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Brief interventions in primary care for hazardous and harmful alcohol consumption: meta-analytic summary of three decades of research to inform clinical practice

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Conflicts of interest

Nicolas Bertholet is salaried by Lausanne University Hospital, a public institution; he has received grants from the Swiss National Science Foundation, the Swiss Foundation for Alcohol Research, and the Department of Community Medicine and Health from the Lausanne University Hospital. He has received no personal support from industry sources such as pharmaceutical, alcohol and tobacco companies and holds no personal stock. Dr Bertholet is an author of a previous systematic review on the subject (Bertholet *et al.*, 2005), and has participated in numerous studies on screening and brief intervention (not limited to the scope of this review), including as a primary investigator of a randomised trial of an electronic screening and brief intervention.

Fiona Beyer, Fiona Campbell, Colin Muirhead, Elizabeth Pienaar and John B Saunders have no conflicts of interest to declare.

Jean-Bernard Daeppen received personal fees from Lundbeck SAS for lectures and advice and was involved in one of the included trials in this systematic review (Daeppen *et al.*, 2007).

Eileen Kaner is an investigator on the ongoing SIPS Junior trial (NIHR programme grant number NIHR RP-PG-0609-10162). She was also involved in included trials in this systematic review (Lock *et al.*, 2006; Kaner *et al.*, 2013).

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Abstract

Aims

An updated Cochrane systematic review assessed effectiveness of screening and brief intervention to reduce hazardous or harmful alcohol consumption in general practice or emergency care settings. This paper summarises the implications of the review for clinicians.

Methods

This review followed Cochrane methods, and is reported according to PRISMA guidance. We searched multiple resources to September 2017, seeking randomised controlled trials of brief interventions to reduce hazardous or harmful alcohol consumption in people attending general practice, emergency care or other primary care settings for reasons other than alcohol treatment. Brief intervention was defined as a conversation comprising five or fewer sessions of brief advice or brief lifestyle counselling and a total duration of less than 60 minutes. Our primary outcome was alcohol consumption, measured as or convertible to grams per week. We conducted meta-analyses to assess change in consumption, and subgroup analyses to explore the impact of participant and intervention characteristics.

Results

We included 69 studies, of which 42 were added for this update. Most studies (88%) compared brief intervention to control. The primary meta-analysis included 34 studies and provided moderate-quality evidence that brief intervention reduced consumption compared to control after one year (mean difference -20 g/week, 95% confidence interval -28 to -12). Subgroup analysis showed a similar effect for men and women.

Conclusions

Brief interventions can reduce harmful and hazardous alcohol consumption in men and women. Short, advice-based interventions may be as effective as extended, counselling-based interventions for patients with harmful levels of alcohol use who are presenting for the first time in a primary care setting.

258 words (excluding subheadings)

Summary

An updated Cochrane systematic review concluded that brief interventions in general practice or emergency care settings can reduce hazardous or harmful alcohol consumption for both men and women after one year. Short, advice-based interventions may be as effective as extended, counselling-based interventions.

42 words

Background

Excessive alcohol use is a leading risk factor for disease burden worldwide, responsible for over 5% of the global burden of disease, disability and death (World Health Organization, 2018). The true impact of alcohol on health of individuals and society is difficult to estimate because of multiple effects resulting from alcohol use (Gakidou *et al.*, 2017). Harmful use of alcohol is a causal factor in more than 200 disease and injury conditions (World Health Organization, 2005). Long-term alcohol use causes several diseases such as alcoholic liver disease, and increases the risk of many partially attributable chronic disorders (e.g. oesophageal cancers). By contrast, single occasion high intensity consumption is associated with acute adverse events such as road traffic accidents, falls and assaults (Rehm *et al.*, 2009). The resulting burden affects quality of life for drinkers and their families and health care resources. In England in 2009 the total annual cost of alcohol related harm to the National Health Service (NHS) was estimated to be £3.5 billion (Home Office, 2012).

