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Restricting or banning alcohol advertising to reduce alcohol consumption in adults and adolescents (Review)

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[Intervention Review]

Restricting or banning alcohol advertising to reduce alcohol consumption in adults and adolescents

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ABSTRACT

Background

Alcohol is estimated to be the fifth leading risk factor for global disability-adjusted life years. Restricting or banning alcohol advertising may reduce exposure to the risk posed by alcohol at the individual and general population level. To date, no systematic review has evaluated the effectiveness, possible harms and cost-effectiveness of this intervention.

Objectives

To evaluate the benefits, harms and costs of restricting or banning the advertising of alcohol, via any format, compared with no restrictions or counter-advertising, on alcohol consumption in adults and adolescents.

Search methods

We searched the Cochrane Drugs and Alcohol Group Specialised Register (May 2014); CENTRAL (Issue 5, 2014); MEDLINE (1966 to 28 May 2014); EMBASE (1974 to 28 May 2014); PsychINFO (June 2013); and five alcohol and marketing databases in October 2013. We also searched seven conference databases and www.clinicaltrials.gov and http://apps.who.int/trialsearch/ in October 2013. We checked the reference lists of all studies identified and those of relevant systematic reviews or guidelines, and contacted researchers, policymakers and other experts in the field for published or unpublished data, regardless of language.

Selection criteria

We included randomised controlled trials (RCTs), controlled clinical trials, prospective and retrospective cohort studies, controlled before-and-after studies and interrupted time series (ITS) studies that evaluated the restriction or banning of alcohol advertising via any format including advertising in the press, on the television, radio, or internet, via billboards, social media or product placement in films. The data could be at the individual (adults or adolescent) or population level.

Data collection and analysis

We used the standard methodological procedures expected by The Cochrane Collaboration.

Main results

We included one small RCT (80 male student participants conducted in the Netherlands and published in 2009) and three ITS studies (general population studies in Canadian provinces conducted in the 1970s and 80s).

The RCT found that young men exposed to movies with a low-alcohol content drank less than men exposed to movies with a high-alcohol content (mean difference (MD) -0.65 drinks; 95% CI -1.2, -0.07; p value = 0.03, very-low-quality evidence). Young men exposed to commercials with a neutral content compared with those exposed to commercials for alcohol drank less (MD -0.73 drinks; 95% CI -1.30, -0.16; p value = 0.01, very-low-quality evidence). Outcomes were assessed immediately after the end of the intervention (lasting 1.5 hours), so no follow-up data were available. Using the Grading of Recommendations Assessment, Development and Evaluation approach, the quality of the evidence was rated as very low due to a serious risk of bias, serious indirectness of the included population and serious level of imprecision.

Two of the ITS studies evaluated the implementation of an advertising ban and one study evaluated the lifting of such a ban. Each of the three ITS studies evaluated a different type of ban (partial or full) compared with different degrees of restrictions or no restrictions during the control period. The results from the three ITS studies were inconsistent. A meta-analysis of the two studies that evaluated the implementation of a ban showed an overall mean non-significant increase in beer consumption in the general population of 1.10% following the ban (95% CI -5.26, 7.47; p value = 0.43; I^2 = 83%, very-low-quality evidence). This finding is consistent with an increase, no difference, or a decrease in alcohol consumption. In the study evaluating the lifting of a total ban on all forms of alcohol advertising to a partial ban on spirits advertising only, which utilised an Abrupt Auto-regressive Integrated Moving Average model, the volume of all forms of alcohol sales decreased by 11.11 kilolitres (95% CI -27.56, 5.34; p value = 0.19) per month after the ban was lifted. In this model, beer and wine sales increased per month by 14.89 kilolitres (95% CI 0.39, 29.39; p value = 0.04) and 1.15 kilolitres (95% CI -0.91, 3.21; p value = 0.27), respectively, and spirits sales decreased statistically significantly by 22.49 kilolitres (95% CI -36.83, -8.15; p value = 0.002). Using the GRADE approach, the evidence from the ITS studies was rated as very low due to a high risk of bias arising from a lack of randomisation and imprecision in the results.

No other prespecified outcomes (including economic loss or hardship due to decreased alcohol sales) were addressed in the included studies and no adverse effects were reported in any of the studies. None of the studies were funded by the alcohol or advertising industries.

Authors' conclusions

There is a lack of robust evidence for or against recommending the implementation of alcohol advertising restrictions. Advertising restrictions should be implemented within a high-quality, well-monitored research programme to ensure the evaluation over time of all relevant outcomes in order to build the evidence base.

PLAIN LANGUAGE SUMMARY

Does banning or restricting advertising for alcohol result in less drinking of alcohol?

Review question

In this review we ask the question whether banning or restricting the advertising of alcohol in any form will lead to people drinking less alcohol. The form of the ban could include banning alcohol advertisements on television, the internet or billboards, or in magazines. We were also interested in the harms that banning advertisements may cause, such as reducing profits in the alcohol and advertising industries, and whether governments would lose taxes if alcohol purchases went down after a ban.

Background

The misuse of alcohol is a significant risk factor for ill health, injury (e.g. through violent behaviour or road traffic collisions), death and social problems around the world. Advertising to promote the drinking of alcohol is widespread. Banning or restricting the advertising of alcohol has been suggested as a possible way to lower the use of alcohol in the general public and to stop young people from starting drinking at an early age.

Study characteristics

The evidence we present is current to May 2014. We found four studies that evaluated the restriction or banning of alcohol advertising via any format. One was a small randomised controlled trial (RCT) that evaluated drinking behaviour in 80 young men in the Netherlands exposed to movies with either a high or low alcohol content combined with a commercial with either a neutral content (interpreted as a ban on alcohol advertising) or a high alcohol content. The other three studies were interrupted time series (ITS) studies. ITS studies are studies in which changes, usually in the general public, are measured at various points before, during and after an intervention such as a change in policy. Two of the three ITS studies evaluated what happened after an advertising ban was introduced by two different Canadian provincial governments. The third ITS study evaluated what happened after a ban was lifted after being in place for 50 years in another Canadian province. Each study evaluated a different category of ban (either partial or full).

None of the above studies were funded by the alcohol or advertising industries.

Key results

The data arising from the included studies did not show a clear effect either for or against the banning or restriction of alcohol advertising.

In the RCT, young men who watched movies with a low-alcohol content drank less than men who watched movies with a high-alcohol content. Young men exposed to commercials with a neutral content compared with those exposed to commercials for alcohol drank less. The trial was one and a half hours, so we do not know how long beyond the trial these effects lasted. The trial did not report on any harmful outcomes.

The results from the three ITS studies were inconsistent. We statistically combined the results of the two studies that assessed what happened after a ban was introduced. This showed an overall increase in beer consumption in the general population following the introduction of the ban, but the results were uncertain and could also be consistent with no difference or an overall decrease in alcohol consumption. The third ITS study, which evaluated the lifting of a total ban on all forms of alcohol advertising to a ban on spirits advertising only, also found uncertain results. None of the studies reported on any harms arising from the bans.

Quality of the evidence

Overall we judged the quality of evidence to be very low in the RCT. This was based on the fact that there were problems with the study methodology, the population included men only and the results were not very accurate. In the ITS studies, the quality was also judged to be very low due to problems with the study methodology and the results not being precise.

Conclusions

The review cannot recommend for or against banning alcohol advertising. Governments that are considering implementing alcohol advertising bans would be advised to implement the ban in a research environment and monitor the effects over time to build the evidence base.

SUMMARY OF FINDINGS

Summary of findings for the main comparison. Non-alcohol commercials compared to alcohol commercials for reduction of alcohol consumption

Non-alcohol commercials compared to alcohol commercials for reduction of alcohol consumption

Patient or population: General population
Settings: General population
Intervention: Non-alcohol commercials
Comparison: Alcohol commercials

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No. of participants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	Alcohol commercials	Non-alcohol commercials				
Total alcohol consumption in number of glasses Follow up: mean 1.5 hours		The mean total alcohol consumption in number of glasses in the intervention groups was **0.73 less** (1.3 to 0.16 less)		80 (1 study)	⊕⊝⊝⊝ **very low** [1,2,3]	
Delayed age of initiation of alcohol use - not measured			Not estimable	-		This outcome was not applicable in this trial
Reduction in rate of reported risk behaviour - not measured			Not estimable	-		
Reduction in alcohol-related injuries or accidents - not measured			Not estimable	-		
Reduction in individual spending on alcohol - not measured			Not estimable	-		
Loss of revenue from alcohol industry - not measured			Not estimable	-		This outcome was not applicable in this trial

Loss of advertising revenue - not measured		Not estimable	-	This outcome was not applicable in this trial

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: Confidence interval;

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[1] Risk of bias: rated as serious. In the Engels 2009 trial, randomisation was inadequate (the groups differed on the baseline prognostic factor prior drinking levels), allocation concealment was unclear and the researchers were not blinded to group allocation so detection bias may be present.
[2] Indirectness: rated as serious. The trial is specific to young men from a university setting in a high-income country and may not be generalisable to other settings.
[3] Imprecision: rated as serious: The 95% CI is wide and the sample size small.

Summary of findings 2. Alcohol ban compared to no ban for the general population

Alcohol ban compared to no ban for the general population

Patient or population: General population
Settings: General population
Intervention: Alcohol ban
Comparison: No ban

Outcomes	Illustrative comparative risks* (95% CI)		Relative effect (95% CI)	No. of pParticipants (studies)	Quality of the evidence (GRADE)	Comments
	Assumed risk	Corresponding risk				
	No ban	Alcohol ban				
Alcohol consumption: % change in beer consumption Follow up: 1.2 to 3 years		The mean % change in beer consumption in the intervention groups was **1.1 more**		2 ITS studies	⊕⊝⊝⊝ **very low** [1,2]	Results for consumption of other types of alcoholic beverages and total consumption were inconsistent in the three ITS studies

		(5.26 less to 7.47 more)				
Reduction in rate of reported risk behaviour - not reported	See comment	See comment	Not estimable	-	See comment	None of the studies measured this outcome
Delayed age of initiation of alcohol use - not reported	See comment	See comment	Not estimable	-	See comment	None of the studies measured this outcome
Reduction in alcohol-related injuries or accidents - not reported	See comment	See comment	Not estimable	-	See comment	None of the studies measured this outcome
Reduction in individual spending on alcohol - not reported	See comment	See comment	Not estimable	-	See comment	None of the studies measured this outcome
Loss of revenue from alcohol industry - not reported	See comment	See comment	Not estimable	-	See comment	None of the studies measured this outcome
Loss of advertising revenue - not reported	See comment	See comment	Not estimable	-	See comment	None of the studies measured this outcome

*The basis for the **assumed risk** (e.g. the median control group risk across studies) is provided in footnotes. The **corresponding risk** (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: Confidence interval; ITS: interrupted time series

GRADE Working Group grades of evidence
High quality: Further research is very unlikely to change our confidence in the estimate of effect.
Moderate quality: Further research is likely to have an important impact on our confidence in the estimate of effect and may change the estimate.
Low quality: Further research is very likely to have an important impact on our confidence in the estimate of effect and is likely to change the estimate.
Very low quality: We are very uncertain about the estimate.

[1] Risk of bias: rated as serious: the risk of a dilution effect is present in both studies (Ogborne 1980 and Smart 1976) and seasonality may not be adequately addressed in the analyses. The studies were not further downgraded for limitations in causal inference due to a lack of randomisation, as the initial GRADE rating commenced at low quality.
[2] Inconsistency: rated as serious. The results from the Smart 1976 study indicate a reduction in beer consumption after implementing a ban on advertising and Ogborne 1980 shows an increase in beer consumption.

BACKGROUND

Description of the condition

Alcohol is estimated to be the fifth leading risk factor for global disability-adjusted life years (DALYs) for all ages and sexes (Lim 2012). This estimate has increased by 32% from 1990 to 2010 (Lim 2012). For people aged 15 to 49 years, alcohol is the leading risk factor for DALYs worldwide (Lim 2012). Over 2.7 million deaths (95% uncertainty index 2,464,575 to 3,006,459) are attributed to alcohol use linked to injury (intentional, unintentional and transport), cardiovascular disease, cirrhosis, cancer, mental and behavioural disorders, human immunodeficiency virus infection/acquired immunodeficiency syndrome, tuberculosis, and neurological disorders (Lim 2012). Alcohol affects not only the health of the drinking individual, but in pregnant women the neurotoxic effects of alcohol may cause a range of congenital defects including foetal alcohol spectrum disorders and foetal death, stillbirth, and infant and child mortality (Burd 2012).

In addition to its effects on mortality and morbidity, alcohol has significant adverse social and economic effects. A 2006 review of studies estimating the global economic burden of alcohol found that alcohol accounts for 1.3% to 3.3% of total health costs, 6.4% to 14.4% of total public order and safety costs, 0.3 to 1.4 per thousand USD of gross domestic product (GDP) for criminal damage costs, 1.0 to 1.7 per thousand USD of GDP for drink-driving costs and 2.7 to 10.9 per thousand USD of GDP for workplace costs (absenteeism, unemployment and premature mortality) (Baumberg 2006).The authors of the review caution readers to consider the methodological differences between studies and inherent design limitations, but these findings are supported by a 2009 analysis conducted in partnership with the World Health Organization (WHO). This aggregate analysis of reviews of published work found that costs associated with alcohol amounted to 1% of GDP in high-income and middle-income countries, with social harm accounting for the greater proportion of these costs, in addition to health costs (Rehm 2009). In a 2010 UK multicriteria decision analysis to assess the relative harms of 20 drugs, harms both to the user and others were greatest for alcohol compared with all other drugs, including heroin and cocaine. Harms assessed included crime, family adversity and a decline in social cohesion within communities (Nutt 2010).

In an overview of systematic reviews and quantitative meta-analyses, Rehm and colleagues evaluated the evidence for a causal impact of average volume of alcohol consumption and pattern of drinking on diseases and injury, and quantified those relationships identified as causal (Rehm 2010). Their findings indicate that alcohol is causally related to many chronic and acute disease outcomes as well as to injury. They report that there is evidence that both the average volume and specific drinking pattern are causally related to ischaemic heart disease, foetal alcohol syndrome, and both intentional and unintentional injury. They postulate that episodes of heavy drinking are likely to influence additional disease outcomes but that epidemiological research to date has had a limited focus on drinking patterns. Due to an absence of research, they were unable to conclude whether the quality of alcohol is a significant factor in disease outcomes.

