

Longer-term primary prevention for alcohol misuse in young people: a systematic review

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ABSTRACT

Objective To identify and summarize rigorous evaluations of psychosocial and educational interventions aimed at the primary prevention of alcohol misuse by young people aged up to 25 years, especially over the longer term (>3 years).

Methods Cochrane Collaboration Systematic Review.

Data sources A comprehensive search of 22 databases and recursive checking of bibliographies for randomized and non-randomized controlled trials and interrupted time-series studies.

Main outcome measures Objective or self-report measures of alcohol use and misuse.

Results Fifty-six studies were selected for inclusion in the systematic review. Twenty of the 56 studies showed evidence of ineffectiveness. No firm conclusions about the effectiveness of prevention interventions in the short- and medium term were possible. Over the longer term (>3 years), the Strengthening Families Programme (SFP) showed promise as an effective prevention intervention. The Number Needed to Treat (NNT) for the SFP over 4 years for three alcohol initiation behaviours (alcohol use, alcohol use without permission and first drunkenness) was 9 (for all three behaviours). One study also highlighted the potential value of culturally focused skills training over the longer-term (NNT = 17 over 3.5 years for 4+ drinks in the last week)

Conclusions (1) Research into important outcome variables needs to be undertaken; (2) the methodology of evaluations needs to be improved; (3) the SFP needs to be evaluated on a larger scale and in different settings; (4) culturally focused interventions require further development and rigorous evaluation; and (5) an international register of alcohol and drug misuse prevention interventions should be established and criteria agreed for rating prevention interventions in terms of safety, efficacy and effectiveness.

KEYWORDS Alcohol, prevention, youth.

INTRODUCTION

World-wide, 5% of all deaths of young people between the ages of 15 and 29 years are attributable to alcohol use (Murray & Lopez 1997; Jernigan 2001). In Europe, one in four deaths of men in the age group 15–29 is related to alcohol. In parts of Eastern Europe, the figure is as high as one in three. Overall, 55 000 young people in Europe died from causes related to alcohol use in 1999 (World Health Organization 2001). The Annual Report of the Chief

Medical Officer of the Department of Health (England and Wales) (Donaldson 2001) highlights a worrying trend for teenagers who drink alcohol to consume larger quantities. In 1998, average consumption among 11–15-year-old drinkers was 9.9 units of alcohol a week (1 unit contains 8 g of pure alcohol), compared to 6.0 units a week in 1992.

The range of harms that are relevant to alcohol use in young people make a straightforward classification and definition of alcohol misuse difficult, but include immedi-

ate harm to self through alcohol overdose and alcohol-related injury, immediate harm to others through drinking and driving and alcohol-related injury, and longer-term harm to self through the development of inappropriate drinking behaviour and patterns. One potentially good indicator of longer-term harm is age of first drinking experiences. Results from the National Longitudinal Alcohol Epidemiologic Survey (Grant & Dawson 1997) ($n=27\ 616$) showed that the life-time alcohol dependence rate of those who initiate alcohol use by age 14 is four times as high as those who start by age 20. Adjusting for potentially confounding variables, the odds of dependence decreased by 14% with each additional year of delayed initiation.

The incidence and prevalence of alcohol misuse and the harm caused by the alcohol misuse of young people is an area of concern for policy makers, health promotion workers, the criminal justice system, youth workers, teachers and parents. It is therefore important to have a clear understanding of the effectiveness of alternative interventions in the primary prevention of alcohol misuse by young people. This paper reports from a Cochrane Collaboration Systematic Review (Foxcroft *et al.* 2002) of the effectiveness of primary prevention interventions for alcohol misuse in young people.

In this systematic review primary prevention is defined as 'projects that have services directed toward reducing the incidence or prevalence of alcohol misuse and related problems or influencing knowledge, attitudes and behaviours related to drinking beverage alcohol' (Staulcup *et al.* 1979). Primary prevention differs from secondary and tertiary prevention in that it deals with populations that are not selected to receive services on the basis of symptomology such as drug abuse. The clients of primary prevention are typically the total populations within particular schools, age levels or neighbourhoods.

There were two objectives in the review: (1) to identify and summarize rigorous evaluations of psychosocial and educational interventions aimed at the primary prevention of alcohol misuse by young people; and (2) to assess the effectiveness of primary prevention interventions over the longer term (>3 years).

METHOD

The systematic review followed the methodological approach of the International Cochrane Collaboration, specifically the Drugs and Alcohol Review Group (Ferri *et al.* 2002). The general approach is to try to find all published and unpublished studies that are relevant and are of sufficient methodological quality. These studies are then evaluated rigorously by two or more Cochrane

reviewers and the results are incorporated onto the Cochrane Collaboration Database of Systematic Reviews. In this systematic review of the effectiveness of primary prevention for alcohol misuse in young people we have not restricted the studies included to randomized controlled trials (RCTs), as is usual in Cochrane Collaboration Systematic Reviews. This is because some prevention programmes are social or organizational interventions where large RCTs are difficult and expensive to undertake, so alternative evaluation designs are used, e.g. matched controlled before-after design and interrupted time-series design. However, in appraising such studies particular attention was given to identifying potential sources of bias.

Inclusion criteria: participants

Participants were young people up to 25 years of age. For the purpose of this systematic review, young people were defined as children, adolescents and young adults aged up to 25 years. This is consistent with a previous systematic review (Foxcroft *et al.* 1997) and, although arbitrary, is specified so that all school- and college-based prevention interventions can be included. If a study contained some individuals aged over 25 then this study was included provided that the results are clearly relevant and generalizable to young people under 26 years. Similarly, if a study of older adults also contains data on young people then these results were included in this systematic review, provided that the results on the young people subset are clearly distinguishable, relevant and generalizable to other young people.

