

Letters to the Editor

“Smoke-free Class Competition”: Far-reaching conclusions based on weak data

The “Smoke-free Class Competition (SFC)” is a European low-level school-based smoking prevention campaign. Classes commit themselves to stay smoke-free for a duration of 6 months (from fall to spring). Those classes that refrain from smoking take part in a prize draw, where they can win a number of attractive prizes, the main prize being a trip to another European country (Hanewinkel et al., 1998).

Up to now, two controlled prospective trials (Vartiainen et al., 1996; Wiborg and Hanewinkel, 2002) and one randomized control trial (Crone et al., 2003) with follow-up periods between 6 and 12 months after the end of the intervention have been conducted to evaluate the campaign under normal classroom conditions. The three studies suggest a short-term effect of SFC in delaying the onset of smoking in adolescence. Recently, another randomized controlled trial on the effectiveness of SFC was published (Schulze et al., 2006). This study has several methodological problems, which are not sufficiently discussed in the paper. This is especially disconcerting since the authors draw far-reaching conclusions for the implementation of school-based prevention programs in general.

The major methodological problem with the paper is that the randomization failed. Why do we randomize? The main reason for using randomization to allocate treatments to subjects (in this case school classes) in a controlled trial is to prevent bias (Altman and Bland, 1999). We want to compare the outcomes of treatments given to groups which do not differ in any systematic way before the treatment starts. Schulze et al. randomly assigned matched pairs of schools to intervention and control group, which were recruited in the Rhine-Neckar County, Germany. Information of the realized sample is given only for the individual level (not for the class or school level) for three variables: sex, age, and smoking status. In two of the variables – age and smoking status – significant differences could be detected at baseline between the groups. These differences are not small, they are substantial. The difference between the two groups with regard to the distribution of never-smokers is nearly 10% points (55.7% vs. 46.4%) and with regard to actual smokers the difference between the groups is about 5% points (11.6% vs. 16.3%). Thus, Schulze et al. have recruited samples which are not comparable.

According to the authors no schools or classes got lost over the course of the study. On an individual level, there was

a loss of 2191 pupils (54.2%). These pupils had to be excluded from the statistical analysis because their personal code was not assignable. Thus, an analyzable sample of 1852 pupils remained. From this sample, 32 out of 980 pupils from the intervention group (3.3%), but 116 out of 872 pupils from the control group (13.3%) did not answer the questions on their smoking status.

This means, compared to the intervention group, in absolute terms more than three times and in relative terms more than four times more pupils in the control group did not give information on their smoking status—the main outcome variable of the study. The authors do not make any effort to either explain or to discuss this substantial difference among the groups.

Overall, fewer pupils with complete data on smoking remained in the control group (40.2%) compared to the intervention group (43.8%; $\chi^2(1)=5.4, P<0.05$). The authors fail to make a correct indication of this systematic difference.

Attrition and missing data are always a problem in longitudinal prevention research. In cessation studies, persons that did not give information on their smoking status (missing data) are regarded as smokers (Adelman et al., 2001). We have used the data from Schulze et al. to carry out such an analysis, in which the pupils without indication of smoking status are regarded as smokers (Table 1).

Based on this analysis, the data could be interpreted as follows: at follow-up, 18 months after the end of the intervention, significantly fewer pupils of the intervention group (40.7%) compared to the control group (48.0%) were actual smokers. Furthermore, 37.4% of the intervention grouped never smoked in their lives compared to only 31.7% of the control group. It becomes clear that the data could be interpreted as a success of the intervention program.

Furthermore, the authors either did not measure the short-term effects of the program or they do not report on it. The effects of the campaign should be measured immediately after the end of the campaign and the follow-up measurement should serve to see how permanent the effect is.

The biggest problems of the paper are the far-reaching conclusions and recommendations of the authors.

The main conclusion of the authors that SFC “did not prevent smoking among adolescents” (p. 33) is derived from the results of one study. This conclusion is based on a weak data basis and methodological shortcomings mentioned above and neglects the results of other related studies in the field.

Table 1
Smoking status at follow-up for intervention and control group (pupils that did not give information on their smoking status are counted as smokers)

	Intervention group		Control group		χ^2 (2)	P
	N	%	N	%		
Smoker	399	40.7	419	48.0	10.6	<0.01
Ex-smoker	214	21.8	177	20.3		
Never-smoker	367	37.4	276	31.7		
Overall	980	100	872	100		

The authors go even further and recommend that “before investing additional funds in preventive school programs, it should be ensured first of all that children can grow up in smoke-free environment” (p. 38) meaning a complete stop of *any* school-based prevention programs.

There is no empirical support which indicates that structural tobacco control measures, such as the complete ban of tobacco advertising, the abolition of vending machines or the creation of smoke-free environments, should be implemented *before* school-based prevention programs can be implemented successfully.

All experiences seem to indicate that programs directed to young people should be continued during adolescence and that one single campaign is very unlikely to have a long-term effect. Therefore, school-based prevention programs need to be embedded in a comprehensive tobacco prevention strategy, including different age appropriate programs and structural control measures.

In a country like Germany, with some modest tobacco control measures only recently implemented, with no health education as a subject in schools (it is organized “cross-curricular” often meaning that health education does not exist), with no regular tobacco prevention activities in schools, with some of the highest smoking prevalence in youth in the Western world (Langness et al., 2005), we urgently need to have effective school-based smoking prevention activities in place. It is unethical to wait with school-based prevention activities until the German Government has implemented all the mentioned structural measures. This could take many years or even decades from now. Therefore, it is counterproductive to create competition between school-based programs and structural tobacco control measures.

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“Smoke-free Class Competition”: A reply to the initiators of the program

The ‘Smoke-free Class Competition’ (SFC), being initiated by Erkki Vartiainen (National Public Health Institute, KTL) and coordinated by Reiner Hanewinkel (Institute for Therapy and Health Research, IFT-Nord) on the European level is the largest primary smoking prevention program for pupils performed in Germany. Based on this initiative, four studies evaluated the effectiveness of the program so far. Three of them find a positive short term effect within 3 to 12 months after the end of the Smoke-free Class Competition (Wiborg and Hanewinkel, 2002; Vartiainen et al., 1996; Crone et al., 2003). The study of Crone et al. (2003) also tested for a sustainable prevention effect after 15 months but could not detect a significant difference between intervention and control group (5%-level). The same applies to our evaluation (Schulze et al., 2006), which provides information about the effectiveness of the program after the even longer period of 18 months.

Following the publication of our data, an intensive debate about school-based prevention programs in general and the effectiveness of the SFC in particular started in Germany, which we explicitly appreciate. In this context, the initiators of the competition Hanewinkel et al. (2006) now disapprove of our results in a comment in this issue of Preventive Medicine. In particular, the following criticisms were expressed: (i) the randomization failed; (ii) the data could be interpreted (with other strategies of analysis) in another and positive way; and (iii) we drew too far reaching conclusions.