The heavy burden that alcohol use imposes on health, and its significant economic consequences, has led to national and international programmes and policies that seek to reduce consumption levels and reduce a primary cause of avoidable ill health (H. M. Government, 2012). Patterns of alcohol use are categorised as hazardous (increased risk of harm) and harmful (Saunders *et al.*, 1993). Harmful alcohol use has detrimental health and social consequences not only for the drinker but for those around them and society as a whole (World Health Organization, 2010). At a population level, the majority of alcohol-related problems are attributable to hazardous and harmful drinking rather than to dependency because the former two are far more prevalent. Consequently, from a public health perspective, it may be more effective to intervene for

hazardous and harmful consumption, noting that many in these categories may be unaware that their drinking is adversely affecting their health and may be more amenable to change than those that need treatment (Geijer-Simpson *et al.*, 2018).

Screening and brief intervention has been advocated as a strategy to reduce excessive drinking (O'Donnell *et al.*, 2014). Brief intervention (BI) is an umbrella term for interventions delivered by generalist practitioners that provide advice or counselling aiming to help hazardous or harmful drinkers understand the risks or adverse impacts of their drinking and explore possible ways to cut down. All BIs typically have similar theoretical underpinnings, namely social cognitive and motivational theory (Heather, 1995) and a component structure (commonly known as FRAMES) (Miller and Sanchez, 1994), which encompasses **F**eedback about existing consumption, **R**esponsibility for change, **A**dvice about practical strategies to reduce drinking; a **M**enu of options for behaviour change; **E**mpathic delivery; and **S**elf-efficacy building. BIs in primary care are not intended for people with alcohol dependence, who typically need more intensive treatment. Evidence that less than 10% of those who drink excessively report having received advice on their alcohol consumption suggests that there are barriers to the use of brief interventions in primary care (Brown *et al.*, 2016).

In 2007 our group published a Cochrane review of BI for hazardous and harmful alcohol consumption in primary care (Kaner *et al.*, 2007). Since then, many relevant new trials have been published. Researchers have developed interventions over time, sometimes adding elements of counselling, motivational interviewing or cognitive behavioural therapy techniques. Consequently, some interventions in recent trials are longer, more complex, or contain more individual sessions than in older studies. Many recent trials have taken place in emergency departments as well as in general practice. Finally, because it may be deemed unethical to conclude from screening that someone has a hazardous or harmful level of consumption and then provide no intervention, control participants are more likely in recent trials to receive simple advice or written information about the risks of such levels of consumption. In light of these changes, we updated our Cochrane review (Kaner *et al.*, 2018). Our objective was to update the evidence on the effectiveness of BI to

reduce hazardous or harmful alcohol consumption in primary care. This paper summarises the results of the systematic review, with a particular focus on implications for practice as well as new analyses of the data.

Methods

The protocol for this review was published on the Cochrane Library (Kaner *et al.*, 2004). The review followed Cochrane methods throughout (Higgins JPT, 2011).

Eligibility criteria

We included randomised controlled trials that included patients who presented to primary (including emergency) care for treatment not related to their alcohol consumption, but who screened positive for hazardous or harmful drinking. Screening could use a tool such as AUDIT (Saunders *et al.*, 1993) or reports of drinking in excess of recommended guidelines. We defined primary care as all immediately accessible health care facilities for which patients did not need a referral (Starfield *et al.*, 2005). Trials in emergency departments and trauma centres were included if it was the patient's first contact following the emergency event.

Brief intervention comprised a single session and up to a maximum of five sessions of verbally-delivered information, advice or counselling (sometimes defined as socio-cognitive) intervention designed to achieve a reduction in risky alcohol consumption, alcohol-related problems, or both (Babor, 1994). Some interventions described as 'brief interventions' in trial reports were based on counselling techniques and were longer or more intensive than could be administered in a standard primary care appointment. We referred to these as 'extended intervention', where the intervention consisted of more than five sessions or a total of more than 60 minutes. The control condition was no intervention, usual treatment for the presenting condition, or minimal information such as feedback and simple advice, or a written leaflet giving general health or alcohol-related information.

Our primary outcome was consumption of alcohol in grams per week (g of ethanol/week, abbreviated to g/wk). Other secondary measures of consumption were number of heavy drinking episodes (HED) per week, drinking days per week, drinks per drinking day, proportion of heavy drinkers, and proportion of heavy

episodic drinkers. Other secondary outcomes were laboratory markers of alcohol consumption, alcohol-related harm, patient satisfaction measures, health-related quality of life, and economic measures. For the analyses we used mean difference and standard deviations for all continuous outcomes, and risk difference and 95% confidence intervals for dichotomous outcomes.

