Effectiveness of brief alcohol interventions in primary care populations (Review)

Kaner EF, Dickinson HO, Beyer FR, Campbell F, Schlesinger C, Heather N, Saunders JB, Burnand B, Pienaar ED

THE COCHRANE COLLABORATION®

This is a reprint of a Cochrane review, prepared and maintained by The Cochrane Collaboration and published in *The Cochrane Library* 2009, Issue 4

http://www.thecochranelibrary.com

TABLE OF CONTENTS

[Intervention Review]

Effectiveness of brief alcohol interventions in primary care populations

Eileen F.S. Kaner[1], Heather O Dickinson[2], Fiona R Beyer[3], Fiona Campbell[4], Carla Schlesinger[5], Nick Heather[6], John B Saunders[7], Bernard Burnand[8], Elizabeth D Pienaar[9]

[1]Institute of Health and Society, The Medical School, Newcastle University, Newcastle upon Tyne, UK. [2]Institute of Health and Society, Newcastle University, Newcastle upon Tyne, UK. [3]Institute of Health & Society, Newcastle University, Newcastle upon Tyne, UK. [4]School of Health and Related Research, University of Sheffield, Sheffield, UK. [5]Centre for Drug & Alcohol Studies, Prince Charles Hospital, Brisbane, Australia. [6]Division of Psychology, University of Northumbria, NEWCASTLE UPON TYNE, UK. [7]Department of Psychiatry, Royal Brisbane and Women's Hospital, University of Queensland/Royal Brisbane Hospital, Australia. [8]Health Care Evaluation Unit & Clinical Epidemiology Centre, Institute of Social and Preventive Medicine, Centre Hospitalier Vaudois and University of Lausanne, Lausanne, Switzerland. [9]South African Cochrane Centre, Medical Research Council, Tygerberg, South Africa

Contact address: Eileen F.S. Kaner, Institute of Health and Society, The Medical School, Newcastle University, Newcastle upon Tyne, NE2 4HH, UK. e.f.s.kaner@newcastle.ac.uk.

Editorial group: Cochrane Drugs and Alcohol Group.
Publication status and date: Edited (no change to conclusions), published in Issue 4, 2009.
Review content assessed as up-to-date: 14 February 2007.

Citation: Kaner EF, Dickinson HO, Beyer FR, Campbell F, Schlesinger C, Heather N, Saunders JB, Burnand B, Pienaar ED. Effectiveness of brief alcohol interventions in primary care populations. *Cochrane Database of Systematic Reviews* 2007, Issue 2. Art. No.: CD004148. DOI: 10.1002/14651858.CD004148.pub3.

ABSTRACT

Background

Many trials reported that brief interventions are effective in reducing excessive drinking. However, some trials have been criticised for being clinically unrepresentative and unable to inform clinical practice.

Objectives

To assess the effectiveness of brief intervention, delivered in general practice or based primary care, to reduce alcohol consumption. To assess whether outcomes differ between trials in research settings and those in routine clinical settings.

Search strategy

We searched the Cochrane Drug and Alcohol Group specialised register (February 2006), MEDLINE (1966 to February 2006), EMBASE (1980 to February 2006), CINAHL (1982 to February 2006), PsycINFO (1840 to February 2006), Science Citation Index (1970 to February 2006), Social Science Citation Index (1970 to February 2006), Alcohol and Alcohol Problems Science Database (1972 to 2003), reference lists of articles.

Selection criteria

Randomised controlled trials, patients presenting to primary care not specifically for alcohol treatment; brief intervention of up to four sessions.

Data collection and analysis

Two authors independently abstracted data and assessed trial quality. Random effects meta-analyses, sub-group, sensitivity analyses, and meta-regression were conducted.

Main results

Meta-analysis of 22 RCTs (enrolling 7,619 participants) showed that participants receiving brief intervention had lower alcohol consumption than the control group after follow-up of one year or longer (mean difference: -38 grams/week, 95% CI: -54 to -23), although there was substantial heterogeneity between trials (I2 = 57%). Sub-group analysis (8 studies, 2,307 participants) confirmed the benefit of brief intervention in men (mean difference: -57 grams/week, 95% CI: -89 to -25, I2 = 56%), but not in women (mean difference: -10 grams/week, 95% CI: -48 to 29, I2 = 45%). Meta-regression showed little evidence of a greater reduction in alcohol consumption with longer treatment exposure or among trials which were less clinically representative. Extended intervention was associated with a non-significantly greater reduction in alcohol consumption than brief intervention (mean difference = -28, 95%CI: -62 to 6 grams/week, I2 = 0%)

Authors' conclusions

Overall, brief interventions lowered alcohol consumption. When data were available by gender, the effect was clear in men at one year of follow up, but not in women. Longer duration of counselling probably has little additional effect. The lack of evidence of any difference in outcomes between efficacy and effectiveness trials suggests that the current literature is relevant to routine primary care. Future trials should focus on women and on delineating the most effective components of interventions.

PLAIN LANGUAGE SUMMARY

Effectiveness of brief interventions in primary care populations

Excessive drinking contributes significantly to social problems, physical and psychological illness, injury and death. Hidden effects include increased levels of violence, accidents and suicide. Most alcohol-related harm is caused by excessive drinkers whose consumption exceeds recommended drinking levels, not the drinkers with severe alcohol dependency problems. One way to reduce consumption levels in a community may be to provide a brief intervention in primary care over one to four sessions. This is provided by healthcare workers such as general physicians, nurses or psychologists. In general practice, patients are routinely asked about alcohol consumption during registration, general health checks and as part of health screening (using a questionnaire). They tend not to be seeking help for alcohol problems when presenting. The intervention they are offered includes feedback on alcohol use and harms, identification of high risk situations for drinking and coping strategies, increased motivation and the development of a personal plan to reduce drinking. It takes place within the time-frame of a standard consultation, 5 to 15 minutes for a general physician, longer for a nurse.

A total of 29 controlled trials from various countries were identified, in general practice (24 trials) or an emergency setting (five trials). Participants drank an average of 306 grams of alcohol (over 30 standard drinks) per week on entry to the trial. Over 7000 participants with a mean age of 43 years were randomised to receive a brief intervention or a control intervention, including assessment only. After one year or more, people who received the brief intervention drank less alcohol than people in the control group (average difference 38 grams/week, range 23 to 54 grams). For men (some 70% of participants), the benefit of brief intervention was a difference of 57 grams/week, range 25 to 89 grams (six trials). The benefit was not clear for women. The benefits of brief intervention were similar in the normal clinical setting and in research settings with greater resources. Longer counselling had little additional benefit.

BACKGROUND

Excessive drinking is a significant cause of mortality, morbidity and social problems, both in developed and in developing countries, with a global cost to health above that of tobacco (WHO 1999). The true impact of alcohol upon the health of individuals and the wider community is however difficult to estimate because of the many hidden effects resulting from its use, including increased levels of violence, accidents and suicide (Anderson 1991). The heavy burden that alcohol use places upon the health of populations, and its significant economic consequences, has led

to national and international programmes and policies that seek to reduce consumption levels and thus reduce a primary cause of avoidable ill health (Alcohol Concern 2000; DoH 1992; WHO 1993). The impetus for a preventive approach to alcohol problems has been reinforced by epidemiological research which shows that, on a population level, the majority of alcohol-related harm is not due to drinkers with severe alcohol dependence but attributable to a much larger group of excessive or hazardous drinkers whose consumption exceeds recommended drinking levels (Anderson 1991) and who experience an increased risk of physical, psychological or social harm.

Early identification and secondary prevention of alcohol problems, using screening and brief interventions in primary care, has increasingly been advocated as the way forward and thus has been the focus of a great deal of research (Anderson 1996; Wutzke 2002). Brief intervention is grounded in social-cognitive theory and typically incorporates some or all of the following elements: feedback on the person's alcohol use and any alcohol-related harm; clarification as to what constitutes low risk alcohol consumption; information on the harms associated with risky alcohol use; benefits of reducing intake; motivational enhancement; analysis of high risk situations for drinking and coping strategies; and the development of a personal plan to reduce consumption. Although the form that brief intervention takes may vary between studies (Heather 1995), core features of these brief interventions in primary care are that they are delivered by generalist health care workers, they target a population of excessive (or hazardous) drinkers that tends not to be seeking help for alcohol problems and they aim for reductions in consumption behaviour and related harm. Brief interventions in primary care have been evaluated less frequently with dependent drinkers as the target group since such individuals often need more intensive treatment than is available in routine primary care and are likely to require a goal of total abstinence.

There are many opportunities for identifying excessive drinking in primary care since patients are routinely asked about alcohol consumption during new patient registrations, general health checks, specific disease clinics (e.g. hypertension, diabetes) and other health screening procedures. Thus brief intervention in routine primary care would typically occur opportunistically, in the sense that drinking problems would often not be the primary reason for the presentation and patients would not actively be seeking treatment. In addition, the intervention would need to be delivered within the limited time-frame of a standard consultation (typically 5-15 minutes for a GP, up to 30 minutes for a nurse) or within the parameters of routine practice (e.g. initial screening plus either referral to a practice colleague or later return for intervention). However, brief intervention trials have evaluated a wide range of activity from a single 5-10 minute session of structured advice delivered by a general medical practitioner or nurse, through to multiple sessions of motivational interviewing or some other form of counselling accompanied by repeated follow-up and delivered by various personnel in primary care. There has also been variability in other features of the intervention, such as the population of patients treated, the training and support of therapists, the theoretical basis underlying the intervention and the use of accompanying written material.

Consequently, although numerous reviews have indicated beneficial outcomes of brief intervention for excessive drinkers (Agosti 1995; Bien 1993; DoH 1992; Moyer 2002; NHS CRD 1993; Poikolainen 1999; Wilk 1997), crucial questions remain concerning its impact in routine practice and its applicability to the broader patient population. Whilst there appears to be little doubt that brief intervention with excessive drinkers can be efficacious in research settings, there is uncertainty about its extrapolation into the real world of primary care (Holder 1999; Kaner 2001). Much of the literature consists of efficacy trials i.e. studies carried out in tightly controlled conditions designed to optimise internal validity (Flay 1986). However, if health professionals are to be encouraged to adopt and administer brief interventions in routine practice, it is necessary to establish a realistic effect size for brief intervention delivered in clinically relevant contexts. In addition, a number of subgroups (e.g. young people, the elderly and ethnic minorities) exist within broad categorisations such as excessive, problematic or hazardous drinkers and little is known about how these subgroups respond to brief intervention in primary care. Differential loss of subjects from brief intervention trials has led to a call for caution in generalizing these results to routine practice (Edwards 1997) but little emphasis has been placed on identifying which patients were not included in the studies at the outset. Thus there is a clear need to characterise the types of drinkers for whom brief interventions have a positive impact and any subgroups that have not been represented in the trials to date.

OBJECTIVES

To determine the effects of brief interventions in reducing excessive alcohol consumption in routine primary health care and characterise the types of drinkers to whom these effects relate. The following comparisons will be made:

(1) Brief intervention versus a control condition (assessment only, standard treatment or non-intervention)

(2) Brief intervention versus a extended psychological intervention.

Subgroup analysis will be undertaken to assess the impact of brief interventions in efficacy (ideal world) and effectiveness (real world) trials using a coding scale developed from the work of Shadish and colleagues (Shadish 2000) and to account for variability in treatment exposure relating to the frequency, duration and theoretical basis of the brief intervention. Intervention outcomes will also be classified according to the time at which they were followed-up to

ascertain the short, medium and long-term effects. Finally, the applicability of brief intervention to different sub-groups of drinkers will be described in narrative form.

METHODS

Criteria for considering studies for this review

Types of studies

Randomised controlled trials, including cluster randomised controlled trials.

Types of participants

Patients who are routinely presenting to primary care for a range of health problems and whose alcohol consumption is identified as being excessive or who have experienced harm as a result of their drinking behaviour. Dependent users of alcohol will not be the main focus of this review.

Primary care will be operationalised to include all immediately accessible, general health care facilities which cover (1) a broad range of possible presenting problems, and (2) which can be accessed by wide range of patients on demand, and not as the result of a referral for specialist care.

Types of interventions

Brief intervention comprises a single session, and up to a maximum of four sessions (Babor 1994), of engagement with a patient and the provision of information and advice that is designed to achieve a reduction in risky alcohol consumption or alcohol-related problems.

Brief interventions are typically compared to control conditions of assessment only or treatment as usual.

Psychologically-based intervention aimed at reducing consumption behaviour or alcohol-related problems that is unlikely to occur in routine practice, for reasons of length or intensity, will be referred to as extended intervention.

Types of outcome measures

Primary outcomes

(1) Self- or other-reports of drinking quantity (e.g. average consumption of alcohol per specified time period)
(2) Self- or other-reports of drinking frequency (e.g. number of drinking occasions per specified time period)
(3) Self- or other-reports of drinking intensity (e.g. number of drinks per drinking day)
(4) Self- or other-reports of drinking within recommended limits (e.g. official recommendations per specified time period).
(5) Levels of laboratory markers of reduced alcohol consumption (e.g. serum GGT (gamma-glutamyltransferase), MCV (mean corpuscular volume)).
(6) Alcohol-related harm to the drinkers or to affected others (e.g. via questionnaires such as the drinking problems index)

Secondary outcomes

(7) Patient satisfaction measures
(8) Health-related quality of life
(9) Economic measures including use of health services
Although not specified in the protocol, we also noted the following outcomes if these were reported:

- participant recorded as a heavy drinker
- participant recorded as a binge drinker
- participant recorded as drinking over 35 units/week

Search methods for identification of studies

Electronic searches

Relevant studies that meet the predefined inclusion criteria were identified by searching the following sources from the earliest available date to 2006:

- MEDLINE (1966 to 2005)
- EMBASE (1980 to 2005)
- PsycINFO (1840 to 2005)
- CINAHL (1982 to 2005)
- Social Sciences Citation Index (SSCI) (1970 to 2005)
- Science Citation Index (SCI) (1970 to 2005)
- Cochrane Drug and Alcohol Group specialised register (February 2006)
- Cochrane Effective Practice and Organisation of Care Group specialised register (2005)
- Alcohol and Alcohol Problems Science Database, ETOH (http://etoh.niaaa.nih.gov/) (1972 to 2003)

The Medline search strategy is reported in detail in Appendix 1. It was adapted as appropriate for other databases.
We did not apply any language restrictions.

Searching other resources

Hand searching and archive searches of relevant journals not covered by the Cochrane library including:

- Journal of Studies on Alcohol

- Alcoholism: Clinical and Experimental Research
- British Journal of General Practice

We hand searched the reference lists of included papers, and relevant systematic reviews.

Key informants and experts were contacted to enquire about unpublished work and ongoing research, particularly through links with the World Health Organisation Collaborative Study group on Brief Interventions in Primary Care.

Data collection and analysis

Selection of studies

Following a search using the strategies and the sources described above, papers were assessed for relevance by Kaner and Schlesinger and the full text retrieved. Selection from this initial search was based on information derived from the title, abstract and keywords. These included the following:

Randomised controlled trial or clinical trialExcessive drinkers (and related terms)Alcohol reduction strategies (and related terms)Primary health care staff and/or settings (and related terms)

If the title, abstract and keywords did not yield enough information to ascertain potential for inclusion then the full paper was retrieved. One author (Schlesinger) rechecked all those papers excluded at this stage to ensure all potentially relevant papers were retrieved.

All retrieved papers were assessed for inclusion in the review using predefined criteria based on those described above (*see* criteria for considering studies for this review). The inclusion criteria were initially piloted on six retrieved papers. Assessment of studies for inclusion in the review was undertaken by two independent authors. Any disagreement was resolved by discussion and consensus or adjudication by a third reviewer.

Data extraction and management

Data were independently extracted by two independent authors using a piloted data extraction form and subsequently entered in the Cochrane Collaboration software (Review Manager Version 4.1).

In the protocol, we specified that we would use performance of an intention-to-treat analysis as a criterion of quality. However, this was revised in the light of guidance in the latest version of the Cochrane Handbook (Deeks 2006b). Intention-to-treat analysis is usually understood to mean that participants were analysed in the groups to which they were randomised regardless of the treatment they actually received. However, it is sometimes understood to imply that, additionally, all participants were included regardless of whether their outcomes were actually collected, which requires imputation of missing outcomes. Rather than using intention-to-treat analysis as a quality criterion, we attempted to abstract data for participants in the groups to which they were randomised, regardless of the treatment they actually received, i.e. corresponding to the more widely agreed definition of intention-to-treat analysis. We performed a sensitivity analysis using imputed data for participants who were lost to follow-up in trials where this was reported, i.e. corresponding to the less widely agreed definition of intention-to-treat analysis.

Assessment of risk of bias in included studies

Assessment of Methodological Quality:

Two independent authors assessed potential biases resulting from the trial design. Any discrepancies between reviewers were resolved by discussion to achieve a consensus. The results of the quality assessment of included studies were tabulated and summarised. Quality assessment was based on the following aspects of methodology:

(1) Selection bias

Did randomisation occur in an unpredictable sequence so that every patient had a equal chance of experiencing control or intervention conditions?

A: adequate allocation concealment (e.g. allocation by a central office unaware of subject characteristics; serially numbered, opaque, sealed envelopes; on-site computer system combined with allocations kept in a locked unreadable computer file that can be accessed only after the characteristics of an enrolled participant have been entered; or other description that contained elements convincing of concealment)

B: unclear allocation concealment (authors either did not report an allocation concealment approach at all or report an approach that did not fall in the category A or C)

C: inadequate allocation concealment (alternation or reference to case numbers, dates of birth, day of the week; any procedure that is entirely transparent before allocation, such as an open list of random numbers or other description that contained elements convincing of not concealment)

(2) Performance bias

Blinding of patients or masking of clinicians regarding treatment condition is difficult to achieve in a trial evaluating a 'talking therapy' although it may be possible in cluster randomized trials and so we noted:

A: double blind

B: single blind (blinding of participants)

C: unclear

D: not applicable

(3) Attrition bias

Differential loss of subjects from comparison groups was explored by recording how many patients were lost to follow up in each, and on each outcome measured.

A: Loss to follow up completely recorded for each group

B: Loss to follow-up incompletely recorded (data reported only for one group or for the overall sample)
C. Unclear or not done
The risk difference (RD) for loss- to follow-up in each study was calculated and these RDs were pooled in a meta-analysis to assess whether there was any systematic difference between loss to follow-up in treatment and control groups.

(4) Detection bias

Efforts to blind the investigator assessing outcomes were appraised.
A: Blind to treatment allocation at outcome assessment
B: Not blind to treatment allocation at outcome assessment
C: Unclear
In recent years, it has become evident that in evaluations of 'talking therapies' there is a need for cluster randomisation (treatment allocation at the level of the practitioner or health setting) in order to prevent therapists from delivering both intervention and control conditions since their practice is likely to be affected by the former. We noted the level at which randomisation and subsequent analysis occurred although it is likely that older trials occurred before such understanding was in place.
Studies with adequate allocation concealment will be classified as A: low risk of bias, studies with unclear allocation concealment will be classified as B: moderate risk of bias and studies with inadequate allocation concealment will be classified as C: high risk of bias"

Measures of treatment effect

Statistical Methods:

For each trial, the outcome data on quantity of alcohol consumed in a specific time period were converted to grams per week if necessary. Drinks and units were converted to grams using either a conversion factor reported in the relevant paper or, if none was reported, using the conversion factor appropriate for the country where the study was conducted, as presented in Gual 1999, Heather 2006 or Miller 1991. Months were converted to weeks by multiplying by 52/12. For intensity of drinking, "drinking days", "drinking sessions" and "occasions" were all assumed to be equivalent to drinking days. For laboratory markers of GGT (gamma-glutamyltransferase) (Israel 1996; Tomson 1998; Wallace 1988) microkatals/litre were converted to International Units/litre by multiplying by 60.
For each of these outcomes, the mean difference (and standard deviation) between the final value of the outcome measure for treatment and control interventions was calculated. If standard deviations of final values were not available, change scores were used if their standard deviations were available. If no standard deviations were available, these trials were omitted from the primary analysis but included in a sensitivity analysis using imputed standard deviations. The weighted mean difference method was used to estimate pooled effect sizes and 95% confidence intervals (95% CI), if sufficient studies reporting the outcome were available. Most trials reported weekly or monthly alcohol consumption and few reported frequency or intensity of drinking. Hence meta-analysis of quantity of alcohol consumed per week provided most information.
For dichotomous outcomes (participant classified as a heavy drinker, a binge drinker or as drinking over 35 units/week, loss to follow-up), relative risks (RR) and 95% CI were calculated and pooled in a meta-analysis using Mantel-Haenzel weighting.
If trials had more than one control arm and the various control arms were very similar (e.g. Heather 1987), they were combined by calculating weighted means of continuous outcomes and summing dichotomous outcomes; likewise for very similar treatment arms (McIntosh 1997).
The magnitude of heterogeneity between trials was assessed using the I2 statistic (Higgins 2002; Higgins 2003); the statistical significance of heterogeneity was assessed using p-values derived from chi-squared tests (Deeks, 2001). In the absence of significant heterogeneity, a fixed effects model was used for the estimation of treatment effects. If there was evidence of significant heterogeneity, the possible reasons for this were investigated and reported and a random effects model was used (DerSimonian 1986).
Funnel plots (plots of the effect estimate from each study against the sample size or effect standard error) were used to assess the potential for bias related to the size of the trials, which could indicate possible publication bias.
For cluster randomised controlled trials, if the analysis accounted for the cluster design we extracted a direct estimate of the desired treatment effect and its standard error; we then assigned imputed standard deviations to the treatment and control groups such that the standard error of the treatment effect which was estimated by the weighted mean difference method in RevMan was the same as the standard error of the treatment effect as reported in analysis which allowed for clustering. If the analysis did not account for the cluster design, we extracted the number of clusters randomized to each intervention, the average cluster size in each intervention group and the outcome data (e.g. number or proportion of individuals with events, or means and standard deviations), ignoring the cluster design, for all individuals in each intervention group. Next using an external estimate of the intra-cluster coefficient (ICC) a design effect was estimated. Hence, we inflated the variance of the effect estimate. It was then possible to enter the data into RevMan and combine the cluster randomised trials with individually randomised trials in the same meta-analysis.

Subgroup analysis and investigation of heterogeneity

Subgroup Analyses:

The following subgroup analyses were planned:

(A) Effectiveness/Efficacy influence on effect-size

Clinical (effectiveness) versus research (efficacy) representativeness of trials was classified using the following criteria and scoring system:

Patients and problems

2=clinically representative subjects initially present with a typically wide range of problems via self-referral or invitation for a health check.

0= research representative subjects may be paid patients, researcher-solicited volunteers (e.g. via advertisement) or referrals from specialist services.

Practice context

2=clinically representative is a community-based setting in which a range of clinical services are usually provided to patients.

0= research representative is a setting in which the research function clearly dominates any clinical one (e.g. clinic at a university or hospital).

Practitioners and therapists

2= clinically representative practitioners are practising doctors, nurses and qualified therapists who earn their main living by providing health services in primary care.

0= research representative practitioners are nonclinicans, or clinicians in training, who are contracted to deliver interventions for the purposes of the study.

Intervention content

2=clinically representative intervention fits with current practice in terms of timing, content or style e.g. 5-15 minutes for a GP; 20-30 minutes for a nurse or initial screening accompanied by a return visit for brief intervention.

0= research representative treatment would not normally occur in routine practice e.g. unusually long consultations.

Therapeutic flexibility

1=clinically representative: allows professional judgement in how an intervention is delivered e.g. freedom to focus on particular issues according to patient need.

0= research representative: strict adherence to a prescribed protocol or script that does not allow for variability in practice.

Pre-therapy training

1=clinically representative training in intervention procedures occurs according to typical CPD/CME procedures, e.g. outreach visits, seminars, one-off training days.

0= research representative training is unusually intensive or requiring of atypical levels of interest or motivation, e.g. prolonged or intensive courses, formal qualification.

Intervention support

1= clinically representative support occurs within standard practice resources e.g. colleague assistance with screening, IT flagging.

0=research representative support would not typically be available e.g. researcher help to flag notes, extra staff for period of the trial.

Intervention monitoring

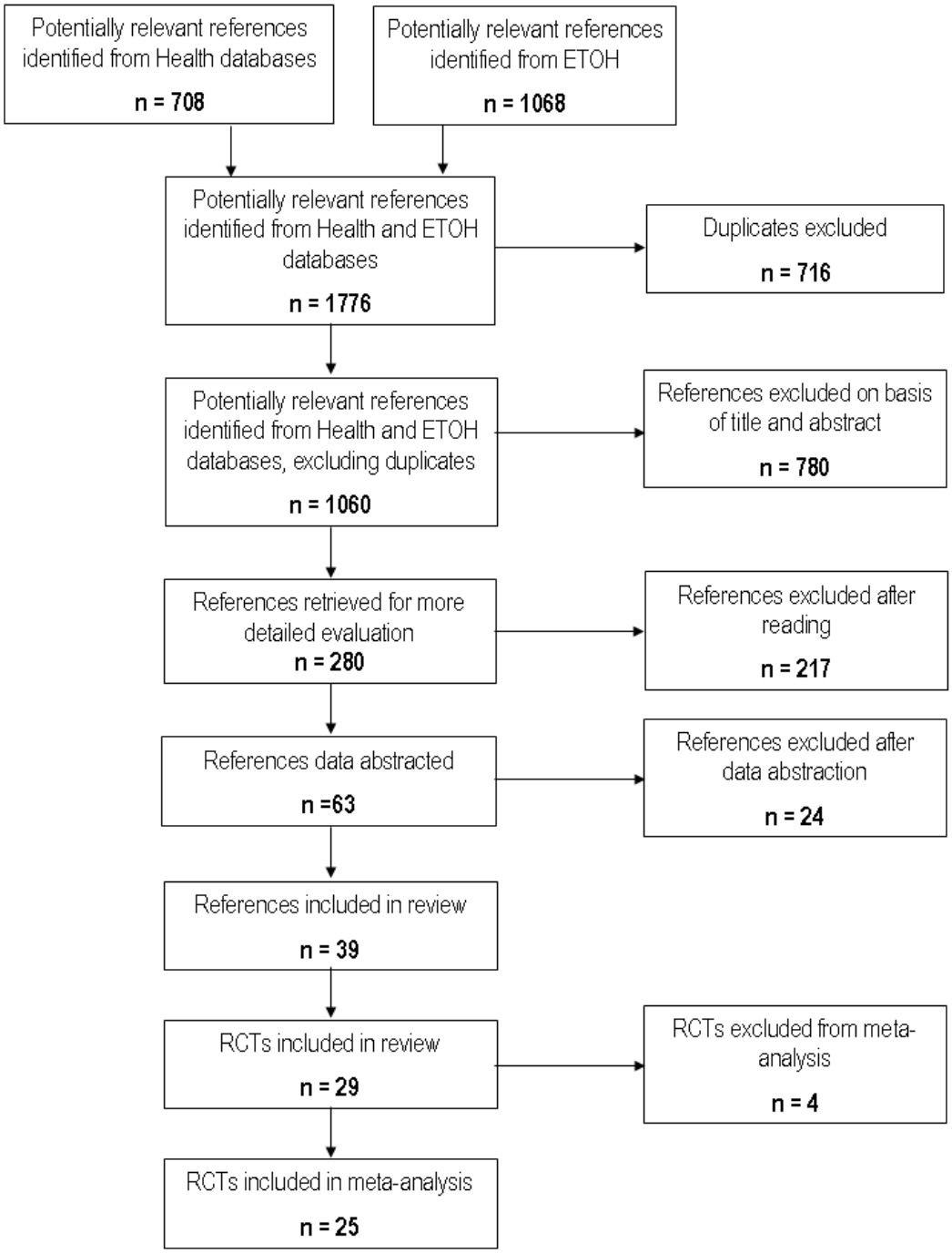
1=clinically representative monitoring of intervention delivery does not interfere with practitioners' behaviour or their relationship with patients.

0= research representative monitoring would be direct observation of therapist behaviour or ongoing/immediate feedback to practitioners after each session.

Each trial was independently classified by two authors. If an item appeared to be partially clinically representative on any item, then a midpoint score was given (either 1 or 0.5 as applicable). If the authors did not report data relating to a particular item, then an intermediate score was allocated so as not to bias the trial towards the effectiveness or efficacy domain. If there was disagreement concerning classification, this was resolved through discussion in order to gain consensus.

For each trial, scores on all items were summed to provide an efficacy/effectiveness score. If a paper scored 12 then it was likely to be highly clinically relevant and so considered to be an effectiveness trial with high external validity. Conversely, if a trial scored 0 then it was highly research relevant and so considered to be an efficacy trial with high internal validity. The effect of brief intervention compared to control, as estimated from random effects meta-analysis, was plotted against the efficacy/effectiveness score (*see* Additional Figure 1). Meta-regression was performed to assess whether this treatment effect was related to the efficacy/efficiency score. Additionally, trials were categorised as effectiveness or efficacy trials based on whether their efficacy/effectiveness score was above or below the median respectively and subgroup analyses were performed.

Figure 1. Flow chart showing identification of included trials.



(B) Variability in problem severity, treatment exposure and follow-up timescales

The severity of participants' drinking behaviour or alcohol-related problems in each study was described in order to facilitate comparison of treatment effects across different trials.

A measure of treatment exposure was calculated as the sum of the duration of the initial brief intervention plus the total duration of all booster sessions, in minutes. If a range of durations was given then the mean was used. If duration was not reported, it was assumed to be 5-10 minutes with a mean of 7.5 minutes. Trials were categorised as high or low treatment exposure trials based on whether the treatment exposure was above or below the median respectively and subgroup analyses were performed.

Different therapists involved at each stage were also noted. Moreover, the precise components of the brief intervention were described, including if an explicit theoretical basis underpinned the intervention (e.g. motivational interviewing, CBT, psychoeducation etc.). A graph of treatment effect sizes was plotted against treatment exposure in the form of clinical time. Finally, the time to follow-up was recorded and an assessment made of the duration of any treatment effects.

(C) Applicability issues

Details of the gender, age, socioeconomic status and ethnic group of participants enrolled in the included trials and followed up at outcome assessment were summarised in order to assess how applicable brief interventions are to the broader population of patients presenting to primary care.

Sensitivity analysis

Sensitivity analyses were performed based on the following characteristics:

(1) Methodological quality: analysis was repeated including only studies with adequate allocation concealment (i.e. class A)

(2) Comparison of outcomes from cluster and individually randomised trials: sensitivity analysis was performed to investigate the robustness of the conclusions, especially of the effect of varying assumptions about the magnitude of the ICC.

(3) Sensitivity analysis was performed including trials which did not report standard deviations by imputing to both their treatment and control groups the median SD of the relevant outcome for trials in which it was reported.

(4) We performed a sensitivity analysis using imputed data for participants who were lost to follow-up in trials where this was reported, i.e. corresponding to the less widely agreed definition of intention-to-treat analysis.

In the protocol, we planned a sensitivity analysis comparing random and fixed effects models if there was unexplained heterogeneity between studies. However, the populations and interventions evaluated by the trials were so heterogeneous that it was deemed more appropriate to use a random effects model for all analyses.

RESULTS

Description of studies

See: Characteristics of included studies; Characteristics of excluded studies.

Results of the search

The search strategy found 1060 potentially relevant references which we electronically screened (*see* Additional Figure 1). We excluded 780 references and retrieved 280 for detailed evaluation, of which we excluded 242; the reasons for exclusion of possibly relevant studies are summarised in *Characteristics of excluded studies table*. The 29 remaining RCTs (reported in 39 references) met our inclusion criteria and are described in detail in *Characteristics of included studies table*.

Three trials (Longabaugh 2001; McIntosh 1997; Rodriguez 2003) did not report the number of participants assessed by treatment arm so could not be included in any meta-analysis.

Twenty-eight trials compared a brief intervention with a control intervention: four of these also included an extended intervention arm (Aalto 2000; Longabaugh 2001; Maisto 2001; Richmond 1995). One of the trials included two control arms (Heather 1987) and one included two intervention arms identical in substance but delivered by different health professionals (McIntosh 1997). One further trial compared an extended intervention with a brief intervention (Israel 1996).

Included studies

Clinical heterogeneity

Participants were screened for inclusion if they were visiting a primary health care clinic or accident and emergency department for any reason, but not specifically for an alcohol-related issue. Potential participants were usually excluded from the trials if they were heavily alcohol dependent or already on an alcohol treatment programme, or had been in the previous year. However some trials did not specify any exclusion criteria. Eleven trials took place in the United States (Chang 1997; Curry 2003; Fleming 1997; Fleming 1999; Fleming 2004; Gentillelo 1999; Kunz 2004; Longabaugh 2001; Maisto 2001; Ockene 1999; Senft 1997), five in the United Kingdom (Crawford 2004; Heather 1987; Lock 2006; Scott 1991; Wallace 1988), five in Spain (Altisent 1997; Cordoba 1998; Diez 2002; Fernandez 1997; Rodriguez 2003), two each in Canada (Israel 1996; McIntosh 1997), Finland (Aalto 2000; Seppa 1992), and Sweden (Romelsjo 1989; Tomson 1998), one in France (Huas 2002) and one in Australia (Richmond 1995).