Description of the intervention

One of the main aims of commercial advertising is to encourage the consumer to use and purchase promoted products. In their extensive 2009 review of the effectiveness and cost-effectiveness of alcohol policies and programmes, Anderson, Chisholm and Fuhr report that alcohol is increasingly marketed using sophisticated advertising in the mainstream media, through the linking of alcohol brands to sports and cultural activities, through sponsorships and product placements, and through direct marketing such as on the internet, and via podcasts and mobile telephones (Anderson 2009). Alcohol marketing campaigns have recently targeted social networking sites such as Facebook and Twitter, which are disproportionately used by young people (Hastings 2013). In a systematic review of 13 longitudinal studies of 38,000 young people, Anderson et al found that longitudinal studies consistently suggest that there is an association between exposure to media/commercial communications and alcohol and adolescents starting to drink alcohol, and with increased drinking among baseline drinkers (Anderson 2009a). In another systematic review of seven cohort studies of young people, Smith and Foxcroft suggest that while there is an association between exposure to alcohol advertising or promotional activity and subsequent alcohol consumption in young people, the modest effect sizes may be limited by the potential influence of residual or unmeasured confounding in the included studies (Smith 2009). Snyder et al, in their longitudinal investigation, found empirical evidence to suggest that exposure to advertising has direct measurable effects on both drink initiation and consumption levels (Snyder 2006).

In their 2008 independent review of the effects of alcohol pricing and promotion for the UK Department of Health, Booth and colleagues identify the methodological complexity of linking advertising to consumption (Booth 2008). Cross-sectional studies will fail to meet the causality criteria of temporality (the intervention predates the effect), and cohort studies and time series analyses may be prone to confounding unless adequately controlled. In addition, they point out that subpopulations such as problem drinkers are likely to be under-represented in general population aggregated data, which are primarily used in national or state-level studies. Despite these methodological limitations, they conclude that there is evidence for an effect of alcohol advertising on underage drinkers and that exposure to television, music videos and billboards that contain alcohol advertising predict the onset of youth drinking and increased drinking (Booth 2008).

How the intervention might work

Prevention strategies to reduce the quantity of alcohol consumed and the age of initiation of alcohol use include several public health interventions targeted at the general population. One such strategy is the restriction or banning of all forms of advertising of alcohol. The reduction in marketing may be voluntary and implemented by the alcohol, media or advertising industries, or mandatory and implemented by government decree.

Theoretically, a restriction or banning of alcohol advertising may reduce the consumption of alcohol across the general population and may raise the age of initiation of drinking in young people. In their 2001 international comparison of bans on the broadcast advertising of alcohol in 17 Organization for Economic and Cooperation Development (OECD) countries between 1977 and 1995, Nelson and Young report that there are several theoretical

models of advertising, including social learning theory, which argues that advertising contributes to normalising perceptions of drinking in society (Nelson 2001). They also describe conflicting economic theories, with advertising either increasing or decreasing consumption because it affects both demand and the levels of prices that sellers find optimal. They warn that partial bans on advertising using specific forms of media may drive substitution towards other advertising media (Nelson 2001).

In their review of policies and programmes, Anderson et al indicate that making alcohol less available and more expensive, and placing a ban on alcohol advertising are the most cost-effective ways to reduce the harm caused by alcohol (Anderson 2009a). However, little evidence is provided to support the statement on banning alcohol advertising. The authors acknowledge that in regions where alcohol marketing relies on self regulation (rather than regulatory banning or restrictions), several studies show that these voluntary systems do not prevent marketing content directed at young people. In another study of pooled time series data from 20 countries over a 26-year period, the authors' primary conclusion was that alcohol advertising bans decrease consumption by 5% to 8% (Saffer 2002). Similarly, a cross-sectional study in the emerging market context of Brazil found evidence of association, but not causation, between alcohol consumption and alcohol promotion (Pinsky 2010).

Why it is important to do this review

In the 2012 Global Burden of Disease report, the authors state that public policy to improve the health of populations will be more effective if policies address the major causes of disease burden. They argue that small reductions in population exposure to large risks will yield substantial health gains (Lim 2012). Reducing or banning alcohol advertising may reduce exposure to the very large risk posed by alcohol both to the individual and to the general population. To date, no systematic review has evaluated the effectiveness, possible harms and cost-effectiveness of this intervention. This Cochrane review aims to evaluate, in a systematic manner, the benefits and harms of reducing or banning alcohol advertising and the cost-effectiveness of such an intervention.

OBJECTIVES

To evaluate the benefits, harms and costs of restricting or banning the advertising of alcohol, via any format, compared with no restrictions or counter-advertising, on alcohol consumption in adults and adolescents

METHODS

Criteria for considering studies for this review

Types of studies

We considered both general population-level studies (where aggregate data from regions are collated before and after a reduction of or ban on advertising) and individual-level studies (where participants may be randomised to different levels of advertising and their subsequent consumption measured) to be applicable to the review.

General population level

a. Randomised controlled trials (RCTs)
b. Controlled clinical trials (CCTs)
c. Prospective cohort studies
d. Retrospective cohort studies if baseline exposure data were collected at time of baseline of study
e. Controlled before and after (CBA) studies, including econometric studies
f. Interrupted time series (ITS) studies. We used the definition for ITS given by the Cochrane Effective Practice and Organization of Care (EPOC) Review Group, viz:
   i. there were at least three time points before and after the intervention, irrespective of the statistical analysis used
   ii. the intervention occurred at a clearly defined point in time
   iii. the study measured provider performance or participant outcome objectively

Individual level

a. RCTs
b. CCTs
c. Prospective cohort studies
d. Retrospective cohort studies if baseline data were collected at time of baseline of study
e. CBA cross-sectional studies
f. ITS studies

NOTE: For both population- and individual-level ITS studies, if the study ignored secular (trend) changes and performed a simple t-test of the pre- versus postintervention periods without further justification, the study was not included in the review unless re-analysis was possible.

Types of participants

Adults of any age and adolescents (defined by WHO as aged 10 to 19 years).

Types of interventions

Intervention

A reduction in or restriction or banning of advertising of alcohol and related products via any format including advertising in the press, on the television, radio, or internet, or via billboards, social media or product placement in films.

We used the broad definition of advertising recommended by the WHO, which defines marketing (with emphasis on its persuasive impact) as: *"any form of commercial communication or message that is designed to increase, or has the effect of increasing, the recognition, appeal and/or consumption of particular products and services. It could comprise anything that acts to advertise or otherwise promote a product or service"* (WHO 2010, page 15). Hence, a restriction on advertising may include restricting responsible drinking campaigns led by the alcohol industry and the marketing of positive associations between industry and socially responsible initiatives.

We attempted to include restrictions on all new forms of marketing, for example those facilitated by digital technologies, but acknowledge that research into the impacts of advertising restrictions is likely to lag behind new marketing technologies.

Comparison

Advertising of alcohol and related products via any format including counter-advertising (defined as the promotion of healthy choices and harm reduction messages).

As for the intervention, we used the definition of advertising recommended by the WHO (WHO 2010).

Types of outcome measures

Primary outcomes

1. Reduction in alcohol consumption

In population-based studies, this may be measured via econometric data (e.g. annual sales of alcohol per capita) and in individual-based studies this may be measured by rate of drinks (number during a specified time).

Secondary outcomes

1. Delayed age of initiation of alcohol use
2. Reduction in rate of reported risk behaviour
3. Reduction in alcohol-related injuries or accidents
4. Reduction in individual spending on alcohol

Adverse effects

1. Loss of revenue from alcohol industry
2. Loss of advertising revenue
3. Reduction in GDP attributable to alcohol sales
4. Loss of employment from alcohol industry
5. Reduction in taxes generated

Search methods for identification of studies

We developed the search strategy with the assistance of the Cochrane Drugs and Alcohol Review Group Trials Search Co-ordinator. We formulated a comprehensive and exhaustive search strategy in an attempt to identify all relevant RCTs, cohort studies and CBA studies, regardless of language or publication status (published, unpublished, in press and in progress).

Electronic searches

As we did not limit the strategy to search for RCTs or cohort studies, we did not use the RCT strategy developed by The Cochrane Collaboration and detailed in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). We used a combination of terms specific to alcohol consumption and to advertising. The search was iterative and used both database-specific syntax and free-text terms. There were no language restrictions.

We searched the following databases.

1. Journal databases

- Cochrane Drugs and Alcohol Group Specialised Register (May 2014)
- MEDLINE (PubMed) (1966 to 28 May 2014); see Appendix 1 for the MEDLINE search strategy
- EMBASE (elsevier.com/online-tools/embase) (1974 to 28 May 2014); see Appendix 2 for the EMBASE search strategy
- *The Cochrane Library* (Issue 5, 2014), which includes the Cochrane Central Register of Controlled Trials (CENTRAL) and the UK National Health Service Economic Evaluations Database (28 May 2014); see Appendix 3 for *The Cochrane Library* search strategy
- PsycINFO (on 14 June 2013); see Appendix 4 for the PsychINFO search strategy

We also search the following additional databases, including economic and marketing databases:

- AgEcon (ageconsearch.umn.edu/) (on 16 October 2013);
- Business Source Premier (on EBSCOHOST) (on 18 October 2013)
- National Institute of Health Alcohol and Alcohol Problems Science Database (1972 to 2003) (http://etoh.niaaa.nih.gov/) (on 22 October 2013;
- Association for Consumer Research (http://www.acrwebsite.org/search/search-conference-proceedings.aspx) (on 22 October 2013);
- Chartered Institute of Marketing (http://library.cim.co.uk/ics-wpd/exec/icswppro.dll) (on 22 October 2013).

2. Conference databases

We attempted to search several relevant conference proceedings. Electronic database searches or reports were available only for the following conferences:

- conference proceedings of the Research Society on Alcoholism (www.rsoa.org);
- conference proceedings of the Kettil Bruun Society 39th Annual Symposium 2013;
- conference proceedings of the International Network on Brief Interventions for Alcohol Problems;
- conference proceedings of the International Health Economics Association (www.ssrn.com);
- meeting reports of the International Center for Alcohol Policies (http://www.icap.org/);
- meeting reports of the European Advertising Standards Alliance (http://www.easa-alliance.org/);
- meeting reports of the The Foundation for Alcohol Research (http://www.abmrf.org/).

3. Ongoing trials

To identify ongoing RCTs we searched ClinicalTrials.gov (www.clinicaltrials.gov/) and the WHO International Clinical Trials Registry Platform (WHO ICTRP) (http://apps.who.int/trialsearch/) (on 10 October 2013). One author, NS, searched both sites using separate terms and combinations of terms. These included [advertising AND alcohol]; [marketing AND alcohol]; [ban AND alcohol]; [restrictions AND alcohol]; [advertis*]; and [ban OR banning].

In the absence of registries for non-RCTs, we contacted experts and researchers in the field, to identify ongoing cohort, CBA and ITS studies.

Searching other resources

We checked the reference lists of all studies identified by the above methods and examined the references of any systematic

reviews, meta-analyses or guidelines we identified during the search process.

During the period of the review, we were in close contact with individual researchers working in the field and policymakers based in inter-governmental organisations including the WHO. We also contacted experts in the field who may have been aware of unpublished or ongoing studies (e.g. Center on Alcohol Marketing and Youth and the European Centre for Monitoring Alcohol Marketing).

We did not conduct handsearching of specific journals other than those searched by the Cochrane Drugs and Alcohol Review Group and already included in CENTRAL.

Data collection and analysis

Selection of studies

Two authors, NS and DCP, read the titles, abstracts and descriptor terms of all downloaded material from the electronic searches to identify potentially eligible reports. We obtained full-text articles for all citations identified as potentially eligible, and NS and DP independently inspected these to establish the relevance of each article according to the prespecified criteria. Where there was any uncertainty as to the eligibility of the record, we obtained the full article.

NS and DCP independently applied the inclusion criteria and any differences of opinion arising were resolved by discussions with a third review author, JEA. We reviewed studies for relevance based on study design, types of participants, exposures and outcome measures.

Data extraction and management

NS and DP independently extracted data into a standardised data extraction form. We piloted the form on two studies to assess its completeness and usability. We extracted the following characteristics from each included study.

- Administrative details: trial or study identification number; author(s); published or unpublished; year of publication; number of studies included in paper; year in which study was conducted; details of other relevant papers cited
- Details of the study: study design; type, duration and completeness of follow up; country and location of study (e.g. higher-income versus lower-income country); informed consent and ethics approval
- Details of participants: setting; numbers; relevant baseline characteristics, including age and sex
- Details of intervention: type of intervention (e.g. restriction, full banning); media setting (e.g. press, television, internet, social media, product placement); timing and duration of intervention; additional co-interventions
- Details of comparison: type and media setting of advertising; timing and duration of current advertising
- Details of outcomes: decreased alcohol consumption; delayed age of initiation of alcohol use; decreased rate of reported risk behaviour; reduction in alcohol-related injuries or accidents; loss of revenue from alcohol industry; loss of revenue from the advertising agency sector; reduction in GDP; loss of employment from alcohol industry; decreased individual spending on alcohol
- Details of the analysis: for RCTs, details of the type of analysis (intention-to-treat or per protocol); for cohort studies, details of the type of adjustment performed in analyses

Assessment of risk of bias in included studies

Assessment of RCTs, CCTs, CBA and cohort studies

For RCTs, CCTs, CBA and cohort studies, NS and DP independently examined the components of each included study for risk of bias using a standard form.

We performed the 'Risk of bias' assessment for RCTs, CCTs, cohort studies and CBAs in this review using the criteria recommended in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). The recommended approach for assessing risk of bias in studies included in a Cochrane Review is a two-part tool, addressing seven specific domains, namely sequence generation and allocation concealment (selection bias), blinding of participants and providers (performance bias), blinding of outcome assessor (detection bias), incomplete outcome data (attrition bias), selective outcome reporting (reporting bias) and other sources of bias. The first part of the tool allows for a description of what was reported to have happened in the study. The second part of the tool involves assigning a judgement relating to the risk of bias for that entry, in terms of low, high or unclear risk. To make these judgements we used the criteria indicated by the *Cochrane Handbook for Systematic Reviews of Interventions* adapted for the addiction field.