Study selection

Searches were updated to include a total of 15 bibliographic databases from inception to September 2017, with no restriction on publication type or language, along with grey literature (supplementary table). Results were imported to Endnote and de-duplicated, and titles and abstracts were screened by two reviewers independently. Full text of papers identified as potentially eligible were assessed by two reviewers independently to confirm the list of included studies. Two reviewers extracted data describing participant characteristics (e.g. gender, age, baseline consumption), intervention detail, and outcomes onto a pre-designed form in Word. Where data were missing, study authors were contacted. All included studies were critically appraised using the Cochrane Risk of Bias tool (Higgins *et al.*, 2011). For each of the above steps, any disagreements were resolved through discussion with a third reviewer.

Data synthesis

All consumption data was converted to g/wk where possible, using data from the paper or standard values for each country (Miller *et al.*, 1991; Gual *et al.*, 1999; Heather and WHO, 2006; Furtwængler and de Visser, 2013), and this comprised the primary meta-analysis. Measures used for other meta-analyses were frequency (drinking days per week) and intensity (drinks per drinking day). For continuous outcomes, the weighted mean difference method was used to estimate pooled effect sizes and 95% confidence intervals (CI), using a random-effects model in RevMan 5.3 (2014). Meta-regression was used to assess any differences in calculated effect associated with the publication date of studies, baseline consumption of participants, or duration of treatment, using the metareg command in Stata version 14.1 (StataCorp, 2015). For dichotomous outcomes (participant classified as a heavy drinker), risk differences and 95% CIs were calculated and pooled in a random-effects meta-analysis using Mantel-Haenszel test weighting. The magnitude of heterogeneity among trials was assessed using the I^2 statistic (Higgins and Thompson, 2002;

Higgins *et al.*, 2003). Statistical significance of heterogeneity was assessed using P values derived from Chi^2 tests (Deeks *et al.*, 2001).

We conducted subgroup analyses to assess the impact on calculated effect sizes of the following. Only gender and age were specified in the review protocol, but we have provided a rationale for the other subgroup analyses here:

- Gender and age of participants.
- Setting: we separated trials taking place in general practice clinics from those in emergency care. If emergency care patients associate their visit with alcohol, they might be more open to messages about changing their consumption. Conversely, their injuries and the stress of the occasion may make the intervention less likely to be effective.
- Type of intervention: we hypothesised that counselling and motivational-based interventions may be more effective than advice-based interventions, due to difference in approach or extra time often needed to deliver them.
- Content of control condition: control participants who receive information about risks of alcohol consumption may be more likely to reduce consumption than those who receive no intervention or general health-related information.
- We used subgroup analysis to separate the follow-up time points and explore intervention decay over the first year.
- We carried out meta-regression analysis to understand whether there was any impact of publication date on calculated effect sizes. Recommended limits for alcohol consumption have reduced over time, so baseline consumption tends to be lower in recent trials, and participants need to reduce their consumption by less in recent trials to achieve non-hazardous levels, a trend which might make more recent trials look less effective.
- We also carried out meta-regression analysis to explore the impact of baseline alcohol consumption and of the amount (duration and frequency) of intervention on the effect size. We calculated an

estimate of exposure in minutes for each trial by adding the duration of all reported sessions. We also compared the effect of extended interventions, relative both to brief interventions and to minimal or no intervention.

We conducted sensitivity analyses to assess the impact of studies at high risk of bias, and of imputing (due to missing) standard deviations.

We used GRADE (Guyatt *et al.*, 2008; Guyatt *et al.*, 2011) to assess overall confidence in the quality of the evidence, via GRADEPro (2015).

Results

Characteristics of included studies

In this update, 42 trials were added to the review, making 69 included trials with 33,642 participants (figure 1).

