined by biochemical indicator only)	Serum thiocyanate or saliva continine
Winkleby [40]	Daily (ever smoking on a daily basis and had smoked one or more cigarettes in the past week)—yes/no	Expired air and plasma thiocyanate

Note. NR, not reported.

and individual subjects [18,22,40] were also used as the unit of assignment. Two quasi-experimental studies selected community as the assignment unit [25,30], and the Hutchinson Smoking Prevention Project [31] randomly assigned entire school districts to treatment conditions. The latter study specifically indicated that it sought to minimize social mixing of subjects between treatment groups by its assignment protocol. The St. Pierre et al. study [34] is assumed to have experienced no treatment diffusion among units of assignment because programming was implemented at separate and distant Boys & Girls Clubs across the country. Further, one other study [22] suggested that it incurred minimal treatment contamination potentially caused by social mixing of participants due to the use of randomized assignment of entire schools to experimental conditions and because interventions were conducted directly through the home rather than school environment.

Evaluation measures included self-report questionnaire items on lifetime or recent use of cigarettes, alcohol, or other drugs for all but 1 of the studies [39]. Of these 24 studies, cigarette smoking was typically assessed by questions relating to the frequency of lifetime, monthly, weekly, or daily use. In the vast majority of studies, each frequency measure was recoded into a dichotomous outcome (yes or no), with lifetime (ever, any, etc.) cigarette use [16,18,20,21,23,24,27,31,32,35,38] being the smoking category most often evaluated in this fashion, followed by monthly (30-day use) [18,19,22,23,26,29,31,37,38] and weekly [19,23,25,28,29,31,35,37,38], and then daily use [18–20,23,25,28,38]. Some studies evaluated smoking by

Table 4
Programming characteristics

Investigators	Program contents	Modality of programming	Teacher in-service	Number of school-based prevention sessions offered/booster programming
Abernathy [16]	Comprehensive social influences	School-based	1	6 sessions (over 3 months)
Aveyard [17]	Social influences (informational), stages of change	School-based, computer	NR	6 sessions (3 class lessons and 3 computer sessions throughout 9th grade)
Bergamaschi [18]	Social influences (normative)	School-based	Yes	6 sessions
Botvin [19]	Comprehensive social influences, Life Skills, STC and LTC	School-based, audiotape	1-day training workshop	15 sessions + 15 boosters (10 in 8th grade and 5 in 9th grade)
Cuijpers [20]	Comprehensive social influences	School-based, video	Several specialized training courses held regularly	9 sessions (3 in 1st year, 3 in 1st or 2nd year, 3 in 2nd or 3rd year)
Del Greco [21]	Social influences (normative), assertiveness training, STC	School-based	NA (researchers taught)	15 sessions (over 3 weeks)
Elder [22]	Comprehensive social influences	School-based	Undergraduate students received 15 h of training	18 sessions (over 7th and 8th grade) + boosters (5 newsletters and 4 phone calls in 9th grade and 2 newsletters and 1 phone call in 11th grade)
Ellickson [23]	Comprehensive social influences	School-based, peer leaders	NR	8 sessions (in 7th grade) + 3 boosters (in 8th grade)
Flay [24]	Comprehensive social influences, public commitment	School-based, video	Specially trained health education specialists	6 sessions + 5 boosters (2 in 6th grade, 2 in 7th grade and 1 in 8th grade)
Flynn [25]	Comprehensive social influences	School-based, community-wide (mass media)	4 annual day-long training workshops	22 sessions (4 per year in 5th thru 8th grades and 3 per year in 9th and 10th grades)
Hansen [26]	Comprehensive social influences, public commitment, STC and LTC	School-based, peer leaders	2-day seminar	15 sessions
Klepp [27]	Comprehensive social influences, public commitment	School-based, peer leaders	NR	10 sessions (over school year)
Murray [28]	Comprehensive social influences, public commitment, STC and LTC	School-based, peer leaders, videos	Teachers trained by project staff	5 sessions (over 6 months)
Pentz [29]	Comprehensive social influences	School-based, peer leaders, community-wide, mass media, parent, local health policy change	Initial 3-day workshop and annual 1-day refresher workshops	10 sessions + 5 boosters (in the 2nd year)
Perry [30]	Comprehensive social influences, public commitment, STC	School-based, peer leaders, community-wide	Peer leaders trained by community staff	5 health education programs (1 session in 6th grade, 6 sessions and 1 booster in 7th grade, 1 session and 1 booster in 8th grade, 6 sessions in 9th grade, and 7 sessions in 10th grade)
Peterson [31]	Comprehensive social influences	School-based	Yes	65 sessions (9 per year in 4th and 5th grades, 10 per year in 6th and 7th grades, 8 in 8th grade, and 5 per year in 9th and 10th grades)
Shean [32]	Comprehensive social influences, STC and LTC	School-based, peer leaders	Yes	5 sessions (over 6 months)
Shope [33]	Social influences (normative), STC	School-based	1 day	23 sessions (7 in 6th grade and 8 per year in 7th and 8th grades)
St. Pierre [34]	Comprehensive social influences, Life Skills	Community-based (Boys & Girls Clubs), peer leaders, videos	Yes	12 sessions + 8 boosters (over 2 years)
Sussman [35]	Comprehensive social influences, component analysis	School-based, videos	Health educators received 3 weeks (120 hours) of training	10 sessions (over 2 weeks) + 1 booster (2 days in 8th grade)
Taylor [36]	Comprehensive social influences	School-based	Project staff received 2 weeks of intensive training	10 sessions

Table 4 (*continued*)

Investigators	Program contents	Modality of programming	Teacher in-service	Number of school-based prevention sessions offered/booster programming
Telch [37]	Comprehensive social influences, public commitment	School-based, peer leaders, films	Peer leaders received 3 2-h trainings	7 sessions (over 9 months)
Vartiainen [38]	Comprehensive social influences, STC and LTC	School-based, community-wide for adults	Health educators taught; Peer leaders received 10 h of training	10 sessions (3 in 7th grade, 5 in 8th grade and 2 in 9th grade)
Walter [39]	Social influences (normative), biofeedback, stress management, LTC	School-based	3 half-day teacher workshops	384 sessions (2 h per week throughout each school year)
Winkleby [40]	Social influences (normative), adult cessation generalization	School-based, peer leaders, community-wide for adults (media, print, and direct education)	NR	7 sessions

Note. NR, not reported; NA, not available; STC, short-term consequences; LTC, long-term consequences.

comparing other dichotomous smoking categories (e.g., light vs heavy, regular vs never) or creating cigarette smoking scales or indices [16,20,21,24,27,28,30,31,33,34,36,40].

More than two-thirds [19,23–32,35,37–40] of the studies utilized biochemical validation or the bogus pipeline technique [22]. Subjects were measured on expired-air carbon monoxide, thiocyanate (saliva or plasma), or cotinine (saliva or plasma) levels in conjunction with self-report questionnaires in all but 1 of these studies. However, Walter et al. [39] determined smoking initiation by biochemical indicator (serum thiocyanate or saliva cotinine) only.

Program contents

Programming characteristics are provided in Table 4. The program contents of all 25 studies included prevention strategies that addressed the issues of social influences to smoke and the development of skills to resist such pressures. Nineteen studies were comprehensive social influences-oriented, designed to impact both normative and informational social influences [16,19,20,22–32,34–38], 5 were aimed at counteracting only normative social influences [18,21,33,39,40], and 1 aimed at counteracting only informational social influences [17]. Normative social influence relates to group influences over an individual that exert pressure on an individual to conform due to a desire to be like others and in order to avoid punishments or obtain rewards, such as group acceptance [20]. To counteract these social pressures to smoke, programs instruct individuals to identify situational pressures and teach skills to refuse offers while maintaining group membership. Informational social influence refers to more covert pressures upon a person to accept information—including peer opinions and beliefs—obtained from another as evidence about reality. The goal of an informational social influences program is to counteract pressures to adopt attitudes and values favorable to tobacco use by providing students with correct perceptions about the prevalence and acceptability of smoking in the peer group.

Two studies [19,34] supplemented their social influences-based curricula with Life Skills Training, programming designed to teach general life skills and competence that could be used in dealing with situations involving peer pressure to smoke, drink, or use drugs, as well as the many other challenges adolescents confront in their everyday lives. Five studies provided factual information about the short- and long-term effects of tobacco use [19,26,28,32,38], while 3 studies focused on only the short-term consequences [21,30,33] and 1 emphasized information on only the long-term health consequences [39]. Six studies included a public commitment component whereby students made a commitment to remain a nonsmoker or not to become a regular smoker [24,26–28,30,37].