The domains of sequence generation and allocation concealment (avoidance of selection bias) were addressed in the tool by a single entry for each study.

Blinding of participants, personnel and outcome assessor (avoidance of performance bias and detection bias) was considered separately for objective outcomes (e.g. use of alcohol measured by biomarker analysis) and subjective outcomes (e.g. patient self-reported use of substance).

The presence of incomplete outcome data (avoidance of attrition bias) was considered separately for all reported outcomes.

We planned to used the criteria drawn from the Newcastle-Ottawa Scale (NOS) (Newcastle-Ottawa) and the criteria developed by the Cochrane Effective Practice and Organization of Care (EPOC) Review Group (EPOC 2008) to assess observational studies. Specifically, the NOS makes judgements in three general areas: selection of study groups, comparability of groups and ascertainment of outcomes (in the case of cohort studies). As a result, this instrument can assess the quality of non-randomised studies so that they can be used in a meta-analysis or systematic review. The 'Risk of bias' tables were adapted to be used for the assessment of RCTs, CCTs, CBA and prospective observational studies according to these criteria. See Appendix 5 for full details. As we did not identify any observational studies for inclusion we did not conduct an assessment using the table.

Assessment for ITS studies

We used the criteria recommended by the Cochrane EPOC Review Group to assess the methodological quality of the ITS studies. The assessment comprises seven standard criteria specific to ITS. See Appendix 6 for full details.

Measures of treatment effect

We conducted data analysis using Review Manager 5 (RevMan 2012).

For RCT data, we calculated outcome measures for dichotomous data (e.g. the proportion of decreasing consumption) as risk ratios with 95% confidence intervals (CIs). For continuous data (e.g. mean age of initiation) we calculated the mean differences (MDs) and standard deviations (SDs) where means were reported.

For cohort and other study design data, we preferentially reported on the adjusted analysis using the estimate of effect reported in the study rather than calculating estimates of effects based on the crude data. Where only crude data were presented, where appropriate, we calculated the crude risk ratios and 95% CIs for dichotomous data and MDs and SDs for continuous data where means were reported, or we reported medians if data were skewed.

Unit of analysis issues

Cluster trials

Studies may employ 'cluster-randomisation' (such as randomisation by student group or region), but analysis and pooling of clustered data poses problems. Authors often fail to account for intraclass correlation in clustered studies, leading to a 'unit of analysis' error (Divine 1992) whereby p values are spuriously low, CIs unduly narrow and statistical significance overestimated. This causes type I errors (Bland 1997).

Where clustering was not accounted for in primary studies, we planned to present data in a table, using a (*) symbol to indicate the presence of a probable unit of analysis error. If cluster studies have been appropriately analysed, taking into account intraclass correlation coefficients, and relevant data documented in the report, synthesis with other studies is possible using the generic inverse variance technique.

Cross-over trials

We did not anticipate that any cross-over trials would have been conducted on this topic.

Dealing with missing data

Where data were missing, we contacted study authors and requested additional data. Where this was not possible, we stated explicitly where calculations were based on assumptions regarding missing data.

Assessment of heterogeneity

For both RCT and cohort meta-analyses, we formally tested for statistical heterogeneity using the Chi^2 test for statistical homogeneity with a 10% level of significance as the cut-off. We quantified the impact of any statistical heterogeneity using the I^2 statistic (Higgins 2002).

Where studies did not have combinable outcomes, we have provided the data in a narrative form.

Data synthesis

Where RCTs were found to be methodologically or clinically comparable, we planned to pool trial results in a meta-analysis. As we anticipated the presence of statistical heterogeneity we planned to combine the data using the random-effects model. As only one RCT was included we did not conduct a meta-analysis. However, if this was possible we had planned to combine the results and calculate the risk ratios and 95% CIs for dichotomous data. For continuous data, we planned to combine the MDs to calculate an overall MD and SD. If time-to-event data were available, we planned to combine the hazard ratios (HRs) reported in the RCTs using the generic inverse variance function.

Where cohort and ITS studies were found to be methodologically or clinically comparable, we pooled the results in a meta-analysis using the generic inverse variance function in RevMan to allow adjusted data to be used in the analysis. We anticipated heterogeneity due to the likelihood of different analytical techniques and different adjusted variables, and combined studies using the random-effects model.

For the cohort and ITS studies, we planned to report on the adjusted analysis using the estimate of effect reported in the study. Where the adjusted estimate of effect was reported with 95% CIs, we calculated the standard error (SE) in order to enter the data into RevMan, using the following formulae for ratio measures:

- lower limit = ln(lower confidence limit given for HR);
- upper limit = ln(upper confidence limit given for HR);
- intervention effect estimate = lnHR;
- SE = (upper limit – lower limit)/3.92.

Subgroup analysis and investigation of heterogeneity

We anticipated statistical heterogeneity due to differences between study populations and interventions. We planned to explore the expected heterogeneity using the following subgroups:

- setting: resource-constrained or resource-rich settings as defined by the World Bank as middle- or low-income countries and high-income countries, respectively;
- setting: international, national, regional or community settings;
- age: adolescent, adult or mixed populations;
- type of advertising: audiovisual, print or social media.

Sensitivity analysis

For RCTs, we planned to explore the effect of study quality on the results by excluding those studies where allocation concealment was unclear or inadequate from the meta-analysis and assessing the effect of this on the overall results. For cohort studies we planned to examine the effect of adjustments for confounding. If data were available, we also planned to explore the effects of funding source (industry versus non-industry) on the meta-analysis. As data were too limited, we were not able to conduct sensitivity analyses.

GRADE assessment

We used GRADEpro version 3.6 to create 'Summary of findings' and evidence profile tables. The GRADEpro software was developed as part of a larger initiative led by the Grading of Recommendations Assessment, Development and Evaluation (GRADE) Working Group. GRADE offers a system for rating the quality of evidence in systematic reviews and guidelines, and grading the strength of recommendations in guidelines (Guyatt 2011). Use of GRADEpro within a Cochrane systematic review facilitates the process of presenting and

grading evidence transparently (http://ims.cochrane.org/revman/other-resources/gradepro/about-gradepro).

In determining the level of evidence for each outcome, we integrated both the efficacy results and the assessment of the risk of bias into a final assessment of the level of evidence and provided full details of the decision in footnotes. For the one RCT identified, the quality of evidence started graded as high and we then downgraded where necessary to reach a final overall quality assessment. For the ITS studies, the quality of evidence started graded as low (due to the lack of randomisation and inherent limitations in inferring causality from this type of study) before we considered other quality parameters for grading.

RESULTS

Description of studies

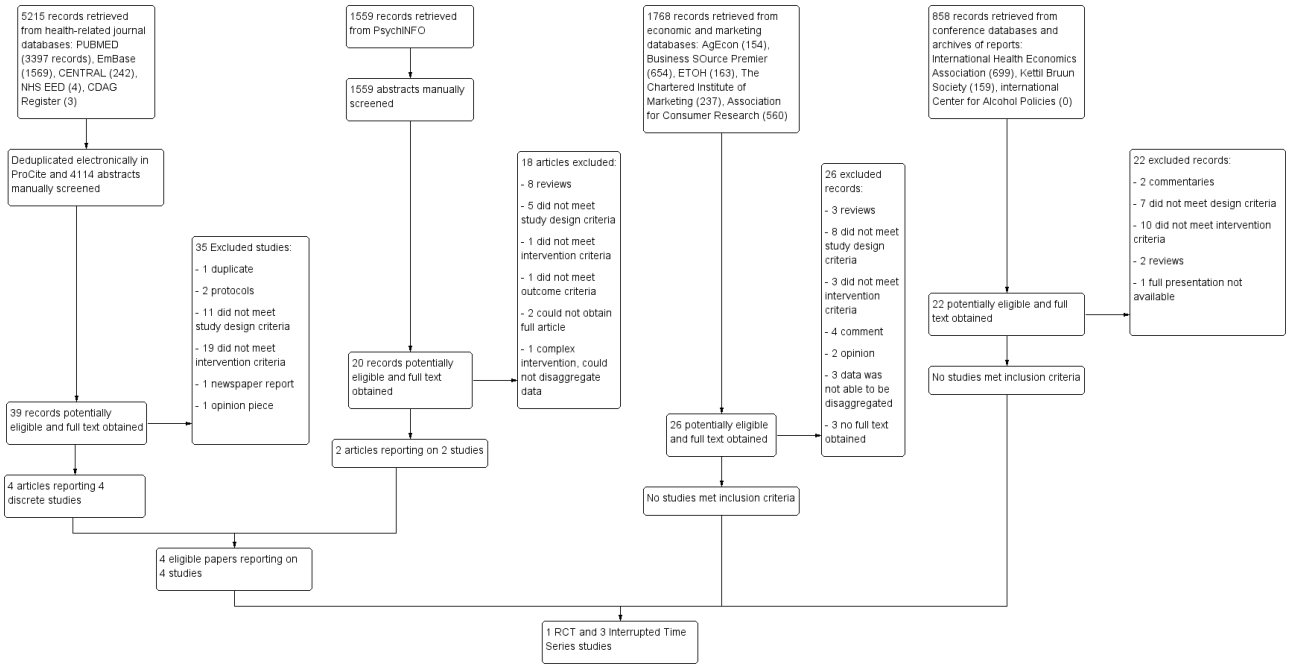
Results of the search

1. Journal databases

1.1 Health-specific databases

The February 2013 search of the electronic journal databases was conducted via OVID and retrieved 4114 records (see Figure 1 for the records retrieved per database).

Figure 1. Flow diagram of screening and eligibility of records of electronic databases: PubMed, EMBASE, The Cochrane Library (CENTRAL and UK National Health Service Economic Evaluations Database) and PsychINFO



After NS and DP manually screened all 4114 abstracts, we identified 39 records as possibly eligible and the full articles were obtained for eligibility assessment. Four of these articles reported on studies which were eligible for inclusion. We also identified a further 18 records which reported reviews or were likely to contain important references and obtained the full articles for these. See Figure 1.

The PsychINFO search was conducted later than the above search (on 14 June 2013) via EBSCOhost and retrieved 1559 records of which we identified 20 records as possibly eligible and obtained the full articles for further scrutiny. Two of these articles reported studies which were eligible for inclusion, both of which were already identified in the earlier search.

The May 2014 updated search retrieved a further 619 records from which none were eligible for inclusion (see Appendix 7 for the records retrieved per database).

1.2 Economic and marketing databases

We searched several other databases which are not specific to health or medical topics in order to ensure we included economic and marketing studies (see Appendix 8 for a full description of the databases, terms used and number of records retrieved). We searched a total of 1768 records of which 26 were potentially eligible and full articles were obtained. Of these none reported on eligible studies.

2. Conference databases

For the search of conference presentations, NS searched the relevant conference databases and archives of manual reports and retrieved 858 records, none of which related to studies that were considered eligible (see Appendix 9 for a full description of the conferences and report archives, responses to requests and number of records retrieved).

3. Trials registries

The search of ClinicalTrials.gov resulted in 159 titles, none of which were relevant to the review. The search of the WHO ICTRP resulted in 66 titles, none of which were relevant to this review.

Included studies

After conducting a full eligibility assessment on all the selected full articles, we identified four eligible studies: one RCT (Engels 2009) and three ITS studies (Makowksy 1991; Ogborne 1980; Smart 1976). Full details of each study is contained in the Characteristics of included studies table.

RCT

Individual level RCT

The RCT (Engels 2009) was conducted in the Netherlands and recruited 40 male pairs aged between 18 and 29 years old. Participants were randomised to one of three intervention groups or to a control group. Participants in the intervention groups watched movie clips containing either a high degree of alcohol content or a low amount of alcohol content interrupted with commercials containing advertising for alcohol products. The control group watched a movie clip containing a low amount of alcohol content and a commercial for neutral products. We interpreted the commercials for neutral products as the equivalent of a ban on alcohol advertising. The observed number of alcohol drinks consumed during the viewing session was counted and self-reported frequency of drinking prior to the trial was recorded.

Population level RCT

No RCT evaluating the effects of a restriction or ban on alcohol advertising at the general population level were identified.

ITS studies

All three ITS studies were conducted in Canada and were published more than 20 years ago (Makowksy 1991; Ogborne 1980; Smart 1976). Each of the studies evaluated a different type of ban (partial or full) compared with different degrees of restrictions or no restrictions during the control period. Ogborne 1980 and Smart 1976 evaluated the effects of an implementation of restrictions, whereas Makowksy 1991 evaluated the effects of lifting a restriction.

Ogborne 1980 compared the effects of a partial ban on beer advertising in print and electronic media implemented from 1974 onwards with the pre-ban period when no ban was in place in Manitoba. Per capita beer consumption was derived from monthly beer sales divided by the year's estimate of the size of the provincial adult population. The consumption rates in Manitoba were compared to those in the province of Ontario where no ban had been in place during the same period.

Smart 1976 evaluated a time-limited total ban on alcohol advertising for beer, wine and spirits in electronic, print and billboard media implemented in British Columbia on 1 September 1971 and continuing to 31 October 1972. Periods before and after the ban was implemented were used as the control period. The outcome assessed was per capita alcohol consumption measured by sales data for alcohol beverages and population estimates from census data.

The third study (Makowksy 1991) compared alcohol consumption before and after the lifting of a total ban on beer and wine advertising on the radio and television and in print media, which had been in effect for 58 years in the province of Saskatchewan. The total ban on advertising for spirits, which was part of the 58-year ban, continued in place and was not lifted. The outcome assessed was alcohol sales by volume derived from monthly sales data and expressed in litres of pure alcohol sold per population aged 15 years and older. The consumption rates were compared to those in the province of New Brunswick where a similar ban was in place and was not lifted during the same period.

Excluded studies

We excluded 35 of the articles retrieved from the combined journal database search, 18 articles from the PsychINFO search, 26 of the articles retrieved in the search of marketing and economic databases and 22 of the reports and presentations retrieved from the conference search. See Figure 1 for reasons for exclusion, which mainly included studies not meeting the study design criteria or not meeting the intervention design criteria.