[Insert figure 1 here]

Participants were 70% male, but more recent trials reported female drinking separately, and one recruited only women (L'Engle *et al.*, 2014). Eight trials focused on adolescents, young adults or both, whilst four recruited only older adults (defined as over 55, 60, or 65 years). Most trials took place in high-income countries, whilst four took place in middle-income countries (Brazil, Kenya, South Africa, and Thailand). Most studies (n = 61) compared BI with 'minimal' or no intervention (table 1). Of these, five also included an extended intervention arm and eight included two minimal or no intervention arms. Four studies delivered a minimal intervention that was sometimes described as control and sometimes as intervention (Heather *et al.*, 1987; Richmond *et al.*, 1995; Sommers *et al.*, 2006; Kaner *et al.*, 2013). One study compared an extended intervention with BI. Four studies compared only an extended intervention with minimal or no intervention. Feedback and structured advice took several formats: described as BI (and assumed to be based on FRAMES where not reported) (n = 27); based on or informed by motivational interviewing, Motivational Enhancement Therapy (MET), or Brief Negotiated Interview (BNI) (n = 32); or Cognitive Behavioural Therapy

(CBT) (n = 2). Some were backed up by diaries or exercises for the participant to complete at home, and follow-up telephone calls. Treatment duration ranged from less than five minutes (Huas *et al.*, 2002; Babor *et al.*, 2006) to 60 minutes (McIntosh *et al.*, 1997) of advice or counselling (median 25 minutes, IQR 7.5 to 30.0).

[Insert table 1 here]

Critical appraisal of included studies

The main sources of bias in trials arose from difficulties blinding participants and providers to the intervention, and from attrition. In some domains risk of bias was difficult to judge due to poor reporting, and trials were assessed as at unclear risk of bias. Some trials were designed (for example through cluster randomisation) so that those delivering the intervention had no contact with the control group participants, so contamination between arms was impossible and these were judged to be at low risk of bias from provider blinding (41%). Researchers reported an attempt to blind participants to the purpose of the study by masking the alcohol focus of the study, and these trials were judged to be at low risk of bias from participant blinding (32%). Many trials (40%) were judged to be at high risk of attrition bias, either because they reported more than 30% drop out rate or because there were unexplained differences in the loss to follow-up between arms.

Effectiveness of interventions – brief interventions versus control

Quantity of alcohol consumed per week

The primary meta-analysis included 34 trials (15,197 participants, median age 43 years) reporting a measure of consumption that could be converted to g/wk, and reported at 12 months (see table 2 for results of all meta-analyses). This demonstrated that after BI, participants drank a mean of 20 g/wk less than those in the control groups (95% CI -28 to -12, I^2=73%) (figure 2). There was substantial heterogeneity among the studies, but the confidence interval for the effect estimate takes this heterogeneity into account.

[Insert figure 2 here]

Three sensitivity analyses separately omitted trials at high risk of attrition bias, omitted trials where allocation concealment was sub-optimal, and included trials where standard deviations had to be imputed. All these sensitivity analyses made little difference to the primary result.

[Insert table 2 here]

The first set of subgroup analyses looked at participant characteristics and setting of interventions (table 3). Eleven trials reported sufficient information about men (3486 participants) and women (1350 participants) to allow a subgroup analysis by gender. Men in the intervention group reduced their consumption by 42 g/wk (95% CI -65 to -20, I^2 = 67%), and women by 30 g/wk (95% CI -59 to -2, I^2 = 78%). The difference in reduction of consumption between men and women was not statistically significant. Three trials (1638 participants) recruited only adolescents or young adults (defined as maximum 21, 24 and 25 years in Bernstein 2010; Fleming 2010; Schaus 2009, respectively). We separated these studies from trials that did not impose age restrictions (13,559 participants). The treatment effect was smaller in the younger population (a mean reduction of 7 g/wk, 95% CI -17 to -3, I^2 = 0%, compared to 23 g/wk, 95% CI -32 to -13, I^2 = 75% for all adults). Ten of the primary meta-analysis trials (6386 participants) took place in emergency care, compared to 24 in general practice (8811 participants). The effect was smaller in emergency care (-10 g/wk, 95% CI -18 to -2, I^2 = 0%) compared to general practice settings (-26 g/wk, 95% CI -37 to -14, I^2 = 79%).