There was substantial heterogeneity between trials in terms of the mechanisms of screening participants for inclusion, and also in the interventions whether in control or intervention arms.

Settings: Most interventions (n=24 trials) were administered in general practice-based primary care (Aalto 2000, Altisent 1997, Chang 1997, Cordoba 1998; Curry 2003; Diez 2002; Fernandez 1997; Fleming 1997, Fleming 1999; Fleming 2004; Heather

1987; Huas 2002; Israel 1996; Lock 2006; Maisto 2001; McIntosh 1997; Ockene 1999; Richmond 1995; Romelsjo 1989; Scott 1991; Senft 1997; Seppa 1992; Tomson 1998; Wallace 1988). Five trials were carried out in accident and emergency departments (Crawford 2004; Gentillelo 1999; Kunz 2004; Longabaugh 2001; Rodriguez 2003). Diez 2002 reported findings for two primary care settings and two other settings; only the former data were included in the meta-analyses.

Screening: Some trials used general health questionnaires, such as the Health and Habits Survey, and some incorporated alcohol consumption questions into these. Some trials used established alcohol screening tools such as CAGE, AUDIT or MAST, or variations on these. Some used a combination of these tools and determined alternative inclusion criteria to fit them, to increase the likelihood of picking up relevant participants. Most trials administered the screening tool by telephone or in the clinic as soon as the patient had registered for their appointment, but one administered the questionnaires by telephone afterwards. Inclusion criteria in terms of consumption were also defined differently, for example by number of units per week, or screening tool score, or level of binging.

Control group intervention: Three categories of control treatment were used. Some trials administered no intervention (Curry 2003; Diez 2002; Fernandez 1997; Gentillelo 1999; Heather 1987; Maisto 2001; Scott 1991; Seppa 1992; Richmond 1995) ; some "usual care", i.e. GP advice to cut down drinking (Aalto 2000; Altisent 1997; Cordoba 1998; Huas 2002; Longabaugh 2001; Romelsjo 1989; Senft 1997; Tomson 1998; Wallace 1988); some gave control participants a leaflet (either on general health issues or specifically about alcohol) (Crawford 2004, Fleming 1997; Fleming 1999; Fleming 2004; Kunz 2004; Ockene 1999); some gave control participants both usual care and a leaflet (Lock 2006; Aalto 2000; Rodriguez 2003). One trial did not have a control condition (Israel 1996), but compared extended intervention with advice from a nurse who gave the participants feedback about their gamma-glutamyltransferase (GGT) levels.

Brief intervention group: Participants received any or all of: motivational interviews; cognitive behavioural therapy; self-completed action plans; leaflets, either on general health issues or specifically about alcohol; requests to keep drinking diaries; written personalised feedback; follow-up telephone counselling; and exercises to complete at home. Thirteen trials evaluated a single brief intervention session (Chang 1997, Crawford 2004; Diez 2002; Fernandez 1997; Gentillelo 1999; Israel 1996, Kunz 2004; Lock 2006; Longabaugh 2001; Maisto 2001; Ockene 1999; Richmond 1995; Rodriguez 2003; Scott 1991). The number of sessions ranged from one to five, individual sessions varied from 1 to 50 minutes, while total intervention exposure time ranged from a mean of 7.5 minutes to 60 minutes. Professionals administering the intervention were general practitioners, nurse practitioners or psychologists.

Extended interventions: Five trials evaluated extended interventions (Aalto 2000; Israel 1996, Longabaugh 2001; Maisto 2001; Richmond 1995) in which the number of sessions delivered to patients ranged from two (Longabaugh 2001) to seven (Aalto 2000; Israel 1996). The duration of initial and booster sessions ranged from 15 minutes (Aalto 2000) to 50 minutes (Longabaugh 2001) . The trial of Longabaugh 2001 could not be included in the meta-analysis as it did not report the number of participants assessed by treatment arm.

Total treatment exposure: This was a combination of the initial session plus any additional booster sessions. Treatment duration in the intervention arm ranged from 5-10 minutes (Lock 2006) to 60 minutes (McIntosh 1997) advice or counselling; the median was 25 minutes and the inter-quartile range 7.5 to 30.0 minutes. Treatment duration in the control group ranged up to 10 minutes (Diez 2002; Rodriguez 2003). In the five extended intervention conditions the treatment exposures ranged from 65 minutes (Maisto 2001) to 175 minutes (Israel 1996).

Efficacy/effectiveness scores: These ranged from 4.5 (Fleming 2004; Romelsjo 1989) to 12 (Lock 2006); the median was 9 and the inter-quartile range 7.5-10.5 (*see* Table 1).

Table 1. Conversion factors for alcohol consumption and efficacy scores of trials.

Trial	Reported units	Conversion factor	Source of conversion	Efficacy score	Treatment exposure
Aalto 2000	grams/week	1	N/A	10.5	45, 105
Altisent 1997	units/week	8	Altisent 1997	8.5	25
Chang 1997	drinks/week	11.671	Miller 1991	9	7.5
Cordoba 1998	units/week	8	Cordoba 1998	11	38.1
Crawford 2004	units/week	8	Miller 1991	10.5	30

Table 1. Conversion factors for alcohol consumption and efficacy scores of trials. (*Continued*)

Curry 2003	drinks/week	1.671	Miller 1991	9	25.5
Diez 2002	units/week	8	Diez 2002	10.5	20
Fernandez 1997	units/week	10	Miller 1991	7.5	10
Fleming 1997	drinks/week	12	Fleming 1997	10.5	25
Fleming 1999	drinks/week	12	Fleming 1999	9	50
Fleming 2004	drinks/month	11.671 x (12/52)	Miller 1991	4.5	30
Gentillelo 1999	drinks/week	11.671	Miller 1991	6	30
Heather 1987	units/month	8 x (12/52)	Heather 1987	8.5	15
Huas 2002	units/week	10	Heather 2006	10	40
Israel 1996	drinks/month	13.456 x (12/52)	Miller 1991	7.5	175
Kunz 2004	drinks/week	11.671	Miller 1991	6	7.5
Lock 2006	drinks/week	8	Miller 1991	12	7.5
Longabaugh 2001	Alcohol consumption	not reported	N/A	6	50
Maisto 2001	drinks/month	11.671 x (12/52)	Miller 1991	5	12.5, 65
McIntosh 1997	drinks/month	13.456 x (12/52)	Miller 1991	10.5	60
Ockene 1999	drinks/week	12.8	Ockene 1999	11	7.5
Richmond 1995	drinks/week	10	Richmond 1995	9.5	5, 52.5
Rodriguez 2003	Alcohol consumption	not reported	N/A	10.5	12.5
Romelsjo 1989	grams/day	1 x 7	N/A	4.5	30
Scott 1991	units/week	8	Miller 1991	10	10
Senft 1997	drinks/3 months	11.671 x (4/52)	Miller 1991	9	15.5
Seppa 1992	Alcohol consumption	not reported	N/A	8.5	37.5
Tomson 1998	grams/week	1	N/A	8.5	22.5

Table 1. Conversion factors for alcohol consumption and efficacy scores of trials. (*Continued*)

Wallace 1988	units/week	8	Miller 1991	8.5	37.5

Baseline consumption of alcohol: This was not reported in the trials of Crawford 2004; Fernandez 1997; Gentillelo 1999; Longabaugh 2001; Rodriguez 2003; Senft 1997; Seppa 1992; Tomson 1998. In the remaining 21 trials, the mean baseline consumption ranged from 89 to 456 grams per week, with an overall mean across trials of 313 grams per week. Six trials reported baseline consumption for men only and also reported the number of men randomised; in these the mean baseline consumption was 377 grams of alcohol per week ; five trials reported baseline consumption for women only and also reported the number of women randomised; in these the mean baseline consumption was 219 grams of alcohol per week. Four trials (Aalto 2000; Chang; Fleming 2004; Senft) reported baseline measures of frequency of drinking in terms of days drinking per week; the mean value was 2.9 days/week. Three trials reported baseline measures of frequency of drinking in terms of number of binges per week; the mean value was 0.9 binges/week. Baseline intensity of drinking was reported in five trials, in which the mean baseline value was 110 grams per drinking day.

Reporting of outcomes

There was substantial heterogeneity between trials in measuring the participants' response to the intervention.

Quantity of alcohol consumed in a specified time period.

24 trials which compared a brief intervention with a control intervention reported the quantity of alcohol consumed in a specified time period, usually a week or a month. 16 trials (Aalto 2000; Altisent 1997; Cordoba 1998; Crawford 2004; Diez 2002; Fleming 1997; Fleming 1999; Fleming 2004; Heather 1987; Kunz 2004; Lock 2006; Maisto 2001; Richmond 1995; Scott 1991; Senft 1997; Wallace 1988) reported the final value at the end of follow-up of the quantity of alcohol consumed in a specified time period and the corresponding standard deviation. One trial (Curry 2003) reported the final value of the quantity of alcohol consumed per week and supplied us with unpublished data on the corresponding standard deviation. A further five trials (Fernandez 1997; Gentillelo 1999; Huas 2002; Ockene 1999; Romelsjo 1989) reported the change between baseline and the end of follow-up (change score) in the quantity of alcohol consumed in a specified time period and the corresponding standard deviation. These 22 trials were included in the primary meta-analysis comparing the effects of a brief intervention with a control intervention on the quantity of alcohol consumed per week. Two of these trials (Scott 1991; Wallace 1988) reported the quantity of alcohol consumed per week both as assessed by structured interview and as reported on a self-completed questionnaire: we used the interview data.

Two further trials (Chang 1997; Tomson 1998) reported the final values of the quantity of alcohol consumed in a specified time period but not the corresponding standard deviations. Although these trials were not included in the primary meta-analysis, they were included in a sensitivity analysis with imputation of the standard deviations.

One further trial (McIntosh 1997) reported the final values of the quantity of alcohol consumed in a specified time period but neither the corresponding standard deviations nor the number of participants assessed and so could not be included in any meta-analysis.

Three trials (Longabaugh 2001; Rodriguez 2003; Seppa 1992) did not report the quantity of alcohol consumed in a specified time period. One of these trials (Longabaugh 2001) reported only the frequency of drinking but not the number of participants assessed; one (Seppa 1992) reported only mean corpuscular volume (MCV); one (Rodriguez 2003) did not report any usable outcomes for quantity of alcohol consumed in a specified time period, frequency or intensity of drinking. Hence these trials could not be included in any meta-analysis.

One further trial (Israel 1996) which compared a brief intervention with an extended intervention reported the quantity of alcohol consumed per month and gamma-glutamyltransferase (GGT).

The units of alcohol used in each trial are presented in Table 1, with the conversion factor used to convert to grams of alcohol.

Frequency of drinking (number of drinking sessions in a specified time period)

Six trials reported the final values and standard deviation of the frequency of drinking in terms of number of days drinking each week or each month (Aalto 2000; Curry 2003; Senft 1997), or in terms of number of binge drinking occasions each week or each month (Fleming 1997; Fleming 1999; Fleming 2004). One trial (Chang 1997) reported the number of days drinking each week but not the corresponding standard deviations. Longabaugh

2001reported the number of days drinking but the number of participants assessed was not reported.

Intensity of drinking (amount of alcohol consumed in a drinking session)

Five trials reported final values and standard deviation of intensity of alcohol consumption in terms of number of drinks per occasion (Aalto 2000; Crawford 2004; Curry 2003; Maisto 2001; Senft 1997). One trial (Chang 1997) reported the number of drinks per occasion but not the corresponding standard deviations.

Laboratory markers

Five trials reported the final values and standard deviation of laboratory markers: either mean corpuscular volume (MCV) (Seppa 1992) or gamma-glutamyltransferase (GGT) (Aalto 2000; Israel 1996; Romelsjo 1989; Wallace 1988). Romelsjo 1989 reported change scores whereas the other trials reported final values.

Heavy drinkers and binge drinkers

Four trials (Altisent 1997; Cordoba 1998; Diez 2002; Fernandez 1997) reported the final percentage of participants who were drinking >35 units/week; one trial (Wallace 1988) reported the final percentage of participants who were heavy drinkers, defined as >35 units/week for men and >21 units/week for women; four trials (Curry 2003; Fleming 1997; Fleming 1999; Fleming 2004) reported the final percentage of participants who were heavy drinkers; and four trials (Curry 2003; Fleming 1997; Fleming 1999; Kunz 2004) reported the final percentage of participants who were binge drinkers, but heavy drinkers and binge drinkers were defined differently in different trials.

Adverse effects

Two trials (Crawford 2004; Gentillelo 1999) reported new injuries necessitating further visits to the Emergency Department or hospital readmission, by treatment arm.

Two trials (Longabaugh 2001; Romelsjo 1989) reported other measures of harms due to alcohol consumption by treatment arm. Longabaugh 2001 reported DrInC (Drinker Inventory of Consequences) and IBC (revised Injury Behaviour Checklist) scores at 11 year and Romelsjo 1989 reported changes in a "problem index", which was a crude summary measure constructed from the answers to six frequency questions.

Patient satisfaction measures

No trials reported patient satisfaction measures.

Health-related quality of life

Crawford 2004 reported GHQ (General Health Questionnaire) score at 6 months and EQ5D (standardised instrument to measure health outcomes) scores at 1 year, by treatment arm. Lock 2006 reported DPI (Drinking Problems Index) and SF-12 (health-related quality of life, in terms of both mental and physical health) at 1 year, by treatment arm.

Economic measures including use of health services

Lock 2006 carried out an economic evaluation, by treatment arm, based on: GP visits, nurse practitioner visits, A&E visits, hospital inpatient care, hospital outpatient visits, total health care costs, total health care costs plus intervention delivery costs, patient costs.

Reporting of outcomes by gender

Some trials reported on male and female participants in separate papers: Anderson 1992 and Scott 1991 reported on the male and female participants respectively from the Cut Down on Drinking trial (Scott 1991); Aalto 2000 reported on the male and female participants respectively from the Lahti trial (Aalto 2000). Some trials (Richmond 1995; Scott 1991; Senft 1997; Seppa 1992; Wallace 1988) reported on male and female participants separately in the same paper. For the primary meta-analysis, data on men and women in the same trial were combined.

Two trials (Fleming 1997; Ockene 1999) reported the final values of the quantity of alcohol consumed in a specified time period by gender but did not report the number of men and women assessed and so could not be included in the meta-analysis of quantity of alcohol consumed in a specified time period, sub-grouped by gender.

Reporting of outcomes at several follow-up time-points

Four trials were reported at several time-points, although not all outcomes were reported at all time-points. TrEAT (Fleming 1997) was reported after follow-up of one and four years by Fleming 1997 and Fleming 2002 respectively; and HEALTH (Ockene 1999) was reported after follow-up of six months and one year by Ockene 1999 and Reiff-Hekking 2005 respectively. Richmond 1995 reported outcomes at 6 months and a year. Curry 2003 reported outcomes at 3 months and a year. If outcomes were reported at several time-points, data for one year follow-up were used in the meta-analyses if available. Aalto 2000 reported outcomes at one year for the intervention groups but not for the control group, so the outcomes at three years (reported for all arms) were used in the meta-analyses.

Cluster randomised trials

Four trials (Cordoba 1998; Huas 2002; Lock 2006; Ockene 1999) were cluster randomised: Cordoba 1998 and Lock 2006 randomised 33 and 40 primary care practices respectively with averages of 6.9 and 2.0 patients/practice; Huas 2002 randomised 88 primary care physicians with an average of 4.8 patients/physician; Ockene 1999 randomised four internal medicine practice

sites with an average of 111 patients/site. Lock 2006 and Ockene 1999 used methods of analysis of final values and change scores respectively which took into account the cluster randomisation. Cordoba 1998 and Huas 2002 did not allow for the cluster randomisation in their analysis; for these trials we abstracted the reported standard deviations of the treatment and control groups and then inflated them, assuming an ICC of 0.06 (Lock 2006) to allow for the clustered design (Deeks 2006a).

Block randomised trials

Three trials (Fernandez 1997; Richmond 1995; Rodriguez 2003) used "block randomisation". Richmond 1995 and Rodriguez 2003 stated that participants were randomly assigned by weeks. Fernandez 1997 stated that participants were block-randomised to control or intervention arms. It is unclear from the reports whether this method of randomisation was randomisation of individuals using random permuted blocks (section 4.2, Matthews 2000) or whether it was randomisation of groups of participants and so should be regarded as cluster randomisation (section 10.6, Matthews 2000).

Intention to Treat Analysis

Ten of the included trials reported that intention-to-treat analysis was done (Aalto 2000; Crawford 2004; Fleming 1997; Fleming 2004; Gentillelo 1999; Lock 2006; Longabaugh 2001; Richmond 1995; Scott 1991; Senft 1997). Three of these trials (Aalto 2000; Richmond 1995; Scott 1991) imputed final outcomes to be the same as baseline values for the 11%, 31% and 44% of participants respectively who were lost to follow-up; these trials also reported data excluding participants lost to follow-up, which we used in the primary meta-analysis. Fleming 2004 imputed values from the interview with the longest follow-up to the 11% of participants who were lost to follow-up. The remaining trials which reported intention-to-treat analysis excluded from analysis participants who were lost to follow-up.

In the remaining trials it was not possible to determine whether or not intention-to-treat analysis was performed (Altisent 1997; Chang 1997; Cordoba 1998; Curry 2003; Diez 2002; Fernandez 1997; Fleming 1999; Heather 1987; Huas 2002; Israel 1996; Kunz 2004; Maisto 2001; McIntosh 1997; Ockene 1999; Romelsjo 1989; Seppa 1992; Tomson 1998; Wallace 1988).

Reporting of sub-groups

Some reports were of sub-groups of trials reported in other references: Manwell 2000 reported a four-year follow-up of a female sub-group of the TrEAT trial (Fleming 1997); Gordon 2003 reported on an elderly sub-group of the ELM trial (Maisto 2001); only data for the entire trial were included in meta-analyses.

Requests to authors for missing data

Authors were contacted for missing data concerning numbers randomised by treatment arm and baseline data for all randomised participants (rather than for those assessed) but unpublished data were obtained only for the trial of Curry 2003.

Risk of bias in included studies

Allocation

Generation of randomisation sequence

Randomisation was confirmed as adequate in 12 trials (Aalto 2000; Altisent 1997; Chang 1997; Crawford 2004; Fernandez 1997; Fleming 1997; Gentillelo 1999; Israel 1996; Lock 2006; Maisto 2001; Ockene 1999; Scott 1991), and in one study (Tomson 1998) the method of randomisation used was inadequate, as participants were assigned to treatment arms on the basis of their date of birth. In the remaining 16 trials the method of randomisation used was unclear (Cordoba 1998; Curry 2003; Diez 2002; Fleming 1999; Fleming 2004; Heather 1987; Huas 2002; Kunz 2004; Longabaugh 2001; McIntosh 1997; Richmond 1995; Rodriguez 2003; Romelsjo 1989; Senft 1997; Seppa 1992; Wallace 1988).

Concealment of allocation

Concealment of allocation was confirmed as adequate in 10 trials (Altisent 1997; Cordoba 1998; Fleming 1997; Fleming 1999; Gentillelo 1999; Maisto 2001; Ockene 1999; Lock 2006; Romelsjo 1989; Scott 1991). In seven trials (Aalto 2000; Crawford 2004; Fernandez 1997; Fleming 2004; Israel 1996; Longabaugh 2001; Rodriguez 2003) it was inadequate and in 11 trials (Chang 1997; Diez 2002; Heather 1987; Huas 2002; Kunz 2004; McIntosh 1997; Richmond 1995; Senft 1997; Seppa 1992; Tomson 1998; Wallace 1988) it was unclear.

Blinding

Due to the nature of the interventions used it was not possible to blind either the participants or the providers of care. In 18 trials the outcome assessors were blinded (Crawford 2004; Curry 2003; Fernandez 1997; Fleming 1997; Fleming 1999; Fleming 2004; Gentillelo 1999; Heather 1987; Kunz 2004; Lock 2006; Longabaugh 2001; Ockene 1999; Richmond 1995; Rodriguez 2003; Romelsjo 1989; Scott 1991; Senft 1997; Wallace 1988). The blinding of the outcome assessors was unclear in 10 trials (Aalto 2000; Altisent 1997; Chang 1997; Cordoba 1998; Diez 2002; Huas 2002; Israel 1996; Maisto 2001; Seppa 1992; Tomson 1998) and inadequate in one study (McIntosh 1997).

Other potential sources of bias

Loss to follow-up

Three trials (Cordoba 1998; Huas 2002; Israel 1996) reported overall loss to follow-up of 49%, 30% and 10% respectively, but did not report the number randomised by treatment arm, so loss to follow-up by treatment arm could not be estimated. One fur-

ther trial (Senft 1997) did not report the number randomised by treatment arm for male and female participants.

Three trials (Longabaugh 2001; McIntosh 1997; Rodriguez 2003) did not report the number assessed by treatment arm; hence loss to follow-up could not be estimated by treatment arm for these trials.

In 11 of the included trials which reported loss to follow-up by individual arm, this was more than 20% in all of the study arms (Aalto 2000; Altisent 1997; Curry 2003; Gentillelo 1999; Kunz 2004; Lock 2006; Maisto 2001; Richmond 1995; Rodriguez 2003; Scott 1991; Seppa 1992).

Effects of interventions

All forest plots, except comparison 1, outcome 5, are presented with trials ordered by their efficacy/effectiveness score, i.e. the most "real world" trial (Lock 2006) is at the top and the most tightly controlled efficacy trial (Fleming 2004) is at the bottom. Comparison 1, outcome 5 is presented with trials ordered by their estimated treatment exposure (mean duration of counselling in the intervention arm).

(1) Brief intervention vs. control

Quantity of alcohol consumed per week

Primary meta-analysis (*see* Analysis 1.1)

The primary meta-analysis included 22 trials (*see* Description of studies: reporting of outcomes) which enrolled 7,619 participants (median 247, range: 83 to 909), with a mean age of 43 years and assessed 5,860 (77%) of participants at the end of follow-up. All trials except one (Diez 2002) rreported gender and in these about 67% of the participants were male. Only eight trials (Cordoba 1998; Curry 2003; Fleming 1997; Fleming 2004; Kunz 2004; Maisto 2001; Ockene 1999; Senft 1997) reported ethnicity and in these about 72% of the participants were white. 18 trials reported outcomes after follow-up of one year; Aalto 2000 , Fernandez 1997, Heather 1987 and Kunz 2004 and had follow-up of three years, 18 months, six months and three months respectively.

Meta-analysis showed that participants receiving brief intervention drank less alcohol per week than those receiving a control intervention (mean difference = -38, 95%CI: -54 to -23 grams/week). There was no significant difference between the pooled findings of the effectiveness trials and the pooled findings of the efficacy trials: the 10 effectiveness trials showed significant benefits of brief intervention (mean difference = -33, 95%CI: -54 to -13 grams/week), and the 12 efficacy trials showed a similar benefit of brief intervention (mean difference = -45, 95%CI: -70 to -20 grams/week). There was substantial heterogeneity (I^2 = 57%) between the findings of the trials: while all trials except Aalto 2000 and Richmond 1995 reported a benefit of brief intervention compared to control, the estimated benefit varied substantially between trials.

The forest plot showed no obvious relationship between the treatment effect and the efficacy/effectiveness score and this was confirmed by meta-regression, performed in Stata, which showed a non-significant increase in the effect of treatment with increasing efficacy/effectiveness score (an increase in the reduction in alcohol consumption of 1.2, 95%CI: -6.5 to 8.9 grams/week, p=0.76, for each increase of one in the efficacy/effectiveness score - *see* Figure 2). A funnel plot (*see* Figure 3) showed little asymmetry.

Figure 2. Estimated treatment effect versus effectiveness/efficacy score. The lines show the predicted meta-regression line and its 95%CI.

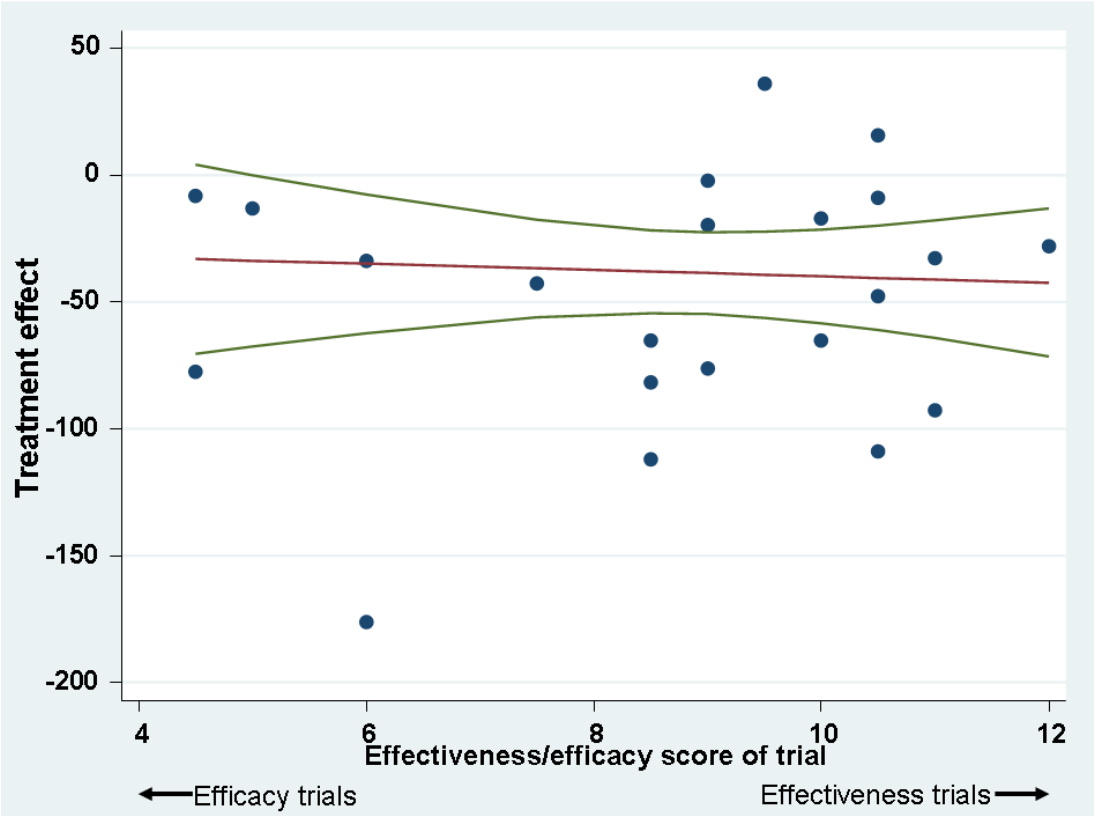
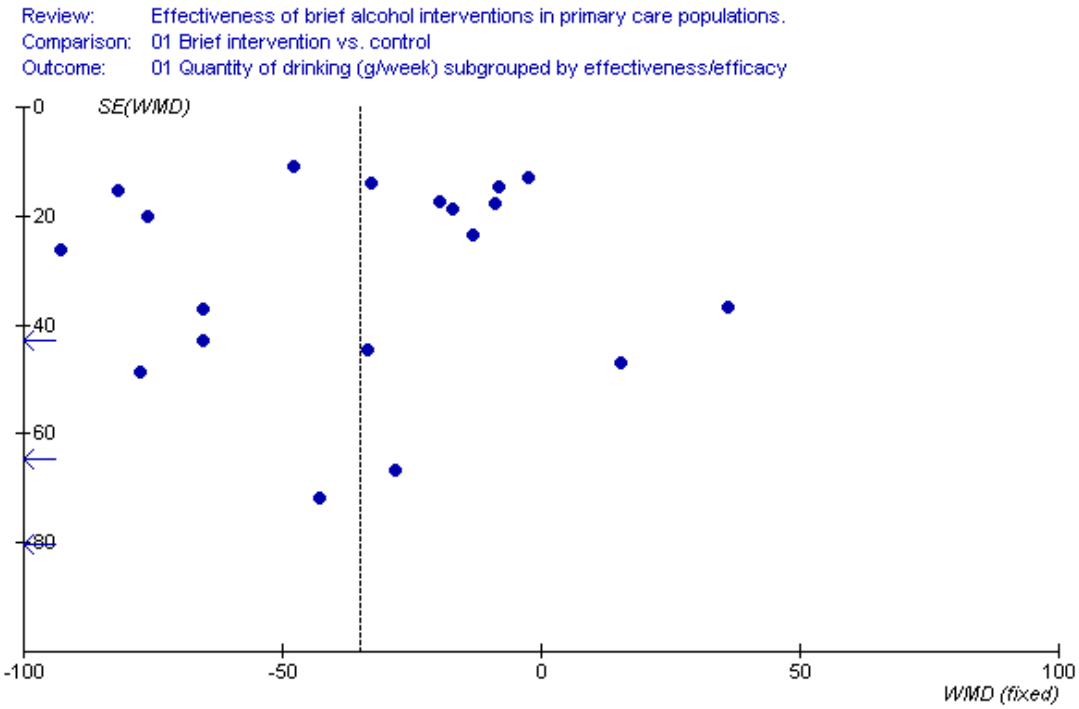


Figure 3. Funnel plot showing precision of trials versus treatment effect.



Sensitivity analysis, restricted to trials with adequate concealment of allocation (*see* Analysis 1.2)

Meta-analysis restricted to the 10 trials that confirmed concealment of allocation showed similar results both for the effectiveness trials (mean difference = -48, 95%CI: -65 to -31 grams/week), the efficacy trials (mean difference = -71, 95%CI: -115 to -26 grams/week) and for all trials (mean difference = -56, 95%CI: -75 to -36 grams/week), with moderate heterogeneity (I2 = 33%) between trials. Seven of these trials reported blinding of the outcome assessor and seven reported adequate randomisation.

Sensitivity analysis, using intention-to-treat data (*see* Analysis 1.3)

The primary meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 11% and 44% of participants respectively who were lost to follow-up in the trials of Aalto 2000 and Scott 1991. This showed similar results both for the effectiveness trials (mean difference = -28, 95%CI: -47 to -10 grams/week), the efficacy trials (mean difference = -51, 95%CI: -77 to -25 grams/week) and for all trials (mean difference = -38, 95%CI: -53 to -23 grams/week), with substantial heterogeneity (I2 = 56%) between trials.

Sensitivity analysis, imputing unknown standard deviations (*see* Analysis 1.4)

Sensitivity analysis was performed including two additional trials which did not report standard deviations (Chang 1997; Tomson 1998). They were included by imputing the median standard deviation of 191 grams of alcohol/week to both treatment and control groups. Inclusion of these trials made little difference to the findings: both of these trials were efficacy trials, so there was no change to the pooled findings of the effectiveness trials. The 13 efficacy trials showed significant benefits of brief intervention (mean difference = -45, 95%CI: -71 to -20 grams/week);pooling of all 23 trials showed significant benefits of brief intervention (mean difference = -36, 95%CI: -52 to -21 grams/week).