Several prominent studies that have previously been included in reviews on this topic were excluded from our review. We document the specific reasons for exclusion of these studies in the Characteristics of excluded studies table. These included regression analyses of large national or regional datasets, which evaluated the association between consumption and whether or not countries or provinces within the regional datasets implemented advertising restrictions (Nelson 2001; Nelson 2003; Nelson 2010; Saffer 1991; Saffer 2002; Young 1993). Although these studies can be viewed as pooled ITS studies, they did not meet all the review study inclusion criteria for ITS studies, specifically that the intervention could be identified as occurring at a clearly defined point in time. Data was aggregated and not analysed or reported within individual countries or states, and no defined points in time were reported for ban implementation. The country-specific data were not available from the authors for further analysis.

The Loi Evin 1999 report of the French government details the consumption of alcohol in France before, during and after the introduction of the *Loi Evin* (ban on alcohol and smoking advertising) implemented in 1991. The law curtails alcohol advertising on television and in cinemas, and disallows sport sponsorship. In the report data are not presented in a manner that allowed us to extract them, as only percentages of use over time are reported. The report states that in France alcohol consumption was declining prior to the introduction of the banning law and that internal surveys have produced contradictory results. The report notes that the proportion of alcohol consumers aged 12 to 18 years had a tendency to decline in the 1980s, but then increased significantly between 1991 and 1995, from 47% in 1991 to 65% in 1995. No variance or significance levels were provided.

Risk of bias in included studies

We assessed the risk of bias using the combination of the standard appraisal for RCTs and the EPOC appraisal specifically for ITS studies (see Appendix 5 and Appendix 6). We provide a full description of the risk of bias for each included study in the Characteristics of included studies table, which is summarised in Figure 2 and Figure 3. None of the included studies were funded by the alcohol or advertising industries.

Figure 2. 'Risk of bias' graph: review authors' judgements about each risk of bias item presented as percentages across all included studies (N = 4).

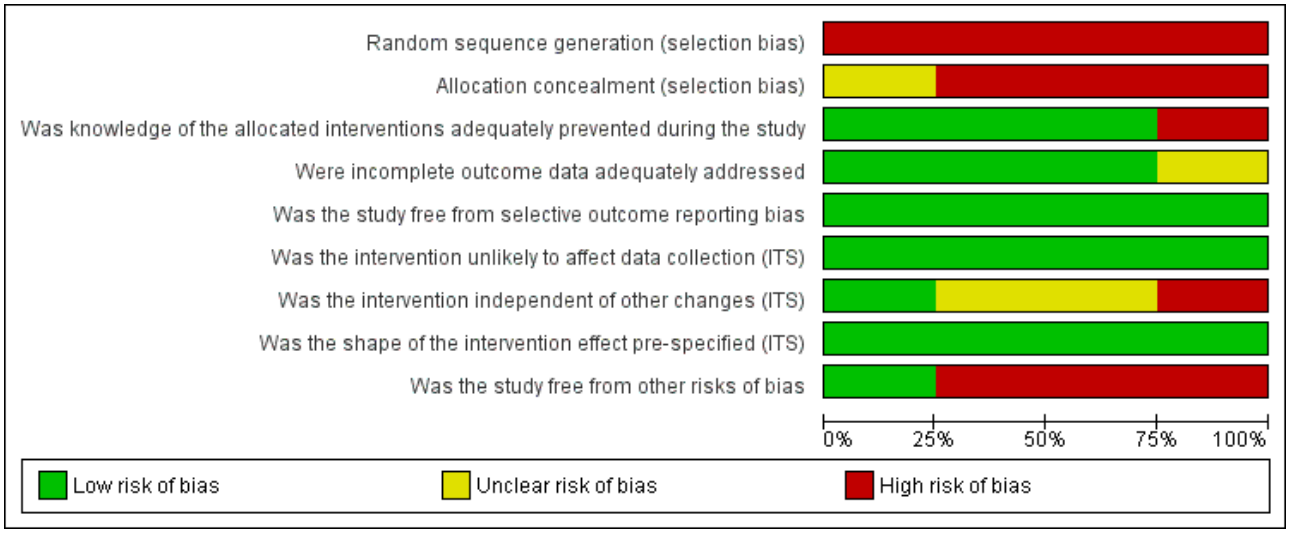
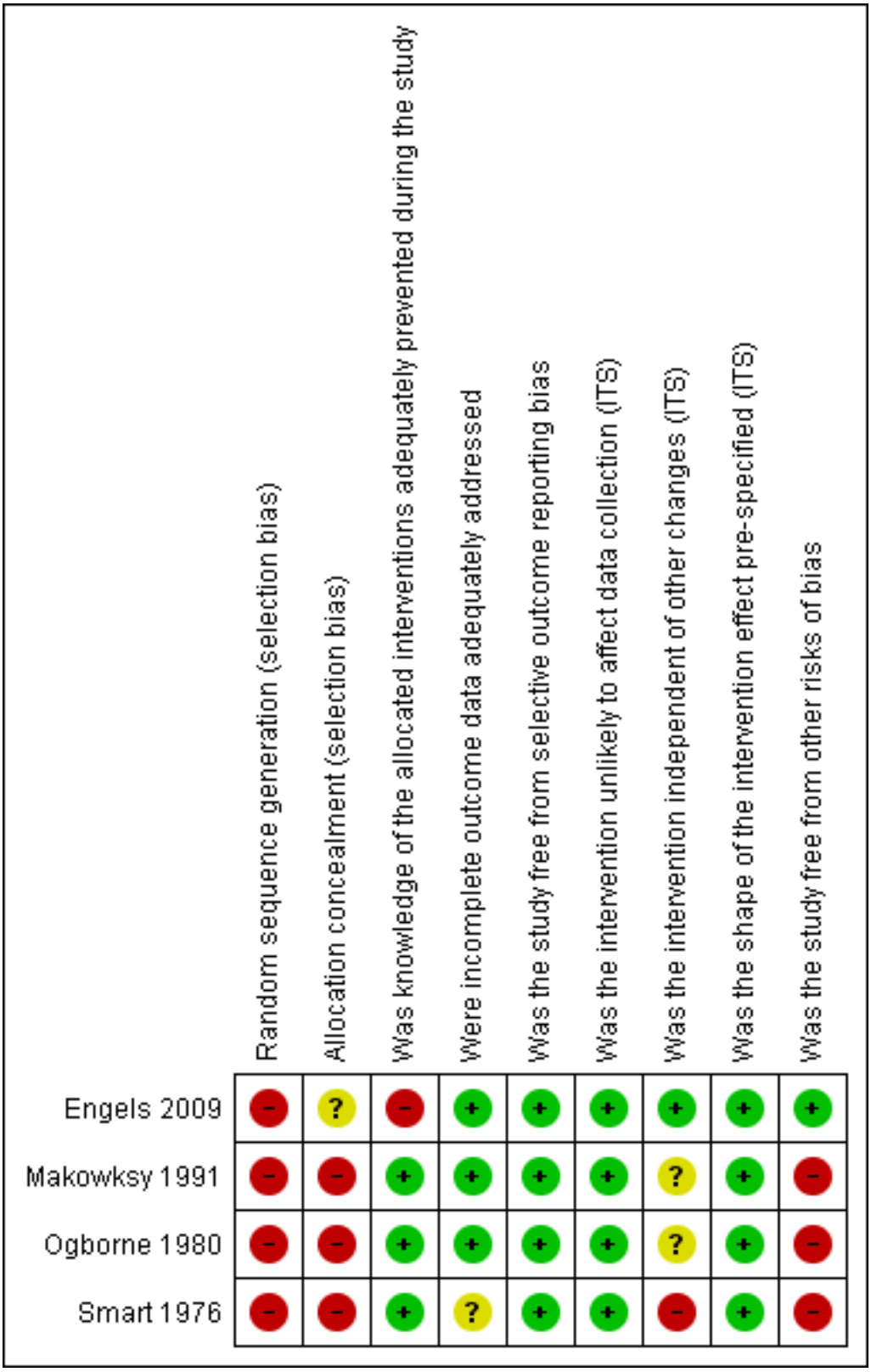


Figure 3. 'Risk of bias' summary: review authors' judgements about each risk of bias item for each included study (N = 4).



Allocation

In Engels 2009 the method of generating the sequence or concealing allocation is not reported. The article states that men who were in the group allocated to watch movies with a high alcohol content reported higher rates of drinking in the week prior to the study indicating that randomisation did not achieve similar baseline differences between groups. We assessed the risk of biasas high.

We assessed all three ITS studies as having a high risk of bias due to a lack of randomisation and allocation concealment.

Blinding

Blinding of research staff was absent in the Engels 2009 trial and detection bias may be present so we rated the risk of bias as high.

We assessed the risk of performance and detection bias to be low in the three ITS studies, as outcomes were objectively measured by routine data collection and the outcomes were unlikely to be influenced by knowledge of the intervention groups.

Incomplete outcome data

We judged the risk of attrition bias to be unclear in Smart 1976, as data were not available for all alcohol types across all the same time periods. We judged the risk of attrition bias as low in the RCT and in the other two ITS studies.

Selective reporting

We considered that none of the studies were at risk of selective reporting bias. The Engels 2009 trial was not registered on a prospective trials registry but results were reported for all the outcomes identified in the methods section. We judged it to be unlikely that the outcomes were changed during the reporting period. For the three ITS studies, there is no indication that other outcomes would be of interest.

Other potential sources of bias

We made three additional assessments of risk of bias specifically pertaining to the ITS studies. These were whether the intervention would affect data collection, whether the intervention was independent of other changes, and whether the shape of the intervention effect had been prespecified.

Data collection influenced by intervention

For all three ITS studies the data were collected from routine sources and we considered the studies to be at low risk of bias.

Intervention independent of other changes

For Makowksy 1991 and Ogborne 1980, there was no report of historical or political reasons underpinning the decisions to lift or implement the ban. In Smart 1976, the advertising ban was initiated by a unanimous political vote, but the ban was stopped after elections when there was a change in political power. There is a likelihood that other political or social changes may have coincided with the period of the ban and as a result we rated this study as at high risk of bias.

Shape of the intervention effect pre-specified

The directional effects of implementing or lifting advertising bans on alcohol consumption or sales were predicted in all three ITS studies prior to testing the intervention effect.

Other forms of bias

We judged all three ITS studies to be at high risk of bias introduced by a possible dilution effect on the advertising restrictions caused by an inability to regulate or control advertising originating in neighbouring provinces or the USA and available in print or electronic media.

Effects of interventions

See: **Summary of findings for the main comparison** Non-alcohol commercials compared to alcohol commercials for reduction of alcohol consumption; **Summary of findings 2** Alcohol ban compared to no ban for the general population

The RCT differed significantly from the three ITS studies in terms of design, participant level (individual versus population level) and duration. For this reason we present the results stratified according to study design and did not seek to conduct meta-analysis across study design.

Alcohol consumption

RCT data

Engels 2009 reported that there were baseline differences between groups with participants in the groups exposed to movie clips with high-alcohol content and commercials for alcohol reporting higher alcohol consumption in the week prior to the trial than those in the groups exposed to low-alcohol content movie clips and neutral commercials. This was reported as a statistically significant difference (t (38) = 2.9; p value < 0.01). The means presented in the trial report were corrected for this difference using analysis of covariance but no further details are provided. Using the corrected means and reported SEs we calculated the SDs using the formula: SD = SE * Sqrt(N) to allow data entry into RevMan.

The Engels 2009 trial found that young men who viewed a movie clip with a low-alcohol content, regardless of the content viewed in the commercial breaks, drank a mean of 1.73 (SD 1.33) glasses of alcoholic drink compared with young men viewing a movie clip with a high-alcohol content who drank a mean of 2.38 (SD 1.33) glasses of alcoholic drink. This was a statistically significant difference (MD -0.65 drinks, 95% CI -1.2, -0.07; p value = 0.03). See Analysis 1.1. The number of alcoholic drinks consumed was 1.69 (SD 1.38) in young men who viewed commercials with no alcohol content compared with a mean of 2.42 (SD 1.25) alcoholic drinks in young men who viewed commercials for alcohol, regardless of the content of alcohol portrayed in the movie clips. This was a statistically significant difference (MD -0.73 drinks, 95% CI -1.30, -0.16); p value = 0.01). See Analysis 2.1.

As participants were recruited in pairs, the investigators conducted an analysis to adjust for clustering effects within pairs. The total alcohol consumption was reported to be statistically significantly higher in young men who viewed movie clips with a high-alcohol content regardless of commercial content compared with young men who viewed movie clips with a low-alcohol content (coefficient 0.74, 95% CI 0.05, 1.43; SE 0.35; p value = 0.03). See Analysis

3.1. Total alcohol consumption was statistically significantly higher in young men who viewed commercials with alcohol content compared with those who viewed commercials with neutral content, regardless of the content of alcohol portrayed in the movie clips (coefficient 0.83, 95% CI 0.14, 1.52; SE 0.35; p value = 0.02). See Analysis 4.1. Outcomes were assessed immediately after the intervention so no follow-up data were available for evaluating the longer-term effects of the low-alcohol content movies or advertising.

ITS data

Due to differences in the reported types of effect estimates between the ITS studies, we were not able to combine these in a meta-analysis, with the exception of Ogborne 1980 and Smart 1976. which both reported the mean percentage change in beer consumption.

Smart 1976 provide graphs and some statistical test results in the text, but we were unable to extract sufficient details for entering into RevMan with the exception of the data for beer consumption. We provide the results in narrative form as reported by the authors. The reported yearly per capita consumption data analysis did not show any effects of the 14-month total ban on alcohol advertising implemented in 1971 and lifted in 1972. The authors reported a Mann-Whitney U-test indicating that there were no statistically significant differences in consumption of beer, wine and spirits during the ban years compared to the pre-ban years (z 0.31, p value > 0.05).

A more detailed analysis using moving averages and a t-test was conducted in Smart 1976 using monthly data in order to account for the ban spanning parts of two calendar years. The article reports that neither of the t values for wine nor beer was significant, with a reduction in wine consumption during (12%) and after (20%) the ban. The authors report that it was not possible to assess the monthly data on spirit consumption as data were not available for a full 24-month period either before or after the ban. The authors report that inspection of the graph of spirit consumption shows similar results as for the beer data, with no graphically noticeable effect on consumption.