[Insert table 3 here]

Further subgroup analyses explored differences in what was provided to the intervention and control groups. Twenty trials (8243 participants) testing advice-based interventions showed greater impact than 12 counselling-based intervention trials (5537 participants) – a reduction of 33 g/wk, 95% CI -46 to -20, I^2 = 68% versus 0 g/wk, 95% CI -3 to 3, I^2 = 0%). Two trials were not included in this subgroup analysis because they contained arms that included both intervention types (Kaner *et al.*, 2013; Drummond *et al.*, 2014). There was no evidence that the treatment effect differed between the two sets of trials after adjusting for year of publication in meta-regression analysis. Half of the trials in the primary meta-analysis provided some kind of alcohol-related advice or leaflet to control participants. The mean reduction in the 16 trials providing

alcohol-related content in the control arm (6591 participants) was lower (a reduction of 13 g/wk, 95% CI -23 to -3, I^2 = 56%) than in 18 trials (8606 participants) where control group participants received no alcohol content (a reduction of 24 g/wk, 95% CI -36 to -12, I^2 = 69%). However, the test for subgroup differences was not significant. For subgroup analyses relating to age, setting and type of intervention, differences between groups appeared to be confounded by the fact that effectiveness was also associated with publication date.

Sensitivity analysis by length of follow-up suggested no decay in the impact of the intervention over the first 12 months. Most trials reported at 6 or 12 months. The pooled effect on consumption at six months (10,313 participants) was similar to 12 months (15,197 participants), a mean reduction of 22 g/wk, 95% CI -32 to -12, I^2 = 70%, versus a reduction of 20 g/wk, 95% CI -28 to -12, I^2 = 73%. Results were similar when the analysis was restricted to studies reporting outcomes at both 6 and 12 months.

The mean weekly baseline consumption of participants enrolled in newly included trials was lower than that reported in 2007 (181g/wk versus 313 g/wk). A meta-regression analysis demonstrated that for every g/wk increase in mean baseline consumption, the mean difference in consumption between BI and control participants decreased by 0.16 g/wk (95% CI -0.23 to -0.10); in other words, the difference in consumption was greater in absolute terms in trials with higher mean baseline consumption. Further meta-regression analysis by publication date showed that for every year going forward in time, the mean difference in consumption between BI and control groups increased by 2.3 g/wk (95% CI 1.3 to 3.4) – in other words, there was a smaller difference in consumption between intervention and control participants in more recent trials. Residual heterogeneity in the latter analysis (I^2 = 42%) was notably lower than in the unadjusted analysis. Including both baseline consumption and year of publication in the meta-regression further reduced residual heterogeneity (I^2 = 29%). The mean difference between BI and minimal or no intervention decreased by 0.10 g/wk (95% CI -0.18 to -0.03) for each increase of 1 g/wk in baseline consumption, and increased by 1.5 g/wk (95% CI 0.1 to 2.9) for each year increment in year of publication.

Meta-regression suggested little association between quantity of alcohol consumed at 12 months and increasing treatment exposure. The mean difference between the BI and control arms was estimated to increase by 0.2 g/wk (95% CI -0.5 to 0.9; P = 0.57), for each increase of 1 minute in the treatment exposure.

Frequency and intensity of consumption and frequency of heavy drinking occasions

Meta-analysis of 11 trials (5469 participants) suggested that, at 12 months, BI participants reduced the frequency of their drinking by 0.13 days/wk (95% CI -0.23 to -0.04, I^2 = 0%) compared to control participants, or around a day every two months. Fifteen trials (6946 participants) suggested very little impact on frequency of heavy episodic drinking (HED) (-0.08 episodes/week, 95% CI -0.14 to -0.02, I^2 = 22%). Ten trials (3128 participants) suggested no meaningful impact in intensity of drinking (-0.2 g/drinking day, 95% CI -3.1 to 2.7, I^2 = 25%). Percentages of heavy or at risk drinkers at 12 months was reported in 18 trials (7623 participants), although the definition of heavy drinking varied between trials. There were 9% fewer heavy drinkers in the intervention group compared to the control group (95% CI -13 to -4, I^2 = 77%). Heavy episodic drinkers also reduced in the intervention group, by 7% (95% CI -12 to -2, I^2 = 76%).

Secondary outcomes

We found little evidence of impact on laboratory markers in seven studies (table 2). Meta-analysis of harms or satisfaction were precluded by the use of multiple different measures, but no studies reported increase in harm or decrease in satisfaction.