The modality of programming of the studies presented was predominantly the single-site school setting. All but 1 of the prevention projects was school-based; the St. Pierre et al. study [34] delivered a prevention program, adapted from a school-based curriculum, to adolescents attending Boys & Girls Clubs of America. Five studies complemented prevention efforts in the schools with a community component that involved intervention programming through such channels as parents, mass media, or health policy change [25,29,30,38,40]. Seven of the studies used videos, films, or audiotapes [19,20,24,28,34,35,37], and 1 study used computers to help deliver the program [17]. Although the majority of the interventions featured implementation of programming by teachers and research staff, 10 studies included peer leaders in the delivery of at least one of the program components [23,26–30,32,34,37,40], in many cases to enhance the believability of normative information on drug use. Across all studies, little or vague information was reported on the type and amount of training provided to the teaching personnel responsible for delivering the programs to the students. All but 4 studies [17,23,27,40] indicated that at least some type of training was provided to classroom teachers, health education specialists, project

staff researchers, or peer leaders who implemented the programming; however, the training practices were, for the most part, virtually unreported. Various study descriptions indicated that in-service training ranged from 1-day workshops for regular classroom teachers [19,33] to 3 weeks (120 h) of training for health educators [35] who had delivered the curricula.

The number of regular (or nonbooster) prevention sessions delivered by these projects varied widely, ranging from 5 to 384, with both a median and a mode of 10 sessions. The period of time over which curriculum programming was implemented also varied greatly, with complete programs being delivered over 2 weeks [35] to 8 years [31]. For example, on the lower side of frequency (or intensity) of programming, Murray and colleagues [28] implemented the Peer Taught Smoking Prevention Project in Minnesota which offered 5 sessions to 7th grade students over a 6-month period. Similarly, Shean and colleagues [32] delivered a 5-session prevention program, based on Murray's curriculum but slightly modified for Western Australian students, to 7th grade students over a 6-month period. As for projects with greater frequency of sessions, the Know Your Body curriculum [39] was taught over a 6-year period for approximately 2 h per week throughout each school year, beginning in 4th grade and continuing consecutively through 9th grade. The Hutchinson Smoking Prevention Project curriculum [31] consisted of 65 lessons delivered over an 8-year period: 9 lessons in grades 3, 4, and 5; 10 lessons in grades 6 and 7; 8 lessons in grade 8; and 5 lessons in grades 9 and 10.

Although only 8 studies specifically indicated that their programs were designed to include a booster component [19,22–24,29,30,34,35], an additional 6 studies implemented additional intervention sessions or components over a span of at least two grades (or years), suggesting booster programming [20,25,31,33,38,39]. Booster components were delivered through various modes including classroom lessons, newsletters, phone calls, media messages, and computer assignments. The number of specifically indicated booster sessions or components ranged from 1 booster session conducted over 2 days [35] to 15 boosters delivered over 2 years [19].

Target population characteristics

Table 5 presents details concerning participant recruitment. The majority of studies recruited 7th grade students, most of whom were between 12 and 13 years old, for participation in the smoking prevention interventions [18–23,26–29,32,35–38,40]. However, several studies chose to implement interventions well before junior high school; these baseline study samples consisted of students from grade 3 [31], grade 4 [25,39], grade 5 [27], and grade 6 [16,24,26,29,30,33] classes. When considering all interventions, participants ranged from approximately 8 to 28 years of age throughout the entire life of the studies ("through-study age range").

Gender was fairly evenly distributed across all of the studies with female representation ranging from 48 to 54% of the sample; however, there was one exception where females composed only 25% of the study sample [34]. Further, 4 studies neglected to report gender composition [24,25,30,38]. In terms of ethnicity, studies predominantly focused on white populations. Although 10 studies did not report ethnicity characteristics, at least 9 studies indicated that study samples consisted of over 75% white participation [2,4,6,10,14,16,18,24,25]. Of the remaining studies, the range of white participants was 45 to 67% of the sample [7,8,11,19,20,21]. The percentage of overall participant recruitment, i.e., the percentage enrolled in the study of those approached, was reported for only 5 studies; recruitment ranged from 82 to 98% [17,27,28,31,39] in these studies. The total number of schools enrolled at baseline varied widely across all studies with a range of 1 to 190 schools. Further, 1 large-scale study enrolled 40 whole school districts at baseline [31], and another 2 large-scale [25,33] and 2 small-scale [18,40] studies neglected to report this information. The number of individual subjects included in baseline samples ranged from 161 to 8388, with an average baseline sample across all studies being 3445 individual subjects.

Follow-up population characteristics

Table 6 presents information on the samples assessed at follow-up. Twenty-two of the studies tracked participants across the entire length of the studies, with 2 of these studies utilizing a combination of cross-sectional and longitudinal data to evaluate long-term program effectiveness [29,30]. However, 3 studies did not follow the same subjects over time, thereby providing only cross-sectional measurements [18,37,40]. The length of follow-up for all 25 studies was a minimum of 24 months, providing a range of 24 to 180 months and a mean of approximately 69 months. Nearly one-third of the studies collected follow-up information covering a 72-month evaluation period [19,24,25,28–30,33,39]. An additional 4 studies reported outcomes over a period of at least 24 months [17,34,35,37], 8 studies had data collected between 36 and 60 months [16,18,20–23,26,36], and the remaining 5 studies were conducted over a period of 84 months or more [27,31,32,38,40]. In fact, the Vartiainen study conducted in Finland was able to provide follow-up information on students at 2-, 3-, 4-, 8-, and 15-year time points, with 71% of the cohort retained at the end of the 15-year study [38].

Despite the impressive lengths of follow-up, retention rates (i.e., percentage of baseline subjects that provided follow-up data) varied greatly for this group of intervention studies under review. The percentage of subjects retained at the last wave of data collection ranged from 18 to 94% with a mean of 64% across all but 3 studies [18,26,36] that failed

Table 5
Target population characteristics

Investigators	Grade of sample at baseline	Approximate "through-study" age range	% Female	% White	% Enrolled in study of those approached	Total No. of schools at baseline	Total No. of subjects at baseline
Abernathy [16]	6	11–15	49	NR	NR	190	7508
Aveyard [17]	9	13–16	50	86	90	52	8352
Bergamaschi [18]	7	13–16	51	NR	NR	NR	NR
Botvin [19]	7	12–19	48	91	NR	56	5954
Cuijpers [20]	7	12–16	51	NR	NR	12	1930
Del Greco [21]	7	12–17	48	100	NR	1	161
Elder [22]	7	12–17	54	59	NR	22	2051
Ellickson [23]	7	12–18	48	67	NR	30	6527
Flay [24]	6	11–18	NR	NR	NR	22	692
Flynn [25]	4–6	9–18	NR	96	NR	NR	5458
Hansen [26]	7 (cohort 1) 6–7 (cohort 2)	12–17 11–17	50	60	NR	14	1221 (cohort 1) 1707 (cohort 2)
Klepp [27]	5–7	10–22	48	NR	82	6	827
Murray [28]	7	12–19	50	NR	94	10	7124
Pentz [29]	6–7	11–18	49	77	NR	48	1607 (longitudinal) 3777 (cross-sectional)
Perry [30]	6	11–18	NR	NR	NR	13	2401 (both longitudinal and cross-sectional)
Peterson [31]	3	8–21	49	90	98 (school districts)	40 school districts	8388
Shean [32]	7	12–20	52	NR	NR	45	2366
Shope [33]	6	11–18	52	94	NR	NR	1057
St. Pierre [34]	7–8	13–16	25	45	NR	14 Boys & Girls Clubs	377
Sussman [35]	7	12–15	50	60	NR	48	6716
Taylor [36]	7	12–17	50	47	NR	12	NR
Telch [37]	7	12–16	48	NR	NR	2	570
Vartiainen [38]	7	13–28	NR	NR	NR	6	903
Walter [39]	4	9–16	48	79	82	15	911
Winkleby [40]	7	12–24	50	78	NR	NR	~650

Note. All percentages are approximate. NR, not reported.

to report this information. Specifically, of the 22 studies that provided subject attrition information at final follow-up, exactly half [19–21,23,27,29,30,35,38–40] lost at least 25%, 3 [32–34] lost at least 50%, and 1 [16] lost over 75%.

Only 8 studies [17,19–21,29,30,37,38] provided the total number of schools followed at the end of the study period; the available numbers ranged from 1 to 56 schools. Failure to report this information may be due to the fact that all of the interventions attempted to follow subjects between the ages of 12–15 and 16–19, a time when students are relocating from junior high to new high schools. Therefore, school counts may not be relevant to many of the studies. One large-scale study, however, reported that it was able to follow all 40 baseline school districts up until the end of the study [31]. Across all longitudinal studies, the number of individual baseline subjects that were tracked at follow-up ranged from 91 to 7864, with a mean of 2235 individual subjects.

Analytic procedures

Study details regarding analytic procedures are presented in Tables 7 and 8. Fourteen studies examined the potential confounding effects of baseline group nonequivalence on smoking, as well as other variables known to be related to the outcome measures [16,17,19,20,22,25,27,29–31,33–35,37]. Of these studies, 3 found statistically significant differences at pretest between treatment groups [17,20,25]; however, only 2 of these studies made adjustments for nonequivalence by controlling for baseline smoking in the outcome analyses [17,20].

Nearly three-quarters of the studies tested or controlled for a possible imbalance of potentially confounding variables that may result in biases on treatment group differences [17,20,23–30,32–36,39,40]. These studies examined the available data for potential confounders such as baseline smoking, age, gender, ethnicity, socioeconomic status, urbanicity, parental, and peer smoking habits, discretionary funds, and acceptability of smoking. Four studies [16,19,22,31] did not attempt to control for possible confounders when making program versus control group comparisons, 1 study [18] did not have baseline data to do so because it utilized a posttest only design, and another 2 studies [37,38] neglected to report any such information.