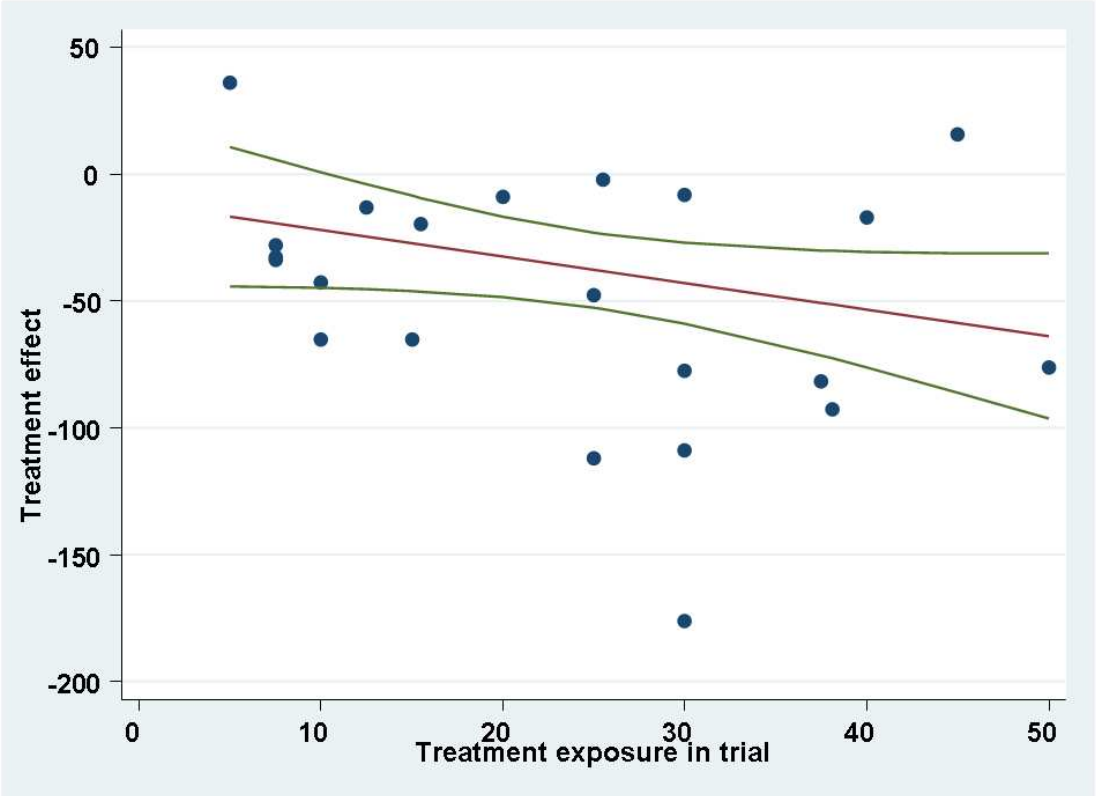
Meta-analysis sub-grouped by high/low treatment exposure (*see* Analysis 1.5)

Although high exposure to treatment resulted in a greater net reduction in alcohol consumption than low exposure to treatment (mean differences of -51, 95%CI: -75 to -27 and -23, 95%CI: -38 to -8 grams/week respectively), the difference between outcomes consequent to these treatment modalities was not statistically significant. Although there was no heterogeneity between the results of trials that had low exposure to treatment, substantial heterogeneity (I2=72%) remained among trials with high exposure to

treatment.

The forest plot is presented with trials ordered by their estimated treatment exposure (mean duration of counselling in the intervention arm minus duration of control treatment) and showed no obvious consistent relationship between the treatment effect and the treatment exposure. This was confirmed by meta-regression, performed in Stata, which showed little evidence of any increase in the effect of treatment with increasing treatment exposure (an increase in the reduction in alcohol consumption of 1.0, 95%CI: -0.1 to 2.2 grams/week, p=0.09, for each increase of one minute in the treatment exposure - *see* Figure 4).

Figure 4. Estimated treatment effect versus treatment exposure (mean duration of counselling for the participants in the trial) for trials comparing brief intervention with control. The lines show the predicted meta-regression line and its 95%CI.



Meta-analysis sub-grouped by gender (*see* Analysis 1.6)

Only 6 trials reported sufficient information (mean, standard deviation and number of participants assessed by treatment arm) by gender about the amount of alcohol consumed per week to allow inclusion in a meta-analysis sub-grouped by gender. Men experienced significant benefits of brief intervention (mean difference = -57, 95%CI: -89 to -25 grams/week) but women did not (mean difference = -10, 95%CI: -48 to 29 grams/week; nevertheless, the difference between men and women was not statistically significant. These results are based on a sample of only 499 women.

Only one trial reporting the effects in women (Scott 1991) confirmed adequate concealment of allocation. The findings of this trial (mean difference = -23, 95%CI: -95 to 48 grams/week) confirmed the overall finding that there was no statistically significant reduction in alcohol consumption in women receiving brief intervention.

The lack of significant reduction in alcohol consumption in women was heavily influenced by one trial (Aalto 2000), which reported a marked reduction in alcohol consumption in women in the control group. However, this trial had inadequate concealment of allocation and the control and intervention groups were not comparable at baseline.

Meta-analysis sub-grouped by gender, excluding trials recruiting only one gender (*see* Analysis 1.7)

In order to compare the effects of brief intervention in men and women with less confounding with other differences between trials, the meta-analysis sub-grouped by gender was repeated excluding three trials which recruited only men. Similar results were obtained: men experienced significant benefits of brief intervention (mean difference = -53, 95%CI: -93 to -13 grams/week), with no significant difference between men and women.

Meta-analysis sub-grouped by cluster/individual randomised (*see* Analysis 1.8)

The four cluster randomised trials (Cordoba 1998; Huas 2002; Lock 2006; Ockene 1999) showed similar results (mean difference = -41, 95%CI: -73 to -9 grams/week) to the 18 individually randomised trials (mean difference = -38, 95%CI: -54 to -23 grams/week).

Sensitivity analysis, varying imputed value of intra-cluster correlation coefficient for cluster randomised trials (*see* Analysis 1.9)

Sensitivity analysis was performed, assuming an intra-cluster correlation coefficient of 0.32 (as found in the trial of Lock 2006, unpublished data) for the two cluster randomised trials (Cordoba 1998; Huas 2002) which did not allow for cluster randomisation in the analysis. This showed similar results for the cluster randomised trials (mean difference = -35, 95%CI: -57 to -12 grams/week).

Frequency of drinking

Frequency of binge drinking, sub-grouped by effectiveness/efficacy (*see* Analysis 1.10)

Only three trials (Fleming 1997; Fleming 1999; Fleming 2004) reported the frequency of binge drinking and, overall, these showed no significant reduction in frequency of binge drinking consequent to brief intervention (mean difference = -0.3, 95%CI: -0.6 to 0.0 binges/week), with little difference between the findings of the two effectiveness trials (Fleming 1997; Fleming 1999) and the efficacy trial (Fleming 2004).

Number of drinking days/week, subgrouped by effectiveness/efficacy (*see* Analysis 1.11)

Two effectiveness trials (Aalto 2000; Curry 2003) and one efficacy trial (Senft 1997) reported the number of drinking days per week and, overall, these showed no significant effect of brief intervention compared to control (mean difference = -0.04, 95%CI: -0.5 to 0.4 drinking days/week) with no significant difference between effectiveness and efficacy trials. When this meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 11% of participants who were lost to follow-up in the trial of Aalto 2000, the results were similar (results not shown).

Number of drinking days/week, subgrouped by gender (*see* Analysis 1.12)

Two trials (Aalto 2000; Senft 1997) reported the number of drinking days per week for men and women separately and, overall, these showed no significant effect of brief intervention compared to control (mean difference = 0.1, 95%CI: -0.6 to 0.4 drinking days/week) with no significant difference between men and women. When this meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 11% of participants who were lost to follow-up in the trial of Aalto 2000, the results were similar (results not shown).

Intensity of drinking

Intensity of drinking, sub-grouped by effectiveness/efficacy (*see* Analysis 1.13)

Only five trials (Aalto 2000; Crawford 2004; Curry 2003; Maisto 2001; Senft 1997) reported the amount of alcohol consumed per drinking day and, overall, these showed no significant reduction in intensity of drinking consequent to brief intervention (mean difference = -3.1, 95%CI: -8.8 to 2.6 grams/drinking day), with no statistically significant difference between the findings of the effectiveness (Aalto 2000; Crawford 2004; Curry 2003) and efficacy (Maisto 2001; Senft 1997) trials. When this meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 11% of participants who were lost to follow-up in the trial of Aalto 2000, the results were similar (results not shown).

Intensity of drinking, sub-grouped by gender (*see* Analysis 1.14)

Two trials (Aalto 2000; Senft 1997) reported the intensity of drinking by gender; this showed no significant difference between men and women. Women demonstrated a statistically non-significant increase in alcohol per drinking day after receiving the brief intervention (mean difference = 24.2, 95%CI: -17.2 to 65.5 grams/drinking day), and men a statistically non-significant decrease (mean difference = -7.4 95%CI: -31.5 to 16.8 grams/drink-

ing day). When this meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 11% of participants who were lost to follow-up in the trial of Aalto 2000, the results were similar (results not shown).

Laboratory markers of drinking

Gamma-glutamyltransferase (GGT) (*see* Analysis 1.15)

Three trials (Aalto 2000; Romelsjo 1989; Wallace 1988) reported GGT; this showed no significant difference between brief intervention and control, with no heterogeneity between trials (mean difference = -1.1 95%CI: -3.9 to 1.7 IU/l, I2=0%). When this meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 11% of participants who were lost to follow-up in the trial of Aalto 2000, the results were similar (results not shown).

Gamma-glutamyltransferase (GGT), subgrouped by gender (*see* Analysis 1.16)

In the two trials (Aalto 2000; Wallace 1988) which reported GGT by gender, there was no significant difference between men and women, although men showed a non-significant decrease in GGT consequent to brief intervention (mean difference = -2.2 95%CI: -6.3 to 2.0 IU/l, I2=0%) while women showed a non-significant increase (mean difference = 3.5 95%CI: -6.0 to 12.9 IU/l, I2=29%).

Mean corpuscular volume (MCV) (*see* Analysis 1.17)

One trial (Seppa 1992) reported MCV; this showed no significant difference between brief intervention and control, both overall (mean difference = 0.6 95%CI: -1.6 to 2.8 fl) and for each gender separately.

Problem drinkers

Heavy drinkers(*see* Analysis 1.18)

Nine trials (Altisent 1997; Cordoba 1998; Curry 2003; Diez 2002; Fernandez 1997; Fleming 1997; Fleming 1999; Fleming 2004; Wallace 1988) reported the percentage of heavy drinkers, although the definition of heavy drinking varied between trials: Altisent 1997, Cordoba 1998 and Fernandez 1997 reported the percentage of participants who drank over 35 units/week; Fleming 1997; Fleming 1999 reported the percentage of men and women who drank over 20 and 13 drinks/week respectively; Fleming 2004 reported the percentage of men and women who drank over 30 and 25 drinks/week respectively); Curry 2003 reported the percentage of men and women who drank over 2 and 1 drinks/day respectively; Wallace 1988 reported the percentage of participants who were heavy drinkers, defined as >35 units/week for men and >21 units/week for women. The findings of these trials are presented on a forest plot, but are not combined in a meta-analysis, because of the varied definitions of heavy drinkers. All trials showed a reduction in the percentage of heavy drinkers in participants receiving the brief intervention, although this reduction was statistically significant for only five trials.

Binge drinkers(*see* Analysis 1.19)

Four trials (Curry 2003; Fleming 1997; Fleming 1999; Kunz 2004) reported the percentage of binge drinkers and, overall, these showed a significant reduction in the percentage of binge drinkers in the brief intervention group compared to the control group (risk difference = -11%, 95%CI: -19% to -3%).

Loss to follow-up (*see* Analysis 1.20)

Twenty-three trials reported loss to follow-up; in these the overall loss to follow-up was 28%. Meta-analysis of these trials showed a significantly higher rate of loss to follow-up in the brief intervention group than the control group (risk difference = 4%, 95%CI: 1% to 7%. Although the higher loss to follow-up in the brief intervention group was more marked in efficacy trials than in effectiveness trials, the difference was not statistically significant.

Adverse effects

Crawford 2004 reported 0.5 fewer visits to Emergency Department by the intervention group during the year after randomisation.

Gentillelo 1999 reported: a reduction of 47% in new injuries requiring either treatment in the emergency department or readmission to the trauma service in the intervention group compared with controls after one year's follow-up, after controlling for alcohol consumption, gender, age, injury severity, injury intent (hazard ratio 0.53, 95% CI 0.26 to 1.07, p=0.07); a reduction of 48% in inpatient hospital readmissions for treatment of a new injury in intervention group compared with controls after three years follow-up (hazard ratio 0.52, 95% CI 0.21 to 1.29); but no significant differences in the death rate between the intervention and control groups (I=2.7%, C=2.3%).

Longabaugh 2001 reported the DrInC (Drinker Inventory of Consequences) score at 1 year: participants who received an extended intervention had significantly fewer DrInC consequences than the control group. Longabaugh also reported the IBC score (revised Injury Behaviour Checklist): participants who received an extended intervention had significantly fewer alcohol-related injuries than control, but participants who received a brief intervention had similar outcomes to the control group.

Romelsjo 1989 reported changes in a "problem index", a crude summary measure constructed from the answers to six questions about the frequency of drinking and of alcohol-related symptoms. However, there was no significant difference between the intervention and control groups in the change in the problem index during the course of the trial.

Health-related quality of life

Crawford 2004 reported GHQ (General Health Questionnaire) score at 6 months and EQ-5D (a standardised instrument to measure health outcomes) scores at 12 months: there was no significant difference between the intervention and control groups in these measures.

Lock 2006 reported DPI (Drinking Problems Index) and SF-12 (health-related quality of life, for both mental and physical health) at 12 months; there was no significant difference between the intervention and control groups in these measures.

Economic measures including use of health services

Lock 2006 carried out an economic evaluation based on: GP visits, nurse practitioner visits, A&E visits, hospital inpatient care, hospital outpatient visits, total health care costs, total health care costs plus intervention delivery costs, patient costs, but found no significant difference between the intervention and control groups in the total healthcare cost including the cost of delivery of the intervention.

(2) Extended intervention vs. brief intervention

Quantity of alcohol consumed per week

Primary meta-analysis (*see* Analysis 2.1)

The primary meta-analysis included four trials (Aalto 2000; Israel 1996; Maisto 2001; Richmond 1995) enrolling 186, 105, 201 and 192 participants respectively. Israel 1996 enrolled participants aged 30-60 years but did not report the mean age; in the trials of Aalto 2000, Maisto 2001 and Richmond 1995, participants had a mean age of 44, 44 and 39 years. Aalto 2000, Maisto 2001and Richmond 1995 reported gender and in these about 70%, 70% and 57% of the participants were male. None of these trials reported ethnicity. Aalto 2000, Israel 1996, Maisto 2001 and Richmond 1995 had follow-up of three years, one year, six months and one year respectively.

Meta-analysis showed that participants receiving an extended intervention drank less alcohol per week than those receiving a brief intervention (mean difference = -28, 95%CI: -62 to 6 grams/week), with little heterogeneity (I2 = 0%) between the findings of the trials. However, the only trial that reported adequate concealment of allocation was Maisto 2001, which showed no significant difference between extended and brief interventions (mean difference = -17, 95%CI: -64 to 29 grams/week).

Sensitivity analysis, using imputed outcomes for participants lost to follow-up (*see* Analysis 2.2)

The primary meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 47% of participants who were lost to follow-up in the trial of Aalto 2000. This showed similar results (mean difference = -27, 95%CI: -59 to 5 grams/week), with no heterogeneity (I2 = 0%) between trials.

Meta-analysis sub-grouped by gender (*see* Analysis 2.3)

Only Aalto 2000 and Richmond 1995 reported outcomes by gender. There was no significant difference between extended and brief interventions either for men (mean difference = -17, 95%CI: -90 to 57 grams/week), or women (mean difference = -52, 95%CI: -181 to 77 grams/week).

Frequency of drinking

Number of drinking days/week (*see* Analysis 2.4)

One trial (Aalto 2000) reported the number of drinking days per week and showed a statistically significant benefit of extended intervention compared to brief intervention (mean difference = 0.7, 95%CI: -1.3 to -0.1 drinking days/week). When this meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 47% of participants who were lost to follow-up, the results were similar (results not shown).

Intensity of drinking

Intensity of drinking, sub-grouped by effectiveness/efficacy (*see* Analysis 2.5)

Only two trials (Aalto 2000; Maisto 2001) reported the amount of alcohol consumed per drinking day and, overall, these showed no significant difference between extended and brief interventions (mean difference = 5.8, 95%CI: -12.7 to 24.4 grams/drinking day), with substantial heterogeneity between trials (I2=53%). When this meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 47% of participants who were lost to follow-up in the trial of Aalto 2000, the results were similar (results not shown).

Laboratory markers of drinking

Gamma-glutamyltransferase (GGT) (*see* Analysis 2.6)

Two trials (Aalto 2000; Israel 1996) reported GGT; this showed no significant difference between extended and brief interventions (mean difference = -2.6 95%CI: -15.7 to 10.4 IU/l) . When this meta-analysis was repeated, assuming final outcomes to be the same as baseline values for the 47% of participants who were lost to follow-up in the trial of Aalto 2000, the results were similar (results not shown).

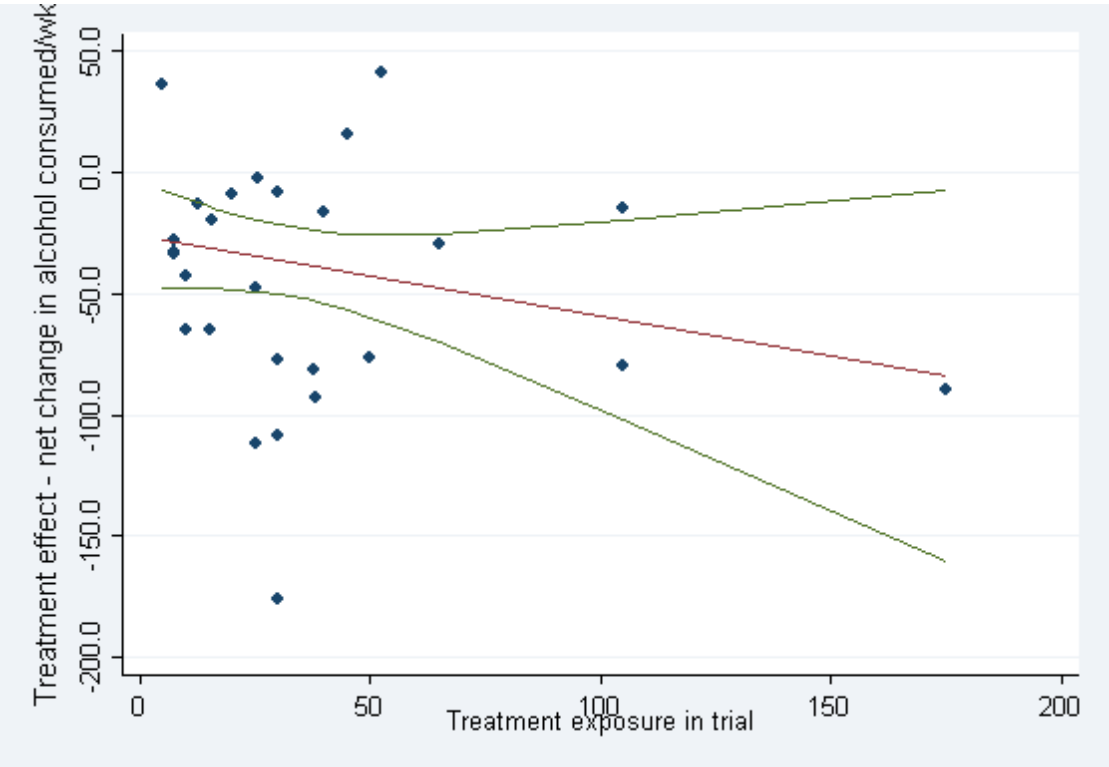
Loss to follow-up (*see* Analysis 2.7)

Three trials (Aalto 2000; Maisto 2001; Richmond 1995) reported loss to follow-up; in these the overall loss to follow-up was 37%. Meta-analysis of these trials showed a non-significantly higher rate of loss to follow-up in the extended compared to the brief intervention group (risk difference = 4%, 95%CI: -5% to 14%).

Meta-regression of treatment effect on treatment exposure, including both brief and extended interventions

The meta-regression of treatment effect on treatment exposure, reported above for trials which compared brief intervention with control, was repeated additionally including the three trials which assessed an extended intervention. This provided greater statistical power for the meta-regression, not only through inclusion of three extra trials, but also through inclusion of trials with greater treatment exposure. For the trials of Aalto 2000 and Maisto 2001, the extended intervention was compared with the control group; the trial of Israel 1996 did not include a control group, so the extended intervention was compared with the brief intervention. The meta-regression showed less marked effects of treatment exposure than the previous meta-regression: a non-significant increase in the effect of treatment with increasing treatment exposure (an increase in the reduction in alcohol consumption of 0.3, 95%CI: -0.2 to 0.8 grams/week, p=0.24, for each increase of one minute in the treatment exposure - *see* Figure 5).

Figure 5. Estimated treatment effect versus treatment exposure (mean duration of counselling for the participants in the trial) for trials evaulating both brief and extended interventions. The lines show the predicted meta-regression line and its 95%CI.



DISCUSSION

Summary of main findings

Primary meta-analysis of 22 trials enrolling 7,619 participants showed that, compared to a control intervention, brief intervention reduced the quantity of alcohol consumed per week by 38 grams (95%CI: 23 to 54), which equates to 4-5 units. These results were robust: several sensitivity analysis were performed and all yielded similar results. Trials of uncertain quality which did not report adequate concealment of allocation were excluded; analysis was repeated imputing data for participants who were lost to follow-up for the two trials which reported these data; trials which did not report standard deviations were included with imputed standard deviations; analysis was repeated varying the assumptions about the intra-cluster correlation coefficient of cluster randomised trials which did not allow for the cluster randomisation in the analysis. All of these sensitivity analyses showed a statistically significant benefit of brief intervention. A funnel plot (*see* Figure 3) showed no evidence of publication bias.

A few trials reported other endpoints: pooling of four trials showed a significant decrease in the percentage of binge drinkers in the brief intervention group; although we did not meta-analyse heavy drinkers due to the different definitions of heavy drinking different trials, all eight trials reporting this outcome showed a decrease in heavy drinking in the brief intervention group. Three and five trials respectively showed non-significant lower frequency and intensity of drinking in the brief intervention group than the control group: the lack of statistical significance in these findings may be due to a lack of statistical power. Only four trials reported laboratory markers of alcohol consumption; three reported GGT and one reported MCV. These showed no significant difference between brief intervention and control, again probably due to low statistical power.

There was substantial heterogeneity between trials in the settings

(primary care or accident and emergency), populations enrolled, screening instruments used, baseline consumption of alcohol, and the active and control interventions delivered. Hence the statistical heterogeneity in the meta-analyses is not surprising. Sub-group analyses showed that heterogeneity of findings was restricted to individually randomised trials and trials with high treatment exposure.

Overall, about 70% of the participants were men. Ethnicity was poorly reported; in those trials which did report it, about 70% of participants were white. On entering the trials, participants consumed, on average, about 310 grams of alcohol per week, but this varied between trials from about 90 to 460 grams per week.

Effectiveness/efficacy

There was no significant difference between trials classified as effectiveness and efficacy trials in the effect of brief intervention on the quantity of alcohol consumed, and meta-regression showed no significant relationship between the estimated treatment effect and the efficacy score of the trial. This lack of evidence of any difference may indicate insensitivity in our classification tool. In some papers, authors did not report information relating to certain items. In these cases we ascribed a mid-value score for that item so as not to tip the study towards the efficacy or effectiveness domain. This may have reduced variation in the final scores (there were not many extreme scores particularly on the efficacy side of the scale) and led to clustering of trials towards the middle of this scale. However, although the trials were skewed towards the effectiveness domain, they were distributed along a continuum. Moreover, it is highly unlikely that there would be any pure efficacy studies since the trial protocol would need to be acceptable and relevant to clinicians (and ethics committees) before it could be enacted in health service contexts. It is possible that the treatment effect may be related to some of the individual factors which were combined in the efficacy score. However, we did not investigate this as it would have been a *post hoc* analysis, not specified in the protocol.

Treatment exposure

Trials classified as high treatment exposure - on the basis of the average length of time which treatment providers spent counselling participants in the intervention group - showed a non-significantly greater net reduction in alcohol consumption than trials with low treatment exposure. However, meta-regression showed little evidence of a trend of a greater reduction in alcohol consumption in trials with greater treatment exposure. The comparison of extended and brief interventions allowed a direct comparison of low and high treatment exposure, free of confounding with other factors. This showed a greater overall reduction in alcohol consumption in the group receiving extended intervention than in the group receiving a brief intervention. Although this reduction was not statistically significant (p=0.07) and was based on four trials, only one of which reported adequate concealment of allocation, it was robust to the conservative assumption that participants lost to follow-up in the trial of Aalto 2000 had no reduction in alcohol consumption. The one good quality trial (Maisto 2001) showed a similar, but less marked reduction in alcohol consumption in the extended intervention group The benefit of extended intervention compared to brief intervention was also apparent in a significant reduction in the frequency of drinking in the one trial (Aalto 2000). Hence there is some weak evidence that a greater length of time spent counselling patients may result in a greater reduction in alcohol consumption, but this is of the order of a possible reduction of one standard drink or less per week for 10 minutes extra counselling. Thus, given the weak relationship between duration of counselling and outcome, the content and structure of brief interventions may be more influential than the total time of intervention delivery.

Gender

Only eight trials reported sufficient information to analyse the outcomes by gender, of which only five included women: in these trials, brief intervention reduced the quantity of alcohol consumed per week by men, but not by women. This result of no significant benefit of brief intervention among women is in contrast to the previous meta-analytic findings of Ballasteros 2004a, largely because of inclusion of different trials. We included the trial of Aalto 2000, whereas Ballasteros 2004a did not: we are unclear why it was excluded. Ballasteros 2004a included the trials of Fleming 1997 and Ockene 1999, which we excluded as they did not report the number of men and women assessed.

Location

Of the 29 trials identified by this review, 19 (66%) were based in English-speaking countries (USA, UK, Canada, Australia), six (21%) in continental Europe, and four (14%) in Scandinavia. No studies were based in transitional or developing countries.

Screening

It was clear in this review (as in other work) that some studies reported reductions in consumption even in control arms. It is difficult to assess if this might be due to regression to the mean or an impact of screening. McIntosh (1997) reported that a significant proportion of patients reduced their drinking between screening and assessment, thus the brief intervention was delivered to some patients who were no longer eligible for such an approach. Thus it is possible that screening alone might represent an impetus for change. This should be investigated in future work.

Strengths and weaknesses

Empirical research has shown that failure to conceal from participants and from treatment providers the allocation of participants to treatment groups is often related to over-estimation of the treatment effect (Moher 1998; Schulz 1995), and that trials

where the participant and treatment provider are not blinded may be more likely to report significant effects of the intervention (Schulz 1995). Although the 21 trials in our review were of variable quality, ten reported adequate concealment of allocation and seven of these also reported adequate blinding. However, as sensitivity analysis restricted to trials which reported adequate concealment of allocation showed a significant benefit of brief intervention similar to that found in the primary meta-analysis, but with less heterogeneity between trials, poor quality trials are unlikely to have introduced much bias in our meta-analysis.

The most likely source of bias is loss to follow-up, which was about 27% overall and significantly higher in the brief intervention arm than the control arm (difference in rates of 3%, 95%CI: 1% to 6%). If participants who dropped out of the brief intervention groups had higher alcohol consumption than those who did not, our estimated reduction in alcohol consumption due to brief intervention would be an over-estimate of the real effect. Nevertheless, the estimated reduction in the quantity of alcohol consumed per week was sufficiently marked that the real effect is likely to be a reduction in alcohol consumption. Furthermore, the random effects model which we used assumes that the effect of treatment is different in different populations and that the estimated reduction in alcohol consumption of 43 grams per week is the mean treatment effect, averaged over all populations. Therefore, the findings provide strong evidence that brief interventions are effective in many populations.

Individual trials had specific weaknesses. Two trials (Chang 1997; Tomson 1998) did not report standard deviations, so these were imputed in a sensitivity analysis; three trials (Longabaugh 2001; McIntosh 1997; Rodriguez 2003) did not report the number of participants assessed by treatment arm so could not be included in any meta-analysis; two cluster randomised trials (Cordoba 1998; Huas 2002) did not allow correctly for the cluster randomisation so their standard deviations were imputed. Two trials (Fernandez 1997; Rodriguez 2003) used "block randomisation". While it is unclear exactly what was meant by this term, this was probably a form of cluster randomisation. However, we were unable to allow for this apparent cluster randomisation as insufficient detail (number of clusters, average number of participants per cluster) was reported. Therefore these trials probably have standard deviations for the treatment effects which are too low and weights which are too high. Nevertheless, this would make little difference to the overall conclusions, as only one of these trials (Fernandez 1997) was included in the primary meta-analysis.

Comparison with other meta-analyses

There are four previous meta-analyses which are directly relevant to this review (Ballesteros 2004; Bertholet 2005; Moyer 2002; Poikolainen 1999). Three of these reviews focused specifically on primary care (Ballesteros 2004; Bertholet 2005; Poikolainen 1999) and these identified 19 trials, 13 trials and 7 trials respectively. The fourth review was the most comprehensive, identifying 56 controlled trials of brief alcohol intervention in total (Moyer 2002). However, these trials incorporated both subjects who were seeking treatment for alcohol problems which is typical of secondary care and specialist alcohol work (n=22 trials) and non-treatment seekers (n=34 trials) which would be more typical of subjects in primary care. However, the non-treatment seeking individuals in the trials identified by Moyer 2002 also came from social care, occupational health settings, hospitals and educational contexts. Moyer 2002 identified 20 trials based in primary care settings. However, in some of these trials, data relating to primary care could not be separated from data provided by other health and social care professionals (as was the case in a large WHO multinational collaborative study 1996 which was reported as 7 separate trials) and so these trials were excluded from our review.

Bertholet 2005 was the most directly comparable meta-analysis, being most recent and with a specific focus on primary care. This review identified 19 relevant trials, of which 9 individual trials were included in their meta-analysis. (There were 12 lines in their forest plot, but 3 trials contributed two lines each as single and multiple session interventions were considered separately). There were several reasons for the differences between this previous meta-analysis and the current one. The search strategy in Bertholet 2005 went up to 2003 whilst we included 2006. Hence we found more recent trials (n=4). In addition, Bertholet 2005 focused only on general practice-based studies whilst our definition of primary care included Accident and Emergency trials (n=4, two of which were published after 2003). Bertholet 2005 included two of the local country studies from the WHO collaborative trial (see above) which were not in our review. Lastly, Bertholet 2005 excluded three trials which we included (Romelsjo 1989; Scott 1991; Wallace 1988) on the basis that screening involved a postal questionnaire survey of lifestyle issues which was not considered representative of routine primary care. However, the patients in these trials were genuine practice patients who were not seeking alcohol treatment and who were unaware of alcohol-related risk or harm. Moreover, we felt that our efficacy/effectiveness classification would address the additional work by practices done in the postal screening stage; hence we included these trials (as did Ballesteros 2004; Moyer 2002; Poikolainen 1999).

Previous meta-analyses reported male and female outcomes from a single trial (judged by the trial protocol) as separate studies (Ballesteros 2004; Bertholet 2005; Moyer 2002; Poikolainen 1999) whilst we have combined them into a single report. Treating findings from male and female participants in the same trial as the results of independent trials gives slightly more weight to the trial than it should be accorded, but is unlikely to lead to any major difference in the overall pooled findings of the meta-analysis.

Bertholet 2005 and Ballesteros 2004 reported an intention-to-treat analysis which imputed zero change to all subjects lost to follow-up. In addition, Moyer 2002 noted that if study results

were only described as, or inferred to be 'non-significant' an effect size of zero was assigned; this related to 13% studies. These interpretations of intention-to-treat analysis are conservative and our analysis, following recommendations in the Cochrane Handbook (Deeks 2006b) attempted to analyse all participants in the groups to which they were randomised but included imputed outcomes only in sensitivity analyses.

Nevertheless, our meta-analysis has yielded broadly similar results to previous work. Bertholet 2005 used a random effects model to analyse alcohol consumption and reported an adjusted ITT analysis showing a mean pooled difference of -38g alcohol per week (95%CI -51 to -24g/week), equating to approximately four fewer drinks per week. The narrower confidence intervals in this report compare to our work is likely to reflect a more conservative analytical approach. Six years earlier, Poikolainen 1999 had reported that multi-session brief interventions produced a pooled effect estimate of change in alcohol consumption of -51g (95% CI -74 to -29g/week). At least some of the change from 1999 to 2005 may reflect the fact that earlier trials of brief alcohol intervention tended to focus on heavier (or harmful) drinkers whilst more recent work has included less heavy or hazardous drinkers with a reduced range for consumption to fall within recommended sensible drinking limits (Ballesteros 2004).

AUTHORS' CONCLUSIONS

Implications for practice

Our data indicate that brief alcohol intervention in primary care contexts results in significant reductions in weekly consumption for men, with an average drop of about 6 standard drinks per week in patients compared to controls. The review showed no significant reduction in alcohol consumption for women; although this may be partly due to low statistical power (as trials reporting outcomes from women only enrolled 499 participants), brief interventions for women are not yet justified.

In the field of brief alcohol intervention, there has been a growing view that most of the trials to date have been tightly controlled efficacy studies and not particularly representative of routine clinical practice (Babor 2006). One could argue that any trial context can never be a true analogue for clinical practice. However, randomised controlled trials remain the gold standard for evaluating the outcomes of psychosocial or pharmacological interventions in health care. Thus, within the context of trial-based evaluation, we feel that the current body of brief alcohol intervention research is applicable to clinical practice. Previous trials have fallen on a continuum from efficacy to effectiveness trials, and the lack of significant difference in outcomes on this dimension suggest that this body of work can inform routine practice.