Inclusion criteria: prevention interventions

Studies included in this systematic review evaluated psychosocial or educational interventions aimed at preventing the onset of alcohol use or alcohol misuse by young people. Psychosocial interventions are defined as those interventions that aim specifically to develop psychological and social skills in young people (e.g. peer resistance) so that they are less likely to misuse alcohol. Educational interventions are defined as those interventions that aim specifically to raise awareness of the potential dangers of alcohol misuse (e.g. increased knowledge) so that young people are less likely to misuse alcohol. Studies were included if the intervention was solely psychosocial or educational or if there was a psychosocial or educational component as part of a larger, multi-component, intervention. Studies were also included if the intervention was not aimed primarily at alcohol misuse prevention but alcohol misuse outcome measures were reported, for example generic drug education programmes or healthy school or community initiatives.

Table 1 Search strategy.

1. Alcohol	2. Population	3. Intervention	4. Outcome	5. Evaluation
drink*	group	intervent*	prevent*	evaluat*
alcohol*	adolescen*	educat*	reduc*	success*
liquor*	teenage*	promot*	improv*	effectiv*
beer*	youth*	programme*	increas*	measur*
wine*	young people	adverti*	decreas*	examin*
spirits	early adult	counsel*	chang*	assess*
drunk*	young adult	treatment*	cessation	compar*
intoxicat*		campaign*	drink driv*	trial*
binge		mass media	dui	rct
		policy	health	
		policies	abstain*	
		legislation	stop*	
		hazard*	problem	
		harm*	intoxicat*	
			drunk*	

*Search term wildcard.

Inclusion criteria: outcome measures

Studies were included if they reported outcomes that were (i) objective or self-report measures of drinking behaviour; or (ii) measures of alcohol-related problems, including alcohol-related risky sex, antisocial behaviour, violence and crime (including drinking and driving).

Search strategy for identification of studies

The Cochrane Collaboration Drugs and Alcohol Collaborative Review Group search strategy was revised for use in this systematic review. Table 1 lists the search terms used, and syntactic variations made for different databases (see Table 2). All searches were undertaken between February 2001 and June 2001.

Critical appraisal and data extraction*Stage 1*

All papers identified through the search were screened by one reviewer for relevance (did the paper report an evaluation of an alcohol or drug misuse prevention programme?), outcome (were alcohol-relevant outcome variables reported?) and design (did the design of the evaluation meet our inclusion criteria?). All searches included non-English language literature and studies with English abstracts were assessed for inclusion (using relevance, outcome and design criteria). When considered likely to meet inclusion criteria, translations were obtained. All obtained papers and review articles were hand-searched for relevant citations.

Table 2 Databases searched.

Project CORK
BIDS ISI (Bath Information and Data Services)
Conference proceedings on BIDS
Current contents on BIDS
PSYCLIT
ERIC (US)
ERIC (Australia, Canada and UK)
ASSIA
MEDLINE
FAMILY RESOURCES DATABASE
HEALTH PERIODICALS DATABASE
EMBASE
Dissertation Abstracts
SIGLE
DRUG INFO
SOMED (Social Medicine)
Social Work Abstracts
National Clearinghouse on Alcohol and Drug Information
Mental Health Abstracts
DRUG INFO.
DRUG database
Alcohol and Alcohol Problems Science Database – ETOH

Stage 2

For those papers that passed Stage 1, data on each study were extracted by two reviewers and confirmed if necessary (in case of discrepancy) by consultation with other reviewers. Discrepancies that could not be resolved easily were referred to the rest of the project team. Data extraction was guided by a detailed proforma for systematically extracting information on the methods, quality and results of each study, including:

- was the study properly controlled;
- what methods of randomization or allocation to intervention groups were used;
- were the groups comparable at baseline;
- were steps taken to maximize the validity of self-reported behaviour;
- were adjustments made for confounding;
- what was the attrition rate and how was this dealt with; and
- did the unit of analysis correspond to the unit of randomization.

Finally, the roles of chance, confounding and bias in the study were considered and a decision taken as to whether or not to include the paper in the systematic review. Studies that had been published in duplicate were only included once. If important data were missing, attempts were made to contact the first author to complete the information necessary for the critical appraisal.

Data analysis

As the heterogeneity of settings, design of studies, source and format of interventions, outcomes measured and target group was substantial, an overall estimate of effect (i.e. a meta-analysis) would have little practical meaning. Therefore the data were analysed in the form of a structured narrative systematic review (Khan *et al.* 2001). An additional intention-to-treat analysis is presented for selected studies reporting positive outcomes in longer-term (>3 years) evaluations. This comprises a re-analysis of study results on an intention-to-treat basis as this information is of more relevance to health-care professionals and policy makers.

Intention-to-treat is an analysis in which data for all subjects are analysed on the basis of the groups/interventions to which participants were allocated, even if some participants did not in fact receive the full intervention. This is an important concept in public health and health policy development, because only a proportion of the population at whom an intervention is targeted would accept/tolerate/receive the intervention, yet the health of the population needs to be considered as a whole and not simply the subset of people who accept or receive an intervention. In this systematic review we have concentrated on an intention-to-treat analysis for those studies reporting longer-term outcomes (>3 years), as the longer-term impact of alcohol misuse prevention programmes is of most interest to public health and health policy makers.

A slightly conservative test of the effectiveness of an intervention is to include all the individuals measured at baseline in the final analysis and to assume that any missing data (i.e. data from those lost to follow-up) would accrue to be equivalent to the results in the control group—regardless of whether the individuals were allo-

cated originally to the intervention or to a control group. The approach used in this re-analysis was based on this assumption that the event rate for the dropouts in both groups was the same as for those individuals in the control group who were assessed. This is a reasonable and slightly conservative approach, provided that attrition is unrelated to factors which influence outcome.

In this systematic review, only the partially effective longer-term studies have been re-analysed on an intention-to-treat basis. The only reason for re-analysing the other studies would be to determine which interventions could be harmful. The appropriate re-analysis would be to assume worst case for intervention dropouts, i.e. the same event rate as for intervention assessed. Because the control dropout rate would again be taken as the same as the control assessed rate, this would have no effect on the analysis. It might be more realistic to expect the dropouts to be the worst cases and therefore more likely to have a poor response (an event). This could be modelled by a higher event rate in both dropout groups, but provided that there is no differential between the groups this will not affect conclusions materially about the intervention effect.