Table 6
Follow-up population characteristics

Investigators	Tracked same subjects	Length of follow-up (months)	% Followed at last wave of collection	Total No. of schools at follow-up	Total No. of subjects at follow-up/total No. of subjects used in analytic sample
Abernathy [16]	Yes	36	18	NR	1368
Aveyard [17]	Yes	24	82	50	6819
Bergamaschi [18]	No	36	NR	NR	2691
Botvin [19]	Yes	72	60	56	3597
Cuijpers [20]	Yes	36	73	11	1405
Del Greco [21]	Yes	48	57	1	91
Elder [22]	Yes	48	75	NR	1545
Ellickson [23]	Yes	60	56	NR	3640
Flay [24]	Yes	72	81	NR	560
Flynn [25]	Yes	72	86	NR	4670
Hansen [26]	Yes	48 (cohort 1) 30 (cohort 2)	NR	NR	NR
Klepp [27]	Yes	120	69	NR	570
Murray [28]	Yes	60 and 72	93	NR	6616
Pentz [29]	Yes (partial)	72	76 (longitudinal) 60 (cross-sectional)	38	1216 (longitudinal) 2239 (cross-sectional)
Perry [30]	Yes (partial)	72	45 (longitudinal) 60 (cross-sectional)	7	1080 (longitudinal) 1439 (cross-sectional)
Peterson [31]	Yes	120 and 144	94	40 school districts	7864
Shean [32]	Yes	84	37	NR	877
Shope [33]	Yes	72	25	NR	262
St. Pierre [34]	Yes	27	43	14 Boys & Girls Clubs	161
Sussman [35]	Yes	24	52	NR	3459
Taylor [36]	Yes	60	NR	NR	3027
Telch [37]	No	33	81	2	463
Vartiainen [38]	Yes	180	71	6	640
Walter [39]	Yes	72	65	NR	593
Winkleby [40]	No	144	56	NR	~423

Note. All percentages are approximate. NR, not reported.

Less than half of the studies that tracked subjects indicated specifically that attrition analyses were performed to determine whether high-risk individuals were more likely to be excluded from the overall follow-up samples [19,24,27–29,32–35,39]. Of these studies, 7 found that there was a statistically significant relationship between high-risk individuals (e.g., baseline smokers or drug users) and attrition from the study [24,27–29,32–34]; specifically, greater attrition was found among high-risk individuals compared to low-risk individuals in each study. Further, half of the longitudinal studies conducted tests to determine whether there was a significant relationship between treatment condition group and attrition status [17,19,22–24,26,27,29,31,32,34]. Four of these 11 studies found a statistically significant relationship, with each study reporting greater loss of individuals from the control group compared to the experimental group [17,26,27,32]. Additional attrition analyses were also performed in 8 studies to assess whether there was greater attrition among smokers by treatment condition [19,24,26–28,30,34,38]. Statistically significant differences were found for only 2 studies [26,30], indicating that smokers in the control conditions were more likely to be excluded (e.g., due to dropping out or absence) from the final analysis compared to smokers in the treatment conditions.

Ten studies assessed implementation fidelity to ascertain whether the program content was delivered as intended [17,19,23–25,29–31,35,39]. However, many of the descriptions were vague or program monitoring was not systematically evaluated; thus, it was difficult to clearly establish whether treatment group students received the programming curriculum as planned. Further, less than one-third of the full group of studies attempted to determine the amount of student exposure to the intervention [16,17,19,23,24,33–35]. Of these, 3 studies conducted separate analyses of program effects by level of student exposure [16,17,19], and 2 studies excluded students who did not receive a substantial portion of the curriculum from treatment group comparisons [33,34].

Across all studies, regardless of unit of assignment, the majority (almost two-thirds) of studies conducted analysis at the individual level [16–18,20–23,26–28,32–34,36,37,40], whereas 7 studies did so at the school level [19,24,29,30,35,38,39]. Twelve of the studies retained the unit of assignment as the unit of analysis. Thus, both unit of assignment and analysis were used at the school

Table 7
Analytic procedures

Investigators	Assessed baseline group equivalence of smoking/adjusted for nonequivalence	Assessed confounders (in treatment vs control group comparisons)	Assessed attrition of high-risk subjects (e.g., baseline smokers) from study	Assessed attrition of subjects from treatment vs control group
Abernathy [16]	Yes, equivalent/NA	No	No	No
Aveyard [17]	Yes, nonequivalent/yes, controlled for baseline smoking	Yes, baseline smoking, age, gender, ethnic group, residence location, and mother, father, sibling and best friend smoking	No	Yes, significantly greater loss of contol vs treatment group subjects
Bergamaschi [18]	NA (posttest only design)	NA (posttest only design)	NA (cross-sectional)	NA (cross-sectional)
Botvin [19]	Yes, equivalent/NA	No	Yes, no differences	Yes, no differences
Cuijpers [20]	Yes, nonequivalent/yes, controlled for baseline smoking	Yes, all variables that were different between groups at pretest	No	No
Del Greco [21]	No	Yes, assertiveness and gender	No	No
Elder [22]	Yes, equivalent/NA	No	No	Yes, no differences
Ellickson [23]	No	Yes, district, race, and propensity to use index	No	Yes, no differences
Flay [24]	No	Yes, baseline smoking, social environment risk, and original school board	Yes, significant loss of pretest smokers vs never smokers	Yes, no differences
Flynn [25]	Yes, nonequivalent/No	Yes	No	No
Hansen [26]	NR	Yes, school district, gender, and ethnicity	No	Yes, significantly greater loss of control vs treatment group subjects
Klepp [27]	Yes, equivalent/NA	Yes, acceptability of smoking, knowledge, parental involvement, discretionary funds, and friend and sibling smoking	Yes, significant loss of pretest weekly smokers vs nonweekly smokers	Yes, significantly greater loss of control vs treatment group subjects
Murray [28]	NR	Yes, age, gender, and baseline parental, peer and sibling smoking	Yes, significant loss of pretest smokers vs never smokers	No
Pentz [29]	Yes, equivalent/NA	Yes, grade, SES, % white, and urbanicity	Yes, significant loss of baseline users vs nonusers	Yes, no differences
Perry [30]	Yes, equivalent/NA	Yes, age and gender	No	No
Peterson [31]	Yes, equivalent/NA	No	No	Yes, no differences
Shean [32]	No	Yes, baseline smoking and gender	Yes, significant loss of those who thought they will smoke in future vs those who did not think they will smoke in future	Yes, significantly greater loss of control vs treatment group subjects
Shope [33]	Yes, equivalent/NA	Yes, gender	Yes, significant loss of baseline drinkers and smokers vs nondrinkers and nonsmokers	No
St. Pierre [34]	Yes, equivalent/NA	Yes, pretest scores, gender, age, and race/ethnicity	Yes, significant loss of baseline marijuana users vs nonmarijuana users	Yes, no differences
Sussman [35]	Yes, equivalent (for 1-year analysis)/NA	Yes, school turnover, region, and gender	Yes, no differences	No
Taylor [36]	No	Yes, pretest drug use, ethnicity, and gender	No	No
Telch [37]	Yes, equivalent/NA	NR	NA (cross-sectional)	NA (cross-sectional)
Vartiainen [38]	NR	NR	No	No
Walter [39]	No	Yes, % white	Yes, no differences	No
Winkleby [40]	No	Yes, age group, city, time of survey, and race	NA (cross-sectional)	NA (cross-sectional)

Note. NR, not reported; NA, not available.

Table 8
Analytic procedures

Investigators	Assessed implementation fidelity (teacher delivered content as intended)	Assessed level of student exposure/analyzed program effects by level of student exposure	Unit of analysis	Statistical analyses (used in treatment vs control group comparisons)
Abernathy [16]	NR	Yes (by teacher and student report)/yes	Individual	χ^2
Aveyard [17]	Yes	Yes/yes	Individual	Logistic regression, odds ratios and 95% CIs
Bergamaschi [18]	NR	NR	Individual	χ^2
Botvin [19]	Yes	Yes/yes	School	Least squares regression
Cuijpers [20]	No	NR	Individual	Multiple and logistic regression
Del Greco [21]	NA (researchers taught)	NR	Individual	Analysis of covariance
Eider [22]	NR	NR	Individual	χ^2
Ellickson [23]	Yes	Yes/no	Individual (with adjustments for within school correlations)	Logistic regression
Flay [24]	Yes	Yes/no	Individual and school	Multiple logistic regression, odds ratios and 95% CIs, χ^2
Flynn [25]	Yes	NR	Individual and community	Stepwise logistic regression, odds ratios and 95% CIs, analysis of variance
Hansen [26]	NR	NR	Individual	χ^2
Klepp [27]	NR	NR	Individual	Logistic regression, odds ratios and 95% CIs, χ^2
Murray [28]	NR	NR	Individual	Logistic regression, analysis of covariance
Pentz [29]	Yes	NR	Individual and school	Multiple and logistic regression, odds ratios and 95% CIs
Perry [30]	Yes	NR	School (with adjustment)	Proc GLM
Peterson [31]	Yes	NR	School district	Group permutation-based test
Shean [32]	NR	NR	Individual	Logistic regression
Shope [33]	NR	Yes/yes, excluded students who did not receive 2 years of curriculum from treatment group comparisons	Individual	Analysis of variance
St. Pierre [34]	NR	Yes/yes, excluded students who did not attend required number of sessions from posttests	Individual	Logistic regression, analysis of covariance, *t* tests
Sussman [35]	Yes	Yes/no	School	*t* tests, analysis of covariance
Taylor [36]	NR	NR	Individual	Growth curve modeling
Telch [37]	NR	NR	Individual	χ^2
Vartiainen [38]	NR	NR	Individual and school	χ^2, analysis of variance
Walter [39]	Yes	NR	School	*t* tests
Winkleby [40]	NR	NR	Individual	Multiple logistic regression

Note. NR, not reported; NA, not available.

level for 6 studies [19,24,29,35,38,39], the individual level for 4 studies [18,22,34,40], the community level for 1 study [25], and the school district level for another study [31].