Effectiveness of interventions – extended interventions

Consumption outcomes

Of eight studies including an extended intervention arm, six (1296 participants) were included in a meta-analysis comparing weekly consumption of those receiving an extended intervention compared to a control group at 12 months; the mean difference was -19.5 g/week, 95% CI -40.5 to 1.5, I^2 = 23%. Two trials (319 participants) were included in an analysis suggesting a reduction in frequency of drinking of around one day per fortnight (-0.45 days/wk, 95% CI -0.81 to -0.09, I^2 = 0%). We found little evidence of an impact on HED and drinking days per week.

Only three trials (552 participants) reporting consumption at 12 months could be included in the meta-analysis comparing extended intervention to BI. These provided no evidence of a meaningful difference in consumption, but the confidence interval was very wide (2 g/week, 95% CI -42 to 45, I^2 = 0%). Only one trial (147 participants) (Maisto *et al.*, 2001) reported frequency and intensity outcomes, with inconclusive evidence. No trials compared extended interventions to brief interventions and reported heavy drinking outcomes.

Discussion

Summary of main results and clinical importance

This review update found a large number of new trials focused on brief alcohol intervention in primary care (69 trials and 33,642 participants). However, just 34 (15,197 participants) could be included in the primary meta-analysis which required an outcome of grams of ethanol consumed per week. At 12 months, participants receiving BI in primary care reduced their consumption by 20 g/week (95% CI -28 to -12) compared to control groups, which equates to two to three UK standard units (8 g in a standard drink unit). There was substantial statistical heterogeneity among these studies. Although this tends to suggest the pooled result is less credible, it is not surprising because there were many differences in the interventions. Nevertheless, three sensitivity analyses accounting for risk of bias indicated that this effect is robust. Although more participants were male (70%), interventions were effective for both men and women. Ethnicity was poorly reported, but in trials that provided these data, most participants were white from countries with high income economies (70%) (World Bank, 2018). Therefore, caution should be used when extrapolating results to other populations. Nevertheless, brief interventions are beginning to be tested in middle income countries as demonstrated by four included studies in this review.

The primary meta-analysis in the 2007 version of this review included 21 trials (7,286 participants), and reported a reduction in consumption of 41 g/wk (95% CI: -57 to -25). Several features of the trials landscape have changed since this previous version that might help to explain the lower effect size in the updated review. Because more recent trials tended to report strikingly lower mean baseline consumption,

participants had less scope to decrease their drinking than in older trials. The meta-regression analysis suggested that those with higher consumption at baseline tend to experience a larger effect, so a trend to lower baseline consumption levels would imply a reduced effect size. This decrease over time in baseline drinking raises important questions: brief interventions were originally developed for individuals with higher baseline consumption compared to the average in more recent trials. Individuals at the lower end may not feel direct effects of changes in drinking which may influence their motivation to change or maintain change, or they may have difficulties in perceiving potential gains in reducing their drinking. Similarly, it may be harder for clinicians to provide feedback with relevant saliency for people with lower levels of drinking. As recommended low risk drinking limits are likely to continue decreasing in the future, this will represent a significant challenge for future interventions (Wood *et al.*, 2018).

Another important change over time is increasing alcohol content in control conditions, which likely decreases the observable differences between intervention and control groups. Whereas a majority of trials in the previous version of the review provided no alcohol-related information to the control group, recent trials are more likely to offer advice or information about alcohol use in the control condition. Subgroup analysis suggested that mean differences in drinking between control and intervention groups were lower for studies providing alcohol-related information to the control groups compared to those that didn't. This fits with our hypotheses that lower consumption eligibility criteria in more recent years gives participants less scope for reducing consumption before they achieve non-risky levels.

Meta-analyses provided no evidence that extended interventions reduce consumption any more or less than brief interventions. These analyses were based on smaller trials where attendance at multiple sessions was not always well reported. It is possible that low attendance after the first session of a trial categorised as 'extended' may have meant that people actually received the equivalent of a brief intervention. This, in addition to the subgroup analysis comparing brief advice to counselling-based interventions and the meta-regression of treatment exposure, provides little evidence of a dose-response intervention effect. This seems counter-intuitive compared to other areas of health care, where more intervention often has a larger

impact. BI opportunistically targets people who are not seeking help with their alcohol consumption, so extended intervention might seem excessive and could alienate people who do not accept that their drinking is problematic. Further research is required to explore why hazardous and harmful drinkers appear to respond more favourably to less intensive intervention. Whereas extended intervention or more complex counselling-based interventions have been seen as options to potentially increase intervention efficacy, this review does not support this approach. Our results do not support the additional burden in time and training or the additional efforts required to recruit patients for multiple sessions. This has important implications for implementation since short and advice-based interventions represent a lesser burden on clinical teams in terms of time and training.