RESULTS

Description of studies

Systematic electronic database searches produced over 6000 titles, although many titles appeared more than once. A number of papers, including unpublished reports, were also found from other sources. After these titles and abstracts were previewed, over 600 papers/reports/dissertations were obtained and screened against the inclusion criterion of design, relevance and outcome and many papers were excluded at this stage. Detailed examination of remaining papers through the completion of systematic data extraction forms led to a substantial number being rejected, leaving 56 studies that met the quality inclusion criteria detailed above. These 56 studies were included in the systematic review and therefore constitute the data for the systematic review, with results from the analysis of these data presented and discussed below. This is an increase of 23 studies to the previous systematic review report (Foxcroft *et al.* 1997). Overall, since the previous systematic review there has been a general improvement in the quality of evaluations, although some methodological concerns persist.

The majority (84%) of the evaluations have taken place in the United States. Of the others, three were Canadian, two British, one Swedish, one Norwegian, one Australian and one was an international study encompassing Australia, Chile, Norway and Swaziland. Thirty-

two interventions were generic drug (including alcohol) education programmes, where alcohol baseline and outcome measures were clearly reported. The remaining 24 studies reported interventions targeted specifically at alcohol.

Methodological quality of included studies

In this systematic review only a small number of the many intervention studies found in the literature search had been well designed and evaluated adequately, even those that were judged to be of sufficient quality to be listed in this systematic review (i.e. sufficient but not excellent quality). Problems with poor quality studies that were excluded from the systematic review included lack of suitable control groups (non-random allocation or non-equivalent groups), lack of pretest information, high levels of attrition, inappropriate analysis for the unit of allocation and more generally a poor quality presentation of results, often in well-respected peer-reviewed journals. The difference between those studies included and those excluded fall into two categories: all RCTs were included, and non-RCTs were included only if, on the whole, the quality of the study was reasonably good. The decision to include all RCTs was taken on the basis that these studies were methodologically stronger and this was clear from the data extraction process (Stage 2, see Method: critical appraisal and data extraction section). For non-RCTs those studies that were judged to be reasonably rigorous were included, based on judgements by the review team of a study's susceptibility to bias. Studies that were fatally flawed were excluded. Studies that may have been partly compromised because of methodological shortcomings were included and these methodological shortcomings were reported in detail (see the full version of the systematic review report on the Cochrane Database of Systematic Reviews, Foxcroft *et al.* 2002).

Of the 56 included studies, 41 were RCTs, 14 were non-randomized control group designs with before and after measures, and there was one interrupted time series (ITS) design. On the whole, the RCTs were of better methodological quality than the non-randomized, matched-control group studies. However, for the RCTs included in this systematic review there was no clear reporting of allocation concealment and for many studies method of randomization was not reported.

Over and above these concerns, there were two major methodological limitations of most included studies. The first is that in most of the studies the unit of allocation (usually class, school or community) and the unit of analysis (usually individuals) were different. The studies of Cook *et al.* (1984); Duryea & Okwumabua (1988); Baldwin (1990); Magura *et al.* (1994); Palinkas *et al.* (1996); Werch *et al.* (1996a); Werch *et al.* (1996b);

Marlatt *et al.* (1998); Werch *et al.* (1998); Monti *et al.* (1999); Werch *et al.* (2000a); and Werch *et al.* (2000b) avoided this problem, as they randomized and analysed by individual. However, in these studies contamination is a concern as interventions are often delivered at a group, rather than an individual, level. For the other studies in which unit of allocation and unit of analysis are different this is potentially a problem as individuals from a single setting (class, school or community) tend to be more like one another than they are like individuals in a different setting, and the within-setting correlation in the data (indexed by the intraclass correlation coefficient, ICC) adds an additional component of the variability to an intervention group's mean over and above that attributable to either the individuals or the interventions themselves (Donnor *et al.* 1990). Unless this component is accounted for in the analysis, the evaluation of the intervention effects will be biased positively in proportion to the magnitude of the ICC and the number of respondents in each setting (Zucker 1991). Only the studies of Clayton *et al.* (1991); Hansen & Graham (1991); Botvin *et al.* (1995a); and Klepp *et al.* (1995) appeared to use a statistical approach, in some analyses, that took account of the ICC. It was beyond the technical scope of this systematic review to reassess each set of results in the light of this analytical shortcoming in most studies. One can only point this out and strongly encourage future evaluations to report, where possible, appropriate analyses.

The second major limitation concerns the often high levels of attrition in some of these studies, especially those with longer-term follow-up. Higher attrition, in the absence of an intention-to-treat (ITT) analysis, threatens the validity of the results. Attrition is the number or proportion of individuals recruited into a study that did not receive the intended intervention or were not assessed at follow-up using the study's instruments. Such dropout can occur at various stages from recruitment to short- or long-term follow-up and may threaten the continued comparability of treatment and control groups and otherwise weaken the internal validity of the study. For example, participants may drop out of a study because they do not like the intervention. Excluding these participants from the analysis could result in an overestimate of the effectiveness of an intervention because it hides information about how acceptable or tolerable an intervention is. Many journal editors and referees like to see attrition rates at no more than 20%, although in large-scale social and community interventions with a medium- to long-term follow-up this is difficult to achieve. In any case, all authors should clearly describe attrition rates, how they varied between different treatment and control groups, and how attrition was dealt with in any statistical analysis, for example through an intention-to-treat analysis. In the studies included in this review the following 21 stud-

ies had attrition rates of less than 20%: Williams *et al.* (1968); Botvin *et al.* (1984); Cook *et al.* (1984); Beaulieu & Jason (1988); Duryea & Okwumabua (1988); Perry & Grant (1988); Allison *et al.* (1990); Hansen & Graham (1991); Ringwalt *et al.* (1991); Caplan *et al.* (1992); Harmon (1993); Bremberg & Arborelius (1994); Wilhelmson *et al.* (1994); Perry *et al.* (1996); Werch *et al.* (1996a); Werch *et al.* (1996b); Marlatt *et al.* (1998); Monti *et al.* (1999); Schinke *et al.* (2000); Werch *et al.* (2000a); Spoth *et al.* (2001a, 2001b).