Program evaluation analyses were accomplished through the use of a variety of statistical procedures, with many studies utilizing more than one type of statistical test due to the use of multiple outcome variables. Almost half of the studies used regression analysis to assess long-term effectiveness of the prevention program, with 9 studies using logistic regression [17,23–25,27,28,32,34,40], 1 study using least squares regression only [19], and 2 studies using both logistic and linear regression [20,29]—with most of these studies providing odds ratios and 95% confidence intervals. Several studies examined treatment effects with χ^2 procedures [16,18,22,24,26,27,37,38], either analysis of variance

[25,33,38] or analysis of covariance [21,28,34,35], and *t* tests [35,39]. General linear model (GLM) [30], group permutation-based [31], and growth curve modeling [36] methods were also employed to determine intervention effectiveness.

Tobacco and other drug use outcomes

Tables 9 and 10 provide program results for tobacco and other drug use. The magnitude of long-term effects for tobacco and other drug use outcomes was evaluated by at least one of the following methods in the vast majority (80%) of prevention studies. In the first method, program effects were determined by calculating differences in percentages of adolescents who smoke or use drugs in experimental versus control conditions (e.g., % of smokers in program - % smokers in control) at posttest. The second method examined programming effects by computing the percentage reduction in smoking or drug use rates (from baseline to follow-up) for experimental conditions relative to control conditions. The formula for percentage reduction in rates is $(X_1 - X_2 = \%\Delta$ program$) - (O_1 - O_2 = \%\Delta$ control). Post hoc test results were not considered in this review's evaluation of intervention outcomes.

Tobacco use outcomes

Overall, a majority (i.e., 15 of 25) of evaluation studies reported at least one (hypothesized) significant positive main effect for long-term (at least 24 months) smoking outcomes for experimental conditions relative to control conditions on such variables as ever, monthly, weekly, or daily smoking among baseline nonsmokers [16,18,19,22,25–27,29,30,32,35–39]. Fifteen studies had available data to determine the percentage reduction in smoking rates for experimental versus control groups, according to only the first calculation method explained above [17–19,21,22,26–28,30–32,37–40]; 3 studies provided data to make evaluations based on the second method only [23,29,35]; and 2 studies had information for both methods [16,20]. Thus, among the 17 studies that reported levels of use by calculating differences in percentages of adolescents who smoke in experimental versus control conditions, 11 studies found statistically significant outcomes for one or more of the experimental groups compared to the control situation [16,18,19,22,26,27,30,32,37,38]. Of these studies that had available data, the long-term mean reduction in the percentage of baseline nonusers who initiated smoking in experimental conditions compared to control conditions was 11.4% with a range of 9 to 14.2% [16,26,27,38,39]. Furthermore, of the studies specifying that their programs were designed to provide booster sessions [19,22–24,29,30,34,35] or suggesting booster programming by delivering curricula over at least a 2-year period [20,25,31,33,38,39], approximately 57% had maintained long-term reductions in cigarette use at final follow-up testing [19,22,25,29,30,35,38,39].

Moreover, prior to the latest long-term results examined in this review, 18 of these 25 prevention projects had also reported initial or interim positive outcomes for tobacco use on follow-up periods ranging from 6 months to 8 years [16,19,20,22–25,27–30,32,33,35–39], whereas 3 studies reported having no statistically significant programming effects [17,26,34], and 4 studies had not published initial shorter term smoking evaluations [18,21,31,40]. Seventy-two percent (13) of 18 studies that had initial positive program effects were also found to have maintenance effects that persisted to the end of the full study period [16,19,22,25,27,29,30,32,35–39].

Other drug use outcomes

Of the 9 studies that provided long-term assessments of other drug use such as alcohol and marijuana incidence and prevalence [19,20,23,25,26,29,33,34,36], two-thirds (6 studies) reported positive program effects [19,20, 29,33,34,36]. Sixteen of the remaining studies did not target these other drugs in their prevention efforts [16–18,22,24,27,28,30–32,35,37–40]. Two studies had available data to determine the percentage reduction in drug use rates for experimental versus control groups, following the first calculation method only [19,26], 2 studies provided data to make evaluations based on the second method only [23,29], and 1 study had information for both methods [20]. For the studies that provided information for the second calculation method (i.e., the percentage reduction in smoking or drug use rates—from baseline to follow-up—for experimental conditions relative to control conditions), long-term reductions ranged from 6.9 to 11.7% for weekly alcohol use [20,29] and a reduction of 5.7% was reported for 30-day marijuana use [29]. Furthermore, of the 7 studies that assessed alcohol or marijuana, and specified [19,23,29,34] or suggested [20,25,33] that their programs were designed to provide booster sessions, 5 (71%) had maintained long-term reductions in alcohol or marijuana use at the end of the long-term study period [19,20,29,33,34].

Initial or interim effects for other drug use were also available for all 9 studies that provided long-term assessments. Eight of the 9 projects reported initial or interim positive outcomes for other drug use on follow-up periods ranging from 3 months to 5 years [19,20,23,25,29,33,34,36]. Seventy-five percent (6 of 8) of studies that had initial positive program effects were also found to have maintenance effects that persisted to the end of the full study period [19,20,29,33,34,36].

Discussion and conclusions

This review of long-term tobacco and drug use prevention intervention studies published since 1966 indicates that school- and community-based programs were effective in preventing or reducing adolescent cigarette, alcohol, and marijuana use across follow-up periods ranging from 2 to 15

Table 9
Initial (or interim) and long-term results for cigarette use

Investigators	Initial or interim effects for cigarette use—% relative reduction for experimental group	Long-term (over 24 months) effects for cigarette use—% relative reduction for experimental group
Abernathy [16]	M1 = At 1 year, 22.7–12.1 = 10.6 (onset) for males only, $P < 0.001$ M2 = At 1 year, (33.6–12.1)–(31.5–22.7) = 12.7 (onset) for males only, $P < 0.001$	M1 = 40.4–31.4 = 9.0 (onset) for males only, $P < 0.05$ M2 = (33.6–31.4)–(31.5–40.4) = 11.1 (onset) for males only, $P < 0.05$
Aveyard [17]	M1 = No significant differences M2 = No data	M1 = No significant differences M2 = No data
Bergamaschi [18]	NA	M1 = 4.1 (within past month), $P < 0.05$ M2 = No data
Botvin [19]	M1 or M2 = No data At 3 years, 1.63–1.46 = .17 (less smoking), $P = 0.003$ (Assessed only this one primary dependent variable)	M1 = 33–26 = 7.0 (monthly), $P < 0.01$ M1 = 27–21 = 6.0 (weekly), $P < 0.05$ M1 = 12–9 = 3.0 (pack a day), $P < 0.05$ M1 = 29–21 = 8.0 (monthly cigarette and alcohol), $P < 0.05$ M1 = 14–10 = 4.0 (weekly cigarette and alcohol), $P < 0.01$ M1 = 8–4 = 4.0 (weekly cigarette and marijuana), $P < 0.05$ M1 = 6–3 = 3.0 (weekly cigarette, alcohol, and marijuana), $P < 0.01$ Plus, even better results with high fidelity sample (too many to document) M2 = No data
Cuijpers [20]	M1 = At 1 year, 13.1–9.2 = 3.9 (current use), $P < 0.05$ M2 = At 1 year, (5.5–9.2)–(5.9–13.1) = 3.5 (current use), $P < 0.05$	M1 = No significant differences M2 = No significant differences
Del Greco [21]	NA	M1 = No significant differences M2 = No data
Elder [22]	M1 = At 3 years, 19.8–13.2 = 6.6 (past month), $P < 0.05$ M2 = At 3 years, (5.0–13.2)–(5.6–19.8) = 6.6 (past month), $P < 0.05$	M1 = 12.6–7.0 = 5.6 (past month), $P < 0.05$ M2 = No data
Ellickson [23]	M1 = At 1 year, 55.2–47 = 8.2 (baseline experimenters had now quit), $P < 0.01$ M1 = At 15 months, 32.3–23.6 = 8.7 (baseline experimenters smoked in the past month), $P < 0.01$ M1 = At 15 months, 22.4–16.5 = 5.9 (baseline experimenters were monthly smokers), $P < 0.05$ M1 = At 15 months, 11.1–5.7 = 5.4 (baseline experimenters were weekly smokers), $P < 0.1$ M1 = At 15 months, 5.1–2.3 = 2.8 (baseline experimenters were daily smokers), $P < 0.05$ M1 = At 15 months, 54.6–44.2 = 10.4 (baseline experimenters had now quit), $P < 0.01$ M2 = No data	M1 = No data M2 = No significant differences
Flay [24]	M1 = At 18 months, 66.7–22.7 = 44.0 (% pretest experimenters were now quitters), $P < 0.003$ M1 = At 18 months, 71–46.7 = 24.3 (% pretest quitters were now quitters), $P < 0.003$ M2 = No data	M1 and M2 = No data No significant differences
Flynn [25]	M1 = At 3 years, 9.25–5.01 = 4.24 (last week), $P < 0.05$ M1 = At 4 years, 14.82–9.10 = 5.7 (last week), $P < 0.05$ M1 = At 4 years, 9.57–5.35 = 4.22 (yesterday), $P < 0.05$ M1 = At 4 years, 8.29–5.16 = 3.13 (smokeless tobacco in past week), $P < 0.05$ M1 = At 5 years, 19.8–12.8 = 7.0 (last week), $P < 0.05$ M1 = At 5 years, 13.09–8.56 = 4.53 (yesterday), $P < .05$ M1 = At 5 years, 4.4–2.6 = 1.8 (No. per week), $P < 0.05$ M2 = At 3 years, (1.29–5.01)–(1.59–9.25) = 3.94 (past week), $P < 0.05$	M1 and M2 = No data OR for being a weekly smoker in the media-plus school group was .79 (CIs = .69, .91) OR for being a daily smoker in the media-plus school group was .78 (CIs = .67, .90)