In Makowksy 1991 the effects of changing a total ban on all forms of alcohol advertising to a partial ban on spirits advertising only was compared for 2.5 years before the lifting of the ban and for 3.5 years after the lifting of the ban in Saskatchewan. Two types of models were applied to the data - Abrupt and Gradual Auto-regressive Integrated Moving Average (ARIMA) models, which adjust for seasonality, trends and random error. Both models assumed the change would be a permanent and not a temporary effect, given that the ban was not reversed. Following the lifting of the ban, the Abrupt ARIMA model indicated that the volume of all forms of alcohol sales decreased by 11.11 kilolitres (95% CI -27.56, 5.34; p value = 0.19) per month. This decrease was not statistically significant. See Analysis 5.1. Each type of alcohol was also examined separately within the model: the volume of beer sales increased statistically significantly by 14.89 kilolitres (95% CI 0.39, 29.39; p value = 0.04) per month following the ban; the volume of wine sales increased by 1.15 kilolitres (95% CI -0.91, 3.21; p value = 0.27) per month following the ban and was not statistically significant; and the volume of spirits decreased statistically significantly by 22.49 kilolitres (95% CI -36.83, -8.15; p value = 0.002). See Analysis 5.2, Analysis 5.3; and Analysis 5.4.

The Gradual ARIMA model (see Analysis 6.1; Analysis 6.2; Analysis 6.3; Analysis 6.4) did not find any statistically significant effects of the ban (under the assumption that prior to the intervention (lifting of the ban) the series was trendless). The authors conclude that the change in legislation regulating advertising of alcoholic beverages cannot be well modelled within the context of a gradual permanent impact on sales volumes due to a lack of statistical significance in the estimates for each type of alcoholic beverage and for total alcohol consumption.

Ogborne 1980 evaluated the effects of a partial ban on beer advertising on beer consumption and not on other forms of alcohol. Smart 1976 and Ogborne 1980 both reported the mean percentage change in beer consumption. In Ogborne 1980 the SD was not explicitly labelled as such so we made an assumption that the reported values were SD. We calculated the SE using the formula: SE = MD/t value.

Ogborne 1980 found a mean percentage increase in beer consumption of 4.5% (SD 2.15) following implementation of the partial ban on beer advertising, and Smart 1976 found a 2% (SD 1.66) decrease in beer consumption following implementation of a total ban on all forms of alcohol advertising. We combined the results in a meta-analysis using the random-effects model producing an overall mean percentage increase in beer consumption of 1.10% following the implementation of the bans (95% CI -5.26, 7.47; p value = 0.43). See Analysis 7.1. The finding was not statistically significant and considerable heterogeneity is present (Chi^2 = 5.72, df = 1 (p value = 0.02); I^2 = 83%) indicating that 83% of the variability in the effect estimate is due to heterogeneity rather than chance alone. We advise that these results should be interpreted with caution.

None of the ITS studies reported on adverse effects, either in terms of economic losses to the alcohol or advertising industries or in reductions in government tax revenues income.

GRADE ASSESSMENTS

GRADE assessments were conducted for all outcomes where data were available to enter into GRADEPro. For the 'Summary of findings' tables we selected seven outcomes per comparison and ranked their importance.

Using the GRADE approach to assess the overall quality of the evidence, we rated the quality of the evidence generated from the RCT as very low for the outcome of alcohol consumption (Summary of findings for the main comparison). This was due to a serious risk of bias, serious indirectness of the included population and serious imprecision present in the results, primarily driven by the small sample size. No other outcomes were measured and therefore could not be graded.

Overall, when using the GRADE approach, we judged the evidence for alcohol consumption arising from the ITS studies to be very low quality. This was due to a high risk of bias arising from a lack of randomisation and imprecision in the results (Summary of findings 2). As for the RCT, no other outcomes were measured and therefore could not be graded.

Using the GRADE approach, we conclude that we have very little confidence in the effect estimates and that the true effect is likely to be substantially different from the estimate of effect.

DISCUSSION

Summary of main results

There is a lack of robust evidence either in support of or against restricting the advertising of alcohol to reduce alcohol consumption. One small RCT and three ITS studies were included in this review. Although the RCT found a statistically significant reduction in alcohol consumption among young men who were not exposed to alcohol advertising compared with young men who were exposed to alcohol advertising, the results should be viewed with caution in the light of the high risk of bias identified within the trial. The RCT did not evaluate any longer-term effects as there was no follow-up period, which limits inferences beyond the immediate effects.

Two of the three included ITS studies evaluated the implementation of a ban on advertising and the other ITS study evaluated the lifting of a ban which had been in place for over 50 years. The results from the three ITS studies were inconsistent. A meta-analysis of the two studies which evaluated the implementation of a ban showed a non-statistically significant mean percentage increase in beer consumption in the general population following the ban. The study evaluating the lifting of a total ban on all forms of alcohol advertising to a partial ban on spirits advertising only indicated that the volume of all forms of alcohol sales decreased per month after the ban was lifted. This was not statistically significant.

Overall completeness and applicability of evidence

The RCT was conducted in young, educated Dutch men in a university setting and may not be generalisable to women, older men or people living in rural and low-resource settings. The results from the trial do, however, provide an indication of the potential for an RCT design to evaluate the immediate response behaviour of study participants to televised alcohol advertising. Stronger evidence would be gained from replicating trials within different age groups, with gender mixes, and in high- and low-resource settings. The use of other electronic media, such as mobile phone messaging, for alcohol advertising and the context in which the advertising is delivered (e.g. television, at a cinema, on the internet) would also require consideration to ensure wider applicability of the results from future trials.

The ITS studies included in this review are over two decades old, thus limiting the utility of the findings to the present day landscape. With the advent of the internet and social media, it may be impossible to implement an advertising ban effectively. All three studies measured general population alcohol consumption using alcohol sales data and population census data. The figures from statistical records were used to estimate per capita alcohol consumption. Such per capita estimates provide an average picture but hide variations in consumption that exist, for instance, between heavy- and moderate-alcohol drinkers, and among young people. An assessment of impact in this regard would require taking into account the patterns of change that occur between specific population groups as a result of a ban or restriction on advertising. Although household surveys may under-report alcohol consumption (Stockwell 1999), they are most likely to show variations in the impact of a ban on advertising in an ITS study. Such an approach may also capture the effects on youth who are under the legal drinking age and who may not have started to drink yet. Hastings and colleagues have emphasized the importance of this group as they argue that the alcohol industry targets advertising to such persons to get them to begin drinking (Hastings 2010).

None of the included studies measured the additional outcomes prespecified in the review, including delaying the age of initiation of alcohol use or rates of reported risk behaviours, alcohol-related injuries or other harms, or individual spending on alcohol. In addition, none of the studies considered the potential adverse effects of advertising restrictions, such as loss of revenue from the alcohol and advertising industries and a reduction in GDP attributable to alcohol sales, nor did any refer to potential job losses in the marketing and communication sectors due to an advertising ban. Future studies should aim to measure outcomes and adverse effects as comprehensively as possible in order to provide a balanced overall assessment of the effects of implementing advertising bans and restrictions.

We did not identify any studies conducted in resource-constrained settings. In an assessment of the international determinants of alcohol advertising restrictions, Gallet and Andres conducted Probit regressions using observations from the year 2002 for 103 countries captured in the Global Information System on Alcohol and Health of the WHO (Gallet 2011). From the analysis they concluded that advertising restrictions were more probable in countries with higher income, higher life expectancy, higher percentage of the population that is young, and with a majority of the population that is Muslim. With the exception of the last observation, the studies included in this review broadly meet these criteria, with all three ITS studies conducted in Canada, a high-income setting. There is therefore a clear gap in the evidence base regarding the influence of advertising restriction on general population alcohol consumption levels in low- and middle-income countries. Authors of an overview of alcohol policy reform in Australia note that population-wide interventions, such as advertising bans, may be more equitable than those interventions aimed at reducing alcohol harms, which rely on a healthcare practitioner for delivery (Doran 2010). This argument suggests that advertising restrictions may be an appropriate intervention for resource-constrained settings should effectiveness be demonstrated.

Quality of the evidence

We judged the single RCT identified to be at a high risk of bias due to inadequate randomisation, uncertain allocation procedure and a lack of blinding. When using the GRADE approach to assess the overall quality of the evidence, we rated the quality of the evidence generated from the RCT as very low for the outcome of alcohol consumption (Summary of findings for the main comparison). This was due to the risk of bias, indirectness of the included population and imprecision present in the results, primarily driven by the small sample size.

The three ITS studies were well conducted and met most of the criteria outlined by the EPOC 'Risk of bias' assessment. However, we identified all three as at risk of a dilution effect because advertising arising from neighbouring Canadian provinces or from the USA was not subject to regulation and the integrity of the intervention was thus compromised. Nevertheless, such dilution effects are a reflection of the current reality as alternative advertising forms, such as social media and internet-driven advertising, arising from regions outside a study area or region

where a ban is implemented, will remain challenging to regulate. In addition, other forms of alcohol control policy within a region may also dilute or (potentially) increase the impact of a reduction in advertising should such an impact exist. We were not able to determine this clearly from the study reports.

Seasonality was addressed inconsistently between the studies and different types of analyses were employed in each study to address it. This difference likely reflects the development of more sophisticated analyses over the 15-year period between publication of the first ITS study in 1976 and the last ITS study in 1991. Overall, when using the GRADE approach, we judged the evidence arising from the three studies to be of very low quality. This was due to the high risk arising from a lack of randomisation and imprecision in the results (Summary of findings 2).

Potential biases in the review process

We minimised possible selection biases in the review process by using a comprehensive search strategy to identify studies and, wherever possible, independently selecting and appraising the studies. In addition to searching journal electronic databases, we also searched conference databases and prospective trials registries, and contacted experts in the field who may have been aware of unpublished or ongoing studies. We contacted several authors of conference abstracts to confirm whether the data in their abstracts corresponded to subsequent journal articles or to assess whether the reported data were eligible for inclusion in this review. It is unlikely that we have missed any important studies given the close partnership we established with agencies and organisations working in this area.

Two authors independently carried out data extraction and quality assessment, which was checked by a third author. We presented the preliminary results at a Global Alcohol Policy Alliance meeting in Seoul, South Korea, in October 2013 and we have incorporated the feedback obtained into the review.

Agreements and disagreements with other studies or reviews

In 1988, Smart published a review of empirical studies on whether alcohol advertising affects overall consumption (Smart 1988). In addition to the two ITS studies we included in this review, which were published at the time (Ogborne 1980; Smart 1976), he reports on the lack of an effect of advertising bans implemented in Norway in 1975 and Finland in 1976, but presents no study designs or data. Despite the 25 years that have passed, our review agrees with his conclusion that no studies have examined the effects of advertising bans on specific segments of the population, such as heavy drinkers or young people. However, our review disagrees with his conclusion that advertising bans do not affect overall alcohol consumption as the data included in our review indicates that there is uncertainty as to whether this effect is beneficial, neutral or harmful. We did not identify any other reviews which specifically focused on the causal relationship between advertising restrictions and alcohol consumption.

During our search for eligible studies, we identified several reviews of the association between advertising and alcohol consumption, many of which were targeted at evaluating the link between advertising and the youth market. An argument can be made that should a causal link be shown between advertising and consumption, then reducing advertising should reduce consumption. It should be noted that we did not conduct a systematic search or critical appraisal of these reviews and we present the results of these reviews as reported by the authors.

A review published online in 2013 (Aspara 2013) reports on a qualitative review of 16 studies which the authors claim are most referred to by alcohol and addiction researchers to show that alcohol advertising increases total consumption. They conclude that the evidence is undermined by several methodological problems including the exclusive use of survey data, use of self-reported data, a lack of exclusive outcomes in young people and the high attrition noted in many of the longitudinal studies. They recommend large-scale field experiments and note that advertising should not be evaluated apart from other marketing variables, especially pricing. In a 2009 systematic review, Smith and Foxcroft (Smith 2009) identified seven cohort studies conducted almost exclusively in young people (more than 13,000) and concluded that the modest association effect size observed between exposure to alcohol advertising and subsequent alcohol consumption is likely to be limited by residual or unmeasured confounding. In another systematic review of 13 longitudinal studies of 38,000 young people, also published in 2009, Anderson et al. found that there was a consistent association between exposure to media/commercial communications and alcohol and adolescents starting to drink alcohol, but the authors acknowledge that they did not attempt to quantify the quality of study characteristics other than the longitudinal design (Anderson 2009a). In a 2010 published summary of the second edition of the book *Alcohol: No ordinary commodity*, the Alcohol and Public Policy Group report that there is consistent evidence to show that alcohol marketing reduces the age of onset of drinking and increases consumption by those who are already drinkers (Alcohol and Public Policy Group). The summary reports that despite the consistent evidence in support of the association, the question of whether restrictions are effective in reducing consumption remains unknown.

AUTHORS' CONCLUSIONS

Implications for practice

There is currently no robust evidence for or against recommending the implementation of alcohol advertising restrictions. Governments and ministries considering implementing restrictions on alcohol advertising should ideally consider delivery of the restrictions within a high-quality, well-monitored research programme to ensure that the intervention is evaluated over time on all relevant outcomes and that useful data to build the evidence base are generated.

Implications for research

Individual level studies

At an individual level, the need for well-conceived and -conducted RCTs exists. Men and women, young and old, and of different prior drinking habits, can be randomised to viewing or receiving marketing media for alcoholic beverages or viewing or receiving neutral marketing media. This can be done as a short-term study or over a longer period of time. Their immediate and ongoing drinking responses to such marketing will provide important evidence to support or refute the use of advertising restrictions to reduce individual alcohol consumption. Consideration will need to be

given to stratification by previous levels of drinking as differences in responses may exist between social and heavy drinkers.

Population level studies

Any country-level ban should be delivered within a research context to ensure data are added to the evidence base.

As the feasibility of conducting an RCT within or between countries is questionable, the recommended study design that can be implemented at a country level is the ITS study. We outline the ideal process for such a study below.

1. Prior to the ban implementation, data are collected for at least a year to 18 months in advance to allow for adequate data collection
2. Data on consumption need to be collected at least three time-points before and after implementation. Data collection would include:
   a. Monthly industry (sales) data to assess general population level consumption
   b. Household or individual surveys to assess individual level consumption
   c. Incidence of alcohol-related mortality and morbidity (e.g. road traffic injuries, deaths from alcohol-related inter-personal violence)
   d. Alcohol industry revenue
   e. Advertising industry revenue
3. Appropriate statistical analysis should be used to analyse the data
4. The ITS can be ongoing with monitoring procedures integrated into routine data collection to observe changes or dilution over time

The length of time required to establish whether a ban has been effective or not is currently unclear. In a seminal experiment in the USA, the effect of a reduction in expenditure on beer advertising was felt within the beer production company as a sales decline 18 months after the cessation of advertising (Ackhoff 1975). Thereafter, it took six months after the reinstatement of normal advertising to restore sales to normal growth rates. This study provided evidence of a so-called carryover effect that once advertising is stopped, it can take a while (18 months in this case) for the effects of advertising to become ineffective. Although there are highly likely to be contextual differences, in the absence of other evidence it seems reasonable to monitor the effects of any country-level ban at least for 18 months.