Findings

Some studies reported partial effectiveness (see below), in that some self-report measures of drinking behaviour (often only one or two measures among several) were influenced positively by the intervention programme. Ineffective interventions were regarded as those that had no statistically significant influence on subsequent self-reported drinking behaviour. There were also some interventions that had an unforeseen opposite effect on subsequent self-reported drinking behaviour (i.e. appeared to increase drinking), and these are reported for completeness. However, it should be noted that this phenomenon may be artefactual, due to poor design, method or analysis (e.g. *post-hoc* tests) and should therefore be interpreted cautiously. Follow-up periods (time from baseline assessment) were split into three groups: short term (1 year or less); medium term (from 1 to 3 years); and long term (over 3 years). More details of the essential components of each intervention programme are contained in the full systematic review report on the Cochrane Database of Systematic Reviews (Foxcroft *et al.* 2002).

Interventions with short-term follow-up (up to 1 year)

Fifteen studies reported partially effective short-term interventions (Williams *et al.* 1968; Gliksman *et al.* 1983; Cook *et al.* 1984; Gilchrist *et al.* 1987; Perry & Grant 1988; Pentz *et al.* 1989b; Baldwin 1990; Hansen & Graham 1991; Caplan *et al.* 1992; Harmon 1993; Wilhelmson *et al.* 1994; Werch *et al.* 1996a, 2000a; Holder 1997; Monti *et al.* 1999). It is difficult to assess the potential of these projects from such short-term results, especially as the pattern and scale of positive outcomes for these studies is unconvincing. Many of these studies reported some effective and some ineffective outcomes, and it is difficult to know what to make of such mixed results.

Twenty-four interventions with only a short-term follow-up reported some non-significant outcomes (Williams *et al.* 1968; Goodstadt & Sheppard 1983; Botvin *et al.* 1984; Cook *et al.* 1984; Moskowitz *et al.*

1984; Durrant 1986; Beaulieu & Jason 1988; Perry & Grant 1988; Allison *et al.* 1990; Brewer 1991; Hansen & Graham 1991; Ringwalt *et al.* 1991; Newman *et al.* 1992; Harmon 1993; Bremberg & Arborelius 1994; Magura *et al.* 1994; Wilhelmson *et al.* 1994; Palinkas *et al.* 1996; Werch *et al.* 1996a, 1996b, 1998, 2000a; Goldberg *et al.* 2000; McBride *et al.* 2000), and there were no clear or systematic differences between those judged partially effective and those judged ineffective. Indeed, some interventions reported both significant and non-significant effects, depending on the outcome variable used.

Four studies reported interventions which appeared to increase drinking behaviour (relative to control groups) in the short term (Goodstadt & Sheppard 1983; Ellickson & Bell 1990; Brewer 1991; Werch *et al.* 2000a). The interventions carried out in these studies did not appear to be characteristically different from the studies described above as partially effective or ineffective: most interventions combined social skills training with knowledge-based education. This phenomenon may be artefactual, due to poor design, method or analysis (e.g. *post-hoc* tests) and should therefore be interpreted cautiously.

Interventions with medium-term follow-up (from 1 to 3 years)

Of the 12 studies reporting medium-term partially effective interventions (Scaggs 1985; Duryea & Okwumabua 1988; Hansen *et al.* 1988; Hopkins *et al.* 1988; Bagnall 1990; Botvin *et al.* 1995a, 1995b; Perry *et al.* 1996; Shope *et al.* 1996a, 1996b; Marlatt *et al.* 1998; Wagenaar *et al.* 2000; Werch *et al.* 2000b), few were convincingly effective, and most were marred by methodological shortcomings. Studies worth noting are (a) the Start Taking Alcohol Risks Seriously (STARS) school and family intervention (Werch *et al.* 2000b), based in two schools, comprising a strong design, low attrition and significant effects on alcohol use and misuse, although the effect sizes seem small; (b) Botvin *et al.* (1995b) culturally focused intervention evaluation, although design limitations hamper generalizability; and (c) Scaggs's (1985) PhD work based on the 'self-in-situation theoretical model', although with this study differential attrition is a problem.

Nineteen studies that carried out a medium-term follow-up found no evidence of intervention effectiveness (Scaggs 1985; Dielman *et al.* 1986; Duryea & Okwumabua 1988; Hopkins *et al.* 1988; Pentz *et al.* 1989a; Bagnall 1990; Ellickson & Bell 1990; Hansen & Graham 1991; St Pierre *et al.* 1992; Rosenbaum *et al.* 1994; Botvin *et al.* 1995a; Klepp *et al.* 1995; Sheehan *et al.* 1996; Shope *et al.* 1996a; 1996b; Marlatt *et al.* 1998; Sussman *et al.* 1998; Wagenaar *et al.* 2000; Werch

et al. 2000b). Several of these had reported previously some short-term significant effects, and this suggests that any early reductions in drinking behaviour achieved by the intervention had eroded in the medium-term.

Two interventions were found to increase drinking behaviour in the medium term. Duryea & Okwumabua (1988) reported that the intervention group (knowledge and social skills programme; US teenagers) reported more excessive drinking than a control group 3 years later. Hopkins *et al.* (1988) found evidence of a negative effect of an intervention (social skills and affective education; US teenagers) in 10% of alcohol-related variables. As mentioned above, this phenomenon may be artefactual, due to poor design, method or analysis (e.g. *post-hoc* tests) and should therefore be interpreted cautiously.

Interventions with long-term follow-up (over 3 years)

Three studies reported effective longer-term interventions. Botvin *et al.* (1995a) followed-up several thousand US teenagers 6 years after initial administration of a life skills training (LST) intervention, which is a multi-modal drug education programme. They reported significantly less self-reported drunkenness in those teenagers who received the intervention compared with a control group, although the effect size seems small. Botvin *et al.* (1995a) also report more convincing results for those teenagers who attended at least 60% of the intervention sessions—a ‘hi-fidelity sample’—but this analysis is flawed as the direct comparability of this subgroup with the full control group is compromised (i.e. this is not an ‘intention-to-treat’ analysis).