Implications for research

There is a clear need for more evaluative research on brief interventions with women, younger people and those from cultural minority groups. In addition there is a need for more research in transitional and developing countries. However, given the large number of trials of brief alcohol intervention showing a positive impact in men, there is no need for more of the same before such interventions are delivered in primary care. Longer treatment appeared to have little effect in significantly improving outcomes. Moreover, there is some suggestion that screening alone may result in alcohol consumption reduction, and this should be investigated further. Finally, future research direction should focus on implementation issues including a more precise specification of brief intervention components.

ACKNOWLEDGEMENTS

We thank Dr Marta Roque of the Iberian Cochrane Centre for assistance with data abstraction. We thank Dr Jean-Bernard Daeppen of the Centre de Traitement en Alcoologie, Lausanne, and Dr Nicolas Bertholet of the Service de Psychiatrie de Liaison, Lausanne, for comments on the review. We thank Dr. Sue Curry of the University of Illinois for supplying unpublished data.

REFERENCES

References to studies included in this review

Aalto 2000 *{published data only}*

Aalto M, Mattila P, Mustonen H, Ruuth KH, Pulkkinen H, Alho H, et al.Brief intervention for male heavy drinkers in routine general practice: A three-year randomized controlled study. *Alcohol & Alcoholism* 2001;**36**(3):224–30.

* Aalto M, Saksanen R, Laine P, Forsstrom R, Raikaa M, Kiviluoto M, et al.Brief intervention for female heavy drinkers in routine general practice: a 3-year randomized, controlled study. *Alcoholism: Clinical and Experimental Research* 2000;**24**(11):1680–6.

Altisent 1997 *{published data only}*

Altisent R, Cordoba R, Delgado MT, Pico MV, Melus E, Aranguren F. Multicenter study on the efficacy of advice for the prevention of alcoholism in primary health care. *Medicina Clinica* 1997;**109**(4):121–4.

Chang 1997 *{published data only}*

Chang G, Behr H, Goetz M, Hiley A, Bigby J. Women and alcohol abuse in primary care: Identification and intervention. *American Journal on Addictions* 1997;**6**(3):183–92.

Cordoba 1998 *{published data only}*

Cordoba R, Delgado MT, Pico V, Altisent R, Fores D, Monreal A. Effectiveness of brief intervention on non-dependent alcohol drinkers (EBIAL): a Spanish multi-centre study. *Family Practice* 1998;**15**(6):562–8.

Crawford 2004 *{published data only}*

Crawford MJ, Patton R, Touquet R, Drummond C, Byford S, Barrett B, et al.Screening and referral for brief intervention of alcohol-misusing patients in an emergency department: a pragmatic randomised controlled trial. *Lancet* 2004;**364**(9442):1334–9.

Curry 2003 *{published data only}*

Curry SJ, Ludman EJ, Grothaus LC, Donovan D, Kim E. A randomized trial of a brief primary-care-based intervention for reducing at-risk drinking practices. Health Psychology 2003; Vol. 22, issue 2:156–65.

Diez 2002 *{published data only}*

Diez JF, Pena C, Garcia E, Gaite L. Brief intervention in Cantabria (Spain) in alcohol related problems [Intervencion breve en Cantabria en problemas relacionados con el alcohol]. *Adicciones* 2002;**14**(1):13–24.

Fernandez 1997 *{published data only}*

Fernandez San Martin MI, Bermejo Caja CJ, Alonso PM, Herreros B, Nieto M, Novoa A, et al.Effectiveness of brief medical counseling to reduce drinkers' alcohol consumption. *Atencion Primaria* 1997;**19**(3):127–32.

Fleming 1997 *{published data only}*

* Fleming MF, Barry KL, Manwell LB, Johnson K, London R. Brief physician advice for problem alcohol drinkers: a randomized controlled trial in community-based primary care practices. *JAMA* 1997;**277**(13):1039–45.

Fleming MF, Mundt MP, French MT, Manwell LB, Stauffacher EA, Barry KL. Benefit-cost analysis of brief physician advice with problem drinkers in primary care settings. *Medical Care* 2000;**38**(1):7–18.

Fleming MF, Mundt MP, French MT, Manwell LB, Stauffacher EA, Barry KL. Brief physician advice for problem drinkers: long-term efficacy and benefit-cost analysis. *Alcoholism: Clinical and Experimental Research* 2002;**26**(1):36–43.

Grossberg PM, Brown DD, Fleming MF. Brief physician advice for high-risk drinking among young adults. *Annals of Family Medicine* 2004;**2**(5):474–80.

Manwell LB, Fleming MF, Mundt MP, Stauffacher EA, Barry KW. Treatment of problem alcohol use in women of childbearing age: results of a brief intervention trial. *Alcoholism: Clinical and Experimental Research* 2000;**24**(10):1517–24.

Fleming 1999 *{published data only}*

Fleming MF, Manwell LB, Barry KL, Adams W, Stauffacher EA. Brief physician advice for alcohol problems in older adults: a randomized community-based trial. *Journal of Family Practice* 1999;**48**(5):378–84.

Fleming 2004 *{published data only}*

Fleming M, Brown R, Brown D. The efficacy of a brief alcohol intervention combined with %CDT feedback in patients being treated for type 2 diabetes and/or hypertension. *Journal of Studies on Alcohol* 2004;**65**(5):631–7.

Gentillelo 1999 *{published data only}*

Gentillelo LM, Rivara FP, Donovan DM, Jurkovich GJK, Daranciang E, Dunn CW, et al.Alcohol interventions in a trauma center as a means of reducing the risk of injury recurrence. *Annals of Surgery* 1999;**230**(4):473–83.

Heather 1987 *{published data only}*

Heather N, Campion PD, Neville RG, Maccabe D. Evaluation of a controlled drinking minimal intervention for problem drinkers in general practice (the DRAMS scheme). *Journal of the Royal College of General Practitioners* 1987;**37**(301):358–63.

Huas 2002 *{published data only}*

Huas D, Pessione F, Bouix J, Demeaux J, Alemand H, Rueff B. Efficacy of a brief intervention for problematic alcohol users [Efficacite a un an d'une intervention breve aupres des consommateurs d'alcool a problemes]. *Rev Pract Med Gen* 2002;**16**:1343–8.

Israel 1996 *{published data only}*

Israel Y, Hollander O, Sanchez-Craig M, Booker S. Screening for problem drinking and counseling by the primary care physician-nurse team. *Alcoholism: Clinical & Experimental Research* 1996;**20**(8):1443–50.

Kunz 2004 *{published data only}*

Kunz MF, French MT, Bazargan-Hejazi S. Cost-effectiveness analysis of a brief intervention delivered to problem drinkers presenting at an inner-city hospital emergency department. *Journal of Studies on Alcohol* 2004;**65**:363–70.

Lock 2006 *{published data only}*

Lock CA, Kaner E, Heather N, Doughty J, Crawshaw A, McNamee P, et al.Effectiveness of nurse-led brief alcohol intervention: a cluster randomized controlled trial. *Journal of Advanced Nursing* 2006;**54**(4):426–39.

Longabaugh 2001 *{published data only}*

Longabaugh R, Woolard RF, Nirenberg TD, Minugh AP, Becker B, Clifford PR, et al.Evaluating the effects of a brief motivational intervention for injured drinkers in the emergency department. *Journal of Studies on Alcohol* 2001;**62**:806–16.

Maisto 2001 *{published data only}*

Gordon AJ, Conigliaro J, Maisto SA, McNeil M, Kraemer KL, Kelley ME. Comparison of consumption effects of brief interventions for hazardous drinking elderly. *Substance Use & Misuse* 2003;**38**(8):1017–35.

* Maisto SA, Conigliaro J, McNeil M, Kraemer K, Conigliaro RL, Kelley ME. Effects of two types of brief intervention and readiness to change on alcohol use in hazadous drinkers. *Journal of Studies on Alcohol* 2001;**62**(5):605–14.

McIntosh 1997 *{published data only}*

McIntosh MC, Leigh G, Baldwin NJ, Marmulak J. Reducing alcohol consumption. Comparing three brief methods in family practice. *Canadian Family Physician* 1997;**43**:1959–62.

Ockene 1999 *{published data only}*

* Ockene JK, Adams A, Hurley TG, Wheeler EV, Hebert JR. Brief physician- and nurse practitioner-delivered counseling for high-risk drinkers: does it work?. *Archives of Internal Medicine* 1999;**159**(18):2198–2205.

Reiff-Hekking S, Ockene JK, Hurley TGRGW. Brief physician and nurse practitioner-delivered counseling for high-risk drinking.

Results at 12 months follow-up. *Journal of General Internal Medicine* 2005;**20**(1):7–13.

Richmond 1995 *{published data only}*
Richmond R, Heather N, Wodak A, Kehoe L, Webster I. Controlled evaluation of a general practice-based brief intervention for excessive drinking. *Addiction* 1995;**90**(1):119–32.

Rodriguez 2003 *{published data only}*
Rodriguez A, Santa-marina E, Martinez X, Torralba L, Escayola M, Marti J, et al.Early identification and brief intervention in alcohol-related traffic casualties: preliminary results [Identificación precoz e intervención breve en lesionados de trafico con presencia d alcohol: primeros resultados]. *Adicciones* 2003;**15**(3):191–202.
Rodriguez-Martos A, Santamarina E, Torralba L, Martinez X, Escayola M, Marti J, et al.Short-term effectiveness of brief interventions in alcohol-positive traffic casualites [Efectividad a corto plazo de las interventiones breves realizadas en pacientes lesionados por accidente de trafico con alcoholemia positiva]. *Gaceta Sanitaria* 2005;**19**:45–9.
* Rodriguez-Martos Dauer A, Santamarina Rubio E, Escayola Coris M, Marti Valls J. Brief intervention in alcohol-positive traffic casualities: is it worth the effort?. *Alcohol and Alcoholism* 2006;**41**(1):76–83.

Romelsjo 1989 *{published data only}*
Romelsjo A, Andersson L, Barrner H, Borg S, Granstrand C, Hultman O, et al.A randomized study of secondary prevention of early stage problem drinkers in primary health care. *British Journal of Addiction* 1989;**84**(11):1319–27.

Scott 1991 *{published data only}*
Anderson P, Scott E. The effect of general practitioners' advice to heavy drinking men. *British Journal of Addiction* 1992;**87**(6):891–900.
* Scott E, Anderson P. Randomized controlled trial of general practitioner intervention in women with excessive alcohol consumption. *Drug & Alcohol Review* 1991;**10**(4):313–21.

Senft 1997 *{published data only}*
Senft RA, Polen MR, Freeborn DK, Hollis JF. Brief intervention in a primary care setting for hazardous drinkers. *American Journal of Preventive Medicine* 1997;**13**(6):464–70.

Seppa 1992 *{published data only}*
Seppa K. Intervention in alcohol abuse among macrocytic patients in general practice. *Scandinavian Journal of Primary Health Care* 1992;**10**(3):217–22.

Tomson 1998 *{published data only}*
Tomson Y, Romelsjo A, Aberg H. Excessive drinking - Brief intervention by a primary health care nurse: A randomized controlled trial. *Scandinavian Journal of Primary Health Care* 1998;**16**(3):188–92.

Wallace 1988 *{published data only}*
Wallace P, Cutler S, Haines A. Randomised controlled trial of general practitioner intervention in patients with excessive alcohol consumption. *British Medical Journal* 1988;**297**(6649):663–8.

References to studies excluded from this review

Andreasson 2002 *{published data only}*
Andreasson S, Hansagi H, Osterlund B. Short-term treatment for alcohol-related problems: four-session guided self-change versus one session of advice: a randomized, controlled trial. *Alcohol: an International Biomedical Journal* 2002;**28**(1):57–62.

Bernstein 1997 *{published data only}*
Bernstein E, Bernstein J, Levenson S. Project ASSERT: an ED-based intervention to increase access to primary care, preventive services, and the substance abuse treatment system. *Annals of Emergency Medicine* 1997;**30**(2):181–9.

Blow 2001 *{published data only}*
Blow F. The effectiveness of an elder-specific brief alcohol intervention for older at-risk drinkers. 23rd-26th February, San Francisco, CA, USA. 2001.

Burge 1997 *{published data only}*
Burge SK, Amodei N, Elkin B, Catala S, Andrew S, Lane PA, et al.An evaluation of two primary care interventions for alcohol abuse among Mexican-American patients. *Addiction* 1997;**92**(12):1705–16.

Chang 1999 *{published data only}*
Chang G, Wilkins-Haug L, Berman S, Goetz M. Brief intervention for alcohol use in pregnancy: A randomized trial. *Addiction* 1999;**94**(10):1499–1508.

Copeland 2003 *{published data only}*
Copeland LA, Blow FC, Barry K. Health care utilization by older alcohol-using veterans: effects of a brief intervention to reduce at-risk drinking. *Health Education & Behavior* 2003;**30**(3):305–21.

Coulter 1995 *{published data only}*
Coulter A, Fowler G, Fuller A, Jones L, Lancaster T, Lawrence M, et al.Effectiveness of health checks conducted by nurses inprimary care: Final results of the OXCHECK study. *British Medical Journal* 1995;**310**:1099–1104.

Dimeff 2000 *{published data only}*
Dimeff LA, McNeely M. Computer-enhanced primary care practitioner advice for high-risk college drinkers in a student primary health-care setting. *Cognitive & Behavioral Practice* 2000;**7**(1):82–100.

Drummond 1990 *{published data only}*
Drummond DC, Thom B, Brown C, Edwards G, Mullan MJ. Specialist versus general practitioner treatment of problem drinkers. *Lancet* 1990;**336**(8720):915–8.

Fernandez 2003 *{published data only}*
Fernandez Moral JA, Ruiz Moral R, Perula de Torres LA, Campos Sanchez L, Lora Cerezo N, Martinez de la Iglesia J. Effectiveness of medical counseling for alcoholic patients and patients with excessive alcohol consumption seen in primary care. *Atencion Primaria* 2003;**31**(3):146–52.

Freeborn 2000 *{published data only}*
Freeborn DK, Polen MR, Hollis JF, Senft RA. Screening and brief intervention for hazardous drinking in an HMO: effects on medical care utilization. *Journal of Behavioral Health Services & Research* 2000;**27**(4):446–53.

Ivanets 1991 *{published data only}*
Ivanets NN, Lukomskaya MI. Evaluation of early intervention strategies used in primary health care: a report on the World Health Organization (WHO) Project on Identification and management of alcohol-related problems in primary health care. *Alcohol & Alcoholism* 1991;**Supplement. 1**:489–91.

Kelly 1988 *{published data only}*
Kelly RB. Controlled trial of a time-efficient method of health promotion. *American Journal of Preventive Medicine* 1988;**4**(4): 200–7.

Kristenson 1983 *{published data only}*
Kristenson H, Ohlin H, Hulten-Nosslin MB, Trell E, Hood B. Identification and intervention of heavy drinking in middle-aged men: results and follow-up of 24-60 months of long-term study with randomized controls. *Alcoholism: Clinical & Experimental Research* 1983;**Spring**:203–9.

Kristenson 2002 *{published data only}*
Kristenson H, Āsterling A, Nilsson JÃ, LindgÃrde F. Prevention of alcohol-related deaths in middle-aged heavy drinkers. *Alcoholism: Clinical and Experimental Research* 2002;**26**(4):478–84.

Logsdon 1989 *{published data only}*
Logsdon DN, Lazaro CM, Meier RV. The feasibility of behavioral risk reduction in primary medical care. *American Journal of Preventive Medicine* 1989;**5**(5):249–56.

Maheswaran 1992 *{published data only}*
Maheswaran R, Beevers M, Beevers DG. Effectiveness of advice to reduce alcohol consumption in hypertensive patients. *Hypertension* 1992;**19**(1):79–84.

Monti 1999 *{published data only}*
Monti PM, Spirito A, Myers MCSM, Barnett NP, Pohsenow DJ, Woolard RLW. Brief intervention for harm reduction with alcohol positive older adolescents in a hospital emergency department. *Journal of Consulting and Clinical Psychology* 1999;**67**(6):989–94.

Nilssen 1991 *{published data only}*
Nilssen O. The Tromso study: identification of a controlled intervention on a population of early stage risk drinkers. *Preventive Medicine* 1991;**20**(4):518–28.

Oliansky 1997 *{published data only}*
Oliansky DM, Wildenhaus KJ, Manlove K, Arnold T, Schoener EP. Effectiveness of brief interventions in reducing substance use among at-risk primary care patients in three community-based clinics. *Substance Abuse* 1997;**18**(3):95–103.

Persson 1989 *{published data only}*
Persson J, Magnussen P. Early intervention in patients with excessive consumption of alcohol: A controlled study. *Alcohol* 1989;**6**(5):403–8.

Richmond 2000 *{published data only}*
Richmond R, Kehoe L, Heather N, Wodak A. Evaluation of a workplace brief intervention for excessive alcohol consumption: the workscreen project. *Preventive Medicine* 2000;**30**(1):51–63.

Saitz 2003 *{published data only}*
Saitz R, Horton NJ, Sullivan LM, Moskowitz MA, Samet JH. Addressing alcohol problems in primary care: a cluster randomized, controlled trial of a systems intervention: the Screening and Intervention in Primary Care (SIP) Study. *Annals of Internal Medicine* 2003;**138**(5):372–82.

Smith 2003 *{published data only}*
Smith AJ, Hodgson RJ, Bridgman K, Sherheard JP. A randomized controlled trial of a brief intervention after alcohol-related facial injury. *Addiction* 2003;**98**(1):43–52.

Vinson 2000 *{published data only}*
Vinson DC, Devera-Sales A. Computer-generated written behavioral contracts with problem drinkers in primary medical care. *Substance Abuse* 2000;**21**(4):215–22.

WHO 1996 *{published data only}*
WHO Brief Intervention Study Group. A cross-national trial of brief interventions with heavy drinkers. *American Journal of Public Health* 1996;**86**(7):948–55.

Woollard 1995 *{published data only}*
Woollard J, Beilin L, Lord T, Puddey I, MacAdam D, Rouse I. A controlled trial of nurse counselling on lifestyle change for hypertensives treated in general practice: preliminary results. *Clinical & Experimental Pharmacology & Physiology* 1995;**22**(6-7): 466–8.

Wutzke 2002 *{published data only}*
Wutzke S, Conigrave K, Saunders J, Hall W. The long-term effectiveness of brief interventions for unsafe alcohol consumption: a 10-year follow-up. *Addiction* 2002;**97**(6):665–75.

Additional references

Agosti 1995
Agosti V. The efficacy of treatments in reducing alcohol consumption: a meta-analysis. *International Journal of the Addictions* 1995;**30**(8):1067–77.

Alcohol Concern 2000
Alcohol Concern. *Britain's Ruin: Meeting Government objectives via a national alcohol strategy*. London: Alcohol Concern, 2000.

Anderson 1991
Anderson P. Alcohol as a key area. *British Medical Journal* 1991;**303** (6805):766–9.

Anderson 1996
Anderson P. *Alcohol and primary health care*. no. 64. Copenhagen: WHO Regional Publications, 1996.

Babor 1994
Babor TE. Avoiding the horrid and beastly sin of drunkenness: does dissuasion make a difference?. *Journal of Consulting and Clinical Psychology* 1994;**62**(6):1127–40.

Babor 2006
Babor T, Higgins-Biddle JC, Dauser D, Burleson JA, Zarkin GA, Bray J. Brief interventions for at-risk drinking: patient outcomes and cost-effectiveness in managed care organizations. *Alcohol and Alcoholism* 2006;**41**(6):624–31.

Ballasteros 2004a
Ballesteros J, Gonzalez-Pinto A, Querejeta I, Arino J. Brief interventions for hazardous drinkers delivered in primary care are equally effective in men and women primary care are equally effective in men and women. *Addiction* 2004;**99**(1):103–8.

Ballesteros 2004
Ballesteros J, Duffy JC, Querejeta I, Arino J, Gonzales-Pinto A. Efficacy of brief interventions for hazardous drinkers in primary care: systematic review and meta-analysis. *Alcoholism: Clinical and Experimental Research* 2004;**28**(4):608–18.

Bertholet 2005
Bertholet N, Daeppen JB, Wietlisbach V, Fleming M, Burnand B. Reduction of alcohol consumption by brief intervention in primary

care: systematic review and meta-analysis. *Archives of Internal Medicine* 2005;**165**(9):986–95.

Bien 1993
Bien TH, Miller WR, Tonigan JS. Brief interventions for alcohol problems: a review. *Addiction* 1993;**90**(8):315–35.

Deeks 2006a
Deeks JJ, Higgins JPT, Altman DG, editors. Approximate analyses of cluster-randomized trials for a meta-analysis: Inflating standard errors. *Cochrane Handbook for Systematic Reviews of Interventions 4.2.6*. Chichester, UK: Jhon Wiley & Sons, Ltd, 2006.

Deeks 2006b
Deeks JJ, Higgins, JPT, Altman DG, editors. Analysing and presenting results ; Section 8.4.1 What are intention-to-treat analyses?. *Cochrane Handbook for Systematic Reviews of Interventions [updated September 2006]*. Chichester, UK: Jhon Wiley & Sons, Ltd, 2005.

Deeks, 2001
Deeks JJ, Altman DG, Bradburn MJ. Chapter 15. Statistical methods for examining heterogeneity and combining results from several studeis in meta-analysis. *Systematic reviews in health care*. Second Edition. London: BMJ books, 2001.

DerSimonian 1986
DerSimonian R, Laird N. Meta-analysis in clinical trials. *Controlled Clinical Trials* 1986;**7**(3):177–88.

DoH 1992
Department of Health. *The Health of the Nation: A summary of the strategy for Health in England*. HMSO, 1992.

Edwards 1997
Edwards AGK, Rollnick S. Outcome studies of brief alcohol intervention in general practice: the problem of lost subjects. *Addiction* 1997;**92**(12):1699–1704.

Flay 1986
Flay BR. Efficacy and effectiveness trials (and other phases of research) in the development of health promotion programs. *Preventive Medicine* 1986;**15**(5):451–74.

Gual 1999
Gual A, Rodriguez Martos A, Lligona A, Llopis J. Does the concept of a standard drink apply to viticultural societies?. *Alcohol & Alcoholism* 1999;**34**(2):153–60.

Heather 1995
Heather N. Interpreting the evidence on brief interventions for excessive drinkers: the need for caution. *Alcohol & Alcoholism* 1995; **30**(3):287–96.

Heather 2006
Heather N. WHO collaborative project on identification & management of alcohol-related problems in primary health care. Report on Phase IV: development of country-wide strategies for implementing early identification and brief intervention in primary health care. http://digbig.com/4meej 2006:133.

Higgins 2002
Higgins JPT, Thompson SG. Quantifying heterogeneity in a meta-analysis. *Statistics in Medicine* 2002;**21**(11):1539–58.

Higgins 2003
Higgins JPT, Thompson SG, Deeks JJ, Altman DG. Measuring inconsistency in meta-analyses. *British Medical Journal* 2003;**327** (7414):557–60.

Holder 1999
Holder H, Flay B, Howard J, Boyd G, Voas R, Grossman M. Phases of alcohol problem prevention research. *Alcoholic, Clinical and Experimental Research* 1999;**23**(1):183–94.

Kaner 2001
Kaner E, Heather N, Brodie J, Lock, McAvoy B. Patient and practitioner characteristics predict brief alcohol intervention in primary care. *British Journal of General Practice* 2001;**51**(471): 822–7.

Matthews 2000
Matthews JNS. *An introduction to randomized controlled clincial trials*. London: Arnold, 2000.

Miller 1991
Miller WR, Heather N, Hall W. Calculating standard drink units: international comparisons. *British Journal of Addiction* 1991;**86**(1): 43–7.

Moher 1998
Moher D, Pham B, Jones A, et al.Does quality of reports of randomsied trials affect est8imates of intervention efficacy reported in meta-analyses?. *Lancet* 1998;**352**(9128):609–13.

Moyer 2002
Moyer A, Finney JW, Swearingen CE, Vergun P. Brief interventions for alcohol problems: a meta-analystic review of controlled investigations in treatment and non-treatment-seeking populations. *Addiction* 2002;**97**(3):279–92.

NHS CRD 1993
University of York. NHS Centre for Reviews and Dissemination. Brief interventions and alcohol use. *Effective Health Care* 1993;**1** (7):13.

Poikolainen 1999
Poikolainen K. Effectiveness of brief interventions to reduce alcohol intake in primary health care populations: a meta-analysis. *Preventive Medicine* 1999;**28**(5):503–9.

Schulz 1995
Schulz KF, Chalmers I, Hayes RJ, Altman D. Empirical evdience of bias. Dimensions of methodological quality associated with estimates of treatment effects in controled trials. *JAMA* 1995;**273** (5):408–12.

Shadish 2000
Shadish WD, Navarro AM, Matt GE, Philips G. The effects of psychological therapies under clinically representative conditions: a meta-analysis. *Psychological Bulletin* 2000;**126**(4):512–29.

WHO 1993
World Health Organization. *European Alcohol Action Plan*. WHO Regional Office for Europe, 1993.

WHO 1999
World Health Organization. *Global Status Report on Alcohol*. Geneva: World Health Organization, 1999.

Wilk 1997

Wilk A, Jensen NM, Havighurst TC. Meta-analysis of randomized control trials addressing brief interventions in heavy alcohol drinkers. *Journal of General Internal Medicine* 1997;**12**(5):274–83.

* *Indicates the major publication for the study*

CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Aalto 2000

Methods	Randomisation: adequate Blinding of outcome assessor - unclear ITT: yes. ITT outcome data were based on imputation of baseline values to participants lost to follow-up.
Participants	Finland. Participants 20-60 yrs consuming => 280 g absolute ethanol/wk or CAGE=>3 for men, => 190 g absolute ethanol/wk or CAGE=>2 for women; excluded if severe psychiatric disease, or at least one detox treatment, or alcohol dependence, or alcohol-related disease; recruited from primary care health clinics; screened by self-administered health questionnaire including CAGE and quantity-frequency consumption questions. Number randomised = 414; 71% male; mean age = 41.6 yrs; 18% comprehensive school, 7.3% vocational school, 12.7% college or university; 18.7% working/studying, 13.6% unemployed, 6% retired At baseline: mean drinking amount per week = 286 g for men, 165.5 g for women; mean drinking times per week = 2.2 for men, 2.1 for women; mean usual drinking amount per occasion = 139.2 g for men, 85.8 g for women; mean CAGE = 3.2 for men, 2.8 for women
Interventions	Group A (n=149) received brief intervention from GP or nurse at baseline, 2, 6, 12, 18, 24 and 30 months. Intervention was 10-20 mins based on FRAMES according to the needs of individual patients. Group B (n=137) received the same intervention less frequently: at baseline, 12 and 24 months Group C (n=128) received advice to reduce drinking and contact their GP in the event of health problems; were not told about 36 month follow-up.
Outcomes	Mean drinking amount per week; drinking times per week; usual drinking amount per occasion; CDT, ASAT, ALAT, GGT, MCV; all assessed at 36 months (blood tests additionally assessed at each brief intervention).
Notes	Lahti project. Loss to follow-up: Group A: 61/149 (41%) Group B: 55/137 (40%) Group C: 55/128 (43%)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	No	C - Inadequate

Altisent 1997

Methods	Randomisation: adequate Blinding of outcome assessor - unclear ITT: unclear
Participants	Spain. Participants 15-75 yrs with a weekly consumption of >280 g for men and >168 g for women; excluded if current treatment for alcohol problems or hepatologic problems or concomitant diseases requiring alcohol abstinence or MALT > 11; recruited from general practice; screened by MALT scale for alcohol dependence. Number randomised = 139; 100% male; mean age = 45 yrs. At baseline: mean weekly alcohol consumption = 57 units (1 unit = 8 g alcohol)
Interventions	Intervention group (n = 75) received 5 mins general advice from GP with support material plus a five-visit program over the year. NB 21 were subsequently excluded, leaving 54 having the intervention treatment. Control group (n = 64) received a single session of brief advice from GP. NB 19 were subsequently excluded, leaving 45 having the control treatment.
Outcomes	% reduction in alcohol consumption; MALT test; Goldberg score; % drinking <35 units/wk; all assessed at 12 months
Notes	Loss to follow-up: Intervention group: 20/54 (37%) Control group: 15/45 (33%)

Risk of bias

Item	**Authors' judgement**	**Description**
Allocation concealment?	Yes	A - Adequate

Chang 1997

Methods	Randomisation: adequate Blinding of outcome assessor - unclear ITT: unclear
Participants	US. Women >=18 yrs consuming > 9 drinks/wk, or >3 drinks per episode of drinking, and positive T-ACE; excluded if current (other) substance abuse, or suicidal tendencies or psychosis, or abstinence from alcohol for 1 month or more before enrollment, or current liver disease or pregnancy, or severe uncontrolled medical illness or organic brain syndromes, or inability to read English or complete study evaluations, or current alcohol or substance abuse treatment; recruited from general internal-medicine practice; screened with Health and Habits Survey Number randomised = 24; 0% male; mean age = 39.3 yrs; 45.9% black, 54.2% white; mean yrs of education = 12.9 At baseline: DSM-IV alcohol diagnoses (mean %): lifetime alcohol abuse = 8.5, lifetime alcohol dependence = 71.0, current alcohol abuse = 8.5, current alcohol dependence = 58.5; mean drinking days/wk = 2.9, mean drinks/drinking day = 4.2, mean drinks/wk = 10.3, mean binges (defined as >3 drinks in one occasion) per wk = 1.0

Chang 1997 (*Continued*)

Interventions	Brief intervention group (n=12) received a session with the study psychiatrist (duration not reported) consisting of a review of: sensible drinking limits, subject's general health, responses to quesionnaire, reasons to modify drinking. Subject recorded in a study booklet drinking goals, motivations, risk situations and alcohol alternatives Alcohol treatment referral group (n=12) received the referral from the research assistant
Outcomes	Mean drinking days/wk, mean drinks/drinking day, mean drinks/wk, mean binges, all assessed at 30 and 60 days.
Notes	Loss to follow-up (at 60 days): Brief intervention group: 1/12 (8%) Treatment referral group: 4/12 (33%)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Cordoba 1998

Methods	Randomisation: unclear. Primary care practices were cluster randomised Blinding of outcome assessor - unclear ITT: unclear
Participants	Spain. Men aged 14-50 yrs with either weekly alcohol consumption over 35 units, or over 10 units on any given day at least once a month (1 unit = 8 g); excluded if alcohol dependency or previous advice to reduce drinking or chronic pathology/treatment requiring >3 months abstinence or CAGE score>1; recruited from general practice; screened by lifestyle questionnaire with embedded CAGE. Number randomised 229; 100% male; mean age 36.5 yrs; 100% Hispanic; 70.1% married; 78.7% middle-lower or lower social status; 95.6% employed; 64.1% further education At baseline: mean weekly alcohol consumption = 54.0 units; CAGE = 1 for 63.2% of participants (data given for 229 heavy throughout-week drinkers only)
Interventions	Intervention group (n = 104 patients) received from the GP 15 mins cognitive-behavioural therapy consisting of a self-informative booklet including day diary for registration of consumption, individualised agreement of consumption targets, and offer of follow-up and support Control group (n = 125) received from the GP 5 min "simple advice" which reproduced usual care
Outcomes	% of participants cutting down to under 35 units/wk; assessed at 12 months
Notes	EBIAL trial Loss to follow-up not reported by arm Number randomised 546; number assessed = 229 patients in 33 centres, average clster size = 6.9.

