To the Editor:  

It is confusing to read some reviews of school-based tobacco use prevention programs. The article by Wiehe et al. [1] and the accompanying editorial by Glantz and Mandel [2] conclude that tobacco use prevention programming may not be effective. We greatly respect both research groups, but the article and accompanying editorial provide an unbalanced picture of the state of teen tobacco prevention research. We provide four examples of literature not reviewed in these articles.

First, a study of a 12-session version of Project Towards No Drug Abuse [3] does provide effects on tobacco use at a 2-year follow-up in a design matching the rigor of the previous studies we completed [1]. Second, we completed a long-term follow-up study of 25 prevention programs [4], which provides a much more optimistic picture of school-based prevention work. That study was the first literature review of an exhaustive sample of 25 tobacco and other drug use prevention studies that provided at least a 2-year follow-up and included at least a quasi-experimental design. The length of follow-up for the 25 studies ranged from 24 to 180 months (mean = 69 months). Fifteen of 25 evaluation studies reported at least one (hypothesized) significant positive main effect for long-term smoking outcomes for experimental conditions relative to control conditions. The long-term mean reduction in the percentage of baseline nonusers who initiated smoking in experimental conditions compared with control conditions was 11.4% (range = 9% to 14.2%). We concluded that more comprehensive (multi-faceted) social influence programs were relatively effective measured initially and at follow-up.

Third, Tobler and colleagues [5] review 207 programs, with a 1-year follow-up, and provide very good information on the relations of intensity of programming, modalities of programming, and program contents to degree of program effects obtained. This meta-analysis, which indicates positive results of school-based programs on tobacco use, was not cited.

Finally, the Hutchinson Smoking Prevention Project study had several difficulties that are acknowledged by its authors. I co-authored two articles that commented on that study [6,7]. These limitations included the need to discern whether or not that program even manipulated mediators of change, and the social climate backdrop (small rural schools) of that study.

We should be cautious about making statements that might deter continued research on the many promising modalities of tobacco use prevention. Science should be able to advance based on balanced perspectives and the shared goal of surmounting obstacles in implementation and evaluation.

Steve Sussman, Ph.D.  
Jennifer Unger, Ph.D.  
Louise Ann Rohrbach, Ph.D., M.P.H.  
C. Anderson Johnson, Ph.D.

Institute for Health Promotion and Disease Prevention Research  
Departments of Preventive Medicine and Psychology  
University of Southern California  
Los Angeles, California

References

To the Editor:

A potential problem with systematic reviews is that they can lead to misleading conclusions if limited criteria are used to select studies. For example, Wiehe et al [1] concluded that there is little to no evidence of long-term effectiveness of school-based smoking prevention programs, a potentially flawed conclusion because of the limited set of studies reviewed. They considered only randomized trials with data reported at grade 12 or age 18. Thus, they should have concluded only that this particular set of studies did not produce strong evidence of effects by grade 12 or age 18. If they had included some other studies, their conclusions might have been very different [2].

As an example, several studies not reviewed, some randomized and some not, report long-term effects at grades 10 or 11 [3,4]. The sizes of these effects suggest that decay of initial effects was not as rapid as it was for most of the programs reviewed by Wiehe et al [1] and many other earlier programs (including Murray et al [5], a randomized trial [6] with follow-up beyond grade 12 that was missed by this “systematic” review). Indeed, in one instance, the effect at grade 11 was larger than the effect obtained before high school [3]. In all cases where programs produced significant long-term effects, the program was more comprehensive and of greater duration (15 or more sessions) than most of those reviewed by Wiehe et al.

Another limitation of the Wiehe review is that they included totally ineffective programs in with programs that had at least demonstrated short-term effects. For example, Peterson et al [7] did not report any effects on expected mediator variables or short-term smoking, so we cannot be sure that they even had an effective program [8]. DARE is another program included by Wiehe et al that is demonstrably ineffective [9].

Thus, by their selection criteria, Wiehe et al were led to a conclusion that could be very misleading for future tobacco control efforts. The editorial comment by Glantz and Mandel [10] exemplifies how misleading conclusions can be misused; they assume that school-based prevention programs do not (and cannot) work. One could reach a more optimistic conclusion with different selection criteria (e.g., comprehensive programs of 15 or more sessions with some long-term follow-up into high school or beyond).

Brian R. Flay, D.Phil.
Institute for Health Research and Policy
University of Illinois at Chicago
Chicago, Illinois

References


To the Editor:

I wish to make an additional comment regarding the article by Wiehe et al entitled, “A systematic review of school-based smoking prevention traits with long term follow-up” [1]. As Stanton Glantz correctly states in his editorial on this article [2], the statistical differences in favor of the Botvin’s Life Skills intervention may have resulted from chance due to multiple univariate testing, as well as the decision by Botvin et al [3] to use one-tail probabilities. I would like to point out that there is another reason why the results from Botvin et al may have showed statistical differences whereas all other studies did not. Botvin et al used schools as the experimental unit of analysis but then used subjects as the unit for statistical analysis. This approach artificially inflates the error degrees of freedom of the test statistic that produces p values that are lower than nominal (i.e., too many statistically significant results). This type of analysis error, which is well documented in the statistical literature [4–6], is commonly referred to as “pseudoreplication.” This problem has also been discussed as it applies specifically to the design of smoking intervention studies [7].