Schinke *et al.* (2000) reported a long-term follow-up of a culturally focused school and community intervention with Native Americans. A skills-based intervention group were around 7% less likely than a control group to be weekly drinkers 3.5 years after baseline measurement. This was statistically significant, although the public health impact of this effect is difficult to judge.

Spoth *et al.* (2001a, 2001b) conducted an evaluation of a family-based intervention using a strong design, and although there was a moderate attrition rate, there was also a consistent pattern of effectiveness across the three drinking behaviour variables they reported. Importantly, the effectiveness of this intervention seemed to increase over time, reflecting the developmentally orientated intervention outcome model on which the intervention is based. This intervention deserves further consideration and study on the basis of these results.

Five other studies reported long-term follow-ups. Ellickson & Bell (1990) reported from a large sample study of US teenagers in a trial of Project ALERT, which incorporated information and social skills education. Early signs of partial effectiveness were not repeated over

the long-term—by the end of high school (5-year follow-up) no effects of the intervention remained. Longer-term outcome results from Project Northland (Perry *et al.* 1996) showed that at 4-year follow-up there were no significant effects of the Project Northland intervention over the control group. Wynn *et al.* (1997) reported a longer-term follow-up of the Alcohol Misuse Prevention Study (AMPS) of Dielman and colleagues (Dielman *et al.* 1986) and stated that there was no significant effect of the AMPS curriculum on 10th-grade alcohol misuse. Clayton *et al.* (1991) followed-up a Drug Abuse Resistance Education (DARE) intervention after 5 and 10 years (when participants were 20 years old), and found that DARE status was unrelated to alcohol use at follow-up. The evaluation by Loveland-Cherry *et al.* (1999) showed a mixed pattern of results. There was a significant but very small positive effect of the intervention on alcohol use, no significant effect on alcohol misuse, and the authors also showed in a *post-hoc* subgroup analysis that those individuals in the intervention group who were already drinkers at baseline were less likely to use and misuse alcohol at follow-up compared with similar controls.

Ineffective interventions

Of the interventions described above as reporting no effects of the intervention over the short, medium or longer term, it may be that some were evaluated poorly and that therefore ineffectiveness has not been confirmed. For many interventions, however, it is probably reasonable to say that the evidence base does not support their continued use in the primary prevention of alcohol misuse for young people, other than in further research studies. These interventions are: ‘DAPPER’ (Allison *et al.* 1990), ‘Alcohol Education in Schools’ (Bagnall 1990), ‘A Drug Abuse Prevention Programme’ (Beaulieu & Jason 1988), ‘It’s your decision’ (Bremberg & Arborelius 1994), ‘DARE’ (Clayton *et al.* 1991; Ringwalt *et al.* 1991), ‘AMPS’ (Dielman *et al.* 1986; Shope *et al.* 1996a, 1996b), ‘Multi-component Inoculation Programme’ (Durrant 1986), ‘Project ALERT’ (Ellickson & Bell 1990), ‘HLAY’ (Hopkins *et al.* 1988), ‘Shifting Gears’ (Klepp *et al.* 1995), ‘A Drug Education Course’ (Moskowitz *et al.* 1984), ‘RPDD’ (Newman *et al.* 1992), ‘PALS’ (Palinkas *et al.* 1996), ‘MPP’ (Pentz *et al.* 1989a), ‘Project Northland’ (Perry *et al.* 1996), ‘PASS’ (Sheehan *et al.* 1996), ‘Stay SMART’ (St Pierre *et al.* 1992) and ‘Towards No Drug Abuse’ (Sussman *et al.* 1998).

Community interventions

It is also worth highlighting separately three recent large-scale community-based interventions. Community interventions have attracted much interest recently as a more

theoretically robust approach than individually orientated interventions (Gorman 2001) and one community trial (Holder 1997) has demonstrated a 10% annual reduction in alcohol-related crashes among all drivers (not specifically youth) across three communities. A cost-effectiveness analysis estimated that for each \$1 spent on the interventions there was a saving of \$2.88. In the same trial, a greater reduction in the number of retail outlets selling alcohol to apparent under-age buyers was found in the intervention communities (around 30%) than in the control communities (12%) in the first year of follow-up. However, it is unclear whether changes in under-age sales will result in reduced alcohol use and misuse (i.e. will young people obtain alcohol elsewhere?).

The second large community trial reported recently is the Communities Mobilizing for Change on Alcohol programme (Wagenaar *et al.* 2000). This study has reported 3-year follow-up results, but there have been no clear statistically significant effects in the intervention communities compared to the control communities for under-age retail sales, self-reported drinking or heavy drinking. One just statistically significant effect ($P=0.05$) that was found concerned arrests for drinking and driving among 18–20-year-olds. The net difference between the intervention and control communities from pre- to post-test measures of drink driving arrests was -30.296 arrests per 100 000 population per year. Put simply, there were around 30 fewer arrests per 100 000 population across the intervention communities than in the control communities. Longer-term results and cost-effectiveness analysis of these data will be important.

The third large community trial is Project Northland (Perry *et al.* 1996) although this intervention is predominantly school-based with strong parental and community involvement. Phase II of the intervention study, currently under way, increases the community aspect significantly. The phase I evaluation included in this systematic review found significant effects of the intervention on drinking behaviour while the intervention was ongoing, but this effect dissipated once the intervention halted. As reported above, at 4-year follow-up there were no significant effects of the Project Northland intervention over the control group.

Intention-to-treat re-analysis

The partially effective longer-term studies included in this intention-to-treat re-analysis are Botvin *et al.* (1995a), Schinke *et al.* (2000) and Spoth *et al.* (2001a, 2001b). It would not be meaningful to re-analyse the others in the same way; however, this does run the danger of presentation bias.