Risk of bias

Cordoba 1998 (*Continued*)

Item	Authors' judgement	Description
Allocation concealment?	Yes	A - Adequate

Crawford 2004

Methods	Randomisation: adequate Blinding of outcome assessor - adequate ITT: yes
Participants	UK. Participants >=18 yrs, English speakers, resident in Greater London, alert & oriented, consuming >8 units in any one session at least once a week for men or >6 units for women, or believing their attendance at A&E is related to alcohol; excluded if already in contact with alcohol services, or requesting help with alcohol problems; recruited from A&E; screened by PAT Number randomised = 599; 78.1% male; mean age = 44 yrs (range 18-90 yrs) At baseline: mean units consumed during drinking session = 21.2
Interventions	Experimental group (n=287) received a health information leaflet containing contact details for national and local alcohol support agencies, and a 30 min session with an experienced alcohol worker to discuss current and previous drinking in a manner tailored to the subject. Control group (n=312) received the leaflet only
Outcomes	Mean weekly units consumed, mean units consumed per drinking day, mean proportion days abstinent all assessed at 6 and 12 months; mean number of attendances at local ED, mean EQ-5D single score both assessed at 12 months only; mean score on GHQ assessed at 6 months only
Notes	Loss to follow-up: Experimental group: 98/287 (34%) Control group: 117/312 (37.5%)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	No	C - Inadequate

Curry 2003

Methods	Randomisation: unclear Blinding of outcome assessor: unclear ITT: Paper reported multiple imputation to impute outcome data for non-respondents; unpublished data supplied to the reviewers was based on 222/333 (67%) of those randomised.
Participants	U.S. No age restrictions. People with AUDIT score <=15 and consuming: a) >= 2 alcoholic drinks/day in past month (chronic drinking), or b) >= 2 episodes of binge drinking (>= 5 drinks) in past week (binge drinkers) , or

Curry 2003 (*Continued*)

	c) >= 1 episode of driving after >=3 drinks. Excluded if alcoholic, pregnant, terminally ill, cogitively impaired. Participants recruited from pre-booked appointment lists of 23 primary care physicians in one urban primary care clinic, screened by telephone interviews based on 85 items. Number radnomised = 333; 65% male; mean age 46.9 yrs; 16% unemployed; 91% post-high school education; 68% income >$35,000/yr; 80% Caucasian. Number assessed = 222 (67%). At baseline: mean drinking amount = 166 gms/week; 42% chronic drinkers; 33% binge drinkers.
Interventions	Intervention: (n=166) a) Brief motivational message of 1-5 minutes from primary care physician during routine visit b) self-help manual c) written personalised feedback d) up to 3 telephone counselling calls over 10 weeks by psychology graduate Control: (n=167) Usual care
Outcomes	No. of drinks/week No. of drinking days/week No. of binges/week No. of gms alcohol/drinking day % binge drinkers % heavy drinkers (average of >1 drink/day for women or >2 drinks/day for men)
Notes	LFU I: 66/166 (40%) C: 45/167 (27%) Analyses of frequency and intensity of drinking are based on unpublished data on 222 cases. Analysis of quaitity of alcohol consumed/week are based on published means and unpublished SDs.

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Diez 2002

Methods	Randomisation: unclear Blinding of outcome assessor - unclear ITT: unclear
Participants	Spain. Men 18-65 yrs with a weekly alcohol consumption of 21-95 units (1 unit = 8 g); excluded if alcohol dependence (defined as alcohol consumption>95 units/wk) or psychiatric disorders; recruited from 1 of 4 settings: university hospital, urban general practice, rural general practice, industrial occupational health clinic; screened by an evaluation survey with drinking questions embedded with general health questions Number randomised = 1022; 100% male; mean age 42.4 yrs (of those evaluated not randomised) At baseline: mean weekly consumption = 47.1 units; % risk drinkers (>35 units/wk) = 62%

Diez 2002 (*Continued*)

Interventions	Intervention group (n =592) received the evaluation survey (10 mins) plus a self-help manual containing methods to evaluate their drinking and its effects on their lifestyle, and guidelines for consumption, with an extra 10 mins of advice and explanation of the manual. Control group (n = 430) received the evaluation survey only (10 mins), with no comment or advice.
Outcomes	Weekly alcohol consumption; % risk drinkers (>35 units/wk); all reported by setting at 6 months for the university hospital, urban general practice, and rural general practice, and 12 months for the industrial occupational health clinic
Notes	Loss to follow-up: Intervention group: 111/592 (19%) Control group: 84/430 (20%) Extracted data for C Urbano and C Rural groups only, others were not based in primary health care Loss to follow-up for these two groups: Intervention group: 49/255 (19%) Control group: 43/229 (19%) No specific alcohol-related baseline data except on table of results "preintervention" - this is what's quoted above

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Fernandez 1997

Methods	Randomisation: adequate Blinding of outcome assessor - adequate ITT: unclear
Participants	Spain. Men 18-64 yrs consuming >21 International Units per week; excluded if other drug consumption or psychiatric disorders or previous attendance at specialised alcohol dependence programs; recruited in general practice; screened by alcohol consumption questionnaires Number randomised = 152; 100% male; mean age = 40.3 yrs; employment: 73.5% employed, 15.9% unemployed, 8.6% retired, 2.0% studying; education: 1.3% higher, 17.8% standard, 78.9% lower At baseline: no alcohol risk data given only >=35 units per week
Interventions	Intervention group (n=67) received 10 minutes counselling backed up by didactic material Control group (n=85) received no intervention
Outcomes	Number of participants with weekly intake >=35 IU at 6-18 months; number of participants with weekly intake >=21 IU at 6-18 months
Notes	Loss to follow-up: Intervention group: 29/67 (43%) Control group: 35/85 (41%)

Fernandez 1997 (*Continued*)

Risk of bias		
Item	**Authors' judgement**	**Description**
Allocation concealment?	No	C - Inadequate

Fleming 1997

Methods	Randomisation: adequate Blinding of outcome assessor - adequate ITT: only in 1 subanalysis paper
Participants	US. Participants 18-65 yrs consuming >14 drinks/wk for men and >11 drinks/wk (1 drink = 8 g) for women; excluded if pregnancies, alcohol treatment in previous yr, advice in previous 3 months from GP to change alcohol use, consumption >50 drinks/wk, reported symptoms of suicide; recruited from GP clinic; screened by general health screening survey containing 4 sets of parallel questions on exercise, smoking, weight & alcohol use. Number randomised = 774; (following numbers are for those assessed): 62% male; 92% White, 1% Hispanic, 4% African American, 3% other; 41% high school or less, 40% some college, 20% college degree or more At baseline: mean consumption in previous 7 days = 19.0 drinks; no binge drinking episodes (defined as >5 drinks for men or >4 drinks for women on 1 occasion) in previous 30 days = 5.5; binge drinkers in previous 30 days = 78%; excessive drinkers (defined as >20 drinks/wk for men or >13 drinks/wk for women) in previous 7 days = 43%
Interventions	Intervention group (n=392) received two 15 min advice sessions 1 month apart from GP, and a workbook containing feedback regarding current health behaviours, review of prevalence of problem drinking, adverse effects of alcohol, worksheet on drinking cues, drinking agreement and diary cards (based on MRC trial). Participants received a follow-up telephone call from the clinic nurse 2 weeks after each meeting with GP. Control group (n=382) received a health booklet on general health issues only
Outcomes	Mean drinks in previous 7 days; binge drinking (defined as >5 drinks for men or >4 drinks for women on 1 occasion); excessive drinking (defined as >20 drinks/wk for men or >13 drinks/wk for women); assessed at 6 and 12 months (further paper gives 48 month data).
Notes	TrEAT trial. Loss to follow-up: Intervention group = 39/392 (10%) Control group = 12/382 (3%) Separate papers on 48 month data; cost-benefit analysis; subgp analysis of women of childbearing age; subgp analysis of young adults.

Risk of bias		
Item	**Authors' judgement**	**Description**

Fleming 1997 (*Continued*)

Allocation concealment?	Yes	A - Adequate

Fleming 1999

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: yes
Participants	US. Adults over 65 yrs consuming >11 drinks for men or >8 drinks (1 drink = 8 g) per week for women, or >=2 positive responses to CAGE, or binge drinking (defined as 4 or more drinks per occasion for men 2 or more times in the last 3 months or 3 or more drinks per occasion for women); excluded if attendance at an alcohol treatment programme or reported symptoms of alcohol withdrawal in the last year, or physician advice received in previous 3 months to change alcohol use, or consumption >50 units per week, or reported thoughts of suicide; recruited from community based primary health care clinics; screened by a modified version of the Health Screening Survey Number randomised = 158; 66% male; age range = 65-75 yrs; 75% married or co-habiting; 26% college educated At baseline: mean weekly alcohol consumption = 16.0 drinks; mean binge drinking episodes (defined as >4 drinks per occasion for men or 3 for women) in previous 30 days = 3.7; binge drinkers in previous 30 days = 44.9%; excessive drinkers (defined as >20 drinks per week for men and >13 for women) in previous 7 days = 29.7%
Interventions	Intervention group (n=87) received 10-15 min brief intervention plus workbook and 10-15 min follow-up reinforcement session, both from GP, plus a phone call from a nurse 2 weeks after each visit. Used same protocols as Medical Research Council trial and Project TrEAT. Control group (n=71) received a general health booklet only.
Outcomes	Number of drinks in previous 7 days; number of binge drinking episodes (defined as >4 drinks per occasion for men or 3 for women) in previous 30 days; % of participants binge drinking in previous 30 days; % of participants drinking excessively (defined as >20 drinks per week for men and >13 for women) in previous 7 days; all assessed at 3, 6 and 12 months.
Notes	GOAL trial. Loss to follow-up: Intervention group: 9/87 (9%) Control group: 4/71 (6%)

Risk of bias

Item	**Authors' judgement**	**Description**
Allocation concealment?	Yes	A - Adequate

Fleming 2004

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: yes
Participants	US. Participants 30-60 yrs on medication for diabetes or hypertension with CDT level>2.5%, or 50 or more drinks in the previous 30 days for men or 30 for women; excluded if current symptoms of alcohol withdrawal, or participation in alcohol treatment programme in previous 12 months; recruited from existing study of %CDT test in 8 primary care clinics; screened by telephone interview Number randomised = 151; 45% male; mean age = 48.7 yrs; 88% white, 8% black, 4% other; high school or less = 41%, college degree = 21%, technical degree = 18%, advanced degree = 19% At baseline: current alcohol abuse = 7.3%, current alcohol dependence = 9.9%, mean drinks in previous 30 days = 33.2, % heavy drinkers (defined as >= 30 drinks in previous 30 days for men or >=25 for women) = 39.1%, mean frequency of binge drinking (defined as >=5 drinks in one occasion for men or >=4 for women) in previous 30 days = 2.6
Interventions	Intervention group (n=81) received two 15 min sessions (a month apart) from nurse practitioners or physician assistants and two 5 min follow-up phone calls from the office nurse. Sessions (based on TrEAT) followed a scripted workbook reviewing prevalence of problem drinking, adverse effects of alcohol, %CDT test result, drinking diary cards and a drinking agreement in the form of a prescription. Control group (n=70) received a general health booklet and were told by researcher to contact physician with health concerns
Outcomes	Mean % of heavy drinkers, mean no drinks in previous 30 days, mean frequency of binge drinking, proportion of subjects who reduced %CDT, all assessed at 2, 4, and 12 months with change scores reported
Notes	Loss to follow-up at 12 months: Intervention group: 11/92 (12%) Control group: 5/75 (7%) Missing data imputed by authors for proportion of subjects reduced %CDT Quantity data is reported as drinks in previous 7 days (not average weekly intake over previous x months)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Gentillelo 1999

Methods	Randomisation: adequate Blinding of outcome assessor - adequate ITT: yes
Participants	US. Participants >=18 yrs who were admitted to trauma centre for treatment of an injury and one of: BAC>=100 mg/dl, or SMAST score >=3, or BAC = 1-99 mg/dl and SMAST = 1 or 2, or BAC = 1-99 and GGT above normal, or SMAST = 1 or 2 and GGT above normal; excluded if discharged within 24 hrs, or did not speak English, or traumatic brain injury that did not resolve by discharge, or died during hospitalisation, or not residents of Washington state, or homeless, or severe psychiatric problems, or discharged to long-term care facility; recruited from level 1 trauma centre, screened by BAC/GGT/

Gentillelo 1999 (*Continued*)

	SMAST. Number randomised = 762; 82% male, mean age = 36.1 yrs; high school or less = 52%, some college or more = 48%; 49.8% employed (all these characteristics reported for all intervention participants and 45% random sample of control participants) At baseline (all intervention & control participants): mean BAC = 152 mg/dl; SMAST score 0-2 = 27.0%, 3-8 = 55.6%, 9-13 = 17.4%; GGT abnormal = 27.0%
Interventions	Intervention group (n=366) received single 30 min motivational interview with trained psychologist consisting of personalised feedback on participant's drinking compared to national norm, injury risk at different BAC levels, negative social consequences of alcohol, negative physical consequences, level of alcohol dependence; focused on participant's personal responsibility for reducing drinking. Participant received a handwritten summarising follow-up letter one month later. Control group (n=396) received no intervention but were helped to find assistance with drinking problem if they requested it.
Outcomes	Trauma recurrence after hospital discharge, changes in mean weekly alcohol intake assessed at 6 and 12 months.
Notes	Loss to follow-up at 12 months: Intervention group: 172/366 (47%) Control group: 181/396 (46%) Table 2 gives changes in mean weekly alcohol intake for those with SMAST = 3-8; text pg 477 gives changes for all participants

Risk of bias

Item	**Authors' judgement**	**Description**
Allocation concealment?	Yes	A - Adequate

Heather 1987

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: unclear
Participants	Scotland. Adults 18-65 yrs with weekly alcohol consumption >35 units for men or >20 units for women (1 unit = 8 g), or clinical suspicion by GP of dependence; recruited from general practice; screened by health questionnaire with alcohol consumption questions embedded. Patients with high consumption or provoking clinical suspicion were given a DRAMS medical questionnaire by GP from which any positive response led to inclusion. Number randomised = 104; 75% men; mean age = 36.4 yrs (range 18-64, SD = 12.2) At baseline: mean consumption in previous month = 194.4 units; mean Michigan alcoholism score = 7.2 (SD = 5.9); mean Ph score = 4.6 (SD = 3.0)
Interventions	DRAMS group (n = 34): received diary sheet to record consumption for 2 weeks plus a blood test; at subsequent appointment received a self-help book plus a pocket-sized, abbreviated version of a manual for controlled drinking, blood tests, advice from GP, more diary sheets; additional appointments are made where progress is reviewed.

Heather 1987 (*Continued*)

	Advice group (n = 32): received strong advice to cut down from GP but no specific targets given and no follow-up consultations arranged Control group (n = 38): had a blood test and assessment interview but did not know the study was about alcohol and had no follow-up consultations
Outcomes	Units of alcohol consumed in previous month, units of alcohol consumed in heaviest month of the previous 6; assessed at 6 months
Notes	DRAMS trial. Loss to follow-up: DRAMS group: 5/34 (15%) Advice group: 2/32 (6%) Control group: 6/38 (16%)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Huas 2002

Methods	Randomisation: unclear Primary care physicians were randomised. Blinding of outcome assessor - unclear ITT: unclear
Participants	France. Participants 18-65 yrs consuming >28 glasses per week and on >=5 days per week; excluded if MAST>=3, or history of alcohol dependence, or in treatment for alcohol problems; recruited from general practice; screened by MAST and declared consumption of alcohol. Number randomised = 541; 100% male; mean age = 51.8 yrs
Interventions	Intervention group (n=?): 10 min intervention focused on reducing alcohol consumption to <28 per week. Patients with physical or biological symptoms invited back every 3 months, otherwise return at 1 year Control group (n=?): usual care
Outcomes	Mean no drinks per week, assessed after 1 yr
Notes	Number of patients assessed = 419, associated with 88 physicians: average cluster size = 4.8.

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Israel 1996

Methods	Randomisation: adequate Blinding of outcome assessor - unclear ITT: unclear
Participants	Canada. Participants drawn from those attending clinic who answered positively to at least 1 of 4 trauma questions and: consumed >=90 drinks in previous 4 weeks (average > 3 drinks per day), or consumed >=5 drinks per day for >=8 days in previous 4 weeks, or CAGE>=2; excluded if severe physical dependence on alcohol, or serum GGT activity >2SD above mean, or treatment for emotional or psychiatric problems, or regular attendance at Alcoholics Anonymous, or current substance abuse (other than alcohol); recruitment from primary care clinics; screened by trauma questionnaire followed by alcohol consumption questionnaire followed by CAGE questionnaire Number randomised = 105; age range = 30-60 yrs At baseline (for those assessed): Mean alcohol consumption in previous 4 weeks = 145.2 drinks; serum GGT = 56.9 U/l
Interventions	Brief counselling group (n=?): one 30 min counselling session from nurse practioner recruited for study on cognitive behavioural techniques to achieve abstinence or sensible drinking, plus a pamphlet with guidelines for achieving abstinence or acceptable drinking, plus 2-monthly 20 min follow-up sessions with the same nurse for a year. Participants recorded daily consumption and tested presence of alcohol saliva daily at bedtime. Participants were informed of their new GGT values and their significance at each session Advice group (n=?): were recommended to reduce their consumption, given the same pamphlet, and informed of GGT values plus significance. No further intervention
Outcomes	Mean alcohol consumption in previous 4 weeks; serum GGT; assessed at 1 yr
Notes	Loss to follow-up not recorded by treatment arm. 30% overall Missing data: gender, number randomised to each arm, baseline data for all randomised patients: requested, not available

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	No	C - Inadequate

Kunz 2004

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: unclear
Participants	US. English or Spanish speaking participants >18 yrs with CAGE score >=1, having used alcohol in previous 12 months; excluded if alcohol counselling in previous yr, or signs of cognitive impairment, or in police custody; recruited from Emergency Department; screened by CAGE Number randomised = 294; (following data are for those assessed), 81% male; mean age = 41.7; 13% married, 66% single, 19% separated/divorced; 70% African American, 30% Hispanic; 29% had health insurance; 44% high school or more education At baseline (for those assessed): mean weekly consumption in previous 3 months = 34.1 drinks; binge

Kunz 2004 (*Continued*)

	drinkers in previous 3 months (defined as >6 drinks on one occasion for men and >4 for women) = 92%; mean AUDIT score = 20.1; injured as a result of drinking in previous 3 months = 27%
Interventions	Intervention group (n=151) received action plans from researchers according to their self-reported levels of readiness to change: seek more information about drinking, think more about negative consequences of drinking, or lower their drinking per day, per week and per occasion. Participants received a copy of their action plan, a packet of health information, and a reminder about a follow-up session. Timings not reported. Control group (n=143) received the packet of health information only
Outcomes	Mean weekly alcohol consumption; % binge drinkers in previous month; AUDIT score; all assessed at 3 months
Notes	Loss to follow-up: Intervention group: 61/151 (40%) Control group: 39/143 (27%) Missing data: baseline data for all randomised participants: requested, no reply Screening, baseline and follow-up questionnaires were not masked as in TrEAT (Fleming 1997) and GOAL (Fleming 1999) trials.

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Lock 2006

Methods	Randomisation: adequate Primary care practices were cluster randomised. Blinding of outcome assessor - adequate ITT: yes
Participants	UK. Participants >=16yrs with AUDIT score >=8 for men or >=7 for women; excluded if current major physical or psychiatric illness, severely alcohol dependent, severely brain damaged or mentally impaired; recruited from general practice, screened by AUDIT Number randomised = 127; 50% male; mean age = 44.1; 72% employed, 5% unemployed, 15% retired, 3% students; 3% primary school, 6% some secondary school, 47% completed secondary school, 21% technical or trade certificate, 23% university or tertiary education. At baseline: mean weekly units consumed = 24.6; mean AUDIT score = 10.5
Interventions	Intervention group (n=67) received 5-10 minute intervention using the "drink-less" protocol including structured advice on standard drink units, recommended consumption & tips on achieving this, benefits of cutting down, goal-setting; they also received a self-help booklet/diary to take away. Control group (n=60) received usual care - nurses advice on cutting down drinking and UK Government Health Education Authority leaflet entitled "Think about Drink" which contained daily benchmark guides and basic advice on alcohol.

Lock 2006 (*Continued*)

Outcomes	AUDIT score, weekly units consumed, DPI, SF-12 physical health, SF-12 mental health; all assessed at 6 and 12 months
Notes	Loss to follow-up at 12 months: Intervention group: 31/67 (46.2%) Control group: 18/60 (30.0%) Number of patients assessed = 78 in 40 GP practices; average cluster size = 2.0.

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Yes	A - Adequate

Longabaugh 2001

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: yes
Participants	US. English or Spanish speaking participants living <1hr from hospital, >=18 yrs, presenting to Emergency Department with an injury that did not require hospitalisation; either breath alcohol positive (BAC>=0.003 mg/dl) in ED, or reported having ingested alcohol in 6 hrs previous to injury, or AUDIT>=8; excluded if homeless, or under arrest, or psychiatric disorders, or previous diagnosis of alcohol dependence or abuse Number randomised = 539; 78% male; mean age = 27 yrs (SD = 9); 72% white, 14% Latino/Hispanic, 10% black, <1% Asian, <1% Native American, 3% other; 77% single; 72% employed At baseline: AUDIT = 12.8; DrInC lifetime negative consequences score = 15.6; mean self-reported alcohol-related injuries in previous yr = 1.6
Interventions	BI group (n=182) received brief intervention by specially trained clinician: one 40-60 min session exploring their alcohol use using motivational interviewing, and helping them to determine goals. Participant completed a Change Plan Worksheet which they kept. BIB group (n=169) received brief intervention (as above) plus a booster session with specially trained clinician 7-10 days after BI, participants were encouraged to discuss postdischarge experiences, provided with additional info about their use of alcohol from baseline screening, and helped to alter Change Plan if required. SC group (n=188) received standard care: treatment of their injury only.
Outcomes	No heavy drinking days per week; alcohol related injuries; negative consequences from drinking; assessed at 1 yr
Notes	Loss to follow-up not given by arm

Risk of bias

Item	Authors' judgement	Description

Longabaugh 2001 (*Continued*)

Allocation concealment?	No	C - Inadequate

Maisto 2001

Methods	Randomisation: adequate Blinding of outcome assessor - unclear ITT: unclear
Participants	US. Participants >=21 yrs with >=8 on AUDIT, or >=16 std drinks per week for men or >=12 for women (1 std drink = 0.6 oz ethanol = 14 g); excluded if other drug abuse or dependence, or alcohol withdrawal for previous year, or participation in substance abuse treament in last year, or unstable psychiatric status; recruited in primary care; screened by a lifestyle survey containing general health, stress and alcohol-specific questions. Number randomised = 301; 70% men; mean age = 45.6 yrs; 77% white, 22% black, 1% other; 89% at least high school; 60% employed At baseline (for previous 30 days): mean number of days abstinent = 16.3; mean no drinks = 75.3; mean no days consumed 1-6 drinks = 10.0; mean drinks per drinking day = 5.7; mean Alcohol Dependence Scale score = 5.2
Interventions	Brief advice group (n=100) received one 10-15 min session from trained interventionist giving feedback from baseline results and implications for participant's drinking, and advice on goal for reducing or stopping drinking, and a booklet on the effects of alcohol Motivational enhancement group (n=101) received one 30-45 min session and 2 15-20 min "booster" sessions after 2 and 6 weeks from trained interventionist using reflective listening and other techniques to enhance the participant's motivation to change their alcohol use. Also received the booklet on the effects of alcohol after the first session Standard care group (n=100) received no intervention from research interventionists but selected data from baseline assessments was forwarded to GP who was not discouraged from acting
Outcomes	Mean number of days abstinent; mean no drinks; mean no days consumed 1-6 drinks; mean drinks per drinking day; all assessed at 6 and 12 months
Notes	ELM trial. Loss to follow-up: Brief advice group: 26/100 (26%) Motivational enhancement group: 28/101 (28%) Standard care group: 15/100 (15%) Separate paper on elderly as subgroup.

Risk of bias

Item	**Authors' judgement**	**Description**
Allocation concealment?	Yes	A - Adequate

McIntosh 1997

Methods	Randomisation: unclear Blinding of outcome assessor - inadequate ITT: unclear
Participants	Canada. Participants >15 yrs who had consumed >=4 std drinks on any day in previous month, or responded positively to >=1 CAGE question; recruited from family practice centre; screened by questionnaire containing CAGE & consumption over previous 28 days. Number randomised = 159; 52% male; mean age = 31.1 yrs; 50% employed full time, 16% employed part time, 34% unemployed
Interventions	Group 1 (n=40) received 30 min session from a physician (not their own) using cognitive behavioural strategies and giving advice on sensible drinking, helping participants understand the function of alcohol within their daily activities, and developing a plan and drinking goals. They received booklets containing this information and diary sheets to record drinking, and returned for second 30 min session 2 weeks later. Group 2 (n=66) received the same intervention and materials from a nurse practioner. Group 3 (n=53) received 5 mins advice from their own family physician on sensible drinking and avoiding risky situations, reinforced with a handout.
Outcomes	Mean monthly quantity frequency of drinking reported at 3, 6 and 12 months.
Notes	Loss to follow-up: not reported by arm; 16/159 (10%) overall Missing data Group 1 and group 2 are aggregated in our analysis as they are an identical intervention.

Risk of bias

Item	**Authors' judgement**	**Description**
Allocation concealment?	Unclear	B - Unclear

Ockene 1999

Methods	Randomisation: adequate. 4 practice sites were cluster randomised Blinding of outcome assessor - adequate ITT: yes
Participants	US. Participants 21-70 yrs consuming > 12 std drinks per week or >=5 std drinks on >=1 occasions in previous month for men, or >9 std drinks per week or >=4 std drinks on >=1 occasions in previous month for women (1 std drink = 12.8 g alcohol); excluded if pregnant, or planning to move away from the area within 1 yr, or didn't have telephone, or already participating in alcohol intervention programme, or psychiatric disorder; recruited from primary care centre; screened by Health Habits Survey with embedded CAGE. Number randomised = 530 (unit of randomisation = practice site, unit of measurement = patient); 64.7% male; mean age = 43.9 yrs; 94.6% white, 5.4% non-white; less than high school level = 8.6%, high school graduate or some college = 51.0%, college graduate or more = 40.4% At baseline: mean no drinks per week = 17.8

Ockene 1999 (*Continued*)

Interventions	SI (Special intervention)group (n=274) received a health booklet including advice on general health issues, and 5-10 min patient-centred alcohol counselling session from trained intervention providers, focusing on weekly consumption and binge drinking UC (Usual care) group (n=256) received the health booklet only
Outcomes	Mean no drinks per week, mean binge drinking episodes (defined as > 5 drinks on one occasion for men and >4 for women), assessed at 12 months. Change in weekly drinking levels and binge drinking episodes also reported from baseline to 12 months
Notes	HEALTH trial. Loss to follow-up: SI group: 39/274 (14%) UC group: 46/256 (18%) Number of patients assessed = 445 in 4 practice sites; average cluster size = 111. 12 month outcome data reported in separate paper, but baseline data for all randomised participants reported only in Ockene 1999.

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Yes	A - Adequate

Richmond 1995

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: yes
Participants	Australia. Participants 18-70yrs consuming >35 standard drinks per week for men or 21 for women; no exclusion criteria listed; recruited from general practice; screen by MAST and Ph score. Number randomised = 378; 57% male; mean age = 37.7 yrs; 74% employed; 67% beyond secondary school. At baseline: mean weekly consumption = 36.8 units.
Interventions	AS (alcoholscreen)group (n=96) received 5 consultations: 1) 5 mins where patients were given self-help manual and day diary to monitor their consumption; 2) 1 week later patients had a 15-20 min consultation where a personalised approach to patient education using a flip-over unit displaying 12 pictorial and text prompts was used, and patients were counselled about recommended limits, problems associated with excessive drinking, alternate activities; 3) 1 month later patients had 5-25 min consultation to reinforce and support new drinking habits; 4) and 5) 5 min sessions for further support. MI (minimal intervention)group (n=96) received 5 mins brief advice and self-help manual NI (no intervention)group (n=93) received no intervention NA (no assessment)group (n=93) received no intervention and no assessments (for comparison with NI group to test whether the assessments make a difference)

Richmond 1995 (*Continued*)

Outcomes	Mean weekly consumption; mean quantity-frequency consumption; GGT; assessed at 6 months and 1 year.
Notes	Alcoholscreen trial. Loss to follow-up: AS group: 32/96 (33%) MI group: 26/96 (27%) NI group: 30/93 (32%) (NA group was not assessed at 1 year)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Rodriguez 2003

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: yes
Participants	Spain. Participants >18 yrs who had been involved in a traffic crash in previous 6 hours and BAC >=0.2 g/l; excluded if unable to speak Spanish, non-resident, very severe medical, psychiatric or social conditions; recruited in emergency department; screened by Alcohol-On-Site saliva test. Number randomised = 85; 88% male; median age = 26 yrs. Baseline AUDIT-C = 4.9
Interventions	BI group (n=40) received 15-20 min intervention based on FRAMES, model of change and motivational interviewing, and an information leaflet. MI group (n=45) received 5 min empathic advice and the same information leaflet.
Outcomes	AUDIT-C positive or negative, % participants who reduced consumption, % reduction in hazardous drinkers; all measure at 1 yr.
Notes	Loss to follow-up: BI group: 17/45 (38%) MI group: 11/40 (28%) Not included in meta-analysis due to unusable outcome data - discussed narratively.

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	No	C - Inadequate

Romelsjo 1989

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: unclear
Participants	Sweden. Participants 18-64 yrs consuming at least 40 g 100% ethanol/day for men and 30 g for women, or drinking in the morning at least every second time when drinking alcohol, or having difficulties restricting drinking at least every second time when drinking alcohol, or CAGE>=3, or elevation of GGT; excluded if inpatient care for alcoholism or alcohol psychosis in previous 3 yrs, or care at an inebriate's institution in previous 3 yrs, or other substance abuse in previous 3 yrs, or ongoing treatment or need for treatment for a mental disorder, or severe somatic disease, or other potential causes of elevated GGT; recruited from existing health study within primary health care teams of district health centres; screened by mailed general health questionnaire incorporating alcohol consumption questions and CAGE, and telephone interview. Number randomised = 83; 84% male; mean age = 46.3 yrs (range = 21-64 yrs); 86% employed At baseline: GGT = 2.0 Ukat/l; daily alcohol consumption = 29.1 g 100% ethanol; "problems index" = 11.3
Interventions	Intervention group (n=41) received advice from GP on cutting down or abstaining from alcohol by the next visit, GGT values were used in discussions in a biofeedback approach; GPs decided frequency of return visits and mean was 3 Control group (n=42) were told by GP to cut down on alcohol and that a follow-up examination would occur after 1 yr.
Outcomes	Change in GGT; change in self-reported alcohol consumption; change in a combined measure of alcohol problems "problem index"; all measured at 1 yr.
Notes	Loss to follow-up: Intervention group = 5/41 (12%) Control group = 6/42 (14%)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Yes	A - Adequate

Scott 1991

Methods	Randomisation: adequate Blinding of outcome assessor - adequate ITT: yes. ITT outcome data were based on imputation of baseline values to participants lost to follow-up.
Participants	England. Participants 17-69 yrs consuming >350 g alcohol for men and >168 g for women in previous week; excluded if consumption > 1050 g/wk for men or 560 g/wk for women, or previous advice to cut down during the previous yr; recruited from general practice; screened by Health Survey Questionnaire including quantity frequency measure of alcohol Number randomised = 226; 68% male; mean age = 44.7 yrs; 47% social class I-IIIn At baseline: mean alcohol consumption for previous wk (from interview)= 526 g for men, 293 g for

Scott 1991 (*Continued*)

	women; mean quantity frequency drinking for previous wk (from HSQ) = 439 g for men, 247 g for women; % binge drinkers = 43 for men (defined as consumption of 140 g on 2+ occasions in previous 3 months), 18 for women (defined as consumption of >14 units on 2+ occasions in previous 3 months)
Interventions	Intervention group (n=113) received 10 mins of feedback of the assessment interview and blood tests, and information on the risks of excessive drinking and the benefits of drinking less, a comparison of participant's drinking compared to average, and advice to reduce alcohol consumption to below 14 units/wk. Participants also received a specially prepared booklet. Control group (n=113) received no advice except at their own request
Outcomes	Change in weekly alcohol consumption measured at 1 yr.
Notes	Cut Down on Drinking trial. Loss to follow-up: Intervention group = 33/113 (29%) Control group = 43/113 (38%)

Risk of bias

Item	**Authors' judgement**	**Description**
Allocation concealment?	Yes	A - Adequate

Senft 1997

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: yes
Participants	US. Participants <21 yrs with total AUDIT score between 8-21, or sum of AUDIT frequency & quantity item scores >=5, or >=6 drinks (defined as 10 oz beer or 4 oz wine or 1 oz liquor) per occasion at least weekly; excluded if pregnant; recruited from primary care clinics; screened by quesionnaire including AUDIT Number randomised = 516; 71% male; mean age = 42.4 yrs; 82% white, 18% non-white; some college or more = 59.5% At baseline: mean AUDIT score = 10.6; mean weekly drinking days = 3.4; mean drinks per drinking day = 4.9; binge drinkers (defined as >=6 drinks per occasion at least weekly) = 28%
Interventions	Intervention group (n=260) received first a 30 second message from the primary care clinician (physician, nurse practitioner or physician assistant)and immediately following a 15 minute session with a health counsellor. This included feedback on participant's drinking compared to national norms, explanation of chronic effects of alcohol use and ways to estimate blood alcohol level, recommendation of restriction or abstinence and creation of plans for this, and building of participant's confidence. They also received a pack of printed material. Control group (n=256) received usual care only
Outcomes	Total standard ethanol content units in previous 3 months; drinking days per week in previous 6 months; drinks per drinking day in previous 6 months; followed up at 6 and 12 months

Senft 1997 (*Continued*)

Notes	Loss to follow-up: Intervention group: 64/260 (25%) Control group: 41/256 (16%) This study did not exclude participants told by their GP to avoid alcohol

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Seppa 1992

Methods	Randomisation: unclear Blinding of outcome assessor - unclear ITT: unclear
Participants	Finland. Adults with MCV >=100 fl, >=2 positive answers to MM-MAST, or macrocytosis for which no other aetiology was found; screened by Malmo Modified Michigan Alcoholism Screening Test; recruited from general practice. Number randomised = 178; 79% male; mean age = 53.2 yrs At baseline: mean MCV = 101.6 fl; GGT = 145.4 U/l
Interventions	Intervention group (n=92) received 5 brief sessions with GP, informed of blood results, asked about alcohol consumption and encouraged to reduce it. Control group (n=86) received no intervention
Outcomes	Self-report of whether alcohol consumption was reduced; MCV values; assessed at 12 months
Notes	Loss to follow-up: Intervention group = 51/92 (55%) Control group = 32/86 (37%)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Tomson 1998

Methods	Randomisation: inadequate (participants were assigned to treatment arms on basis of date of birth) Blinding of outcome assessor - unclear ITT: unclear

Tomson 1998 (*Continued*)

Participants	Sweden. Participants 25-54 yrs with GGT >0.89 microkatals/l; excluded if chronic alcoholic; recruited from primary healthcare centre; screened by health check including GGT. Number randomised = 222; (but following data is for n=75 who were not excluded and then were followed up): 81% male; mean age = 45.2 yrs; 73% blue collar, 27% white collar At baseline: mean weekly consumption given at baseline only for intervention group = 337 g; mean S-GGT = 1.7 microkatals/l
Interventions	Intervention group (n=100) received 2 consultations from nurse discussing lifestyle in general and alcohol consumption in particular, focusing on factors that facilitate or make controlled drinking more difficult, using GGT as biochemical feedback. Control group (n=122) received 1 appointment with GP to discuss lifestyle in general
Outcomes	GGT measure at 1 and 2 years
Notes	Loss to follow-up: Intervention group: 70/100 (70%) Control group: 77/122 (63%) Randomisation (n=222) then assessment & exclusion (leaving n=75 who actually had the intervention or control sessions)

Risk of bias

Item	Authors' judgement	Description
Allocation concealment?	Unclear	B - Unclear

Wallace 1988

Methods	Randomisation: unclear Blinding of outcome assessor - adequate ITT: unclear
Participants	UK. Participants 17-69 yrs who had consumed >=35 units of alcohol in previous week for men and 21 units for women, or had >= postive CAGE responses; excluded if serious illness, or received medical advice about drinking in the previous year, or requested help from nurse with drinking, or GGT>150 IU/l; recruited from general practice; screened by self-administered health survey questionnaire including quantity/frequency scale & modified CAGE Number randomised = 909; 71% male; mean age = 42.4 yrs At baseline: alcohol consumption for previous week (from interview)= 55.0 units; alcohol consumption according to questionnaire (quantity/frequency)= 44.1 units; GGT = 23.0 IU/l
Interventions	Intervention group (n=450) received advice from GP about potential harmful effects of their current consumption, illustration of general population consumption level compared to theirs, and an information booklet. Men advised to drink no more than 18 units/wk, women 9 units/wk, all received a drinking diary to keep; all participants returned for follow-up interview after 1 month, some (at GP discretion)after 4, 7, 10 months, when drinking diary was reviewed and feedback given on the results of blood tests. Control group (n=459) received brief advice on general health from research nurse as part of screening interview, no further advice from GP

Wallace 1988 (*Continued*)

Outcomes	Weekly alcohol consumption at 6 and 12 months.
Notes	Loss to follow-up: Intervention group: 87/450 (19%) Control group: 74/459 (16%) Outcome assessors were blinded at the start but learned allocation from many participants during the trial.