Table 9 (*continued*)

Investigators	Initial or interim effects for cigarette use—% relative reduction for experimental group	Long-term (over 24 months) effects for cigarette use—% relative reduction for experimental group
	M2 = At 4 years, (1.29–9.10)–(1.59–14.82) = 5.42 (past week), $P < 0.05$ M2 = At 4 years, (.61–5.35)–(.49–9.57) = 4.34 (yesterday), $P < 0.05$ M2 = At 4 years, (2.66–5.16)–(2.54–8.29) = 3.25 (smokeless tobacco in past week), $P < 0.05$ M2 = At 5 years, (1.29–12.81)–(1.59–19.80) = 6.69 (past week), $P < 0.05$ M2 = At 5 years, (.61–8.56)–(.49–13.09) = 4.65 (yesterday), $P < 0.05$	
Hansen [26]	M1 = Cohort 1—At 6 months, no significant differences M1 = Cohort 2—At 6 months, no significant differences M2 = No data	M1 = Cohort 1—38–22 = 16.0 (30-day use) prevalence, $P < 0.05$ M1 = Cohort 2—16.3–3.9 = 12.4 (30-day use) incidence, $P = .00$ M1 = Cohort 2—16.2–5.6 = 10.6 (30-day use) prevalence, p = .03 M2 = No data
Klepp [27]	M1 = At 2 years, 26.9–16.5 = 10.4 (onset), $P < 0.001$ M2 = No data	M1 = 55.8–41.6 = 14.2 (ever) males only, $P < 0.05$ M1 = 50.0–35.0 (weekly) males only, $P < 0.05$ M2 = No data
Murray [28]	M1 = At 1 year, 8.3–2.9 = 5.4 (weekly among baseline nonusers), $P < 0.05$ M1 = At 1 year, 7.1–2.1 = 5 (daily among baseline nonusers), $P < 0.01$ M1 = At 1 year, 12.3–2.0 = 10.3 (average No. of cigarettes per week among baseline nonusers), $P < 0.001$ M1 = At 1 year, 8.0–4.3 = 3.7 (average No. of cigarettes per week among baseline experimental smokers), $P < 0.001$ M1 = At 4 years, no significant differences M2 = No data	M1 = No significant differences M2 = No data
Pentz [29]	Cross sectional: M1 = No data M2 = At 1 year, 7.3 (past month), $P < 0.01$ M2 = At 1 year, 4.6 (past week), $P < 0.01$ M2 = At 2 years, 10.8 (past month), $P < 0.01$ M2 = At 2 years, 7.2 (past week), $P < 0.01$ M2 = At 2 years, 3.4 (heavy use), $P < 0.05$ M2 = At 3 years, 10.5 (past month), $P < 0.01$ M2 = At 3 years, 8.5 (past week), $P < 0.01$ M2 = At 3 years, 6.2 (heavy use), $P < 0.001$	Cross sectional: M1 = No data M2 = 10.5 (past month), $P < 0.05$ M2 = 7.5 (past week), $P < 0.05$ M2 = 4.8 (heavy use), $P < 0.01$
Perry [30]	M1 = At 4 years, 22–11 = 11.0 (current), $P < 0.0001$ M1 = At 4 years, 41–31 = 10.0 (never tried), $P < 0.04$ M1 = At 4 years, 16.9–6.2 = 10.7 (average No. of cigarettes per week), $P < 0.0001$ M2 = No data	M1 = 24.1–14.6 = 9.5 weekly, $P < 0.05$ M1 = Average No. of cigarettes per week significantly lower in intervention community (no numbers reported) M2 = No data
Peterson [31]	NA	M1 = No significant differences M2 = No data
Shean [32]	M1 = At 1 year, 33.1–24.1 = 7.0 (smoked one or more a day regularly for 6 months) among baseline nonusers (females only), $P = 0.04$ M1 = At 1 year, 29.4–15.4 = 14.0 (smoked one or more a day regularly for 6 months) among baseline nonusers (males only), $P = 0.002$ M1 = At 2 years, 49.7–37.8 = 11.9 (smoked one or more a day regularly for 6 months) among baseline nonusers (females only), $P = 0.03$ M2 = No data	M1 = 28–16 = 12.0 (smoked one or more a day regularly for 6 months) among baseline nonusers (females only), OR = .50 (CIs = .26, .98) M2 = No data
Shope [33]	M1 and M2 = No data At 1 year, .47–.17 = .30 (overall mean level on scale: 0 = never to 5 = every day frequency of use), $P = 0.05$	M1 and M2 = No data No significant differences

(*continued on next page*)

Table 9 (*continued*)

Investigators	Initial or interim effects for cigarette use—% relative reduction for experimental group	Long-term (over 24 months) effects for cigarette use—% relative reduction for experimental group
St. Pierre [34]	M1 and M2 = No data At 15 months, no significant differences	M1 and M2 = No data No significant differences
Sussman [35]	M1 = No data M2 = At 1 year, 9.3–6.1 = 3.2 (ever tried), $P < 0.05$ M2 = At 1 year, 5.6–2.0 = 3.6 (weekly), $P < 0.05$ M2 = At 1 year, 4.1–1.7 = 2.4 (ever tried smokeless tobacco), $P < 0.05$ M2 = At 1 year, −.5–−.4 = .90 (weekly smokeless tobacco), $P < 0.05$	M1 = No data M2 = 23–16 = 7.0 (ever tried), $P < 0.05$ M2 = 9–4 = 5.0 (weekly), $P < 0.05$ M2 = 7.0–7.0 (ever tried smokeless tobacco), $P < 0.05$ M2 = 1–−2.0 (weekly smokeless tobacco), $P < 0.05$
Taylor [36]	M1 and M2 = No data At 1 year, significantly reduced rates of cigarette consumption (no numbers reported), $P = 0.03$	M1 and M2 = No data (Growth curve modeling) Recent and lifetime cigarette use were significantly lower (no numbers reported), $P = 0.01$ and $P = 0.003$, respectively
Telch [37]	M1 = At 9 months, 10.3–5.3 = 5.0 (weekly), $P < 0.05$ M1 = At 21 months, 18.8–7.1 = 11.7 (weekly), $P < 0.001$ M2 = No data	M1 = 14.8–5.1 = 9.7 (weekly), $P < 0.001$ M2 = No data
Vartiainen [38]	M1 = At 2 years, 23.6–13.3 = 10.3 (monthly), $P = 0.002$ M1 = At 2 years, 21.6–12.1 = 9.5 (weekly), $P = 0.004$ M1 = At 4 years, 34.5–21.3 = 13.2 (monthly), $P = 0.001$ M1 = At 4 years, 28.6–17.3 = 11.3 (weekly), $P = 0.005$ M1 = At 4 years, 25.2–13.8 = 11.4 (daily), $P = 0.002$ M1 = At 4 years, 41.2–25.8 = 15.4 (any among baseline nonsmokers), $P = 0.001$ M1 = At 8 years, 40.6–26 = 14.6 (monthly), $P = 0.012$ M1 = At 8 years, 35.3–21.4 = 13.9 (weekly), $P = 0.012$ M1 = At 8 years, 46.9–30.1 = 16.8 (any among baseline nonsmokers), $P = 0.003$ M2 = No data	M1 = 41.2–29.3 = 11.9 (any among baseline nonsmokers), $P = 0.026$ M1 = 35–28 = 7.0 (monthly), $P = 0.045$ M2 = No data
Walter [39]	M1 = At 1 year, 40.9–36.4 = 4.5 observed mean change (mol/L) in level of serum thiocyanate, $P = 0.000$ M2 = At 1 year, (35.8–36.4)–(35.6–40.9) = 4.7 observed mean change (mol/L) in level of serum thiocyanate, $P = 0.000$	M1 = 13.1–3.5 = 9.6 (any), $P < 0.005$ M2 = No data
Winkleby [40]	NA	M1 = No significant differences M2 = No data