Strength of evidence

For brief intervention compared to control, the GRADE assessment was moderate for all outcomes. This judgement was downgraded due to difficulties with blinding participants and practitioners, and from participant attrition in many of the studies. However, there is a sizable body of evidence, from which the direction of effect is consistent; 82% of studies in the primary meta-analysis reported a reduction in consumption for brief intervention compared to minimal or no intervention participants.

For extended intervention compared to control, fewer studies were available. The GRADE assessment was moderate for each of the outcomes except intensity, where it was low because only one study provided data. For brief compared to extended intervention, the strength of evidence was low or very low because there were few studies and the effect estimates were very imprecise.

Strengths and limitations

This review followed robust Cochrane methods throughout (Higgins JPT, 2011). The addition of 42 new studies provided relevant data that could be added to existing analyses, but also provided scope for new analyses that were not possible in the previous version of the review. Although it can introduce bias to conduct analyses that are not pre-specified in the protocol, all the new subgroup analyses are clinically plausible. A potential weakness of this field is that many of the outcomes were self-reported, and may be

susceptible to social desirability bias. Many of the studies made efforts to minimise this effect, and a recent trial suggests that the effect may be small for questions about alcohol consumption (Kypri *et al.*, 2016). Laboratory markers are not so susceptible to this type of bias. Two biomarkers (GGT - serum gamma-glutamyltransferase and MCV - mean corpuscular volume) were reported in only seven included trials. These indirect biomarkers measure biological processes that can be affected by alcohol use, but also by an array of other things. Although they are not subject to social desirability bias like self-reported measures, these particular biomarkers lack sensitivity and specificity so their performance is sub-optimal for this purpose (Bertholet *et al.*, 2014). Recently, more direct markers of alcohol consumption have been isolated (Liangpunsakul *et al.*, 2015) and these would be more appropriate for future brief intervention trials.

A potential limitation of some of our subgroup analyses is that they may be confounded by the overall trend that more recent trials tended to report lower differences between control and intervention group. For subgroup analyses according to age, setting and intervention type, the subgroup that has become more common in recent trials - younger age group, emergency care setting, counselling-based intervention - showed a lower reduction in consumption, but this may be because these trials were more recent rather than a true subgroup effect.

Comparison with other studies

The results of this review are consistent with others in suggesting a small but significant impact on alcohol consumption in hazardous or harmful drinkers (O'Donnell *et al.*, 2014). Other reviews have explored the impact of differences in intervention and control groups. A review of seven systematic reviews suggested that 15 minutes of brief intervention was more effective than usual care or longer input, and that more sessions were better than a single session (Álvarez-Bueno *et al.*, 2015). Another review reported the best evidence was for brief (10-15 minute) multi-contact interventions (Jonas *et al.*, 2012). Whereas our review suggests little extra impact from longer duration, this indicates that splitting that longer duration into multiple sessions may increase the effect. A review of brief interventions in emergency care and another in

college students reported modest intervention effects but no impact of intervention length on the effect estimates (Samson and Tanner-Smith, 2015; Schmidt *et al.*, 2015).

Conclusions

This updated Cochrane systematic review concluded that brief interventions can reduce hazardous or harmful alcohol consumption by an average of 20 grams of ethanol, or 2.5 UK units, per week, and that both men and women can benefit. Our analyses of the impact of setting, participant age and type of intervention were confounded by the tendency for more recent trials to report smaller effects, and further research is required to understand this phenomenon. The notably decreased levels of alcohol consumption seen at enrolment in recent trials may help explain this finding. Nevertheless, this review suggests that short, advice-based interventions may be as effective as extended, counselling-based interventions for patients with harmful levels of alcohol use who are presenting for the first time in a primary care setting.

4794 words

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