Risk of bias

Item	**Authors' judgement**	**Description**
Allocation concealment?	Unclear	B - Unclear

Characteristics of excluded studies *[ordered by study ID]*

Andreasson 2002	Participants were not recruited from primary care
Bernstein 1997	Doesn't assess effectiveness of brief interventions
Blow 2001	Screening study
Burge 1997	Participants were severely dependent alcoholics
Chang 1999	Participants were not recruited from primary care; intervention was too long to be "brief"
Copeland 2003	No prespecified outcomes
Coulter 1995	No prespecified outcomes
Dimeff 2000	No prespecified outcomes
Drummond 1990	Participants were not recruited from primary care
Fernandez 2003	Not RCT
Freeborn 2000	No prespecified outcomes
Ivanets 1991	Some participants were not recruited from primary care: unable to separate outcome data
Kelly 1988	No prespecified outcomes
Kristenson 1983	Participants were not recruited from primary care
Kristenson 2002	Participants were not recruited from primary care
Logsdon 1989	Not RCT
Maheswaran 1992	Participants were not recruited from primary care
Monti 1999	No prespecified outcomes
Nilssen 1991	No prespecified outcomes
Oliansky 1997	No prespecified outcomes
Persson 1989	Some participants were not recruited from primary care: unable to separate outcome data
Richmond 2000	Not RCT; participants were not recruited from primary care
Saitz 2003	Intervention concerns prompting physicians to give advice (generally), not the effect of a brief intervention for alcohol

(*Continued*)

Smith 2003	Participants were not recruited from primary care
Vinson 2000	Study protocol changed over time
WHO 1996	Some participants were not recruited from primary care: unable to separate outcome data
Woollard 1995	Not primarily an alcohol reduction trial
Wutzke 2002	Some participants were not recruited from primary care: unable to separate outcome data

DATA AND ANALYSES

Comparison 1. Brief intervention vs. control

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Quantity of drinking (g/week) subgrouped by effectiveness/ efficacy	22	5860	Mean Difference (IV, Random, 95% CI)	-38.42 [-54.16, -22.67]
1.1 Effectiveness trials	10	3106	Mean Difference (IV, Random, 95% CI)	-33.35 [-53.62, -13.07]
1.2 Efficacy trials	12	2754	Mean Difference (IV, Random, 95% CI)	-45.07 [-70.07, -20.07]
2 Quantity of drinking (g/week) restricted to trials with adequate allocation concealment	10	2474	Mean Difference (IV, Random, 95% CI)	-55.77 [-75.17, -36.38]
2.1 Effectiveness	5	1625	Mean Difference (IV, Random, 95% CI)	-48.17 [-65.44, -30.91]
2.2 Efficacy	5	849	Mean Difference (IV, Random, 95% CI)	-70.68 [-115.21, -26.15]
3 Quantity of drinking (g/week) using imputed values for participants lost to follow-up	22	6038	Mean Difference (IV, Random, 95% CI)	-38.04 [-53.41, -22.68]
3.1 Effectiveness	11	3506	Mean Difference (IV, Random, 95% CI)	-28.33 [-46.91, -9.75]
3.2 Efficacy	11	2532	Mean Difference (IV, Random, 95% CI)	-51.36 [-77.27, -25.44]
4 Quantity of drinking (g/week) - with imputation of unknown standard deviations	24	5954	Mean Difference (IV, Random, 95% CI)	-36.33 [-51.86, -20.80]
4.1 Effectiveness	11	3328	Mean Difference (IV, Random, 95% CI)	-28.46 [-48.01, -8.92]
4.2 Efficacy	13	2626	Mean Difference (IV, Random, 95% CI)	-45.49 [-70.80, -20.19]
5 Quantity of drinking (g/week) subgrouped by high/low treatment exposure	22	5860	Mean Difference (IV, Random, 95% CI)	-38.42 [-54.16, -22.67]
5.1 Low treatment exposure	10	2139	Mean Difference (IV, Random, 95% CI)	-22.88 [-38.18, -7.58]
5.2 High treatment exposure	12	3721	Mean Difference (IV, Random, 95% CI)	-51.02 [-75.16, -26.88]
6 Quantity of drinking (g/week) subgrouped by gender	8	2307	Mean Difference (IV, Random, 95% CI)	-38.66 [-64.91, -12.42]
6.1 Men	8	1808	Mean Difference (IV, Random, 95% CI)	-57.06 [-88.72, -25.39]
6.2 Women	5	499	Mean Difference (IV, Random, 95% CI)	-9.54 [-48.32, 29.24]

7 Quantity of drinking (g/ week) subgrouped by gender, excluding trials of men only	5	1670	Mean Difference (IV, Random, 95% CI)	-29.61 [-60.28, 1.05]
7.1 Men	5	1171	Mean Difference (IV, Random, 95% CI)	-53.03 [-93.32, -12.74]
7.2 Women	5	499	Mean Difference (IV, Random, 95% CI)	-9.54 [-48.32, 29.24]
8 Quantity of drinking (g/ week) subgrouped by cluster/ individual randomisation	22	6019	Mean Difference (IV, Random, 95% CI)	-38.22 [-53.85, -22.60]
8.1 Cluster randomised	4	1171	Mean Difference (IV, Random, 95% CI)	-40.98 [-72.72, -9.24]
8.2 Individual randomised	18	4848	Mean Difference (IV, Random, 95% CI)	-37.80 [-56.33, -19.26]
9 Quantity of drinking (g/ week) subgrouped by cluster/ individual randomisation, varying imputed ICC	22	6019	Mean Difference (IV, Random, 95% CI)	-37.38 [-52.96, -21.80]
9.1 Cluster randomised	4	1171	Mean Difference (IV, Random, 95% CI)	-34.64 [-56.91, -12.36]
9.2 Individual randomised	18	4848	Mean Difference (IV, Random, 95% CI)	-37.80 [-56.33, -19.26]
10 Frequency of drinking (no. binges/wk)	3	1003	Mean Difference (IV, Random, 95% CI)	-0.27 [-0.58, 0.04]
10.1 Effectiveness	1	723	Mean Difference (IV, Random, 95% CI)	-0.26 [-0.44, -0.08]
10.2 Efficacy	2	280	Mean Difference (IV, Random, 95% CI)	-0.38 [-1.15, 0.40]
11 Frequency of drinking (no. days drinking/wk) subgrouped by effectiveness/efficacy	3	795	Mean Difference (IV, Random, 95% CI)	-0.04 [-0.51, 0.43]
11.1 Effectiveness	2	382	Mean Difference (IV, Random, 95% CI)	0.20 [-0.24, 0.64]
11.2 Efficacy	1	413	Mean Difference (IV, Random, 95% CI)	-0.40 [-0.84, 0.04]
12 Frequency of drinking (no. days drinking/wk) subgrouped by gender	2	575	Mean Difference (IV, Random, 95% CI)	-0.13 [-0.61, 0.36]
12.1 Men	2	403	Mean Difference (IV, Random, 95% CI)	0.05 [-0.73, 0.83]
12.2 Women	2	172	Mean Difference (IV, Random, 95% CI)	-0.33 [-1.11, 0.45]
13 Intensity of drinking (g/ drinking day) subgrouped by effectiveness/efficacy	5	1334	Mean Difference (IV, Random, 95% CI)	-3.10 [-8.85, 2.64]
13.1 Effectiveness	3	761	Mean Difference (IV, Random, 95% CI)	-6.50 [-16.17, 3.16]
13.2 Efficacy	2	573	Mean Difference (IV, Random, 95% CI)	0.73 [-6.87, 8.32]
14 Intensity of drinking (g/ drinking day) subgrouped by gender	2	569	Mean Difference (IV, Random, 95% CI)	4.21 [-15.33, 23.74]
14.1 Men	2	399	Mean Difference (IV, Random, 95% CI)	-7.35 [-31.48, 16.77]
14.2 Women	2	170	Mean Difference (IV, Random, 95% CI)	24.18 [-17.18, 65.54]
15 Laboratory markers - GGT (IU/l)	3	1008	Mean Difference (IV, Random, 95% CI)	-1.11 [-3.89, 1.67]

16 Laboratory markers - GGT (IU/l), subgrouped by gender	2	936	Mean Difference (IV, Random, 95% CI)	0.32 [-3.48, 4.13]
16.1 Men	2	664	Mean Difference (IV, Random, 95% CI)	-2.17 [-6.30, 1.96]
16.2 Women	2	272	Mean Difference (IV, Random, 95% CI)	3.47 [-5.99, 12.94]
17 Laboratory markers - MCV (fl)	1	95	Mean Difference (IV, Random, 95% CI)	0.62 [-1.58, 2.81]
17.1 Men	1	81	Mean Difference (IV, Random, 95% CI)	0.80 [-1.70, 3.30]
17.2 Women	1	14	Mean Difference (IV, Random, 95% CI)	Not estimable
18 Heavy drinkers	9		Risk Difference (M-H, Random, 95% CI)	Subtotals only
19 Binge drinkers	4	1010	Risk Difference (M-H, Random, 95% CI)	-0.11 [-0.19, -0.03]
20 Loss to follow-up in assessment of quantity of alcohol consumed	23	7395	Risk Difference (M-H, Random, 95% CI)	0.04 [0.01, 0.07]
20.1 Effectiveness trials	10	4436	Risk Difference (M-H, Random, 95% CI)	0.02 [-0.02, 0.06]
20.2 Efficacy trials	13	2959	Risk Difference (M-H, Random, 95% CI)	0.06 [0.03, 0.09]

Comparison 2. Extended vs. brief intervention

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Quantity of drinking (g/week)	4	508	Mean Difference (IV, Random, 95% CI)	-27.96 [-62.19, 6.26]
2 Quantity of drinking (g/week) using imputed values for participants lost to follow-up	4	631	Mean Difference (IV, Fixed, 95% CI)	-27.01 [-59.31, 5.30]
3 Quantity of drinking (g/week), subgrouped by gender	2	288	Mean Difference (IV, Random, 95% CI)	-22.76 [-74.26, 28.74]
3.1 Men	2	189	Mean Difference (IV, Random, 95% CI)	-16.63 [-90.20, 56.93]
3.2 Women	2	99	Mean Difference (IV, Random, 95% CI)	-51.89 [-180.62, 76.85]
4 Frequency of drinking (no. days drinking/week)	1	157	Mean Difference (IV, Random, 95% CI)	-0.69 [-1.28, -0.10]
5 Intensity of drinking (g/drinking day)	2	299	Mean Difference (IV, Random, 95% CI)	5.84 [-12.73, 24.40]
6 Laboratory markers - GGT (IU/l)	2	243	Mean Difference (IV, Random, 95% CI)	-2.64 [-15.67, 10.39]
7 Loss to follow-up in assessment of quantity of alcohol consumed	3	676	Risk Difference (M-H, Random, 95% CI)	0.04 [-0.05, 0.14]

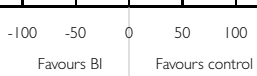
Analysis 1.1. Comparison 1 Brief intervention vs. control, Outcome 1 Quantity of drinking (g/week) subgrouped by effectiveness/efficacy.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 1 Quantity of drinking (g/week) subgrouped by effectiveness/efficacy

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Effectiveness trials							
Lock 2006	36	128.64 (293.28)	42	156.8 (293.28)		1.3 %	-28.16 [-158.72, 102.40]
Cordoba 1998	104	202.4 (183.27)	125	295.2 (215.22)		4.9 %	-92.80 [-144.42, -41.18]
Ockene 1999	235	-73.34 (146.41)	210	-40.58 (146.41)		7.9 %	-32.76 [-60.01, -5.51]
Aalto 2000	82	278.3 (280.69)	73	262.79 (299.4)		2.3 %	15.51 [-76.19, 107.21]
Crawford 2004	189	457.6 (547.2)	195	566.4 (710.4)		1.3 %	-108.80 [-235.40, 17.80]
Diez 2002	206	293.74 (186.21)	186	302.74 (163.01)		6.9 %	-9.00 [-43.57, 25.57]
Fleming 1997	353	137.76 (135.72)	370	185.52 (155.16)		8.7 %	-47.76 [-68.98, -26.54]
Huas 2002	270	-109 (164.73)	149	-92 (190.35)		6.6 %	-17.00 [-53.33, 19.33]
Scott 1991	80	245.38 (191.3)	70	310.64 (252.17)		3.2 %	-65.26 [-137.70, 7.18]
Richmond 1995	70	326 (211)	61	290 (208)		3.3 %	36.00 [-35.89, 107.89]
Subtotal (95% CI)	**1625**		**1481**			**46.3 %**	**-33.35 [-53.62, -13.07]**
Heterogeneity: Tau² = 391.17; Chi² = 16.24, df = 9 (P = 0.06); I² =45%							
Test for overall effect: Z = 3.22 (P = 0.0013)							
2 Efficacy trials							
Curry 2003	100	108.5 (98.5)	122	110.9 (93.25)		8.1 %	-2.40 [-27.83, 23.03]
Fleming 1999	78	119.04 (83.64)	67	195.24 (146.04)		6.2 %	-76.20 [-115.79, -36.61]
Senft 1997	196	140.95 (177.31)	215	160.7 (177.31)		6.9 %	-19.75 [-54.07, 14.57]
Wallace 1988	363	304.34 (184.94)	385	386.15 (230.97)		7.5 %	-81.81 [-111.71, -51.91]
Altisent 1997	34	168 (167.2)	30	280 (174.4)		2.6 %	-112.00 [-195.98, -28.02]
Heather 1987	29	252.55 (156.37)	62	317.76 (246.68)		2.6 %	-65.21 [-148.93, 18.51]
Fernandez 1997	38	-107.4 (370.4)	50	-64.6 (278.3)		1.1 %	-42.80 [-183.58, 97.98]
Gentillelo 1999	194	-254.43 (601.47)	215	-78.2 (992.56)		0.9 %	-176.23 [-333.60, -18.86]
Kunz 2004	90	200.62 (308.93)	104	234.35 (312.2)		2.5 %	-33.73 [-121.33, 53.87]
Maisto 2001	74	133.98 (147.52)	85	147.33 (147.72)		5.5 %	-13.35 [-59.35, 32.65]
Fleming 2004	81	57.64 (106.39)	70	65.99 (74.34)		7.6 %	-8.35 [-37.33, 20.63]



(*Continued . . .*)

(. . . *Continued*)

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Romelsjo 1989	36	-34.86 (209.04)	36	42.64 (202.26)		2.2 %	-77.50 [-172.52, 17.52]
Subtotal (95% CI)	**1313**		**1441**			**53.7 %**	**-45.07 [-70.07, -20.07]**
Heterogeneity: Tau² = 1024.80; Chi² = 32.35, df = 11 (P = 0.00067); I² =66%							
Test for overall effect: Z = 3.53 (P = 0.00041)							
Total (95% CI)	**2938**		**2922**			**100.0 %**	**-38.42 [-54.16, -22.67]**
Heterogeneity: Tau² = 628.95; Chi² = 48.60, df = 21 (P = 0.00057); I² =57%							
Test for overall effect: Z = 4.78 (P < 0.00001)							

Analysis 1.2. Comparison 1 Brief intervention vs. control, Outcome 2 Quantity of drinking (g/week) restricted to trials with adequate allocation concealment.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 2 Quantity of drinking (g/week) restricted to trials with adequate allocation concealment

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Effectiveness							
Lock 2006	36	128.64 (293.28)	42	156.8 (293.28)		2.1 %	-28.16 [-158.72, 102.40]
Cordoba 1998	104	202.4 (183.27)	125	295.2 (215.22)		10.1 %	-92.80 [-144.42, -41.18]
Ockene 1999	235	-73.34 (146.41)	210	-40.58 (146.41)		20.9 %	-32.76 [-60.01, -5.51]
Fleming 1997	353	137.76 (135.72)	370	185.52 (155.16)		24.9 %	-47.76 [-68.98, -26.54]
Scott 1991	80	245.38 (191.3)	70	310.64 (252.17)		6.0 %	-65.26 [-137.70, 7.18]
Subtotal (95% CI)	**808**		**817**			**64.0 %**	**-48.17 [-65.44, -30.91]**
Heterogeneity: Tau² = 40.88; Chi² = 4.40, df = 4 (P = 0.35); I² =9%							
Test for overall effect: Z = 5.47 (P < 0.00001)							
2 Efficacy							
Fleming 1999	78	119.04 (83.64)	67	195.24 (146.04)		14.3 %	-76.20 [-115.79, -36.61]
Altisent 1997	34	168 (167.2)	30	280 (174.4)		4.6 %	-112.00 [-195.98, -28.02]
Gentillelo 1999	194	-254.43 (601.47)	215	-78.2 (992.56)		1.5 %	-176.23 [-333.60, -18.86]
Maisto 2001	74	133.98 (147.52)	85	147.33 (147.72)		11.9 %	-13.35 [-59.35, 32.65]

(*Continued* . . .)

(. . . *Continued*)

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Romelsjo 1989	36	-34.86 (209.04)	36	42.64 (202.26)		3.7 %	-77.50 [-172.52, 17.52]
Subtotal (95% CI)	**416**		**433**			**36.0 %**	**-70.68 [-115.21, -26.15]**
Heterogeneity: Tau^2 = 1197.34; Chi^2 = 8.28, df = 4 (P = 0.08); I^2 =52%							
Test for overall effect: Z = 3.11 (P = 0.0019)							
Total (95% CI)	**1224**		**1250**			**100.0 %**	**-55.77 [-75.17, -36.38]**
Heterogeneity: Tau^2 = 275.40; Chi^2 = 13.48, df = 9 (P = 0.14); I^2 =33%							
Test for overall effect: Z = 5.64 (P < 0.00001)							

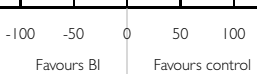
Analysis 1.3. Comparison 1 Brief intervention vs. control, Outcome 3 Quantity of drinking (g/week) using imputed values for participants lost to follow-up.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 3 Quantity of drinking (g/week) using imputed values for participants lost to follow-up

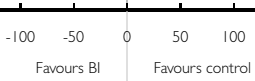
Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Effectiveness							
Lock 2006	36	128.64 (293.28)	42	156.8 (293.28)		1.2 %	-28.16 [-158.72, 102.40]
Cordoba 1998	104	202.4 (183.27)	125	295.2 (215.22)		4.8 %	-92.80 [-144.42, -41.18]
Ockene 1999	235	-73.34 (146.41)	210	-40.58 (146.41)		7.8 %	-32.76 [-60.01, -5.51]
Aalto 2000	134	277.02 (291.16)	123	278.71 (318)		3.0 %	-1.69 [-76.45, 73.07]
Crawford 2004	189	457.6 (547.2)	195	566.4 (710.4)		1.3 %	-108.80 [-235.40, 17.80]
Diez 2002	206	293.74 (186.21)	186	302.74 (163.01)		6.7 %	-9.00 [-43.57, 25.57]
Fleming 1997	353	137.76 (135.72)	370	185.52 (155.16)		8.6 %	-47.76 [-68.98, -26.54]
Huas 2002	270	-109 (164.73)	149	-92 (190.35)		6.5 %	-17.00 [-53.33, 19.33]
Scott 1991	113	312.36 (198.36)	113	361.59 (223.71)		4.4 %	-49.23 [-104.36, 5.90]
Richmond 1995	70	326 (211)	61	290 (208)		3.2 %	36.00 [-35.89, 107.89]
Curry 2003	100	108.5 (98.5)	122	110.9 (93.25)		8.0 %	-2.40 [-27.83, 23.03]
Subtotal (95% CI)	**1810**		**1696**			**55.4 %**	**-28.33 [-46.91, -9.75]**



(*Continued* . . .)

(. . . *Continued*)

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Heterogeneity: Tau² = 419.33; Chi² = 20.55, df = 10 (P = 0.02); I² =51%							
Test for overall effect: Z = 2.99 (P = 0.0028)							
2 Efficacy							
Fleming 1999	78	119.04 (83.64)	67	195.24 (146.04)		6.1 %	-76.20 [-115.79, -36.61]
Senft 1997	196	140.95 (177.31)	215	160.7 (177.31)		6.8 %	-19.75 [-54.07, 14.57]
Wallace 1988	363	304.34 (184.94)	385	386.15 (230.97)		7.4 %	-81.81 [-111.71, -51.91]
Altisent 1997	34	168 (167.2)	30	280 (174.4)		2.5 %	-112.00 [-195.98, -28.02]
Heather 1987	29	252.55 (156.37)	62	317.76 (246.68)		2.5 %	-65.21 [-148.93, 18.51]
Fernandez 1997	38	-107.4 (370.4)	50	-64.6 (278.3)		1.1 %	-42.80 [-183.58, 97.98]
Gentillelo 1999	194	-254.43 (601.47)	215	-78.2 (992.56)		0.9 %	-176.23 [-333.60, -18.86]
Kunz 2004	90	200.62 (308.93)	104	234.35 (312.2)		2.4 %	-33.73 [-121.33, 53.87]
Maisto 2001	74	133.98 (147.52)	85	147.33 (147.72)		5.3 %	-13.35 [-59.35, 32.65]
Fleming 2004	81	57.64 (106.39)	70	65.99 (74.34)		7.5 %	-8.35 [-37.33, 20.63]
Romelsjo 1989	36	-34.86 (209.04)	36	42.64 (202.26)		2.1 %	-77.50 [-172.52, 17.52]
Subtotal (95% CI)	**1213**		**1319**			**44.6 %**	**-51.36 [-77.27, -25.44]**
Heterogeneity: Tau² = 907.03; Chi² = 24.10, df = 10 (P = 0.01); I² =59%							
Test for overall effect: Z = 3.88 (P = 0.00010)							
Total (95% CI)	**3023**		**3015**			**100.0 %**	**-38.04 [-53.41, -22.68]**
Heterogeneity: Tau² = 599.33; Chi² = 47.78, df = 21 (P = 0.00074); I² =56%							
Test for overall effect: Z = 4.85 (P < 0.00001)							



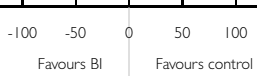
Analysis 1.4. Comparison 1 Brief intervention vs. control, Outcome 4 Quantity of drinking (g/week) - with imputation of unknown standard deviations.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 4 Quantity of drinking (g/week) - with imputation of unknown standard deviations

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Effectiveness							
Lock 2006	36	128.64 (293.28)	42	156.8 (293.28)		1.2 %	-28.16 [-158.72, 102.40]
Cordoba 1998	104	202.4 (183.27)	125	295.2 (215.22)		4.7 %	-92.80 [-144.42, -41.18]
Ockene 1999	235	-73.34 (146.41)	210	-40.58 (146.41)		7.6 %	-32.76 [-60.01, -5.51]
Aalto 2000	82	278.3 (280.69)	73	262.79 (299.4)		2.2 %	15.51 [-76.19, 107.21]
Crawford 2004	189	457.6 (547.2)	195	566.4 (710.4)		1.3 %	-108.80 [-235.40, 17.80]
Diez 2002	206	293.74 (186.21)	186	302.74 (163.01)		6.7 %	-9.00 [-43.57, 25.57]
Fleming 1997	353	137.76 (135.72)	370	185.52 (155.16)		8.4 %	-47.76 [-68.98, -26.54]
Huas 2002	270	-109 (164.73)	149	-92 (190.35)		6.4 %	-17.00 [-53.33, 19.33]
Scott 1991	80	245.38 (191.3)	70	310.64 (252.17)		3.1 %	-65.26 [-137.70, 7.18]
Richmond 1995	70	326 (211)	61	290 (208)		3.2 %	36.00 [-35.89, 107.89]
Curry 2003	100	108.5 (98.5)	122	110.9 (93.25)		7.8 %	-2.40 [-27.83, 23.03]
Subtotal (95% CI)	**1725**		**1603**			**52.7 %**	**-28.46 [-48.01, -8.92]**
Heterogeneity: Tau² = 468.49; Chi² = 21.38, df = 10 (P = 0.02); I² =53%							
Test for overall effect: Z = 2.85 (P = 0.0043)							
2 Efficacy							
Chang 1997	11	130.7 (191)	8	109.71 (191)		0.7 %	20.99 [-152.96, 194.94]
Fleming 1999	78	119.04 (83.64)	67	195.24 (146.04)		6.0 %	-76.20 [-115.79, -36.61]
Senft 1997	196	140.95 (177.31)	215	160.7 (177.31)		6.7 %	-19.75 [-54.07, 14.57]
Wallace 1988	363	304.34 (184.94)	385	386.15 (230.97)		7.3 %	-81.81 [-111.71, -51.91]
Altisent 1997	34	168 (167.2)	30	280 (174.4)		2.5 %	-112.00 [-195.98, -28.02]
Heather 1987	29	252.55 (156.37)	62	317.76 (246.68)		2.6 %	-65.21 [-148.93, 18.51]
Fernandez 1997	38	-107.4 (370.4)	50	-64.6 (278.3)		1.1 %	-42.80 [-183.58, 97.98]
Tomson 1998	30	228 (191)	45	196 (191)		2.4 %	32.00 [-56.24, 120.24]
Gentillelo 1999	194	-254.43 (601.47)	215	-78.2 (992.56)		0.9 %	-176.23 [-333.60, -18.86]
Kunz 2004	90	200.62 (308.93)	104	234.35 (312.2)		2.4 %	-33.73 [-121.33, 53.87]



(*Continued . . .*)

(. . . *Continued*)

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Maisto 2001	74	133.98 (147.52)	85	147.33 (147.72)		5.3 %	-13.35 [-59.35, 32.65]
Fleming 2004	81	57.64 (106.39)	70	65.99 (74.34)		7.4 %	-8.35 [-37.33, 20.63]
Romelsjo 1989	36	-34.86 (209.04)	36	42.64 (202.26)		2.1 %	-77.50 [-172.52, 17.52]
Subtotal (95% CI)	**1254**		**1372**			**47.3 %**	**-45.49 [-70.80, -20.19]**
Heterogeneity: Tau^2 = 952.38; Chi^2 = 27.48, df = 12 (P = 0.01); I^2 =56%							
Test for overall effect: Z = 3.52 (P = 0.00042)							
Total (95% CI)	**2979**		**2975**			**100.0 %**	**-36.33 [-51.86, -20.80]**
Heterogeneity: Tau^2 = 632.63; Chi^2 = 51.18, df = 23 (P = 0.00064); I^2 =55%							
Test for overall effect: Z = 4.59 (P < 0.00001)							

-100 -50 0 50 100

Favours BI Favours control

Analysis 1.5. Comparison 1 Brief intervention vs. control, Outcome 5 Quantity of drinking (g/week) subgrouped by high/low treatment exposure.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 5 Quantity of drinking (g/week) subgrouped by high/low treatment exposure

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Low treatment exposure							
Lock 2006	36	128.64 (293.28)	42	156.8 (293.28)		1.3 %	-28.16 [-158.72, 102.40]
Richmond 1995	70	326 (211)	61	290 (208)		3.3 %	36.00 [-35.89, 107.89]
Ockene 1999	235	-73.34 (146.41)	210	-40.58 (146.41)		7.9 %	-32.76 [-60.01, -5.51]
Kunz 2004	90	200.62 (308.93)	104	234.35 (312.2)		2.5 %	-33.73 [-121.33, 53.87]
Scott 1991	80	245.38 (191.3)	70	310.64 (252.17)		3.2 %	-65.26 [-137.70, 7.18]
Fernandez 1997	38	-107.4 (370.4)	50	-64.6 (278.3)		1.1 %	-42.80 [-183.58, 97.98]
Diez 2002	206	293.74 (186.21)	186	302.74 (163.01)		6.9 %	-9.00 [-43.57, 25.57]
Maisto 2001	74	133.98 (147.52)	85	147.33 (147.72)		5.5 %	-13.35 [-59.35, 32.65]
Heather 1987	29	252.55 (156.37)	62	317.76 (246.68)		2.6 %	-65.21 [-148.93, 18.51]
Senft 1997	196	140.95 (177.31)	215	160.7 (177.31)		6.9 %	-19.75 [-54.07, 14.57]

-100 -50 0 50 100

Favours BI Favours control

(*Continued* . . .)