David A. Ludwig, Ph.D.
Medical College of Georgia
Augusta, Georgia
out additional limitations to an included study. Our study that do not meet these criteria. Third, they point to systematic reviews and meta-analyses with results contrary to Second, they question our inclusion criteria and cite sys-

First, they invoke publications not included in our review. The smoking prevention study by Murray et al [3,4] did have follow-up to at least 12th grade but did not have a randomized control group, and thus did not meet our inclusion criteria. They had randomized schools to one of two interventions, and then at a later date added additional no-intervention control schools. Had it been included, it would have added to the limited number of studies meeting our criteria. Among these studies, there was little evidence of long-term reduction in smoking prevalence. This is especially true given the variety of approaches and populations studied. Criteria for our systematic review, as with all good reviews, are set a priori without regard to whether the studies were effective or ineffective; thus, including ineffective studies that meet inclusion criteria is indeed appropriate.

The short-term impact demonstrated by a number of tobacco reduction programs may wane by the time teens graduate from high school, and thus these programs may reflect socially desirable self-report of tobacco use, or may only temporarily delay smoking among youths inclined to smoke. We therefore included only studies with long-term follow-up of smoking prevalence to 18 years of age.

We do not claim to have cited all previous systematic reviews nor meta-analyses on school-based smoking prevention trials, and there have been many. Those mentioned in the letters did not have similarly rigorous criteria, including the systematic review by Skara et al [5] and the meta-analysis by Tobler et al [6]. The Skara et al review included studies with a quasi-experimental design and did not require that evaluations be done through the 12th grade or age 18 (rather age 16 to 19) [5]. The more significant criterion is the former. Effect sizes in quasi-experimental studies are larger than in randomized studies, and current evidence-based recommendations suggest that studies be reviewed separately for each type of design [7]. The Tobler et al meta-analysis reviewed school-based drug prevention programs with a sub-analysis among a “high-quality set” of studies [6]. These studies included randomized controlled studies but allowed these studies to have a post-test as early as three months after the intervention and did not specify age at follow-up. As discussed in our review paper and above, short periods of follow-up may contribute to inaccurate self-reported smoking behavior due to social desirability. Younger ages of follow-up are not as predictive of adult smoking status, and thus we used a more rigorous standard of follow-up to at least 12th grade or age 18.

Although stratified analyses by various study characteristics may have elucidated factors contributing to a study’s (in)effectiveness, we were unable to perform these analyses due to the limited number of studies meeting our criteria. The limitations to the Peterson et al study [8] are important to discuss but do not change the overall conclusions of our review paper. We concluded that there have been a limited number of studies that met our inclusion criteria. Among these studies, there was little evidence of long-term reduction in smoking prevalence. This is especially true given the variety of approaches and populations studied. Criteria for our systematic review, as with all good reviews, are set a priori without regard to whether the studies were effective or ineffective; thus, including ineffective studies that meet inclusion criteria is indeed appropriate.

Ludwig is incorrect in his interpretation of Botvin et al’s analysis [9]. Botvin et al state in their Methods section, “school means for each drug use variable were then analyzed using the ordinary least-squares regression statistical procedures. . .with the school being used as the unit of analysis.”
Overall, we are hopeful that the attention and discussion this article has produced will lead to further research that meets the criteria of good scientific design and methodology. The conclusions of our article did not extend beyond the scope of the data, and hence are not misleading. Although several subsequent editorials calling for shifts in public health spending and research have been of concern to some, this is an appropriate forum to engage in such debate, focusing on the evidence at hand. We welcome any further discussion and look forward to future work on this important public health issue.

Sarah E. Wiehe, M.D., M.P.H.
Indiana University School of Medicine Indianapolis, Indiana

Michelle M. Garrison, M.P.H.
Dimitri A. Christakis, M.D., M.P.H.
Beth E. Ebel, M.D., M.Sc., M.P.H.
Frederick P. Rivara, M.D., M.P.H.
University of Washington Seattle, Washington

References


The Guest Editors reply:

Our editorial [1] did not argue against continuing research on school-based smoking prevention. Rather, we concluded that there is strong and consistent evidence that these programs are not effective at reducing smoking in the long run (i.e., at the time of high school graduation), so these programs are not a good use of public health resources allocated to reduce tobacco use. Nothing that Sussman et al or Flay present changes that conclusion.

The Project TND report [2] is a single study of an intensive intervention with a high-risk group (students in continuation high schools). It says nothing about the general population of students who receive most school-based tobacco programs. Few of the studies included in Skara and Sussman’s review of 25 adolescent tobacco prevention programs followed students until they graduated, and most of the few that did failed to show significant differences between treatment and control groups at that time [3]. (In any event, the studies that did follow students to graduation and used smoking prevalence as the end point were included in Wiehe et al’s article [4].) Likewise, the four studies included in Tobler et al’s review that followed students to high school graduation did not report any long-term benefits [5]. Finally, although Sussman et al quote their earlier letter criticizing the Hutchinson Smoking Prevention Project [6], they ignored the compelling responses to those criticisms [7].

Flay’s criticism that Wiehe et al [4] “included totally ineffective programs in with programs that had at least demonstrated short-term effects” misses the entire point of doing a meta-analysis. Of course one would draw positive conclusions from an analysis that excludes papers with negative findings. Doing so, however, would hardly represent an objective analysis of the evidence on the long-term effectiveness of school-based programs.

Although didactic material could (and should) be included in the regular curriculum through infusion in writing, social science, or science to provide objective information about the dangers of tobacco use and the behavior of the tobacco industry at little or no cost and without taking valuable class time from regular academic programming, smoking prevention programs are better done through mechanisms outside the curriculum. Given that there are effective tobacco control interventions—including aggressive media campaigns and smoke-free workplaces and homes—increasingly scarce tobacco control resources should be directed to these effective (and cost-effective) interventions, not expensive and ineffective school-based programs.

Stanton A. Glantz, Ph.D.
Lev M. Mandel, M.Sc.
Center for Tobacco Control Research and Education
University of California, San Francisco
San Francisco, California
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