Note. M1, Method 1: Examined program effects by calculating differences in percentages of adolescents who smoke in experimental versus control conditions at posttest: % of smokers in program - % smokers in control. M2, Method 2: Examined program effects by calculating the percentage reduction in smoking rates (from baseline to follow-up) for experimental conditions relative to control conditions: $(X_1 - X_2 = \%\Delta$ program$) - (O_1 - O_2 = \%\Delta$ control). NA, not available.

years. Long-term prevention studies conducted thus far have predominantly focused on cigarette use, with all 25 studies under review providing assessments of tobacco use incidence and/or prevalence. The majority of these evaluations reported statistically significant program effects for smoking outcomes, indicating reductions in the percentage of baseline nonusers who initiated smoking in experimental versus control conditions ranging from 9 to 14.2%—lasting for up to 15 years. Only 9 of the 25 studies provided long-term assessments of the impact that social influences programs have on other substances such as alcohol and marijuana use. Results for long-term drug use reductions ranged from 6.9 to 11.7% for weekly alcohol use, and a reduction of 5.7% was reported for 30-day marijuana use.

The magnitude of effects was fairly consistent across the individual studies, adding further to the evidence indicating that the prevention approaches were effective in preventing or reducing tobacco and other drug use. Moreover, consistent with previous research [24,60], results indicated that program effects were less likely to decay among studies that delivered booster programming sessions as a supplement to the program curricula. Of the studies that provided booster sessions, the majority had maintained long-term reductions for all three substances (cigarette, alcohol, and marijuana use) at final follow-up testing.

Initial or interim published results were available for the vast majority of studies that measured either tobacco or alcohol and marijuana use. Of the studies that had these data available, it was found that the large majority of interventions that produced initial positive program effects tended to maintain long-term reductions in substance use incidence and prevalence beyond 2 years. Specifically, preventive effects were maintained over the long term for nearly three-quarters of the interventions (72% that assessed tobacco use and 75% that assessed alcohol or marijuana use) that had demonstrated initial programming effects. A plausible ex-

planation for this observed pattern of results is that prevention programs must have strong initial positive effects if maintenance effects are expected to persist over time especially across the transitional period between junior high and high school. This possibility is illustrated most notably in the evaluation of the Hutchinson Smoking Prevention Project [31], one of the more methodologically rigorous trials conducted by Peterson and colleagues in which the prevention program proved to be ineffective at both the 10- and 12-year follow-ups. In the absence of published initial or short-term results (for changes in key mediators or tobacco use behaviors) for this project, it is possible to speculate that the particular intervention program was initially ineffective among the target audience, thereby decreasing the possibility that program effects could be observed at any long-term follow-up evaluation.

By the same logic, it could further be hypothesized that all empirically evaluated short-term studies which have produced strong positive behavioral differences between intervention and control conditions would be more likely to evidence maintenance effects—that are stronger and last longer—compared to studies that failed to find positive effects or found weak or negative program results. A recent meta-analysis has gathered information on over 200 studies, a large number of which showed positive program effects [13]. This possibility would suggest that many more existing prevention programs (that have only assessed short-term results) may also be effective in reducing or preventing adolescent substance use over the long-term. However, such long-term information is relatively rare, mostly due to a lack of funding for evaluation research proposals. That is, only studies that receive long-term evaluation funding (as did most of the studies under review here) are able to empirically test and publish results. Such a bias may be operating to reduce the probability of finding long-term program effects that might be present.

Although previous literature reviews [8–10] and meta-analyses [11–13] have well-established short-term prevention effects of 24 months or less, this paper is the first to demonstrate the presence of positive program maintenance effects that last for up to 15 years after completion of substance use prevention programming. However, despite this apparent progress in science-based preventive interventions, caution should be exercised when interpreting the overall success of these studies because of numerous methodological shortcomings and substantial variation between programs and the reporting of data—all of which reflects the current status of adolescent prevention programming in school- and community-based populations.

Methodological issues and challenges

Overall methodological design

Information provided in these 25 evaluation reports suggests that the investigators were concerned with the overall design methodology of their prevention interventions; however, across all of the studies, common weaknesses in research designs posed serious threats to internal validity. In particular, the majority of studies used the quasi-experimental design, selected school as the unit of assignment but analyzed data at the individual level, and showed great variability in the selection and use of substance use outcome measures.

The most common treatment design utilized by the studies was the quasi-experimental nonequivalent group, pretest–posttest design. Although no design is perfect, the experimental design controls for the most threats to internal validity and should be used in evaluating program effectiveness because it increases our confidence that observed outcomes are the result of a given treatment program instead of a function of extraneous variables or events. However, given the difficulties of conducting such large-scale projects, it is not uncommon that system-wide evaluation studies on psychosocial influences substance use prevention programs tend not to utilize true experimental designs.

Another methodological design issue that may have posed threats to the interpretability of the reported findings concerns the appropriate unit of assignment to experimental conditions. Fortunately, all but 5 of the studies assigned whole schools or communities—and in one case [34], whole Boys & Girls Clubs—to treatment groups. Despite the rationale for using school as the unit of assignment [8,61], large-scale investigations that would enable assignment of higher-order units (e.g., schools) are not always possible when only a small number of schools or communities are available for experimental and control conditions. Furthermore, much more research is needed to determine whether contamination, for example, at the classroom level of assignment, is significant and indeed detrimental.

Across all studies, there was great variability in the selection and use of outcome measures. For example, some studies measured reduction of onset of smoking among baseline nonsmokers, while other studies assessed reductions in the prevalence of monthly and weekly cigarette use and/or the transition from weekly to daily smoking. In many cases, reporting was unclear regarding what and how many measures were being assessed. Studies also failed to report how outcome measures where dichotomized or scored (when scales were created). Many studies selected multiple outcome measures but neglected to indicate whether adjustments were made to decrease the Type I error rate that might cause studies to incorrectly declare a difference or relationship to be true when in fact there is no treatment effect. Further, the psychometric adequacy of the substance use-related measures was not reported for the most part in theses studies. However, research suggests that self-report measures of substance use in prevention research perform well among adolescents, indicating good reliability and predictive validity [35,62,63].