(. . . *Continued*)

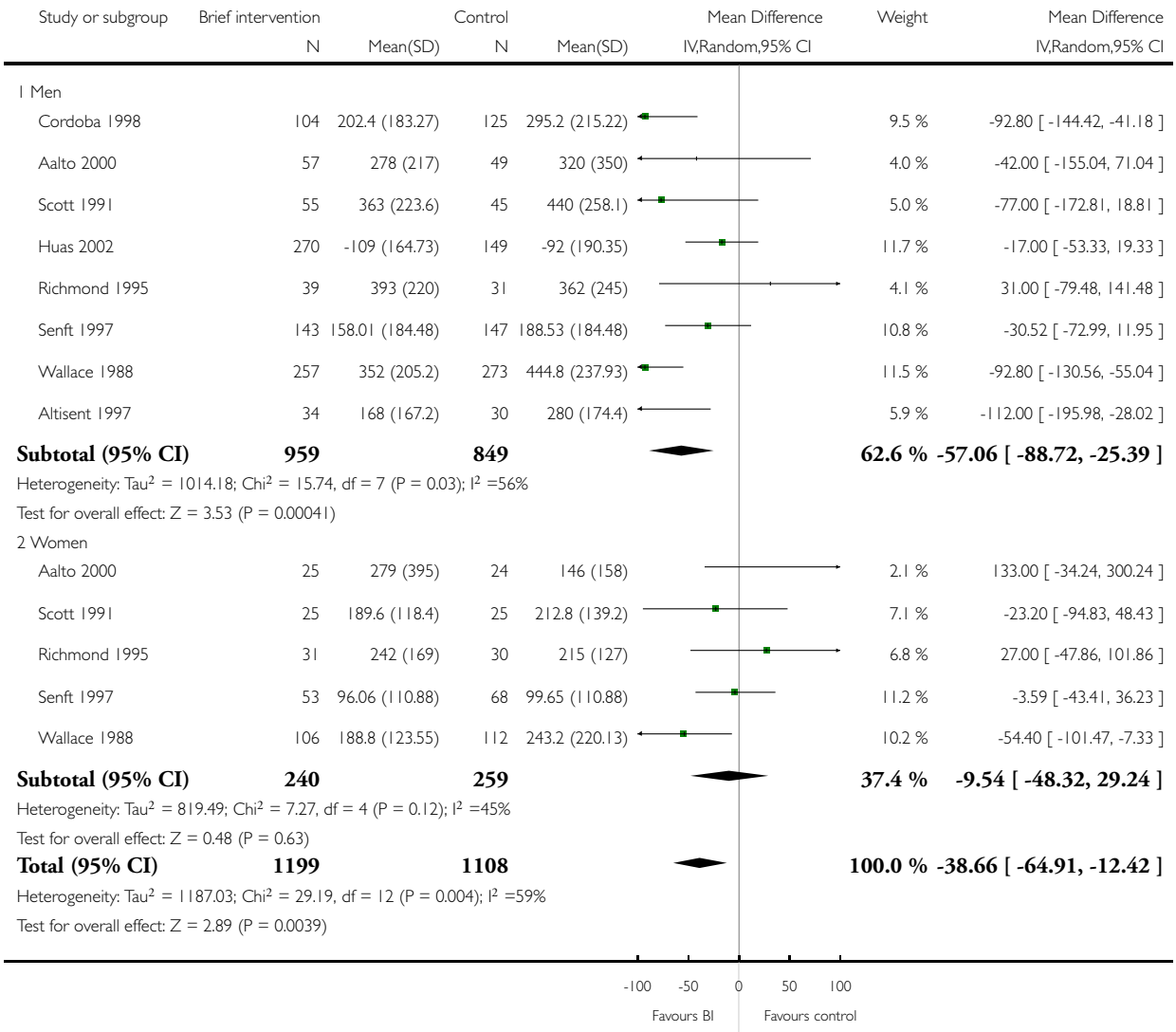
Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Subtotal (95% CI)	**1054**		**1085**			**41.1 %**	**-22.88 [-38.18, -7.58]**
Heterogeneity: Tau^2 = 0.0; Chi^2 = 6.34, df = 9 (P = 0.71); I^2 =0.0%							
Test for overall effect: Z = 2.93 (P = 0.0034)							
2 High treatment exposure							
Fleming 1999	78	119.04 (83.64)	67	195.24 (146.04)		6.2 %	-76.20 [-115.79, -36.61]
Aalto 2000	82	278.3 (280.69)	73	262.79 (299.4)		2.3 %	15.51 [-76.19, 107.21]
Huas 2002	270	-109 (164.73)	149	-92 (190.35)		6.6 %	-17.00 [-53.33, 19.33]
Cordoba 1998	104	202.4 (183.27)	125	295.2 (215.22)		4.9 %	-92.80 [-144.42, -41.18]
Wallace 1988	363	304.34 (184.94)	385	386.15 (230.97)		7.5 %	-81.81 [-111.71, -51.91]
Crawford 2004	189	457.6 (547.2)	195	566.4 (710.4)		1.3 %	-108.80 [-235.40, 17.80]
Fleming 2004	81	57.64 (106.39)	70	65.99 (74.34)		7.6 %	-8.35 [-37.33, 20.63]
Gentillelo 1999	194	-254.43 (601.47)	215	-78.2 (992.56)		0.9 %	-176.23 [-333.60, -18.86]
Romelsjo 1989	36	-34.86 (209.04)	36	42.64 (202.26)		2.2 %	-77.50 [-172.52, 17.52]
Curry 2003	100	108.5 (98.5)	122	110.9 (93.25)		8.1 %	-2.40 [-27.83, 23.03]
Altisent 1997	34	168 (167.2)	30	280 (174.4)		2.6 %	-112.00 [-195.98, -28.02]
Fleming 1997	353	137.76 (135.72)	370	185.52 (155.16)		8.7 %	-47.76 [-68.98, -26.54]
Subtotal (95% CI)	**1884**		**1837**			**58.9 %**	**-51.02 [-75.16, -26.88]**
Heterogeneity: Tau^2 = 1027.96; Chi^2 = 38.56, df = 11 (P = 0.00006); I^2 =71%							
Test for overall effect: Z = 4.14 (P = 0.000034)							
Total (95% CI)	**2938**		**2922**			**100.0 %**	**-38.42 [-54.16, -22.67]**
Heterogeneity: Tau^2 = 628.95; Chi^2 = 48.60, df = 21 (P = 0.00057); I^2 =57%							
Test for overall effect: Z = 4.78 (P < 0.00001)							

Analysis 1.6. Comparison 1 Brief intervention vs. control, Outcome 6 Quantity of drinking (g/week) subgrouped by gender.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 6 Quantity of drinking (g/week) subgrouped by gender



Analysis 1.7. Comparison 1 Brief intervention vs. control, Outcome 7 Quantity of drinking (g/week) subgrouped by gender, excluding trials of men only.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 7 Quantity of drinking (g/week) subgrouped by gender, excluding trials of men only

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Men							
Aalto 2000	97	290 (273)	84	338 (371)		6.8 %	-48.00 [-144.16, 48.16]
Scott 1991	55	363 (223.6)	45	440 (258.1)		6.8 %	-77.00 [-172.81, 18.81]
Richmond 1995	39	393 (220)	31	362 (245)		5.6 %	31.00 [-79.48, 141.48]
Senft 1997	143	158.01 (184.48)	147	188.53 (184.48)		14.6 %	-30.52 [-72.99, 11.95]
Wallace 1988	257	352 (205.2)	273	444.8 (237.93)		15.6 %	-92.80 [-130.56, -55.04]
Subtotal (95% CI)	**591**		**580**			**49.4 %**	**-53.03 [-93.32, -12.74]**
Heterogeneity: Tau² = 901.38; Chi² = 7.54, df = 4 (P = 0.11); I² =47%							
Test for overall effect: Z = 2.58 (P = 0.0099)							
2 Women							
Aalto 2000	25	279 (395)	24	146 (158)		2.9 %	133.00 [-34.24, 300.24]
Scott 1991	25	189.6 (118.4)	25	212.8 (139.2)		9.6 %	-23.20 [-94.83, 48.43]
Richmond 1995	31	242 (169)	30	215 (127)		9.2 %	27.00 [-47.86, 101.86]
Senft 1997	53	96.06 (110.88)	68	99.65 (110.88)		15.2 %	-3.59 [-43.41, 36.23]
Wallace 1988	106	188.8 (123.55)	112	243.2 (220.13)		13.8 %	-54.40 [-101.47, -7.33]
Subtotal (95% CI)	**240**		**259**			**50.6 %**	**-9.54 [-48.32, 29.24]**
Heterogeneity: Tau² = 819.49; Chi² = 7.27, df = 4 (P = 0.12); I² =45%							
Test for overall effect: Z = 0.48 (P = 0.63)							
Total (95% CI)	**831**		**839**			**100.0 %**	**-29.61 [-60.28, 1.05]**
Heterogeneity: Tau² = 1202.84; Chi² = 20.80, df = 9 (P = 0.01); I² =57%							
Test for overall effect: Z = 1.89 (P = 0.058)							

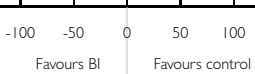
Analysis 1.8. Comparison 1 Brief intervention vs. control, Outcome 8 Quantity of drinking (g/week) subgrouped by cluster/individual randomisation.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 8 Quantity of drinking (g/week) subgrouped by cluster/individual randomisation

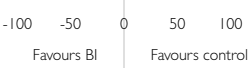
Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Cluster randomised							
Lock 2006	36	128.64 (293.28)	42	156.8 (293.28)		1.3 %	-28.16 [-158.72, 102.40]
Cordoba 1998	104	202.4 (183.27)	125	295.2 (215.22)		4.9 %	-92.80 [-144.42, -41.18]
Ockene 1999	235	-73.34 (146.41)	210	-40.58 (146.41)		7.9 %	-32.76 [-60.01, -5.51]
Huas 2002	270	-109 (164.73)	149	-92 (190.35)		6.6 %	-17.00 [-53.33, 19.33]
Subtotal (95% CI)	**645**		**526**			**20.7 %**	**-40.98 [-72.72, -9.24]**
Heterogeneity: Tau² = 466.96; Chi² = 5.76, df = 3 (P = 0.12); I² =48%							
Test for overall effect: Z = 2.53 (P = 0.011)							
2 Individual randomised							
Scott 1991	80	245.38 (191.3)	70	310.64 (252.17)		3.2 %	-65.26 [-137.70, 7.18]
Aalto 2000	82	278.3 (280.69)	73	262.79 (299.4)		2.3 %	15.51 [-76.19, 107.21]
Crawford 2004	189	457.6 (547.2)	195	566.4 (710.4)		1.3 %	-108.80 [-235.40, 17.80]
Diez 2002	206	293.74 (186.21)	186	302.74 (163.01)		6.9 %	-9.00 [-43.57, 25.57]
Fleming 1997	353	137.76 (135.72)	370	185.52 (155.16)		8.7 %	-47.76 [-68.98, -26.54]
Richmond 1995	70	326 (211)	61	290 (208)		3.2 %	36.00 [-35.89, 107.89]
Wallace 1988	448	304.64 (205.79)	459	384.63 (251.98)		7.5 %	-79.99 [-109.90, -50.08]
Fleming 1999	78	119.04 (83.64)	67	195.24 (146.04)		6.2 %	-76.20 [-115.79, -36.61]
Senft 1997	196	140.95 (177.31)	215	160.7 (177.31)		6.9 %	-19.75 [-54.07, 14.57]
Altisent 1997	34	168 (167.2)	30	280 (174.4)		2.6 %	-112.00 [-195.98, -28.02]
Curry 2003	100	108.5 (98.5)	122	110.9 (93.25)		8.1 %	-2.40 [-27.83, 23.03]
Heather 1987	29	252.55 (156.37)	62	317.76 (246.68)		2.6 %	-65.21 [-148.93, 18.51]
Fernandez 1997	38	-107.4 (370.4)	50	-64.6 (278.3)		1.1 %	-42.80 [-183.58, 97.98]
Gentillelo 1999	194	-254.43 (601.47)	215	-78.2 (992.56)		0.9 %	-176.23 [-333.60, -18.86]
Kunz 2004	90	200.62 (308.93)	104	234.35 (312.2)		2.4 %	-33.73 [-121.33, 53.87]
Maisto 2001	74	133.98 (147.52)	85	147.33 (147.72)		5.5 %	-13.35 [-59.35, 32.65]
Fleming 2004	81	57.64 (106.39)	70	65.99 (74.34)		7.6 %	-8.35 [-37.33, 20.63]



(*Continued . . .*)

(. . . *Continued*)

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Romelsjo 1989	36	-34.86 (209.04)	36	42.64 (202.26)		2.1 %	-77.50 [-172.52, 17.52]
Subtotal (95% CI)	**2378**		**2470**			**79.3 %**	**-37.80 [-56.33, -19.26]**
Heterogeneity: Tau² = 738.21; Chi² = 42.07, df = 17 (P = 0.00066); I² =60%							
Test for overall effect: Z = 4.00 (P = 0.000064)							
Total (95% CI)	**3023**		**2996**			**100.0 %**	**-38.22 [-53.85, -22.60]**
Heterogeneity: Tau² = 612.55; Chi² = 47.87, df = 21 (P = 0.00072); I² =56%							
Test for overall effect: Z = 4.79 (P < 0.00001)							



Analysis 1.9. Comparison 1 Brief intervention vs. control, Outcome 9 Quantity of drinking (g/week) subgrouped by cluster/individual randomisation, varying imputed ICC.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 9 Quantity of drinking (g/week) subgrouped by cluster/individual randomisation, varying imputed ICC

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Cluster randomised							
Lock 2006	36	128.64 (293.28)	42	156.8 (293.28)		1.3 %	-28.16 [-158.72, 102.40]
Cordoba 1998	104	202.4 (264.35)	125	295.2 (310.44)		3.1 %	-92.80 [-167.25, -18.35]
Ockene 1999	235	-73.34 (146.41)	210	-40.58 (146.41)		8.3 %	-32.76 [-60.01, -5.51]
Huas 2002	270	-109 (218.61)	149	-92 (252.62)		5.4 %	-17.00 [-65.22, 31.22]
Subtotal (95% CI)	**645**		**526**			**18.0 %**	**-34.64 [-56.91, -12.36]**
Heterogeneity: Tau² = 0.0; Chi² = 2.89, df = 3 (P = 0.41); I² =0.0%							
Test for overall effect: Z = 3.05 (P = 0.0023)							
2 Individual randomised							
Scott 1991	80	245.38 (191.3)	70	310.64 (252.17)		3.3 %	-65.26 [-137.70, 7.18]
Aalto 2000	82	278.3 (280.69)	73	262.79 (299.4)		2.3 %	15.51 [-76.19, 107.21]
Crawford 2004	189	457.6 (547.2)	195	566.4 (710.4)		1.3 %	-108.80 [-235.40, 17.80]
Diez 2002	206	293.74 (186.21)	186	302.74 (163.01)		7.2 %	-9.00 [-43.57, 25.57]
Fleming 1997	353	137.76 (135.72)	370	185.52 (155.16)		9.2 %	-47.76 [-68.98, -26.54]

(*Continued* . . .)

(. . . *Continued*)

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Richmond 1995	70	326 (211)	61	290 (208)		3.3 %	36.00 [-35.89, 107.89]
Wallace 1988	448	304.64 (205.79)	459	384.63 (251.98)		7.8 %	-79.99 [-109.90, -50.08]
Fleming 1999	78	119.04 (83.64)	67	195.24 (146.04)		6.4 %	-76.20 [-115.79, -36.61]
Senft 1997	196	140.95 (177.31)	215	160.7 (177.31)		7.2 %	-19.75 [-54.07, 14.57]
Altisent 1997	34	168 (167.2)	30	280 (174.4)		2.6 %	-112.00 [-195.98, -28.02]
Curry 2003	100	108.5 (98.5)	122	110.9 (93.25)		8.5 %	-2.40 [-27.83, 23.03]
Heather 1987	29	252.55 (156.37)	62	317.76 (246.68)		2.6 %	-65.21 [-148.93, 18.51]
Fernandez 1997	38	-107.4 (370.4)	50	-64.6 (278.3)		1.1 %	-42.80 [-183.58, 97.98]
Gentillelo 1999	194	-254.43 (601.47)	215	-78.2 (992.56)		0.9 %	-176.23 [-333.60, -18.86]
Kunz 2004	90	200.62 (308.93)	104	234.35 (312.2)		2.5 %	-33.73 [-121.33, 53.87]
Maisto 2001	74	133.98 (147.52)	85	147.33 (147.72)		5.6 %	-13.35 [-59.35, 32.65]
Fleming 2004	81	57.64 (106.39)	70	65.99 (74.34)		8.0 %	-8.35 [-37.33, 20.63]
Romelsjo 1989	36	-34.86 (209.04)	36	42.64 (202.26)		2.2 %	-77.50 [-172.52, 17.52]
Subtotal (95% CI)	**2378**		**2470**			**82.0 %**	**-37.80 [-56.33, -19.26]**
Heterogeneity: Tau^2 = 738.21; Chi^2 = 42.07, df = 17 (P = 0.00066); I^2 =60%							
Test for overall effect: Z = 4.00 (P = 0.000064)							
Total (95% CI)	**3023**		**2996**			**100.0 %**	**-37.38 [-52.96, -21.80]**
Heterogeneity: Tau^2 = 572.61; Chi^2 = 44.95, df = 21 (P = 0.002); I^2 =53%							
Test for overall effect: Z = 4.70 (P < 0.00001)							

Analysis 1.10. Comparison 1 Brief intervention vs. control, Outcome 10 Frequency of drinking (no. binges/wk).

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 10 Frequency of drinking (no. binges/wk)

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Effectiveness							
Fleming 1997	353	0.71 (1.21)	370	0.97 (1.27)		44.8 %	-0.26 [-0.44, -0.08]
Subtotal (95% CI)	**353**		**370**			**44.8 %**	**-0.26 [-0.44, -0.08]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.82 (P = 0.0048)							
2 Efficacy							
Fleming 1999	78	0.42 (1.37)	67	1.24 (2.13)		18.0 %	-0.82 [-1.41, -0.23]
Fleming 2004	70	0.28 (0.9)	65	0.3 (0.74)		37.2 %	-0.02 [-0.30, 0.26]
Subtotal (95% CI)	**148**		**132**			**55.2 %**	**-0.38 [-1.15, 0.40]**
Heterogeneity: $Tau^2 = 0.26$; $Chi^2 = 5.73$, df = 1 (P = 0.02); $I^2 = 83\%$							
Test for overall effect: Z = 0.94 (P = 0.35)							
Total (95% CI)	**501**		**502**			**100.0 %**	**-0.27 [-0.58, 0.04]**
Heterogeneity: $Tau^2 = 0.05$; $Chi^2 = 6.10$, df = 2 (P = 0.05); $I^2 = 67\%$							
Test for overall effect: Z = 1.71 (P = 0.086)							

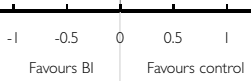
Analysis 1.11. Comparison 1 Brief intervention vs. control, Outcome 11 Frequency of drinking (no. days drinking/wk) subgrouped by effectiveness/efficacy.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 11 Frequency of drinking (no. days drinking/wk) subgrouped by effectiveness/efficacy

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Effectiveness							
Aalto 2000	84	2.55 (1.8)	76	2.17 (2.09)		30.7 %	0.38 [-0.23, 0.99]
Curry 2003	100	3.56 (2.4)	122	3.55 (2.44)		29.1 %	0.01 [-0.63, 0.65]
Subtotal (95% CI)	**184**		**198**			**59.8 %**	**0.20 [-0.24, 0.64]**
Heterogeneity: $Tau^2 = 0.0$; $Chi^2 = 0.68$, df = 1 (P = 0.41); $I^2 = 0.0\%$							
Test for overall effect: Z = 0.91 (P = 0.36)							
2 Efficacy							
Senft 1997	198	2.7 (2.3)	215	3.1 (2.3)		40.2 %	-0.40 [-0.84, 0.04]
Subtotal (95% CI)	**198**		**215**			**40.2 %**	**-0.40 [-0.84, 0.04]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.77 (P = 0.077)							
Total (95% CI)	**382**		**413**			**100.0 %**	**-0.04 [-0.51, 0.43]**
Heterogeneity: $Tau^2 = 0.09$; $Chi^2 = 4.27$, df = 2 (P = 0.12); $I^2 = 53\%$							
Test for overall effect: Z = 0.17 (P = 0.86)							

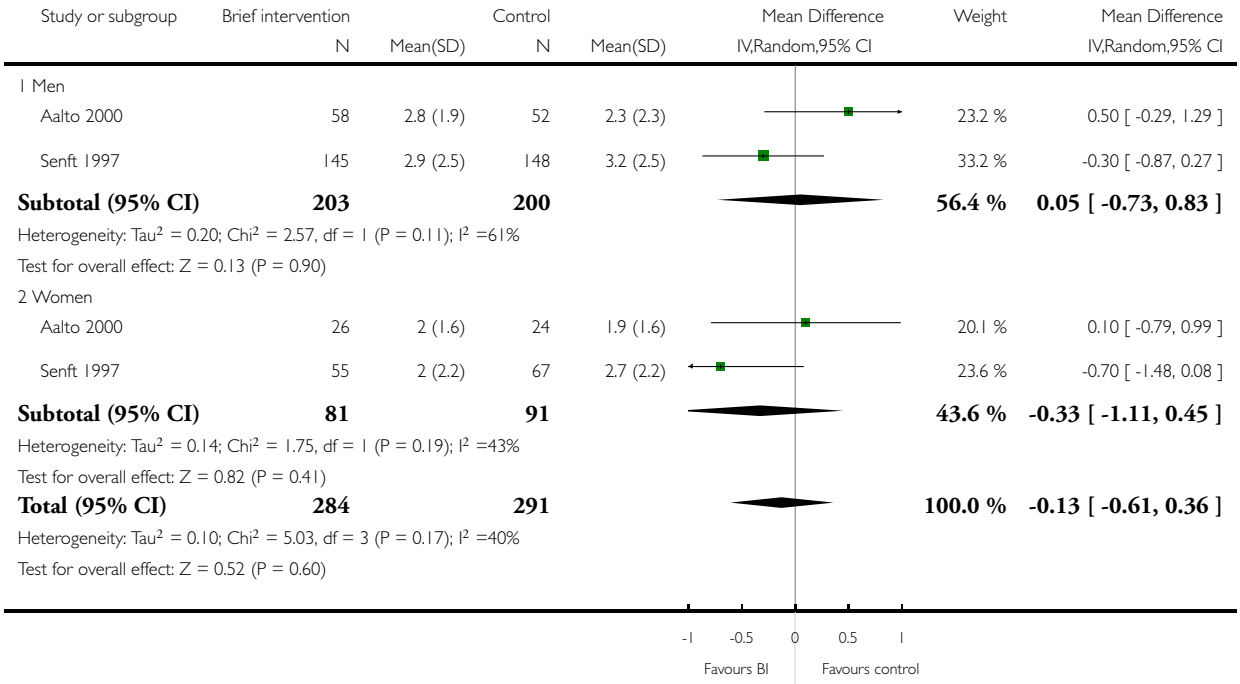


Analysis 1.12. Comparison 1 Brief intervention vs. control, Outcome 12 Frequency of drinking (no. days drinking/wk) subgrouped by gender.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 12 Frequency of drinking (no. days drinking/wk) subgrouped by gender



Analysis 1.13. Comparison 1 Brief intervention vs. control, Outcome 13 Intensity of drinking (g/drinking day) subgrouped by effectiveness/efficacy.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 13 Intensity of drinking (g/drinking day) subgrouped by effectiveness/efficacy

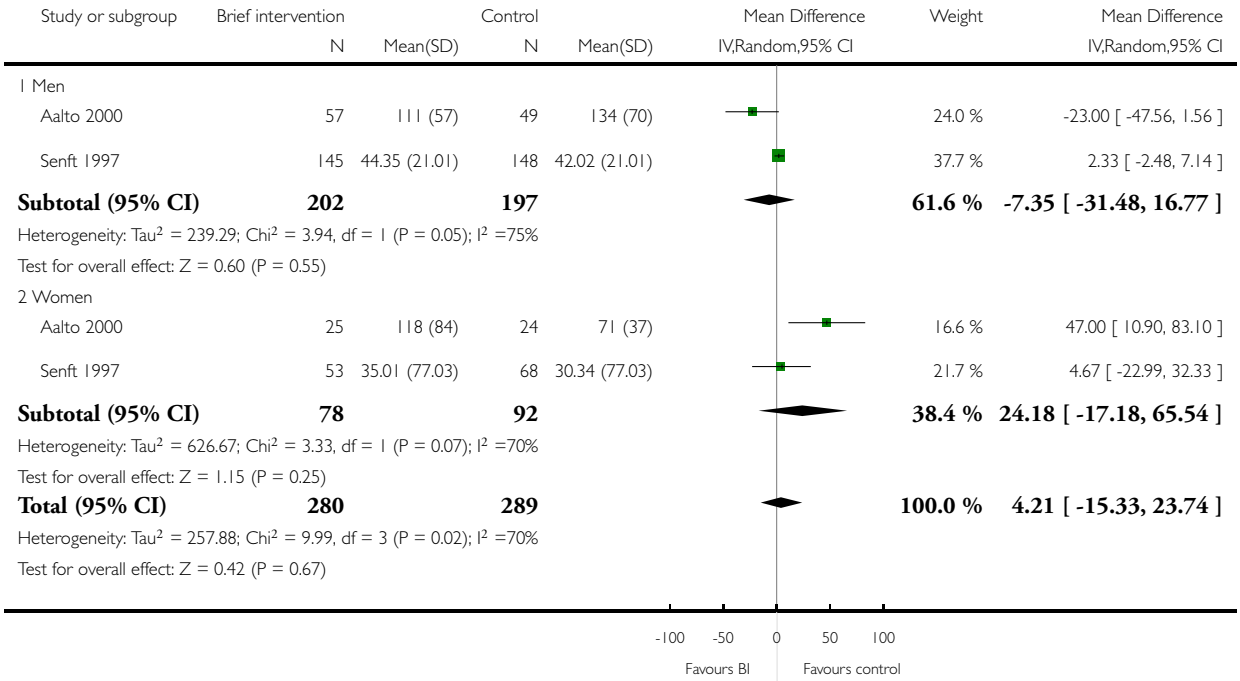
Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Effectiveness							
Aalto 2000	82	113.13 (65.86)	73	113.29 (60.86)		7.3 %	-0.16 [-20.11, 19.79]
Crawford 2004	189	104.8 (88.8)	195	128 (124.8)		6.3 %	-23.20 [-44.81, -1.59]
Curry 2003	100	37.78 (16.49)	122	42 (17.48)		43.2 %	-4.22 [-8.70, 0.26]
Subtotal (95% CI)	**371**		**390**			**56.8 %**	**-6.50 [-16.17, 3.16]**
Heterogeneity: Tau^2 = 31.58; Chi^2 = 3.06, df = 2 (P = 0.22); I^2 =35%							
Test for overall effect: Z = 1.32 (P = 0.19)							
2 Efficacy							
Maisto 2001	74	47.85 (37.35)	85	52.52 (40.85)		16.2 %	-4.67 [-16.83, 7.49]
Senft 1997	198	42.02 (42.02)	216	38.51 (42.02)		27.1 %	3.51 [-4.59, 11.61]
Subtotal (95% CI)	**272**		**301**			**43.2 %**	**0.73 [-6.87, 8.32]**
Heterogeneity: Tau^2 = 5.67; Chi^2 = 1.20, df = 1 (P = 0.27); I^2 =17%							
Test for overall effect: Z = 0.19 (P = 0.85)							
Total (95% CI)	**643**		**691**			**100.0 %**	**-3.10 [-8.85, 2.64]**
Heterogeneity: Tau^2 = 14.66; Chi^2 = 6.27, df = 4 (P = 0.18); I^2 =36%							
Test for overall effect: Z = 1.06 (P = 0.29)							

Analysis 1.14. Comparison 1 Brief intervention vs. control, Outcome 14 Intensity of drinking (g/drinking day) subgrouped by gender.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 14 Intensity of drinking (g/drinking day) subgrouped by gender



Analysis 1.15. Comparison 1 Brief intervention vs. control, Outcome 15 Laboratory markers - GGT (IU/l).

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 15 Laboratory markers - GGT (IU/l)

Study or subgroup	Breif intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Aalto 2000	96	79.68 (90.89)	92	66.44 (76.89)		1.3 %	13.24 [-10.79, 37.27]
Romelsjo 1989	36	-2.81 (11.94)	36	-1.39 (15.65)		18.7 %	-1.42 [-7.85, 5.01]
Wallace 1988	363	22.07 (20.63)	385	23.35 (22.69)		80.0 %	-1.28 [-4.38, 1.82]
Total (95% CI)	**495**		**513**			**100.0 %**	**-1.11 [-3.89, 1.67]**

Heterogeneity: Tau² = 0.0; Chi² = 1.39, df = 2 (P = 0.50); I² =0.0%

Test for overall effect: Z = 0.78 (P = 0.43)

Analysis 1.16. Comparison 1 Brief intervention vs. control, Outcome 16 Laboratory markers - GGT (IU/l), subgrouped by gender.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

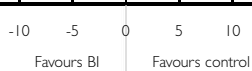
Outcome: 16 Laboratory markers - GGT (IU/l), subgrouped by gender

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Men							
Aalto 2000	69	89.1 (102.7)	65	77.3 (89.1)		1.3 %	11.80 [-20.70, 44.30]
Wallace 1988	257	25.4 (22.44)	273	27.8 (26.44)		43.1 %	-2.40 [-6.57, 1.77]
Subtotal (95% CI)	**326**		**338**			**44.4 %**	**-2.17 [-6.30, 1.96]**

Heterogeneity: Tau² = 0.0; Chi² = 0.72, df = 1 (P = 0.40); I² =0.0%

Test for overall effect: Z = 1.03 (P = 0.30)

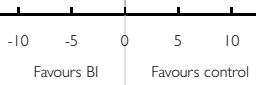
2 Women							
Aalto 2000	27	55.6 (51)	27	40.3 (33.9)		2.6 %	15.30 [-7.80, 38.40]
Wallace 1988	106	14 (15.44)	112	12.5 (8.47)		52.9 %	1.50 [-1.83, 4.83]
Subtotal (95% CI)	**133**		**139**			**55.6 %**	**3.47 [-5.99, 12.94]**



(*Continued . . .*)

(. . . *Continued*)

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Heterogeneity: Tau² = 24.33; Chi² = 1.34, df = 1 (P = 0.25); I² =26%							
Test for overall effect: Z = 0.72 (P = 0.47)							
Total (95% CI)	**459**		**477**			**100.0 %**	**0.32 [-3.48, 4.13]**
Heterogeneity: Tau² = 4.23; Chi² = 4.21, df = 3 (P = 0.24); I² =29%							
Test for overall effect: Z = 0.17 (P = 0.87)							



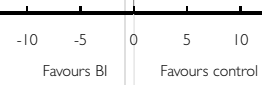
Analysis 1.17. Comparison 1 Brief intervention vs. control, Outcome 17 Laboratory markers - MCV (fl).