The design of all three studies (Botvin *et al.* 1995a; Schinke *et al.* 2000; Spoth *et al.* 2001a, 2001b) is hierar-

chical. Intervention was at the school level (schools were randomized to receive one of two interventions or no intervention, sometimes with blocking). The response variable summarized in this systematic review is dichotomous (yes/no to a relevant event). Botvin *et al.* (1995a) used school as the unit of measurement. If interest focuses on the effect of intervention on the individual student then school level analysis is inappropriate, the well-known 'ecological fallacy'. However, it could be argued that the social/community effect is the focus of interest and that it is appropriate to estimate the average effect of intervention by school. Schinke *et al.* (2000) used individual student as the unit of analysis. This is erroneous and will result in inflated type I errors (or equivalently too narrow confidence intervals) with even quite small intraclass correlation (ICC), especially with such large within-school samples. Spoth *et al.* (2001a, 2001b) used multi-level modelling for analysis of frequency of past month/year use of alcohol, but used student as the unit of analysis for the dichotomous responses referred to in this review, rather than a multi-level binomial model. This is erroneous, as described above.

As mentioned earlier, it was beyond the scope of the systematic review paper to allow for ICC and generally not possible with the information available. Moreover, the re-analysis presented in this section relies on the results presented in the papers and will therefore carry forward the erroneous original analysis. The impact of this threat to internal validity is not known for Schinke *et al.* (2000) and Spoth *et al.* (2001a, 2001b), although it can be estimated for Botvin *et al.* (1995a) by duplicating the ITT analysis at the level of the individual and the level of the school. When this was employed (not presented, but details available from the author) there was little practical difference between the results.

In Table 3 and Table 4 the proportions (proportion Intervention: p_I ; and proportion Control: p_C) of individuals with events (including the estimated number of events for dropouts) in each group were calculated. The absolute risk reduction (ARR) was the difference between the proportions ($p_C - p_I$). A 95% confidence interval was found for the ARR using the normal approximation for the difference between proportions. The standard error of each proportion was estimated using the number assessed (not the number analysed/estimated; it should be noted that this explains why re-analysis of ineffective or negative studies would give the same result for the confidence intervals as well as for the estimate) as estimated data do not provide extra information. The ARR and its confidence limits were inverted to give the number needed to treat (NNT) (Cook & Sackett 1995) with 95% confidence interval.

In Table 3 (Spoth *et al.* 2001a, 2001b) only new-users were counted as events and the total assessed were the

Table 3 Intention-to-treat analysis for selected studies (student as unit of analysis).

Programme (follow-up)	Outcome	Baseline n	Follow-up n	Outcome event rate (follow-up only)	Outcome event n (follow-up only)	Estimated outcome event N (attrition only: control event rate used as basis for estimation in all groups)	Total outcome event N (actual + estimated)	ARR (95% CI)	NNT (95% CI) (rounded up)
Skills training (culturally focused) (Schinke et al. 2000) (3.5 years)	4+ drinks in last week	A: 455	A: 388	A: 0.23	A: 89	A: 20	A: 109	A versus C: 6.23% (0.09–12.36%)	A versus C: 17 (9–1149)
		B: 462	B: 399	B: 0.25	B: 102	B: 19	B: 121	B versus C: 4.09 (–2.17–10.27%)	B versus C: 25 (10–∞)
		C: 479	C: 412	C: 0.30	C: 124	C: 20	C: 145		
Strengthening Families Program (SFP) (Spoth et al. 2001b) (4 years)	Ever used alcohol	ISFP: 205	ISFP: 131	ISFP: 0.50	ISFP: 65	ISFP: 50	ISFP: 115	ISFP versus Ctrl: 11.39% (–0.40–23.19%)	ISFP versus Ctrl: 9 (5–∞)
		PDFY: 187	PDFY: 122	PDFY: 0.60	PDFY: 73	PDFY: 44	PDFY: 117	PDFY versus Ctrl: 4.97% (–6.90–16.83%)	PDFY versus Ctrl: 21 (6–∞)
		Ctrl: 174	Ctrl: 126	Ctrl: 0.67	Ctrl: 85	Ctrl: 32	Ctrl: 117		
Strengthening Families Program (SFP) (Spoth et al. 2001b) (4 years)	Ever used alcohol without permission	ISFP: 232	ISFP: 148	ISFP: 0.40	ISFP: 59	ISFP: 49	ISFP: 108	ISFP versus Ctrl: 11.98% (0.63–23.33%)	ISFP versus Ctrl: 9 (5–160)
		PDFY: 215	PDFY: 140	PDFY: 0.51	PDFY: 72	PDFY: 44	PDFY: 116	PDFY versus Ctrl: 4.69% (–6.82–16.19%)	PDFY versus Ctrl: 22 (7–∞)
		Ctrl: 200	Ctrl: 145	Ctrl: 0.59	Ctrl: 85	Ctrl: 32	Ctrl: 117		
Strengthening Families Program (SFP) (Spoth et al. 2001b) (4 years)	Ever been drunk	ISFP: 232	ISFP: 148	ISFP: 0.26	ISFP: 39	ISFP: 37	ISFP: 76	ISFP versus Ctrl: 11.27% (0.31–22.24%)	ISFP versus Ctrl: 9 (5–327)
		PDFY: 216	PDFY: 141	PDFY: 0.35	PDFY: 50	PDFY: 33	PDFY: 83	PDFY versus Ctrl: 5.56% (–5.73–16.86%)	PDFY versus Ctrl: 18 (6–∞)
		Ctrl: 207	Ctrl: 150	Ctrl: 0.44	Ctrl: 66	Ctrl: 25	Ctrl: 91		

Schinke: A, problem-solving, personal coping, interpersonal communication—all incorporating native American myths, legends and stories; B, as A but also involving local community residents; C, Control. Spoth: ISFP, Iowa Strengthening Families Programme; PDFY, Preparing for the Drug Free Years programme; Ctrl, control group.

Table 4 Intention-to-treat analysis for selected studies (school as unit of analysis).