Table 10
Initial (or interim) and long-term results for other drug use

Investigators	Initial or interim effects for other drug use—% relative reduction for experimental group	Long-term (over 24 months) effects for other drug use—% relative reduction for experimental group
Abernathy [16]	NA	NA
Aveyard [17]	NA	NA
Bergamaschi [18]	NA	NA
Botvin [19]	M1 and M2 = No data At 3 years, 2.32–2.19 = 0.13 (less drunkenness), $P = 0.04$ At 3 years, 1.66–1.51 = 0.15 (less marijuana use), $P < 0.02$	M1 = 40–33 = 7.0 (monthly drunkenness), $P < .01$ Plus, even better results with high fidelity sample M2 = No data
Cuijpers [20]	M1 = At 1 year, 42.8–32.8 = 10.0 (current alcohol use), $P < 0.01$ M1 = At 1 year, 2.10–1.96 = 0.14 (alcoholic drinks per occasion), $P < 0.01$ M1 = At 2 years, 65.4–56.6 = 8.8 (current alcohol use), $P < 0.001$ M1 = At 2 years, 11.2–7.1 = 4.1 (current marijuana use), $P < 0.05$ M2 = At 1 year, (26.9–32.8)–(31.8–42.8) = 5.1 (current alcohol use), $P < 0.01$ M2 = At 1 year, (1.89–1.96)–(1.71–2.10) = 0.32 (alcoholic drinks per occasion), $P < 0.01$ M2 = At 2 years, (26.9–56.6)–(31.8–65.4) = 3.9 (current alcohol use), $P < 0.001$	M1 = 80.5–73.8 = 6.7 (current alcohol use), $P < 0.001$ M1 = 56.9–44.2 = 12.7 (current weekly alcohol use), $P < 0.05$ M1 = 5.27–4.06 = 1.21 (alcoholic drinks per week), $P < 0.01$ M1 = 5.82–4.79 = 1.03 (alcoholic drinks per occasion), $P < 0.001$ M2 = (26.9–73.8)–(31.8–80.5) = 1.8 (current alcohol use), $P < 0.001$ M2 = (12.0–44.2)–(13.0–56.9) = 11.7 (current weekly alcohol use), $P < 0.05$ M2 = (0.58–4.06)–(0.53–5.27) = 1.26 (alcoholic drinks per week), $P < 0.01$ M2 = (1.89–4.79)–(1.71–5.82) = 1.21 (alcoholic drinks per occasion), $P < 0.001$
Del Greco [21]	NA	NA
Elder [22]	NA	NA
Ellickson [23]	M1 = At 3 months, 22.8–16.3 = 6.5 (ever use alcohol among baseline nondrinkers), $P < 0.05$ M1 = At 3 months, 10.8–5.9 = 4.9 (alcohol use in past month among baseline nondrinkers), $P < 0.05$ M1 = At 3 months, 10.7–5.6 = 5.1 (weekly marijuana use among baseline marijuana users), $P < 0.05$ M1 = At 12 months, 7.7–4.9 = 2.8 (ever use marijuana among baseline marijuana and cigarette nonusers), $P < 0.05$ M1 = At 12 months, 6.4–3.3 = 3.1 (monthly marijuana use among baseline marijuana nonusers but cigarette users), $P < 0.05$ M1 = At 15 months, 12.1–8.3 = 3.8 (ever use marijuana among baseline marijuana and cigarette nonusers), $P < 0.05$ M1 = At 15 months, 3.7–1.4 = 2.3 (marijuana use in past month among baseline marijuana and cigarette nonusers), $P < 0.01$ M2 = No data	M1 = No data M2 = No significant differences
Flay [24]	NA	NA
Flynn [25]	Although not targeted by the intervention: M1 = At 5 years, 55.80–49.48 = 6.32 beer drinking more than once, $P < 0.05$ M2 = At 5 years, (7.36–49.48) − (8.58–55.80) = 5.1 beer drinking more than once, $P < 0.05$	Although not targeted by the intervention: M1 and M2 = No data No significant differences for alcohol
Hansen [26]	M1 = Cohort 1- At 6 months, no significant differences for alcohol M1 = Cohort 2- At 6 months, no significant differences for alcohol M2 = No data	M1 = Cohort 1—no significant differences for alcohol M1 = Cohort 2—no significant differences for alcohol M2 = No data
Klepp [27]	NA	NA
Murray [28]	NA	NA
Pentz [29]	Cross sectional: M1 = No data M2 = At 2 years, 7.2 (past month alcohol use), $P < 0.01$ M2 = At 2 years, 6.8 (heavy alcohol use), $P < 0.05$ M2 = At 2 years, 3.1 (heavy marijuana use), $P < 0.01$ M2 = At 3 years, 7.4 (past month alcohol use), $P < 0.05$ M2 = At 3 years, 5.2 (past week alcohol use), $P < 0.05$	Cross sectional: M1 = No data M2 = 8.9 (past month alcohol use), $P < 0.01$ M2 = 6.9 (past week alcohol use), $P < 0.05$ M2 = 7.2 (heavy alcohol use), $P < 0.05$ M2 = 5.7 (past month marijuana use), $P < 0.05$ M2 = 2.9 (heavy marijuana use), $P < 0.01$

Table 10 (*continued*)

Investigators	Initial or interim effects for other drug use—% relative reduction for experimental group	Long-term (over 24 months) effects for other drug use—% relative reduction for experimental group
	M2 = At 3 years, 7.1 (heavy use alcohol use), $P < 0.01$ M2 = At 3 years, 5.9 (past month marijuana use), $P < 0.01$ M2 = At 3 years, 3.4 (past week marijuana use), $P < 0.01$ M2 = At 3 years, 3.2 (heavy use marijuana use), $P < 0.01$	
Perry [30]	NA	NA
Peterson [31]	NA	NA
Shean [32]	NA	NA
Shope [33]	M1 and M2 = No data At 1 year, significant difference for drinks per week (1.15–0.56 = 0.59 overall mean level; on scale: 0 = no alcohol to 6 = 8 or more drinks per week), $P = 0.002$ At 1 year, significant difference for marijuana use (no numbers reported) for males, $P < 0.03$ At 1 year, significant difference for cocaine use (no numbers reported) for males, $P < 0.03$ At 1 year, significant difference for drug use (no numbers reported), $P < .00$	M1 and M2 = No data Significant difference for cocaine use (no numbers reported) for females, $P = 0.04$ No significant differences for alcohol use No significant differences for marijuana use
St. Pierre [34]	M1 and M2 = No data At 15 months, significantly less marijuana-related behavior (on scale; no numbers reported), $P = 0.05$	M1 and M2 = No data 1.38–1.25 = 0.13 marijuana-related behavior (on scale), $P = 0.05$
Sussman [35]	NA	NA
Taylor [36]	M1 and M2 = No data At 1 year, significantly reduced rates of alcohol consumption (no numbers reported), $P = 0.001$. At 1 year, significantly reduced rates of marijuana use (no numbers reported), $P = 0.009$	M1 and M2 = No data (Growth curve modeling) Recent and lifetime alcohol use were significantly lower (no numbers reported), $P = 0.007$ and $P = 0.004$, respectively Lifetime drunkenness was significantly lower (no numbers reported), $P = 0.026$
Telch [37]	M1 = At 21 months, 16.2–5.6 = 10.6 (“high” or drunk on alcohol during the past week or day), $P < 0.01$ M1 = At 21 months, 14.9–7.6 = 7.3 (marijuana use during the past week or day), $P < 0.01$ M2 = No data	NA
Vartiainen [38]	NA	NA
Walter [39]	NA	NA
Winkleby [40]	NA	NA

Note. M1, Method 1: Examined program effects by calculating differences in percentages of adolescents who use drugs in experimental versus control conditions at posttest: % of drug users in program - % drug users in control. M2, Method 2: Examined program effects by calculating the percent reduction in drug use rates (from baseline to follow-up) for experimental conditions relative to control conditions: $(X_1 - X_2 = \%\Delta$ program$) - (O_1 - O_2 = \%\Delta$ control). NA, not available.

Programming characteristics

Although all of the studies indicated that they implemented psychosocial influences programming, large differences exist in intervention design and the application of social learning principles. Across the studies, little information was provided to ascertain the quality and content of programming. Many studies completely neglected to describe or outline their programmatic approaches. Further, the programs varied greatly in terms of frequency (from 5 to 384 sessions) and length (over 2 weeks to 8 years) and providers (project staff, health educators, classroom teachers, peer leaders). Similarly, provider training ranged from as little as 1-day workshops for classroom teachers to as much as 3 weeks (120 h) of training for health educators who had delivered the program curricula. Fourteen studies indicated that their programs offered some type of booster programming; however, again, the quality, frequency, and length of these booster sessions varied widely. Given the success that booster programming may have on the maintenance of long-term reductions of cigarette, alcohol, and marijuana use, it is unfortunate that more booster programming information is not available in these evaluation reports.

Information related to the construct validity of the intervention components was not reported in most studies. Unfortunately, studies did not test program effects on mediating variables and, further, studies did not attempt to link changes in mediating constructs to substance use outcome measures. Therefore, it remains unclear whether the program components are actually building or changing the skills of interest and whether these skills are affecting subsequent drug use behavior.

Recruitment and retention of participants

Selection bias may exist as a limitation of many of the studies. Although the reporting was often unclear with re-

gard to the sampling process, it is assumed that many studies did not employ random sampling techniques; instead they recruited subjects based on availability (i.e., samples of convenience). Thus, this type of sample selection impacts external validity because the information obtained from the study sample is limited to describing the current study sample only—and generalizing the current findings to a larger population or to other hypothetical populations is not always possible.

Encouragingly, the longitudinal studies conducted thus far have indicated that it is possible to follow cohorts for very long periods of time—ranging from 2 to 15 years; however, the retention rates indicate that long-term tracking of individuals is still a very serious and problematic issue in school- and community-based prevention research. The percentage of retention at follow-up varied widely across both the small- and large-scale studies, with most studies indicating losses of 25%, 50% or more of the original target sample. The mention of subject tracking protocols was almost nonexistent in all but a few studies. Procedures for tracking study participants require considerable attention.

Statistical analysis of data

Considerable variability existed in the manner in which analytic procedures were reported. Although it was not always clear precisely what analyses methods were employed, sufficient information was obtained from most studies regarding baseline group equivalence, confounding variables, differential attrition, implementation fidelity, unit of assignment, and statistical testing. Based on this reporting across studies, it was evident that these studies sought to maximize the validity of their respective studies.

The majority of studies addressed the issues of baseline group nonequivalence and the possible confounding effects of known risk factors associated with the dependent variables. Results indicated that pretest nonequivalence of experimental conditions was not generally a problem since all but 3 studies tested and found comparable baseline levels of smoking and other drug use behaviors such as alcohol and marijuana use. Similarly, most studies tested or controlled for possible imbalances of potentially confounding variables (e.g., ethnicity, socioeconomic status, and parental and peer smoking habits) that may have created the observed effects across studies. Thus, for the most part, baseline nonequivalence and confounding variables may be reasonably ruled out as threats to internal validity for this group of studies.