The proposed approach to a country-level ITS study outlined above is in agreement with the International Alcohol Control study, a multi-country collaborative project that aims to assess the impact of alcohol control policy and policy changes in a longitudinal survey of drinkers from Australia, England, Mongolia, New Zealand, Peru, Saint Kitts and Nevis, Scotland, South Africa, South Korea, Thailand and Vietnam (Casswell 2012) . Data are collected annually in repeated surveys of 2000 respondents aged between 18 and 65 years per country. Outcome variables will provide comprehensive alcohol consumption data.

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* Indicates the major publication for the study

CHARACTERISTICS OF STUDIES

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

Engels 2009

Methods	STUDY TYPE: • Randomised controlled trial COUNTRY: • Netherlands SETTING: • Radboud University Nijmegen campus in a bar laboratory equipped as a relaxing room with a comfortable couch and a big screen television. Ashtray, nuts and chips were provided and a refrigerator was stocked with soft-alcoholic drinks (beer and wine) and soft drinks DURATION OF RECRUITMENT: • Not reported DURATION OF TRIAL: • Not reported. The intervention took 1.5 hours FOLLOW UP: • Not applicable as the outcomes were measured during the intervention process • A questionnaire was conducted with participants on completion of the intervention
Participants	INCLUSION CRITERIA: • Males aged 18 to 29 years • Each male was invited to attend with a male friend so the units of analysis was at the pair level EXCLUSION CRITERIA: • Not explicitly reported

Engels 2009 *(Continued)*

	Number of participants randomised: 80 in 40 pairs, each pair randomised to one of four exposure groups (20 participants in each group) Baseline data: • No numeric data reported according to group allocation • Mean age was 21.45 years with a SD 2.1 • There were reported differences in weekly drinking between allocated groups: previous week's alcohol consumption was higher in the Alcohol Movie/Alcohol Commercial (AM/AC) group than in the Non-alcohol Movie/Non-alcohol Commercial (NM/NC) group (mean 31.2 drinks, SD 17.1 versus mean 17.8 drinks, SD 11.7; t(38) = 2.9; p value < 0.01)

Interventions	Three discrete interventions and one control group were provided.
	
	Prior to the interventions, all participant pairs were told that they would see a movie clip interrupted by two commercial breaks and to act like they were relaxing at home. Free drinks were available in the refrigerator, nuts and chips were offered and smoking was allowed. Taxi fare was provided for men who drank three or more bottles of wine or beer and all participants received nine euros for their participation
	
	INTERVENTION AM/AC (20 participants):
	
	• Alcohol movie with alcohol commercials
	  ◦ Each pair watched a 1 hour movie clip from 'American Pie 2' - a comedy containing strong sexual content and nudity, and crude humour and drinking content (characters drank alcohol 18 times and alcoholic beverages were portrayed an additional 23 times)
	  ◦ After 14 and 33 minutes, the movie clip was interrupted with a commercial break for 3.5 minutes for neutral content (cars or a video camera) and alcohol content
	
	INTERVENTION Alcohol Movie/Neutral Commercial (AM/NC) (20 participants):
	
	• Alcohol movie with neutral commercials
	  ◦ Each pair watched a 1 hour movie clip from 'American Pie 2' (as above)
	  ◦ After 14 and 33 minutes, the movie clip was interrupted with a commercial break for 3.5 minutes for neutral content (cars or a video camera) only
	
	INTERVENTION Neutral Movie/Alcohol Commercial (NM/AC) (20 participants):
	
	• Non-alcoholic movie with alcohol commercials
	  ◦ Each pair watched a 1 hour movie clip from '40 days and 40 nights' - a comedy containing strong sexual content and nudity and limited drinking content (characters drank alcohol 3 times and alcoholic beverages were portrayed an additional 15 times)
	  ◦ After 14 and 33 minutes, the movie clip was interrupted with a commercial break for 3.5 minutes for neutral content (cars or a video camera) and alcohol content
	
	CONTROL NM/NC (20 participants):
	
	• Non-alcoholic movie with neutral commercials
	  ◦ Each pair watched a 1 hour movie clip from '40 days and 40 nights' (as above)
	  ◦ After 14 and 33 minutes, the movie clip was interrupted with a commercial break for 3.5 minutes for neutral content (cars or a video camera) only
	
	The commercials were selected to be similar in terms of number, length and diversity of the presented products

Outcomes	The outcomes were not clearly reported as primary or secondary. OUTCOMES: • Alcohol consumption: ◦ Observed number of drinks consumed in the 1 hour movie session. Bottles of beer contained 200 mL; bottles of wine contained 250 mL. To assess the total amount of alcohol consumed, the count-

Engels 2009 *(Continued)*

ed number of bottles of wine consumed was multiplied by 1.6, to attain an outcome relating to the amount of alcohol in one bottle of beer
    - Self-reported number of drinks drunk during a typical 1 hour television viewing (via questionnaire)
    - Self-reported frequency of drinking
    - Self-reported incidence of drunkenness in past 12 months
- Appreciation of the movie: nine question 5-point rating scale

Notes	ETHICS: The local ethics committee approved the laboratory protocols INFORMED CONSENT: This is unlikely as the article states that none of the participants guessed the real aim of the study indicating that this was withheld from them. Participants provided written permission to be video and audio recorded and to allow the footage to be used afterwards FUNDING: The lead author was funded by a fellowship of the Netherlands Organisation for Scientific Research. Funding for the study was received from The Netherlands Organisation for Scientific Research and a private organisation called STAP, an organisation against alcohol misuse and its consequences. The report states that both organisations were not involved in the development of design, collection of the data, writing the paper or decision to submit the paper for publication

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	The method of generating the sequence is not reported. The article states that men who were in the group allocated to watch movies with a high alcohol content reported higher rates of drinking in the week prior to the study indicating randomisation was not successful
Allocation concealment (selection bias)	Unclear risk	Not reported
Was knowledge of the allocated interventions adequately prevented during the study	High risk	The research staff were aware of the allocated groups. Participants were aware of the content they were watching but were unaware whether they were in an intervention or control group
Were incomplete outcome data adequately addressed	Low risk	All participants completed the trial and outcomes were available for all 80 participants
Was the study free from selective outcome reporting bias	Low risk	The trial was not registered on a trial database but results were reported for all outcomes identified in the methods section of the paper
Was the intervention unlikely to affect data collection (ITS)	Low risk	Not applicable to RCT
Was the intervention independent of other changes (ITS)	Low risk	Not applicable to RCT

Engels 2009 *(Continued)*

Was the shape of the intervention effect pre-specified (ITS)	Low risk	Not applicable to RCT
Was the study free from other risks of bias	Low risk	There is no indication of other bias

Makowksy 1991

Methods	STUDY TYPE: • Interrupted Time Series COUNTRY: • Canada SETTING: • Provinces of Saskatchewan and New Brunswick DURATION OF STUDY PERIOD: • 1 April 1981 to 31 March 1987 ANALYSIS TYPE: Reported as time series analysis using the methods of Box and Jenkins (1970). Auto-regressive, integrated moving average (ARIMA) models were used
Participants	Adult population 15 years and older purchasing alcohol
Interventions	INTERVENTION: *Type:* • Total ban on beer, wine and spirits advertising (the report describes the ban as partial as advertising from other media originating from outside the province (e.g. cable television), was not possible to ban; for the purposes of this review, the ban is considered total within the province) *Media:* • Radio (beer, wine, spirits) • Television (beer, wine, spirits) • Newspapers and magazines (beer, wine and spirits) *Duration of intervention:* • 1 April 1981 to 3 October 1983 • The ban had been in effect for 58 years prior to being lifted in October 1983. The intervention period includes the final two years of the ban period, i.e. 1981 to 1983. CONTROL: *Type*: • Partial ban for spirits only (the ban on advertising for spirits continued to be applied, with the exception ofthe print media where spirits could be advertised) *Media*:

Makowksy 1991 *(Continued)*

- Radio (spirits)
- Television (spirits)

Duration of control:

- Post-ban after lifting of the ban in October 1983 until 31 March 1987

COMPARISON:

The consumption rates were compared to those in the province of New Brunswick where a similar ban had been in place and was not lifted during the same period

Outcomes	PRIMARY OUTCOME:
	• Per capita consumption:
	  ○ The initial unit of measure was monthly sales data for alcohol beverages across the province. Sales data were derived from monthly reports of the Saskatchewan and New Brunswick Liquor Commissions that were sent to Statistics Canada. Total volume of sales was measured in terms of sales of absolute alcohol per litres for the population 15 years and older. Volumes of absolute alcohol were derived from the relative alcohol content using the following percentages per alcohol type:
	  ■ Spirits: 39%
	  ■ Wine: 10%
	  ■ Beer: 5%
	SECONDARY OUTCOMES:
	• None reported

Notes	ETHICS: Not applicable as nationally aggregated data. FUNDING: Not clearly reported; study undertaken by employees of Health Services and Promotion branch of the Health and Welfare Canada

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Not a RCT
Allocation concealment (selection bias)	High risk	Not a RCT
Was knowledge of the allocated interventions adequately prevented during the study	Low risk	The outcome of monthly alcohol sales was objectively measured by routine data collection and was thus unlikely to have been influenced by knowledge of the intervention
Were incomplete outcome data adequately addressed	Low risk	There is no report of missing data as each month is accounted for. The methodology that the liquor commissions used to collect data was not reported
Was the study free from selective outcome reporting bias	Low risk	There is no indication that other outcomes would be of interest

Makowksy 1991 *(Continued)*

Was the intervention unlikely to affect data collection (ITS)	Low risk	The data were collected from routine source before and after the lifting of the ban
Was the intervention independent of other changes (ITS)	Unclear risk	No report of historical or political reasons underpinning decision to lift the ban
Was the shape of the intervention effect pre-specified (ITS)	Low risk	Yes, the lifting of the ban was predicted to increase sales of alcohol
Was the study free from other risks of bias	High risk	There is an acknowledged possibility that advertising from other provinces and countries would not have been stopped by the ban, causing a dilution effect. Seasonality may have affected results and this is addressed in the analysis

Ogborne 1980

Methods	STUDY TYPE: • Interrupted Time Series COUNTRY: • Canada SETTING: • Province of Manitoba DURATION OF STUDY PERIOD: • January 1970 to January 1978 ANALYSIS TYPE: Reported as time series analysis using the methods of Glass, Wilson and Gottman. t test values reported
Participants	Defined as adult population purchasing alcohol
Interventions	INTERVENTION: *Type:* • Partial ban on beer advertising *Media:* • Print • Electronic *Duration of intervention:* • 1974 to 1978 CONTROL: *Type*: • No ban

Ogborne 1980 *(Continued)*

	Duration of control: • Pre-ban before 1974 COMPARISON: The beer consumption rates were compared to those in the province of Alberta where no ban had been in place during the same period
Outcomes	PRIMARY OUTCOME: • Per capita alcohol consumption: ○ Monthly beer sales were obtained from the Brewers' Association of Canada and sales data for alcohol beverages from Statistics Canada for British Columbia and Ontario. Per capita consumption was calculated for each month by dividing the monthly sales figures by the year's estimate of the size of the provincial adult population (over 15 years of age) published by Statistics Canada SECONDARY OUTCOMES: • None reported
Notes	ETHICS: Not applicable as nationally aggregated data FUNDING: Conducted by the Addiction Research Foundation, Canada, and assumed to be the funding source

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Not a RCT
Allocation concealment (selection bias)	High risk	Not a RCT
Was knowledge of the allocated interventions adequately prevented during the study	Low risk	The outcome of consumption was objectively measured by routine data collection and was thus unlikely to have been influenced by knowledge of the intervention
Were incomplete outcome data adequately addressed	Low risk	There is no report of missing data as each month is accounted for
Was the study free from selective outcome reporting bias	Low risk	There is no indication that other outcomes would be of interest
Was the intervention unlikely to affect data collection (ITS)	Low risk	The data were collected from routine source before and after the ban
Was the intervention independent of other changes (ITS)	Unclear risk	No report of historical or political reasons underpinning decision to implement ban

Ogborne 1980 *(Continued)*

Was the shape of the intervention effect pre-specified (ITS)	Low risk	It was predicted that beer sales would decrease
Was the study free from other risks of bias	High risk	Seasonality was not addressed although the analysis may have adjusted for this but no details are given. Broadcast and printed media originating outside the province were not subject to regulation or control by the Manitoba Provincial Liquor Commission

Smart 1976

Methods	STUDY TYPE: • Interrupted Time Series COUNTRY: • Canada SETTING: • Provinces of British Columbia and Ontario DURATION OF STUDY PERIOD: • 1962 to 1972 ANALYSIS: Simple mean comparisons using t test on de-trended data
Participants	Adult population purchasing alcohol
Interventions	INTERVENTION: *Type:* • Complete ban on alcohol (beer, wine and spirits) and tobacco advertising *Media:* • Newspaper • Radio • Television • Billboards • Notice-boards *Duration of intervention:* • 1 September 1971 to 31 October 1972 CONTROL: *Type*: • No ban *Duration of control:* • Pre-ban before 1 September 1971 ○ Variable depending on data type (monthly or yearly) and type of alcohol

Smart 1976 *(Continued)*

- Monthly data:
  - Beer: 1968 to 1 September 1971
  - Wine: 1968 to 1 September 1971
  - Spirits: October 1970 to 1 September 1971
- Post-ban after 31 October 1972
  - Variable depending on data type (monthly or yearly) and type of alcohol
  - Monthly data:
    - Beer: 31 October 1972 to August 1972 (note no monthly data for post-ban period)
    - Wine: 31 October 1972 to 1974
    - Spirits: 31 October 1972 to December 1973

COMPARISON:

The consumption rates were compared to those in the province of Ontario where no ban had been in place during the same period