Programme (follow-up)	Outcome	No. of schools	Outcome event rate (follow-up only)	Estimated event (ITT) rate	SE of event rate	ARR (95% CI)	NNT (95% CI) (rounded up)
Life Skills Training (LST) (Botvin et al. 1995) (6 years)	Monthly alcohol use	A: 18	A: 0.61	A: 0.61	A: 0.03	A versus C: N/A	A versus C: N/A
		B: 16	B: 0.57	B: 0.58	B: 0.03	B versus C: 1.81% (-5.25% to 8.88%)	B versus C: 56 (12-∞)
		C: 22	C: 0.60	C: 0.60	C: 0.02		
Life Skills Training (LST) (Botvin et al. 1995) (6 years)	Weekly alcohol use	A: 18	A: 0.29	A: 0.29	A: 0.02	A versus C: N/A	A versus C: N/A
		B: 16	B: 0.24	B: 0.26	B: 0.02	B versus C: 3.02% (-2.52% to 8.56%)	B versus C: 34 (12-∞)
		C: 22	C: 0.29	C: 0.29	C: 0.02		
Life Skills Training (LST) (Botvin et al. 1995) (6 years)	3+ drinks per occasion	A: 18	A: 0.57	A: 0.58	A: 0.02	A versus C: 1.21% (-4.34% to 6.75%)	A versus C: 83 (15-∞)
		B: 16	B: 0.55	B: 0.57	B: 0.05	B versus C: 2.42% (-105.46% to 110.29%)	B versus C: 42 (1-∞)
		C: 22	C: 0.59	C: 0.59	C: 0.02		
Life Skills Training (LST) (Botvin et al. 1995) (6 years)	Drunkness in last month	A: 18	A: 0.34	A: 0.36	A: 0.02	A versus C: 3.62% (-1.92% to 9.17%)	A versus C: 28 (11-∞)
		B: 16	B: 0.33	B: 0.36	B: 0.03	B versus C: 4.23% (-2.84% to 11.30%)	B versus C: 24 (9-∞)
		C: 22	C: 0.40	C: 0.40	C: 0.02		

A, teacher-delivered (formal training + feedback); B, teacher-delivered (video training); C, control.

non-users at baseline who were assessed subsequently (estimate of non-users at baseline = (total at baseline) × (10th-grade follow-up assessed)/(10th-grade follow-up completed). For the ITT analysis it is therefore more appropriate to adjust the numbers at baseline to reflect only non-users. This has been done in Table 3. In Table 4 the ITT results are given for Botvin based on school as the unit of analysis. The standard error for ARR was estimated using the standard errors given in the paper for the proportions of students in each group with events. [NB: one of these, 0.55, is unusually large and may be an error in the paper.] When the proportion of events in the intervention group is the same or higher than the control group the re-analysis is not appropriate and so has been omitted from Table 3 and Table 4.

The most interesting result from the intention-to-treat re-analysis is the NNT of 9 (Spath *et al.* 2001a, 2001b; see Table 3). This indicates that for every nine individuals who receive the intervention, there will be one fewer person reporting that they have ever used alcohol, used alcohol without permission, or ever been drunk, 4 years later. The 95% confidence interval indicates that the true (population) value will be in this range 95% of the time. When the 95% confidence interval for the NNT includes infinity, then this indicates simply that the statistic is not significant. However, the sample size needs to be considered, as the width of the confidence interval is directly proportional to the size of the sample. It may be that a more promising intervention has a wider confidence interval simply because of a smaller sample size in the evaluation, but this does not mean that the intervention has less potential (Sterne & Davey Smith 2001).

DISCUSSION

The 56 studies included in the systematic review reported a range of different prevention interventions over the short, medium and longer term. These different prevention interventions represented a number of different theoretical perspectives, from knowledge-only programmes through to normative, social learning and multi-component community-based interventions. Different settings for prevention programmes and a range of different outcome measures added to the diversity of studies included in this systematic review. Although 56 studies is a large number of studies to include in a Cochrane systematic review the diversity of approaches to prevention, of settings, and of outcome measurement precluded a formal meta-analytical synthesis of results: no two studies were sufficiently similar. Moreover, there are strengths in narrative synthesis that are not found in meta-analysis, including more detailed consideration of the component and methodological shortcomings of

individual studies. Such detail is lost in meta-analysis, leading potentially to an ecological fallacy. Although there are ways of dealing with diversity in meta-analysis (Lipsey & Wilson 1993), this is only appropriate if the results of a meta-analysis can be interpreted usefully by policy makers and practitioners. In many instances where diverse interventions are analysed together in a single meta-analysis this is not the case. Therefore the main results of this systematic review have been presented in the form of a narrative synthesis, structured by follow-up period.

In the previous systematic review (Foxcroft *et al.* 1997) we reported that no firm conclusions could be drawn about the effectiveness of prevention interventions. In the few years that have passed since that first report 23 more evaluations have been identified and included in this updated systematic review. Overall the methodological quality of the more recent evaluations is better than those reported in the previous systematic review report. More importantly, the updated systematic review is able to make some stronger recommendations in terms of policy and practice, both in terms of ineffective interventions and potentially effective interventions.

Evidence of ineffectiveness is also an important consideration for policy makers and prevention workers, and in this regard we have identified a number of prevention interventions (see Results section) where the evaluation evidence shows evidence of ineffectiveness, despite limitations of the evaluations. It was more difficult to draw conclusions about evidence of effectiveness in the short and medium term. Studies with a short-term follow-up provided no clear evidence of effectiveness that would be useful to policy makers and prevention workers. Over the medium term three interventions were highlighted but two of these had limiting methodological shortcomings (Scaggs 1985; Botvin *et al.* 1995b) and in the third (Werch *et al.* 2000b) the effect sizes were small and of questionable public health, and therefore policy, relevance.

Over the longer term, the results of this systematic review point to the potential value of the Strengthening Families Program (SFP; Spoth *et al.* 2001a, 2001b) as an effective intervention for the primary prevention of alcohol misuse. The Number Needed to Treat (NNT) for the SFP over 4 years for three alcohol initiation behaviours (alcohol use, alcohol use without permission and first drunkenness) was 9 (for all three outcomes). Importantly, the SFP reported age of first drinking experiences as the comparative outcome measure and, as mentioned earlier, this may be a more useful indicator of future drinking problems than the outcome measures reported in other studies. Other interventions worth considering are culturally focused interventions. One study highlighted the potential value of culturally focused skills training over

the longer term (NNT = 17 over 3.5 years for 4+ drinks in the last week). The Life Skills Training (LST) approach showed less promise, and this finding should be noted by policy makers and practitioners given the widespread application of LST. It should, however, also be noted that direct comparison of NNTs across these different studies is problematic because of different study designs, methods, populations, outcome measures and follow-up periods.