The issue of attrition was identified in this review as one of the most serious problems across all of the longitudinal studies. The vast majority of studies reported that they had indeed suffered from large attrition rates at final posttest, ranging from 6 to 82% across the full group of studies. Subject attrition has the potential to seriously compromise both the external and the internal validity of drug use prevention evaluations. The main factor in determining the impact of attrition on external validity is not so much the percentage of people responding (i.e., response rate) but rather whether there was a systematic bias between missing and remaining subjects that was acting to affect responses [64]. Less than half of the 22 longitudinal studies tested a smoking $\times$ attrition status interaction and subsequently reported significantly greater attrition from the study among high-risk individuals compared to low-risk individuals. Students who were absent at testing or dropped out of the study were more likely to use tobacco, alcohol, and marijuana at baseline compared to those who remained in the study until the final posttest. Unfortunately, the authors generally did not provide additional information (demographics, pertinent psychosocial information, etc.) about other potential differences between respondents and nonrespondents. Although the available data confirm that threats to external validity exist among these evaluation studies, research indicates that program effectiveness may be underestimated when high-risk subjects who might best respond to the intervention are lost at follow-up [64]. Therefore, the bias operating would be against finding positive program effects. Furthermore, replication of results by other substance use prevention studies suggests that the results found in this review are generalizable to similar populations despite apparent effects of differential attrition.

With regard to internal validity and attrition, only half of the longitudinal studies tested a treatment condition $\times$ attrition status interaction that would indicate whether there was greater attrition among subjects from the intervention group than among subjects from the control group. Only a few of the studies found a significant interaction confirming differential attrition across conditions. Further, fewer than a third of the studies that tracked the same subjects tested a smoking $\times$ treatment condition $\times$ attrition status interaction, with only 2 studies indicating statistically significant differences. Results from these 2 studies showed that baseline smokers in the control group were more likely to be lost to follow-up compared to baseline smokers in the treatment group. Although the findings suggest that subjects' experimental condition may have interacted with attrition status to influence the major tobacco, alcohol, and marijuana drug use measures, research indicates that the magnitude of the treatment condition difference would have decreased because more control group smokers had dropped out, resulting in less smoking in the control group [65,66]. More importantly, this review reveals the general lack of attrition analyses demonstrating whether attrition affects the internal validity of these prevention studies.

Although research indicates that the most effective interventions are implemented with strong fidelity by trained staff [67], fewer than half of the studies under review assessed implementation fidelity to determine whether the program curricula and its contents were actually delivered as intended. Of the studies that measured program implementation fidelity, only a handful of studies conducted separate analyses of program effects by level of student exposure. For example, evaluations of the Life Skills Training

curriculum intervention conducted by Botvin and colleagues lend support to previous findings on the positive effects associated with high-fidelity interventions. Specifically, this study found the strongest long-term effects among students who received at least 60% of the intervention (over the 3 intervention years) compared to students in the no-treatment control group. At the 6-year follow-up, the high-fidelity sample had 44% fewer drug users and 66% fewer polydrug (tobacco, alcohol, and marijuana) users. These results are very promising; however, the case for effectiveness would be considerably strengthened if more long-term studies placed an emphasis on implementation fidelity evaluation.

Another important issue faced in prevention research conducted at the school and community levels is the selection of the proper unit of analysis. It has been argued that the unit of analysis should be the same as the unit of assignment [68]. However, fewer than half of the studies under review actually retained the unit of assignment as the unit of analysis. In many cases, the judgment to select units other than school (i.e., grades, classrooms, or individuals) has been made on the basis of limited budgets and resources that result in a relatively small number of schools or classes to be assigned to conditions. Moreover, at least two other reasons may persuade researchers against selecting school as the unit of assignment and analysis. First, when analysis is conducted at the more aggregated level (e.g., the school), fewer degrees of freedom are available, providing less statistical power to detect true differences. Second, if the school is the unit of assignment and analysis, statistical interactions involving lower-order units (e.g., individual characteristics) will remain unexamined. Thus, decisions about the preferred level of aggregation depend on individual study designs.

Reporting of tobacco and other drug use outcomes

The multifaceted and unique nature of these intervention projects added further to the challenge of reporting their results. Thus, across studies, there was great variability in the reporting of program effects, making it difficult to interpret findings and make comparisons across studies. Oftentimes, information was simply not presented or reporting was unclear. For example, many studies provided inadequate data regarding pretest smoking and other drug use measures, as well as incomplete data on posttest smoking, alcohol, or marijuana use behavior. More often than not, it was unclear whether only selected significant findings were being reported and whether nonsignificant findings were not mentioned. Moreover, virtually all of the studies failed to measure the impact of the intervention on hypothesized mediating variables. Such information would greatly contribute to the understanding of how and why these social influences programs work.

Implications/directions for future research

Despite the methodological and logistical difficulties of conducting large-scale school- and community-based research projects, the results of these individual studies provide long-term empirical evidence of the effectiveness of social influences programs in preventing or reducing tobacco, alcohol, and marijuana use among adolescents. However, this conclusion is still somewhat tenuous given the lack of significant program effects reported in several studies and the great variability that existed in the level of internal and external validity across all studies.

As has been indicated in the discussion above, there is enormous scope for research in the area of drug prevention using improved methodologies. Primarily, better quality research is needed that builds upon the most rigorous scientific evidence accumulated from past generations of prevention trials. To address this need, future studies must place emphasis on the development of detailed conceptual models that are translated into specific hypotheses that, in turn, serve to guide the evaluation design and analysis plan from the very outset of the study. Adequate design conceptualization would then include development of detailed protocols for well-controlled randomized experiments and/or quasi-experiments that clearly define the appropriate target population, conduct statistical power analyses to determine appropriate sample size, and consider feasible strategies for achieving randomization, blocked and/or matched comparison groups, and proper unit of assignment to experimental conditions. Studies should utilize longitudinal or combinations of cross-sectional and longitudinal designs that evaluate long-term outcomes and maintenance effects as adolescents transition from junior high school to senior high school—a time when program effects are likely to dissipate. Special attention should be given to achieving a high response rate and minimizing selective attrition. However, given the length of the study period and current lack of subject tracking procedures, high attrition rates remain inevitable in this current generation of studies; thus, the identification and systematic use of appropriate statistical procedures for dealing with differential attrition are encouraged.

Investigators should also consider ethnic differences in the association between the particular curriculum and subsequent substance use rates. As it stands now, the prevention literature on adolescents is lacking information about the predictors and patterns of substance use among minorities. Even less information is available to determine whether existing interventions that have proved effective among predominantly white, middle-class populations are applicable to other minority populations. Such research could maximize the quality of culturally sensitive prevention and cessation programming and thereby reduce rates in our large subpopulations.

Issues concerning measurement should also receive more attention in future prevention program evaluations. Studies

should identify, further validate, and/or develop proper instrumentation for evaluating presumed mediating and moderating variables in addition to measuring the final behavioral outcomes. For example, studies should utilize instrumentation that is linked directly to operationally defined curriculum goals for producing mediating effects of specific knowledge, skills, attitudes, and behaviors and then link changes in these variables to subsequent drug use behavior. Also, whenever appropriate and possible, multiple measures of factors should be used including standardized teacher and parent reports, direct observations, and individual assessments.

Research designs should also be conceptualized in terms of specific protocols for implementation of existing, modified, or newly developed curricula that include strategies for maintaining and evaluating fidelity of the intervention. In addition to quantitative methodologies, study designs should include qualitative methodologies that aid in the identification of factors that are related to intervention effectiveness or ineffectiveness. For example, qualitative data should be collected on factors that affect the fidelity of program implementation including the completeness of curriculum as actually delivered, adaptation of the intervention protocol, characteristics of the provider, and student involvement or acceptance of program. Qualitative information on social environment characteristics regarding target individuals and their family, classroom, school, and community is also needed to assess the generalizability of psychosocial programs to drug abuse prevention. More research, therefore, is needed in the development, validation, and piloting of instruments that measure such qualitative data. Designs that allow for evaluation of, for example, how individual and contextual factors interact with high-fidelity programming to influence smoking rates and patterns of change are also encouraged.

To further advance the field, studies should develop detailed evaluation plans for data analysis using state of the art techniques for measuring change across long-term periods that involve multiple time point measurements. Designs should allow evaluation of how the duration, intensity, and frequency of the intervention influence initial short-term outcomes as well as the multiple long-term outcomes. Studies should systematically evaluate whether programs produce positive maintenance effects for substances other than cigarettes, such as alcohol, marijuana, and other drug use. The effects of booster programming on the maintenance of long-term reductions in substance use should also be evaluated on a systematic basis. Regarding analytic procedures, additional research is necessary to address the several challenges related to baseline group nonequivalence, testing for potential confounding variables, differential attrition, and proper unit of analysis. To date, there exists a strong need for routine analyses and precise reporting of the techniques utilized and their subsequent effects on substance use programs.

Another area that merits further attention concerns the reporting of data. Considerable variation remains in the descriptions of the interventions, recruitment procedures, subjects, treatments and facilitators, data collection, tracking procedures, attrition analysis, definition and classification of measures, and the manner in which substance use was measured and successes were determined. Future research should include the development of standardized descriptors of the intervention and evaluation that should be used in every substance use prevention study that is published. Moreover, in order to make more meaningful interpretations of the results, it will be necessary to report all categories of substance use behavior, not just selected significant findings. These recommendations serve as a good starting point for the next generation of prevention studies. However, until then, there remains a clear need for investigators to continue to develop and evaluate appropriate interventions designed to address substance use among the youth population.

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