Outcomes	PRIMARY OUTCOME: • Per capita alcohol consumption: ○ Measured by sales data for alcohol beverages from Statistics Canada for British Columbia and Ontario. Using population estimates from the dicennial censuses (1961 to 1971) per capita consumption estimates were made for beer, wine and spirits SECONDARY OUTCOMES: • None reported
Notes	ETHICS: Not applicable as nationally aggregated data. FUNDING: Addiction Research Foundation, Canada and Alcoholism Foundation of British Columbia

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Not a RCT
Allocation concealment (selection bias)	High risk	Not a RCT
Was knowledge of the allocated interventions adequately prevented during the study	Low risk	The outcome of consumption was objectively measured by routine data collection and was thus unlikely to have been influenced by knowledge of the intervention
Were incomplete outcome data adequately addressed	Unclear risk	Data were not available for all alcohol types across all the same periods. The author states that he was unable to obtain the data despite requests
Was the study free from selective outcome reporting bias	Low risk	There is no indication that other outcomes would be of interest

Smart 1976 *(Continued)*

Was the intervention unlikely to affect data collection (ITS)	Low risk	The data were collected from routine sources before and after the ban
Was the intervention independent of other changes (ITS)	High risk	The ban was initiated by a unanimous political vote, but the ban was stopped after elections when there was a change in political power. There is a likelihood that other political or social changes may have coincided with the period of the ban
Was the shape of the intervention effect pre-specified (ITS)	Low risk	An increase in consumption was predicted after the ban was removed. This was tested and the point was dated
Was the study free from other risks of bias	High risk	There is an acknowledged possibility that advertising from other states would not have been stopped by the ban, causing a dilution effect. Seasonality may have affected results and this is addressed in the analysis. Mediators of alcohol use, other than advertising, are not discussed

RCT: randomised controlled trial
SD: standard deviation

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

Study	Reason for exclusion
Ackhoff 1975	This marketing study performed several interrupted time series of restrictions of advertising on beer sales within Anheuser-Busch Inc. (the company that manufacturer BUDWEISER beer) between 1963 and 1968. No numerical data were presented in the report, only graphical representation of the stimuli-response curve and we were therefore unable to extract useful data
Calfee 1994	This econometric analysis of four European nations (France, Germany, Netherlands, UK) evaluated the effects of advertising using two different models between years spanning 1968 to 1991. Bans were not in place in these countries. The authors also consider Sweden in the years 1970 to 1989 with a ban implemented in 1979. The data are not presented but the authors report that the results did not differ between the dataset spanning the period 1970 to 1989 compared with the period after the ban (1979 to 1989). The actual data are not presented and we could not therefore extract them
Gallet 2007	This is a meta-regression of elasticities of alcohol demand in 132 studies. The specific intervention time point was not possible to identify for the individual studies from the aggregated data
Goldfarb 2011	This US-based study used data from a large database of surveys collected by a media metrics agency to measure the effectiveness of 275 different online alcohol advertising campaigns between 2001 and 2008. 61,580 consumers browsing the website on which a campaign ran were either exposed to an advertisement for alcohol or a dummy advertisement for a neutral product, based on a randomised numerical algorithm placed on the advertisement server. Both exposed and not exposed (control) respondents were then recruited using an online survey invitation typically issued by a pop-up window. Respondents were asked whether they were likely or not likely to purchase a variety of products including the alcohol product advertised. These results were then evaluated against the background advertising restrictions of the relevant state. The study reported that results show that people are 8% less likely to say that they will purchase an alcoholic beverage in states that have alcohol advertising bans compared with states that do not. For consumers exposed to online advertising, this gap narrows to 3%. We excluded this study as the outcome measured intent to purchase and not sales and consumption data
Loi Evin 1999	This French government report of 1999 details the consumption of alcohol in France before, during and after the introduction of the *Loi Evin* (ban on alcohol and smoking advertising) implemented in

Study	Reason for exclusion
	1991. The law curtails alcohol advertising on television and in cinemas, and disallows sport sponsorship. Data are not presented in a manner which allowed us to extract them and are in the form of reporting of cross-sectional surveys. Only annual percentages of consumption are presented as reported in different surveys. No methodology, variance or significance levels were provided. The report states that in France alcohol consumption was declining prior to the introduction of the banning law and that internal surveys have produced contradictory results. The report notes that the proportion of alcohol consumers aged 12 to 18 years had a tendency to decline in the 1980s, but then increased significantly between 1991 and 1995, from 47% in 1991 to 65% in 1995
Midford 2010	This pre-post controlled study was conducted in an Australian community with a recognised substantial alcohol problem. Restriction of promotion or advertising of full strength beer, spirits mixers or 2 litre casks of wine was introduced simultaneously with restrictions on hours of sales of alcohol and container types for selling alcohol. The intervention was thus complex and the effects could not be disaggregated to restrictions on advertising only
Nelson 2001	This study conducted regression analyses on cross-country panel data from seventeen OECD countries for the period 1977 to 1995. Within the aggregated data, there was no indication of a specific point in time where the restrictions were implemented within individual countries. The country-specific data were not available from the author for further analysis
Nelson 2003	This study analysed panel data from 45 US states for the period 1982 to 1997. Within the aggregated data, there was no indication of a specific point in time where the restrictions were implemented within states. The state-specific data were not available from the author for further analysis
Nelson 2010	This study conducted regression analyses of cross-country panel data from seventeen OECD countries for the period 1975 to 2000. It is an update of the Nelson 2001 study. Within the aggregated data, there was no indication of a specific point in time where the restrictions were implemented within individual countries. The country-specific data were not available from the author for further analysis
Saffer 1991	This is a pooled time series from 17 OECD countries for the period 1970 to 1983. Within the aggregated data, there was no indication of a specific point in time where the restrictions were implemented within individual countries. The country-specific data were not available from the author for further analysis
Saffer 2002	This economic analysis evaluates a pooled time series of data from 20 OECD countries for the period from 1970 to 1995. It is an update of the earlier analysis by Saffer 1991. Within the aggregated data, there was no indication of a specific point in time where the restrictions were implemented within individual countries. The country-specific data were not available from the author for further analysis
Young 1993	This analysis re-examines the same dataset from Saffer 1991 evaluating 17 OECD countries from 1970 to 1983 and employs a different analysis and set of assumptions. An additional reference, Saffer 1993, offers a response to this analysis. Within the aggregated data, there was no indication of a specific point in time where the restrictions were implemented within individual countries

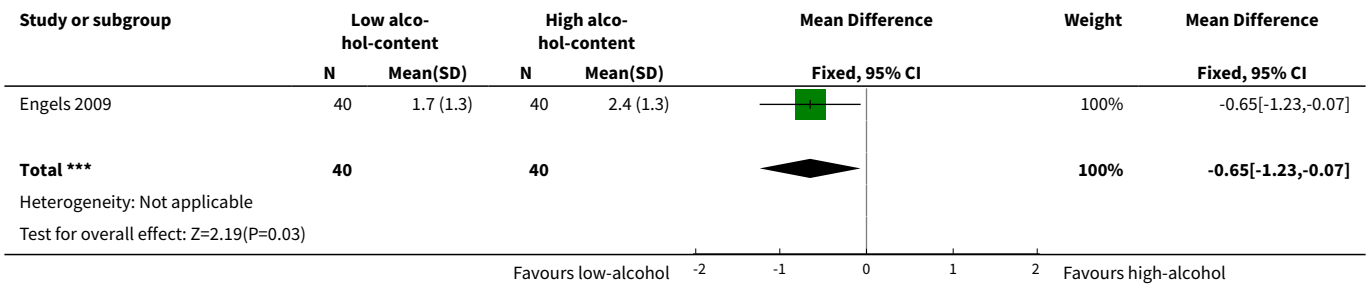
OECD: Organization for Economic and Cooperation Development

DATA AND ANALYSES

Comparison 1. Low-alcohol content movies versus high-alcohol content movies

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total alcohol consumption in number of glasses	1	80	Mean Difference (IV, Fixed, 95% CI)	-0.65 [-1.23, -0.07]

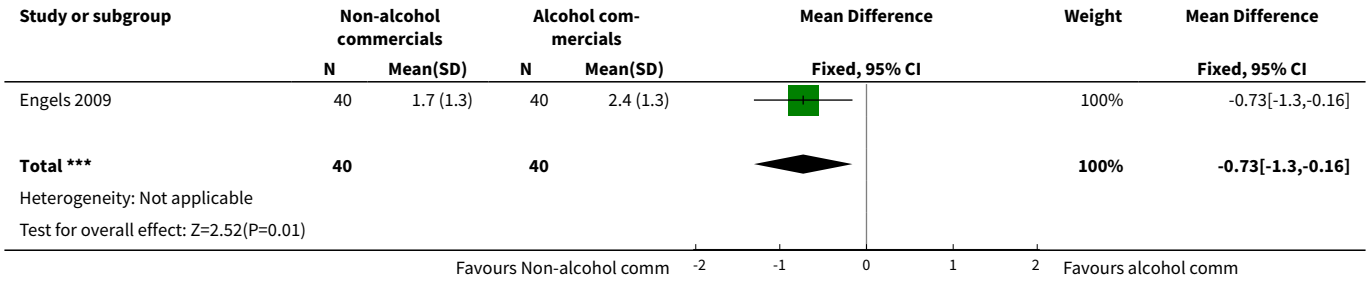
Analysis 1.1. Comparison 1 Low-alcohol content movies versus high-alcohol content movies, Outcome 1 Total alcohol consumption in number of glasses.



Comparison 2. Non-alcohol commercials versus alcohol commercials

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total alcohol consumption in number of glasses	1	80	Mean Difference (IV, Fixed, 95% CI)	-0.73 [-1.30, -0.16]

Analysis 2.1. Comparison 2 Non-alcohol commercials versus alcohol commercials, Outcome 1 Total alcohol consumption in number of glasses.

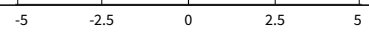


Comparison 3. High-alcohol content movies versus low-alcohol content movies adjusted for clustering effects

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total alcohol consumption	1		Coefficient (Fixed, 95% CI)	0.74 [0.05, 1.43]

Analysis 3.1. Comparison 3 High-alcohol content movies versus low-alcohol content movies adjusted for clustering effects, Outcome 1 Total alcohol consumption.

Study or subgroup	High-alcohol content N	Low-alcohol content N	Coefficient (SE)	Coefficient IV, Fixed, 95% CI	Weight	Coefficient IV, Fixed, 95% CI
Engels 2009	0	0	0.7 (0.35)		100%	0.74[0.05,1.43]
Total (95% CI)					**100%**	**0.74[0.05,1.43]**
Heterogeneity: Not applicable						
Test for overall effect: Z=2.11(P=0.03)						



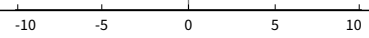
Favours high-alcohol — Favours low-alcohol

Comparison 4. Alcohol commercials versus non-alcohol commercials adjusted for clustering effects

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Total alcohol consumption	1		Coefficient (Fixed, 95% CI)	0.83 [0.14, 1.52]

Analysis 4.1. Comparison 4 Alcohol commercials versus non-alcohol commercials adjusted for clustering effects, Outcome 1 Total alcohol consumption.

Study or subgroup	Alcohol commercials N	Non-alcohol commercials N	Coefficient (SE)	Coefficient IV, Fixed, 95% CI	Weight	Coefficient IV, Fixed, 95% CI
Engels 2009	0	0	0.8 (0.35)		100%	0.83[0.14,1.52]
Total (95% CI)					**100%**	**0.83[0.14,1.52]**
Heterogeneity: Not applicable						
Test for overall effect: Z=2.37(P=0.02)						

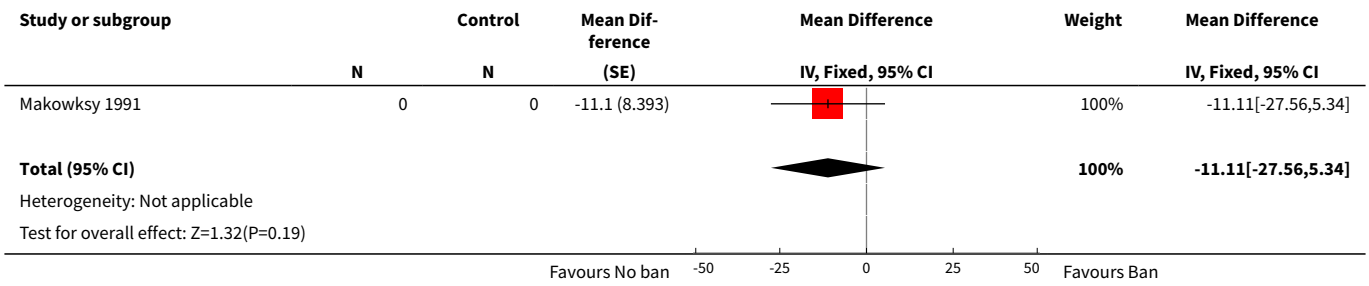


Favours Alcohol comm — Favours non-alcohol comm

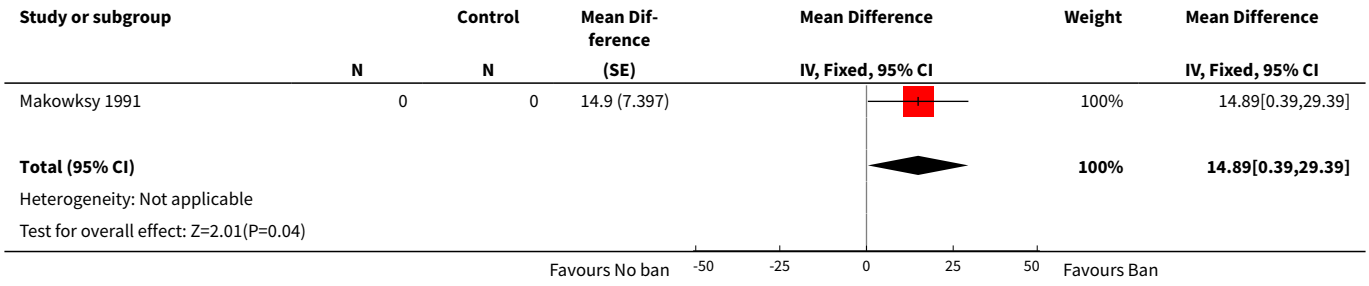
Comparison 5. Total advertising ban versus Partial advertising ban Abrupt permanent model