Community interventions also need to be considered by policy makers as the potential benefit goes beyond youth. If community interventions can have a significant impact on important youth alcohol misuse outcomes at the same time as impacting on other groups within a community (e.g. Holder 1997) then there may ultimately be an economy of scale. Instead of different interventions for different groups, a single community intervention that covers all these groups may be more cost-effective.

Whether interventions focused on alcohol alone, or alcohol as one of a number of drugs, appeared to have no effect on outcome in the studies reviewed. However, the majority of these mainly school-based studies were conducted in the United States, where the goal of misuse prevention programmes for adolescents tends to be abstinence from any substance use (including alcohol). This may not be the target outcome for drinking behaviour in some other countries, where the emphasis tends to be sensible drinking rather than abstinence. Different philosophies underlie the two approaches so caution must be taken if the adoption of intervention programmes from the United States is contemplated. For example, in Britain different messages are given for alcohol compared with tobacco or illegal drugs—sensible age-related use for the former, abstinence for the latter—whereas in certain Nordic countries the approach is similar to that in the United States.

Furthermore, it is difficult to judge the relative merits of different interventions if evaluations report different outcomes and the public health relevance of these different outcomes is unknown. What is needed is a thorough systematic review of the evidence for subsequent alcohol misuse and alcohol-related problems provided by such indicators, using the International Guide for Monitoring Alcohol Consumption and Relation Harm (World Health Organization 2000) as a framework for systematically reviewing these indicators. This would draw together the evidence on age of initiation (e.g. Grant & Dawson 1997), age of first drunkenness, regular consumption and mean drinking frequency index scores as predictor variables. This should lead to greater clarity over the sort of measures to be included in future intervention evaluations.

This review of the methodological quality of evaluations and effectiveness of interventions highlights a num-

ber of important issues. The poor quality of much research into the effectiveness of interventions must be stressed. If we want to know if an intervention is working then a proper assessment of its effectiveness should be made, preferably over the longer term. In this systematic review only a small number of the many intervention studies found in the literature search have been well designed and evaluated adequately, even those that were judged to be of sufficient quality to be listed in this systematic review. Problems with poor quality studies included lack of suitable control groups (non-random allocation or nonequivalent groups), lack of pretest information, high levels of attrition, inappropriate analysis for the unit of allocation, and more generally a poor quality presentation of results, often in well-respected peer-reviewed journals.

It might be helpful, in terms of promoting better quality evaluations, if public health policy makers were to develop and use a register for the adoption and use of prevention interventions based on what is known about the effectiveness of the intervention. A suggested framework for classifying prevention interventions for adoption by policy makers and prevention workers is:

- 1 Safety, efficacy and effectiveness established: prevention intervention may be used.
- 2 Efficacy established. Further evaluation required to confirm effectiveness and safety: prevention intervention can be used as part of a primary research programme or a surveillance programme.
- 3 Safety and efficacy not proven: prevention intervention should be used only as part of a primary research programme, using appropriate methodology.
- 4 Safety and/or efficacy shown to be unsatisfactory: prevention intervention should not be used.

The World Health Organization is probably best placed to take the lead on the development and promotion of a prevention intervention register. Only those interventions where there is good evidence for efficacy, effectiveness and safety would be given the highest rating and approved for use, whereas interventions where less is known would not be approved for widespread use until further evidence is provided.

CONCLUSIONS

Implications for practice

- 1 The results of this systematic review point to the potential value of the Strengthening Families Program (Spath *et al.* 2001a, 2001b) as an effective intervention over the longer term for the primary prevention of alcohol misuse. The Strengthening Families Program needs to be evaluated on a larger scale and in different settings to confirm the current results.

- 2 Other interventions worth considering are culturally focused interventions. Schinke *et al.* (2000) highlighted the potential value of culturally focused interventions over the longer term, but as with the Strengthening Families Program, further study is required.
- 3 Evidence for the Life Skills Training (LST) programme is less convincing and policy makers and practitioners should be informed of this finding, given the widespread acceptance and use of LST.
- 4 Community interventions also need to be considered by policy makers as the potential benefit goes beyond youth. If community interventions can have a significant impact on important youth alcohol misuse outcomes at the same time as impacting on other groups within a community (e.g. Holder 1997), then there may ultimately be an economy of scale. Instead of different interventions for different groups, a single community intervention that covers all these groups may be more cost-effective.
- 5 An international register of alcohol and drug misuse prevention interventions should be established and criteria agreed for rating prevention intervention in terms of safety, efficacy and effectiveness. This register should be aimed at public health policy makers and prevention workers.

Implications for research

- 1 Research into the important outcome variables needs to be undertaken. There is no single outcome measure of youth drinking behaviour that is used in evaluation studies, and no clear understanding of which outcome measures are important predictors of alcohol misuse, morbidity and mortality in later life.
- 2 The methodology of evaluations needs to be improved. Large-scale RCTs are possible and preferable for rigorous scientific evaluation of discrete interventions, but appropriate statistical analysis needs to be undertaken to take account of the intraclass correlation coefficient (ICC). For large community interventions where RCTs are not practical then a Comparative Interrupted Time Series (CITS) design with sufficient pre- and postintervention measurement time points should be considered. In addition, all researchers should clearly describe attrition rates, how they varied between different treatment and control groups, and how attrition was dealt with in any statistical analysis, for example through an intention-to-treat analysis.
- 3 The Strengthening Families Programme (SFP) needs to be evaluated on a larger scale and in different settings to confirm the current results. Although earlier evaluations of the SFP have been reported (Kumpfer *et al.* 1996) these were not included in this systematic

review because of design limitations. Future evaluations should be of high scientific quality. In addition, cost-effectiveness analyses would be useful.

- 4 Culturally focused interventions require further development and rigorous evaluation, including cost-effectiveness assessment.

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DECLARATION OF INTEREST

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