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 17 Laboratory markers - MCV (fl)

Study or subgroup	Brief intervention N	Mean(SD)	Control N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
1 Men							
Seppa 1992	35	101.9 (5.9)	46	101.1 (5.4)		76.9 %	0.80 [-1.70, 3.30]
Subtotal (95% CI)	**35**		**46**			**76.9 %**	**0.80 [-1.70, 3.30]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.63 (P = 0.53)							
2 Women							
Seppa 1992	6	98.5 (4.4)	8	98.5 (4.2)		23.1 %	0.0 [-4.57, 4.57]
Subtotal (95% CI)	**6**		**8**			**23.1 %**	**0.0 [-4.57, 4.57]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.0 (P = 1.0)							
Total (95% CI)	**41**		**54**			**100.0 %**	**0.62 [-1.58, 2.81]**
Heterogeneity: Tau² = 0.0; Chi² = 0.09, df = 1 (P = 0.76); I² =0.0%							
Test for overall effect: Z = 0.55 (P = 0.58)							



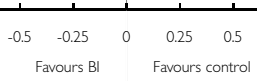
Analysis 1.18. Comparison 1 Brief intervention vs. control, Outcome 18 Heavy drinkers.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

Outcome: 18 Heavy drinkers

Study or subgroup	Brief intervention n/N	Control n/N	Risk Difference M-H,Random,95% CI	Risk Difference M-H,Random,95% CI
Cordoba 1998	34/104	70/125		-0.23 [-0.36, -0.11]
Diez 2002	96/206	94/186		-0.04 [-0.14, 0.06]
Fleming 1997	60/353	119/370		-0.15 [-0.21, -0.09]
Fleming 1999	12/78	23/67		-0.19 [-0.33, -0.05]
Altisent 1997	6/34	16/30		-0.36 [-0.58, -0.14]
Curry 2003	33/99	47/122		-0.05 [-0.18, 0.07]
Fernandez 1997	15/38	26/50		-0.13 [-0.33, 0.08]
Fleming 2004	20/81	28/70		-0.15 [-0.30, 0.00]
Wallace 1988	247/363	337/385		-0.19 [-0.25, -0.14]



Analysis 1.19. Comparison 1 Brief intervention vs. control, Outcome 19 Binge drinkers.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

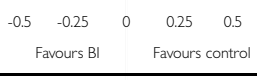
Outcome: 19 Binge drinkers

Study or subgroup	Brief intervention n/N	Control n/N	Risk Difference M-H,Random,95% CI	Weight	Risk Difference M-H,Random,95% CI
Fleming 1997	105/188	186/261		30.8 %	-0.15 [-0.24, -0.06]
Fleming 1999	24/78	33/67		17.2 %	-0.18 [-0.34, -0.03]
Curry 2003	12/100	17/122		31.1 %	-0.02 [-0.11, 0.07]
Kunz 2004	53/90	72/104		20.8 %	-0.10 [-0.24, 0.03]
Total (95% CI)	**456**	**554**		**100.0 %**	**-0.11 [-0.19, -0.03]**

Total events: 194 (Brief intervention), 308 (Control)

Heterogeneity: $Tau^2 = 0.00$; $Chi^2 = 6.18$, df = 3 (P = 0.10); $I^2 = 51\%$

Test for overall effect: Z = 2.59 (P = 0.0095)



Analysis 1.20. Comparison 1 Brief intervention vs. control, Outcome 20 Loss to follow-up in assessment of quantity of alcohol consumed.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 1 Brief intervention vs. control

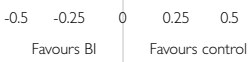
Outcome: 20 Loss to follow-up in assessment of quantity of alcohol consumed

Study or subgroup	Brief intervention n/N	Control n/N	Risk Difference M-H,Random,95% CI	Weight	Risk Difference M-H,Random,95% CI
1 Effectiveness trials					
Lock 2006	31/67	18/60		2.0 %	0.16 [0.00, 0.33]
Ockene 1999	39/274	46/256		7.1 %	-0.04 [-0.10, 0.03]
Aalto 2000	55/137	55/128		3.4 %	-0.03 [-0.15, 0.09]
Crawford 2004	98/287	117/312		5.9 %	-0.03 [-0.11, 0.04]
Diez 2002	49/255	43/229		6.4 %	0.00 [-0.07, 0.07]
Fleming 1997	39/392	12/382		9.9 %	0.07 [0.03, 0.10]
Scott 1991	33/113	43/113		3.3 %	-0.09 [-0.21, 0.03]
Wallace 1988	87/450	74/459		8.3 %	0.03 [-0.02, 0.08]
Richmond 1995	70/96	61/93		2.9 %	0.07 [-0.06, 0.20]
Curry 2003	66/166	45/167		4.3 %	0.13 [0.03, 0.23]
Subtotal (95% CI)	**2237**	**2199**		**53.6 %**	**0.02 [-0.02, 0.06]**
Total events: 567 (Brief intervention), 514 (Control)					
Heterogeneity: $Tau^2 = 0.00$; $Chi^2 = 24.33$, df = 9 (P = 0.004); $I^2 = 63\%$					
Test for overall effect: Z = 1.10 (P = 0.27)					
2 Efficacy trials					
Chang 1997	1/12	4/12		0.7 %	-0.25 [-0.56, 0.06]
Fleming 1999	9/87	4/71		5.4 %	0.05 [-0.04, 0.13]
Senft 1997	64/260	41/256		6.5 %	0.09 [0.02, 0.15]
Altisent 1997	20/54	15/45		1.6 %	0.04 [-0.15, 0.23]
Heather 1987	5/34	8/70		2.7 %	0.03 [-0.11, 0.17]
Fernandez 1997	29/67	35/85		2.2 %	0.02 [-0.14, 0.18]
Seppa 1992	51/92	32/86		2.6 %	0.18 [0.04, 0.33]
Tomson 1998	70/100	77/122		3.2 %	0.07 [-0.06, 0.19]
Gentillelo 1999	172/366	181/396		6.3 %	0.01 [-0.06, 0.08]

(*Continued* . . .)

(. . . *Continued*)

Study or subgroup	Brief intervention n/N	Control n/N	Risk Difference M-H,Random,95% CI	Weight	Risk Difference M-H,Random,95% CI
Kunz 2004	61/151	39/143		3.9 %	0.13 [0.02, 0.24]
Fleming 2004	11/92	5/75		5.1 %	0.05 [-0.03, 0.14]
Maisto 2001	26/100	15/100		3.8 %	0.11 [0.00, 0.22]
Romelsjo 1989	5/41	6/42		2.5 %	-0.02 [-0.17, 0.12]
Subtotal (95% CI)	**1456**	**1503**		**46.4 %**	**0.06 [0.03, 0.09]**
Total events: 524 (Brief intervention), 462 (Control)					
Heterogeneity: Tau² = 0.00; Chi² = 13.13, df = 12 (P = 0.36); I² =9%					
Test for overall effect: Z = 3.70 (P = 0.00022)					
Total (95% CI)	**3693**	**3702**		**100.0 %**	**0.04 [0.01, 0.07]**
Total events: 1091 (Brief intervention), 976 (Control)					
Heterogeneity: Tau² = 0.00; Chi² = 38.80, df = 22 (P = 0.01); I² =43%					
Test for overall effect: Z = 3.00 (P = 0.0027)					



Analysis 2.1. Comparison 2 Extended vs. brief intervention, Outcome 1 Quantity of drinking (g/week).

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 2 Extended vs. brief intervention

Outcome: 1 Quantity of drinking (g/week)

Study or subgroup	Extended N	Mean(SD)	Brief N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Aalto 2000	70	221.31 (263.14)	82	278.3 (280.69)		15.6 %	-56.99 [-143.54, 29.56]
Israel 1996	35	142.84 (170.79)	38	232.58 (256.49)		11.9 %	-89.74 [-189.00, 9.52]
Maisto 2001	73	116.83 (140.06)	74	133.98 (147.52)		54.2 %	-17.15 [-63.65, 29.35]
Richmond 1995	66	331 (261)	70	326 (211)		18.3 %	5.00 [-75.05, 85.05]
Total (95% CI)	**244**		**264**			**100.0 %**	**-27.96 [-62.19, 6.26]**
Heterogeneity: Tau² = 0.0; Chi² = 2.78, df = 3 (P = 0.43); I² =0.0%							
Test for overall effect: Z = 1.60 (P = 0.11)							

Analysis 2.2. Comparison 2 Extended vs. brief intervention, Outcome 2 Quantity of drinking (g/week) using imputed values for participants lost to follow-up.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 2 Extended vs. brief intervention

Outcome: 2 Quantity of drinking (g/week) using imputed values for participants lost to follow-up

Study or subgroup	Extended N	Mean(SD)	Brief N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Aalto 2000	141	235.21 (268.15)	134	277.02 (291.06)		23.8 %	-41.81 [-108.05, 24.43]
Israel 1996	35	142.84 (170.79)	38	232.58 (256.49)		10.6 %	-89.74 [-189.00, 9.52]
Maisto 2001	73	115.54 (138.44)	74	132.51 (145.98)		49.3 %	-16.97 [-62.96, 29.02]
Richmond 1995	66	331 (261)	70	326 (211)		16.3 %	5.00 [-75.05, 85.05]
Total (95% CI)	**315**		**316**			**100.0 %**	**-27.01 [-59.31, 5.30]**

Heterogeneity: $Chi^2 = 2.52$, df = 3 (P = 0.47); $I^2 = 0.0\%$

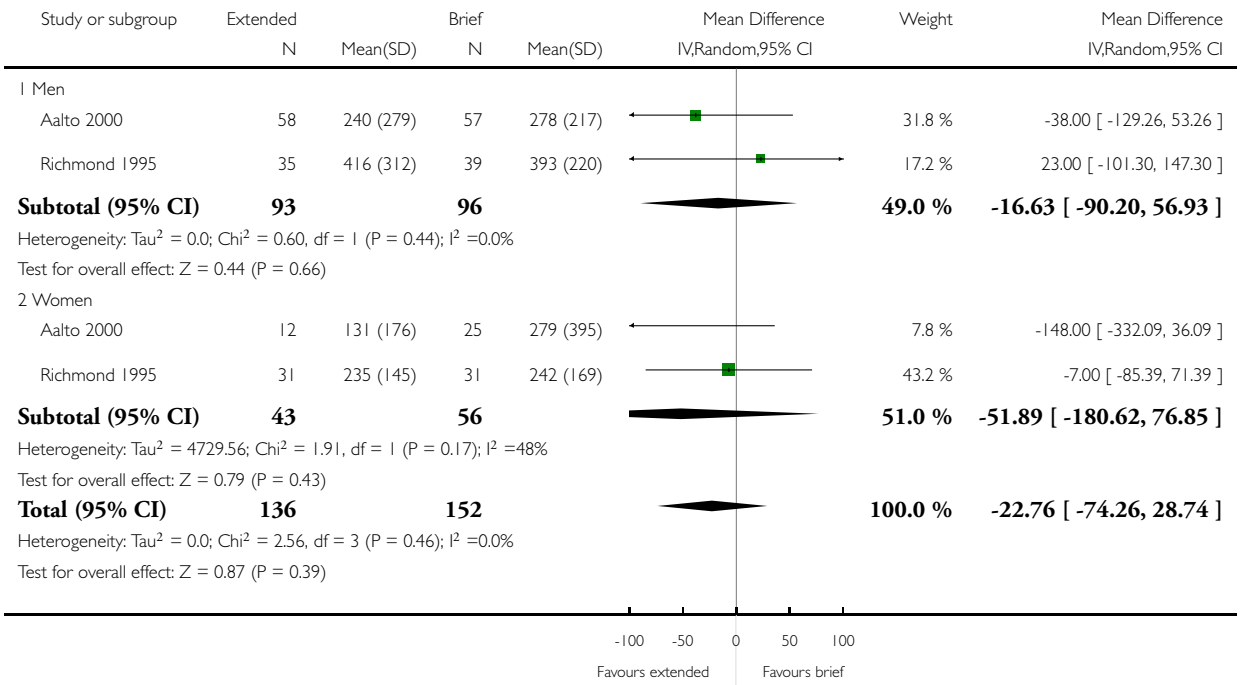
Test for overall effect: Z = 1.64 (P = 0.10)

Analysis 2.3. Comparison 2 Extended vs. brief intervention, Outcome 3 Quantity of drinking (g/week), subgrouped by gender.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 2 Extended vs. brief intervention

Outcome: 3 Quantity of drinking (g/week), subgrouped by gender

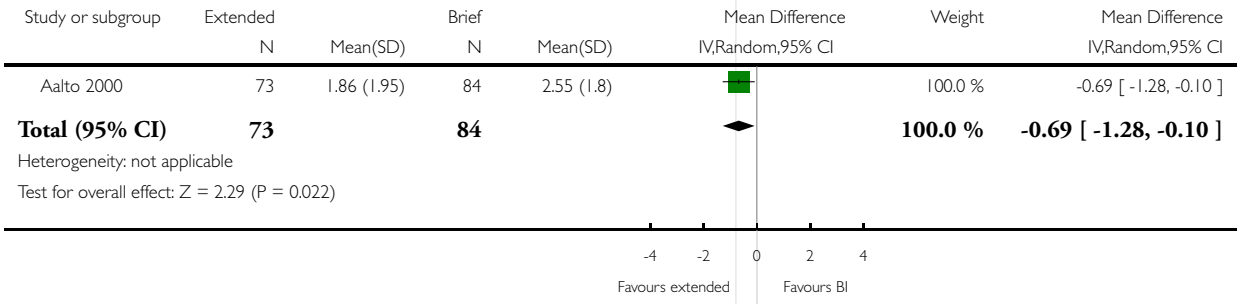


Analysis 2.4. Comparison 2 Extended vs. brief intervention, Outcome 4 Frequency of drinking (no. days drinking/week).

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 2 Extended vs. brief intervention

Outcome: 4 Frequency of drinking (no. days drinking/week)



Analysis 2.5. Comparison 2 Extended vs. brief intervention, Outcome 5 Intensity of drinking (g/drinking day).

Review: Effectiveness of brief alcohol interventions in primary care populations

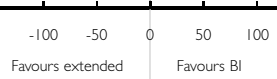
Comparison: 2 Extended vs. brief intervention

Outcome: 5 Intensity of drinking (g/drinking day)

Study or subgroup	Extended N	Mean(SD)	Brief N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Aalto 2000	70	131.77 (82.63)	82	113.13 (65.86)		35.4 %	18.64 [-5.40, 42.68]
Maisto 2001	73	46.68 (35.01)	74	47.85 (37.35)		64.6 %	-1.17 [-12.87, 10.53]
Total (95% CI)	**143**		**156**			**100.0 %**	**5.84 [-12.73, 24.40]**

Heterogeneity: Tau² = 103.18; Chi² = 2.11, df = 1 (P = 0.15); I² =53%

Test for overall effect: Z = 0.62 (P = 0.54)



Analysis 2.6. Comparison 2 Extended vs. brief intervention, Outcome 6 Laboratory markers - GGT (IU/l).

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 2 Extended vs. brief intervention

Outcome: 6 Laboratory markers - GGT (IU/l)

Study or subgroup	Extended N	Mean(SD)	Brief N	Mean(SD)	Mean Difference IV,Random,95% CI	Weight	Mean Difference IV,Random,95% CI
Aalto 2000	88	89.28 (109.08)	96	79.68 (90.89)		20.0 %	9.60 [-19.55, 38.75]
Israel 1996	32	41.7 (25.5)	27	47.4 (30.7)		80.0 %	-5.70 [-20.27, 8.87]
Total (95% CI)	**120**		**123**			**100.0 %**	**-2.64 [-15.67, 10.39]**

Heterogeneity: Tau² = 0.0; Chi² = 0.85, df = 1 (P = 0.36); I² =0.0%

Test for overall effect: Z = 0.40 (P = 0.69)

Analysis 2.7. Comparison 2 Extended vs. brief intervention, Outcome 7 Loss to follow-up in assessment of quantity of alcohol consumed.

Review: Effectiveness of brief alcohol interventions in primary care populations

Comparison: 2 Extended vs. brief intervention

Outcome: 7 Loss to follow-up in assessment of quantity of alcohol consumed

Study or subgroup	Extended n/N	Brief n/N	Risk Difference M-H,Random,95% CI	Weight	Risk Difference M-H,Random,95% CI
Aalto 2000	79/149	55/137		36.1 %	0.13 [0.01, 0.24]
Maisto 2001	28/101	26/100		33.6 %	0.02 [-0.11, 0.14]
Richmond 1995	30/96	32/93		30.3 %	-0.03 [-0.17, 0.10]
Total (95% CI)	**346**	**330**		**100.0 %**	**0.04 [-0.05, 0.14]**
Total events: 137 (Extended), 113 (Brief)					
Heterogeneity: $Tau^2 = 0.00$; $Chi^2 = 3.52$, df = 2 (P = 0.17); $I^2 = 43\%$					
Test for overall effect: Z = 0.89 (P = 0.38)					

APPENDICES

Appendix 1. MEDLINE Search strategy

1. family practice/
2. family pract$.tw.
3. general practice.sh.
4. general pract$.tw.
5. primary health care/
6. primary care/
7. community health services/
8. Community Care/
9. shared care.mp.
10. Patient Care/ or patient care team.mp.
11. family medicine/
12. family physician/
13. family phys$.tw.
14. exp alcohol/
15. alcohol$.tw.
16. 1 or 2 or 3 or 4 or 5 or 6 or 7 or 8 or 9 or 10 or 11 or 12 or 13
17. 14 or 15
18. 16 and 17
19. alcohol reduction.mp.
20. brief intervention.mp.
21. early intervention.mp.
22. minimal intervention.mp.
23. alcohol therapy.mp.

24. harm reduction,.mp.
25. screening.mp.
26. (counseling or counselling).mp.
27. controlled drinking.mp.
28. (brief counseling or brief counselling).mp.
29. physician based intervention.mp.
30. general practitioner intervention.mp.
31. secondary prevention.mp.
32. general practitioner's advice.mp.
33. brief physician-delivered counseling.mp.
34. brief nurse-delivered counseling.mp.
35. identification.mp.
36. intervention.mp.
37. 19 or 20 or 21 or 22 or 23 or 24 or 25 or 26 or 27 or 28 or 29 or 30 or 31 or 32 or 33 or 34 or 35 or 36
38. (alcohol or alcohol consumption).mp.
39. 37 and 38
40. 39 and 18
41. randomized controlled trial.mp.
42. controlled clinical trial.mp.
43. randomized controlled trials.mp.
44. random allocation.mp.
45. double blind method.mp.
46. single blind method.mp.
47. or/41-46
48. (animal not human).mp.
49. 47 not 48
50. clinical trial.mp.
51. exp clinical trials/
52. (clin$ adj2 trial$).ti,ab.
53. ((singl$ or doubl$ or trebl$ or tripl$) adj2 (blind$ or mask$)).ti,ab.
54. placebos.mp.
55. placebo$.ti,ab.
56. random$.ti,ab.
57. research design.mp.
58. or/50-57
59. 58 not 48
60. 59 not 49
61. comparative study.mp.
62. exp evaluation studies/
63. follow up studies.mp.
64. prospective studies.mp.
65. (control$ or prospectiv$ or volunteer$).ti,ab.
66. or/61-65
67. 66 not 48
68. 66 not (49 or 60)
69. 49 or 60 or 68
70. 69 and 40

FEEDBACK

Anders Beich, 27 May 2009

Summary

The effect of brief interventions for hazardous drinking among primary care patients has been investigated in several controlled trials and meta-analyses. In most trials accomplished during the last 25 years intervention groups, mostly men, have subsequently reported lower alcohol consumption than their matching controls at follow-up. Therefore, it is no surprise that Kaner et al. in their meta-analysis [1] conclude that brief alcohol intervention in primary care contexts results in significant reductions in weekly consumption at least for men.

The crucial point is whether these results represent effectiveness rather than efficacy [2], because if so then health care professionals should be able to contribute to less hazardous drinking habits among their patients by adopting this technology. One question to address could then be how the technology fits within the context of primary care and how these results could be used out there where health professionals meet patients on a continuous basis.

The Cochrane-analysis [1] aims to examine to which degree the included trials have reported results that are externally valid and it is claimed that ?The lack of evidence of any difference in outcomes between efficacy and effectiveness trials suggests that the current literature is relevant to routine primary care and we feel that the current body of brief alcohol intervention research is applicable to clinical practice. I find this part of the analysis/interpretation to be problematic and the methods they use for reaching this conclusion to be inappropriate. The strength of their argument is obviously weak and their way of reasoning is deceptive. Just because two objects have the same colour you can hardly conclude, that this suggests they can be used for a specific purpose, and feelings should in my opinion be kept out of the argumentation.

I find the analysis and the discussion of the meta-analysis results to have several serious weaknesses:

1) Which drinkers are we dealing with?

The heterogeneity among trial groups in relation to baseline consumption is substantial among the included studies. Average consumption ranges from about 17 drinks/week to 54, that is, from slightly hazardous drinking to obviously harmful consumption and possible dependency. The risk reduction by reducing from 54 to 49 drinks per week can hardly be comparable to a reduction from 17 to 12 drinks per week and the report lacks a qualified discussion of this issue.

2) Study quality analysis as an invocation

Although a quality analysis of the included studies is carried out, the results are not allowed to affect the calculation of average intervention effects: Poor quality studies are given the same weight as high quality studies. I am aware that this is often the case in meta-analyses, but when results rely on self-reports I believe this issue is at least worth a discussion, which leads to the next weakness

3) The ”Garbage in“ ”garbage out“ problem

The analysis is not concerned with the quality and quality control of data (self-reported drinking). Self-reported drinking has been shown to be influenced by situational and contextual matters and the impossibility of blinding of study subjects may constitute a major source of bias in several of the included studies and this is not discussed in the report. A sensitivity analysis focusing on effects in studies that had other more objective data available could have been attempted or at least discussed.

4) Biased effectiveness perspective

One of the aims of the work is to investigate to which degree the included trials can be characterized as effectiveness trials and thereby relevant to routine primary care. This is done by analysing to which degree the setting can be characterized as a clinical rather than a research setting (reasons for encounter, the advisor/interventionist and the character of the clinic, as well as support, help and supervision provided in clinical trials).

The analysis avoids to analyse to which degree the trial groups can be characterized as clinical rather than research groups, that is, no attempts were made to find out how representative the included individuals were in relation to how many were actually screened hazardous drinking positive in the first place. Because screening and brief intervention recommendations come together [3] brief intervention seems to involve some kind of screening as it did the included trials it should at least have been clarified how refined the trial groups were compared to the groups that screened positive at baseline.

An example of the problem: A heavy weight study in the present meta-analysis is a study by Fleming et al.[4], who screened 17,695 patients, found 2,925 (17%) to be hazardous drinkers, of which 774 were included in the trial after all. That is, 2,151 hazardous drinkers were lost before the trial started for reasons poorly described. It is indeed possible that the most interested (/ready /motivated)

drinkers were over represented in the trial groups. This may in more than one way lead to biased results and it is not justified to claim that the results do also count for the majority of hazardous drinkers who were excluded in this study.

This issue is not at all new to most of the Cochrane-group though they manage to maintain absolute silence about the lost-subjects-before-randomization issue and a meta-analysis on exactly that issue in 2003[5], one that caused them to react very strongly at the time and later on in public accuse the authors of the analysis for causing the death of hazardous drinkers because these discussions might delay implementation efforts that they were in charge of.

5) Conflict of interests

The trustworthiness of the interpretation by Kaner et al. is in my opinion severely weakened by the fact that the authors claim no conflict of interests when more of them are well known for their involvement in implementation activities regarding brief interventions for decades. It is an open question what should be included as conflict of interests, but when more of the authors, the first author inclusive, have had implementation of these brief intervention technologies as a main employment for years, it seems unlikely that this would not affect which questions they wish to answer and the basic choices they make in the process of a meta-analysis of the effectiveness the very technology.

When the authors state that they have made inquiries on non-published studies I find it strange that they do not mention a Danish pragmatic randomised trial [7] that was not yet published when they ended their literature search, but I have personally presented the results for at least three of the authors more than once, results indicating that brief intervention had no effect on men and might be worse than no intervention for women when it was put out in real life circumstances and all recommendations were followed. Also, other results from this Danish group of independent researchers indicating that screening based brief intervention is not compatible with the work of the general practitioner [8] and that for the documentation so far rest on highly refined trial groups [5] are carefully ignored in this Cochrane-paper.

This meta-analysis tells us that some hazardous drinking male patients in primary health care can benefit from a brief intervention. But we can not say anything about the proportion, how much they will reduce their drinking or how we should find them from this analysis. I find the approach of this meta-analysis to be selective and biased and the purpose of it to be doubtful.

I propose that The Cochrane Collaboration encourage future author groups to declare all possible conflict of interests, not just obviously commercial ones.

References

1. Kaner EF, Beyer F, Dickinson HO et al. Effectiveness of brief alcohol interventions in primary care populations. Cochrane Database Syst Rev 2007:CD004148.

2. Flay BR. Efficacy and effectiveness trials (and other phases of research) in the development of health promotion programs. Prev Med 1986;15:451-74.

3. Babor TF, Higgins-Biddle JC. Brief Intervention for Hazardous and Harmful Drinking - A Manual for Use in Primary Care. Geneva: World Health Organization, 2001: 1-29

4. Fleming MF, Barry KL, Manwell LB et al. Brief physician advice for problem alcohol drinkers - A randomised controlled trial in community-based primary care practices. JAMA 1997;277:1039-45.

5. Beich A, Thorsen T, Rollnick S. Screening in brief intervention trials targeting excessive drinkers in general practice: systematic review and meta-analysis. BMJ 2003;327:536-40.

6. Kaner EF, Lock CA, McAvoy BR, Heather N, Gilvarry E. A RCT of three training and support strategies to encourage implementation of screening and brief alcohol intervention by general practitioners. Br J GenPract 1999;49:699-703.

7. Beich A, Gannik D, Saelan H et al. Screening and Brief Intervention Targeting Risky Drinkers in Danish General Practice A Pragmatic Controlled Trial. Alcohol Alcohol 2007.

8. Beich A, Gannik D, Malterud K. Screening and brief intervention for excessive alcohol use: qualitative interview study of the experiences of general practitioners. BMJ 2002;325:870-3.

Reply

We are pleased that the commentator confirms the validity of our primary review finding that brief interventions are effective at reducing excessive alcohol consumption, particularly in men. We welcome critique of our approach in exploring the efficacy-effectiveness question indeed we pointed out in our discussion some weaknesses that we felt were inherent in our approach. Despite this, we felt it was worth trying to unravel the efficacy-effectiveness issue and believe our work has made a start, even if this is an imperfect approach. This said, we feel there is merit in trying to clarify important parameters such as whether if the interventions are delivered in typical practices by practitioners who deliver primary care as their main occupation and if the interventions are delivered to routinely presenting patients. Furthermore, we also tried to assess whether brief interventions were delivered within normal consultation times and if preparatory

training is what most practitioners are likely to be able access. These are just 4 of the 8 parameters we assessed trials on in our attempt to establish the interval or external validity of the trial design.

1) We have described the characteristics of subjects in the brief intervention trials and we state both narratively and statistically in the review that there is a great deal of heterogeneity in the field.

2) The extensive sensitivity analysis that we carried out deals with the range of well-accepted quality criteria for research trials. Studies that were lacking on various quality criteria were omitted from the numerous sensitivity analyses that we reported and the key findings of the review were not substantially altered by this process.

3) This review was based on a range of outcome measures reported in 29 trials, some of which were self-reported by patients and some of which were more objectively measured. Thomas Greenfield and Lorraine Midanik have published a number of papers on the issue of self-report as a measure of alcohol use and have found this to be a valid approach. Furthermore, Babor and colleagues have confirmed that self-reported data if collected in the right way are sufficiently valid for research and less intrusive for patients [Babor T, Steinberg K, Anton RF, Del Boca F. Talk is cheap: measuring drinking outcomes in clinical trials. Journal of Studies on Alcohol 2000;61:55-63]. In addition, whilst self-reported drinking may not be a gold-standard measure of alcohol consumption, this approach was employed in both the control and treatment arms of the trials and so this issue would not essentially alter the outcome findings of the review. Lastly, we conducted a meta-analysis which considered the outcomes of individually randomised trials compared to cluster randomised trials (analysis 01.08) and found no statistically significant difference in outcome. Thus we do not think that the self-reported outcomes or the lack of blinding in individually randomised trials substantively affects the findings of our review. Finally, two included trials reported both self-reported outcomes and laboratory markers (GGT) of alcohol intake; both studies were consistent in finding that the intervention was more effective than control on the basis of both self-reported and objective outcomes.

4) We are aware that more individuals are screened than enrolled into the treatment or control conditions of the trials, this is indeed the purpose of screening. The number of patients that need to be screened in order to identify patients suitable for intervention is an important implementation issue for practice (and practitioners) which has been specifically investigated elsewhere (reference 5 below) but it was not central to the aims of our effectiveness review.

In our review, we specifically report the loss to follow-up of patients post intervention which was greater in brief intervention conditions compared to control conditions. We cite this as a weakness in the brief intervention evidence-base. However, the loss of patients between screening and enrolment into the trial was not relevant to the aim of our review which was to ascertain the relative effectiveness of brief intervention versus control conditions in reducing excessive drinking. Since allocation at enrolment was randomly determined, it is clear that post-screening attrition would be equally experienced in both control and brief intervention conditions. Thus this issue would not substantively alter the findings of our review.

The efficiency of screening prior to brief intervention was covered in a BMJ review in 2003 and there was a considerable debate on this issue at the time (in which 2 out of the nine authors of the current review participated). Since the majority of the co-authors were not involved in that debate, it is not accurate or helpful to state that most of the Cochrane group have maintained a silence on the discussion. As stated above, the issue as to whether screening is an efficient means of identifying excessive drinkers in primary care was not relevant to our key aim of evaluating the effectiveness of brief intervention at reducing heavy drinking.

5) Of the nine co-authors of the current review, three have been involved in both implementation research and outcome evaluation trials in the field of brief alcohol interventions. Indeed, two of these individuals were involved in conducting three of the null-effect trials that contributed to the review (Heather et al. 1987, Richmond et al. 1995 and Lock et al. 2006). The statement that implementation of brief intervention technologies has been the main employment for years of any of the review authors is inaccurate. In addition, we do not agree with the proposition that involvement in implementation research is in of itself favourable towards brief intervention (for an example see reference 5) and the findings of null-effect outcome evaluation research highlighted above substantiates this view. Finally, all the meta-analysis conducted in the Cochrane review was conducted by two research scientists who have never previously worked in the brief intervention field the respective contribution of all the review co-authors to each aspect of the review process is clearly outlined on the published review (page 44). Thus we feel confident about our statement concerning conflict of interest regarding the review.

The current review clearly reported that its search strategy extended to 2006. Thus it obviously would not have included reference 7 which was published in 2007. Indeed the initial review analysis was completed in November 2006 (the group was awarded a small grant by the Cochrane Collaboration to complete the review by this time). However, we re-ran our analysis in February 2007 after correspondence from a trial author (Curry 2003) whose work had been excluded from the review in error. The published version of reference 7 states that it was published (following amendment made after peer-review) in July 2007. Thus the peer-reviewed version of this work was not available for this review. However, this work will, of course, be included in any future update of the Cochrane review. Nevertheless, since the current review clearly contains both null-effect and positive-effect trials of brief intervention, it is clear that we have not selectively included only work with positive outcomes. The Cochrane review meta-analysis clearly reports both the average reduction in the percentage of heavy drinkers (Analysis 1.18) and the average weekly reduction in drinking after brief alcohol intervention (Analysis 1.1) along with a wide range of other relevant outcome measures.

References

1. Kaner EF, Beyer F, Dickinson HO et al. Effectiveness of brief alcohol interventions in primary care populations. Cochrane Database Syst Rev 2007:CD004148.
2.Flay BR. Efficacy and effectiveness trials (and other phases of research) in the development of health promotion programs. Prev Med 1986;15:451-74.
3.Babor TF, Higgins-Biddle JC. Brief Intervention for Hazardous and Harmful Drinking - A Manual for Use in Primary Care. Geneva: World Health Organization, 2001: 1-29
4.Fleming MF, Barry KL, Manwell LB et al. Brief physician advice for problem alcohol drinkers - A randomised controlled trial in community-based primary care practices. JAMA 1997;277:1039-45.
5.Beich A, Thorsen T, Rollnick S. Screening in brief intervention trials targeting excessive drinkers in general practice: systematic review and meta-analysis. BMJ 2003;327:536-40.
6. Kaner EF, Lock CA, McAvoy BR, Heather N, Gilvarry E. A RCT of three training and support strategies to encourage implementation of screening and brief alcohol intervention by general practitioners. Br J Gen Pract 1999;49:699-703.
7. Beich A, Gannik D, Saelan H et al. Screening and Brief Intervention Targeting Risky Drinkers in Danish General Practice A Pragmatic Controlled Trial. Alcohol Alcohol 2007.
8. Beich A, Gannik D, Malterud K. Screening and brief intervention for excessive alcohol use: qualitative interview study of the experiences of general practitioners. BMJ 2002;325:870-3.

Contributors

Feedback from Anders Beich
Reply from Kaner EF, Dickinson HO, Beyer FR, Campbell F, Schlesinger C, Heather N, Saunders JB, Burnand B, Pienaar ED

WHAT'S NEW

Last assessed as up-to-date: 14 February 2007.

28 July 2009	Feedback has been incorporated	We have replied to feedback from Dr Anders Beich

HISTORY

Protocol first published: Issue 2, 2003

Review first published: Issue 2, 2007

19 June 2008	Amended	corrected figures, estimates and add minor changes
29 April 2008	New search has been performed	Add one study but conclusions are not changed
29 April 2008	Amended	little changes
15 February 2007	New citation required and conclusions have changed	Substantive amendment

CONTRIBUTIONS OF AUTHORS

Kaner conceived, designed and coordinated the review. Kaner and Campbell wrote the protocol with advice from Heather and Saunders to develop the efficacy/effectiveness scale, from Dickinson on statistical issues and from Piennar on other issues. Piennar, Campbell and Kaner conducted the searches. Campbell managed the reference databases. Kaner and Schlesinger sifted the references. Kaner, Schlesinger, Piennar, Campbell, Beyer and Dickinson abstracted data. Dickinson and Beyer performed statistical analysis. Dickinson, Beyer and Kainer wrote the review.

DECLARATIONS OF INTEREST

The authors have no known conflict of interest.

SOURCES OF SUPPORT

Internal sources

- Small Grant from the University of Newcastle upon Tyne, UK.

External sources

- Department of Health Primary Care Career Scientist Award, UK.
- Cochrane Collaboration, UK.

INDEX TERMS

Medical Subject Headings (MeSH)

Alcohol Drinking [*therapy]; Alcoholism [*therapy]; Emergencies; Family Practice; Randomized Controlled Trials as Topic

MeSH check words

Humans