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Volume of alcohol (beer, wine and spirits) sales in kilolitres	1		Mean Difference (Fixed, 95% CI)	-11.11 [-27.56, 5.34]
2 Volume of beer sales in kilolitres	1		Mean Difference (Fixed, 95% CI)	14.89 [0.39, 29.39]
3 Volume of wine sales in kilolitres	1		Mean Difference (Fixed, 95% CI)	1.15 [-0.91, 3.21]
4 Volume of spirits sales in kilolitres	1		Mean Difference (Fixed, 95% CI)	-22.49 [-36.83, -8.15]

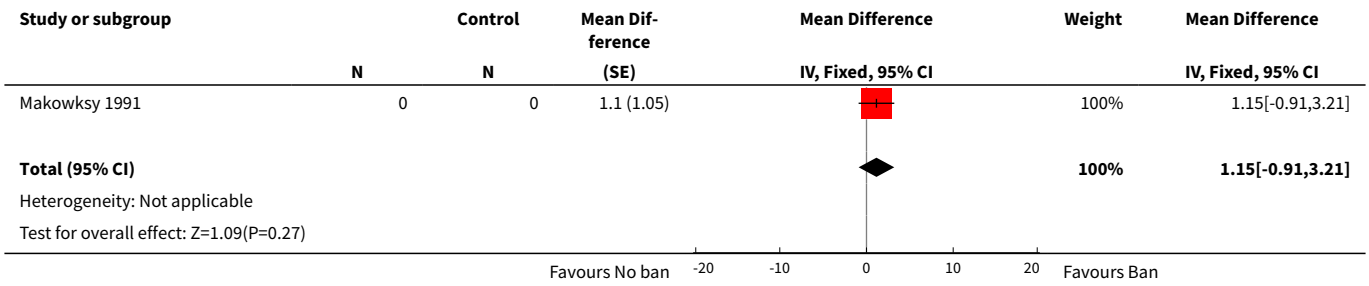
Analysis 5.1. Comparison 5 Total advertising ban versus Partial advertising ban Abrupt permanent model, Outcome 1 Volume of alcohol (beer, wine and spirits) sales in kilolitres.



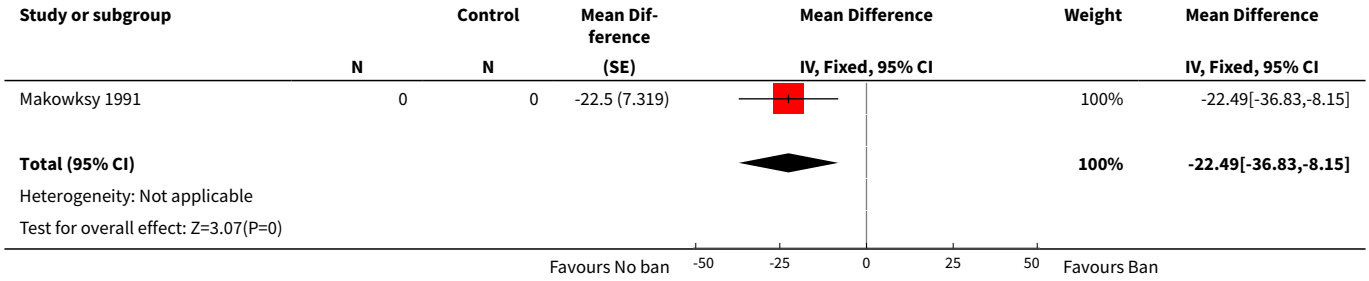
Analysis 5.2. Comparison 5 Total advertising ban versus Partial advertising ban Abrupt permanent model, Outcome 2 Volume of beer sales in kilolitres.



Analysis 5.3. Comparison 5 Total advertising ban versus Partial advertising ban Abrupt permanent model, Outcome 3 Volume of wine sales in kilolitres.



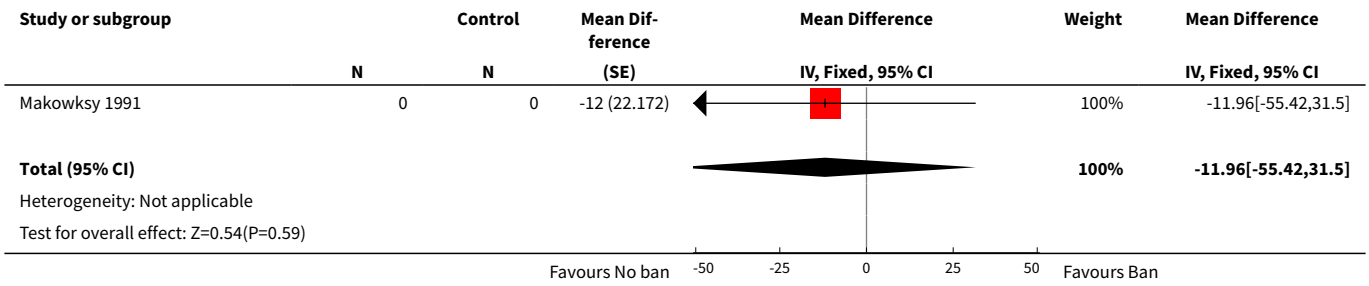
Analysis 5.4. Comparison 5 Total advertising ban versus Partial advertising ban Abrupt permanent model, Outcome 4 Volume of spirits sales in kilolitres.



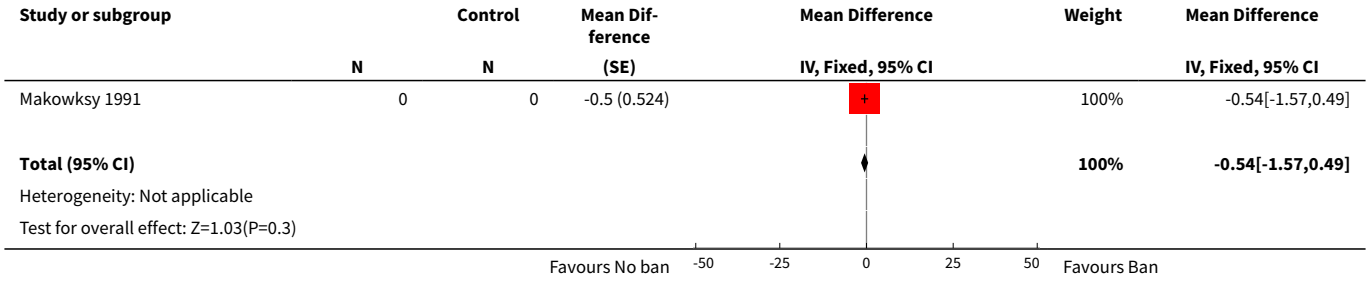
Comparison 6. Total advertising ban versus Partial advertising ban Gradual permanent model

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Volume of alcohol (beer, wine and spirits) sales in kilolitres	1		Mean Difference (Fixed, 95% CI)	-11.96 [-55.42, 31.50]
2 Volume of beer sales in kilolitres	1		Mean Difference (Fixed, 95% CI)	-0.54 [-1.57, 0.49]
3 Volume of wine sales in kilolitres	1		Mean Difference (Fixed, 95% CI)	-0.00 [-0.04, 0.04]
4 Volume of spirits sales in kilolitres	1		Mean Difference (Fixed, 95% CI)	-27.8 [-59.34, 3.74]

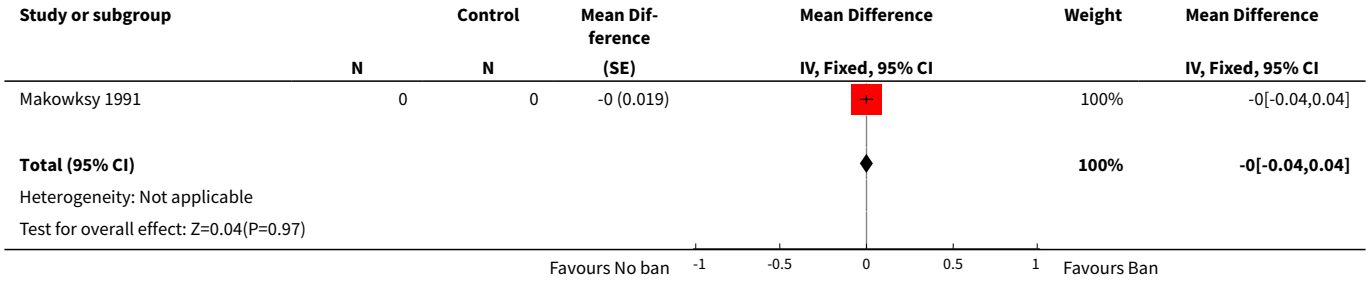
Analysis 6.1. Comparison 6 Total advertising ban versus Partial advertising ban Gradual permanent model, Outcome 1 Volume of alcohol (beer, wine and spirits) sales in kilolitres.



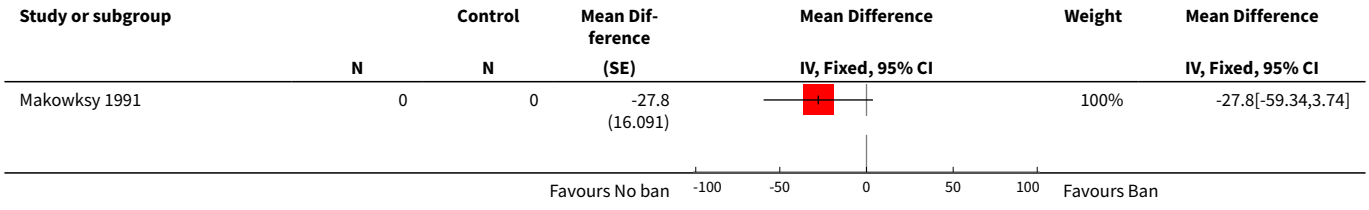
Analysis 6.2. Comparison 6 Total advertising ban versus Partial advertising ban Gradual permanent model, Outcome 2 Volume of beer sales in kilolitres.

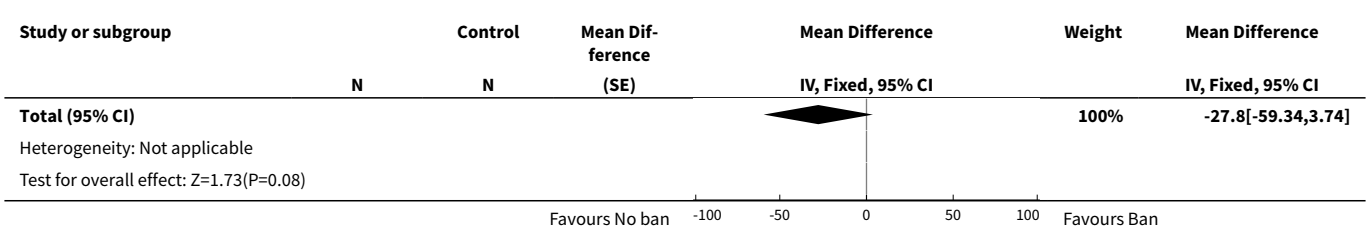


Analysis 6.3. Comparison 6 Total advertising ban versus Partial advertising ban Gradual permanent model, Outcome 3 Volume of wine sales in kilolitres.



Analysis 6.4. Comparison 6 Total advertising ban versus Partial advertising ban Gradual permanent model, Outcome 4 Volume of spirits sales in kilolitres.

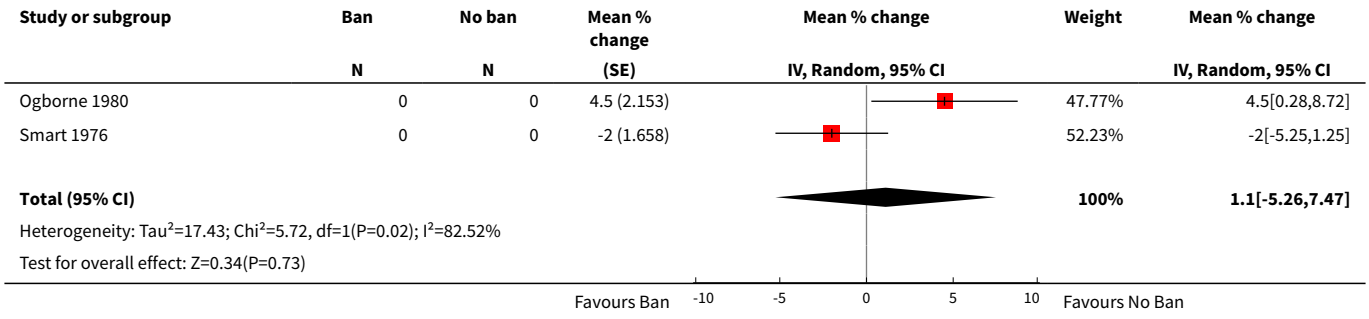




Comparison 7. Alcohol ban versus no ban

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 % Change in beer consumption	2		Mean % change (Random, 95% CI)	1.10 [-5.26, 7.47]

Analysis 7.1. Comparison 7 Alcohol ban versus no ban, Outcome 1 % Change in beer consumption.



APPENDICES

Appendix 1. PubMed search strategy

Search	Query
#22	Search **(#20) NOT #21**
#21	Search **animals [mh] NOT humans [mh]**
#20	Search **((#6) AND #10) AND #19**
#19	Search **(((((((#11) OR #12) OR #13) OR #14) OR #15) OR #16) OR #17) OR #18**
#18	Search **policy[tiab] OR policies[tiab]**

(Continued)

#17	Search **forbid*[tiab] OR prohibit*[tiab] OR interdict*[tiab] OR regulat*[tiab] OR reducing[tiab] OR reduce[tiab] OR reduced[tiab] OR reduction*[tiab] OR restrict*[tiab]**
#16	Search **ban[tiab] OR bans[tiab] OR banned[tiab] OR banning[tiab]**
#15	Search **limit*[tiab]**
#14	Search **law[tiab] OR laws[tiab]**
#13	Search **"Legislation as Topic"[MeSH]**
#12	Search **"Health Policy"[MeSH]**
#11	Search **"Policy"[MeSH]**
#10	Search **((#7) OR #8) OR #9**
#9	Search **((ad[tiab] OR ads[tiab] OR spot[tiab]) AND (Televis*[tiab] OR TV*[tiab] OR Radio[tiab] OR Radios[tiab] OR Movie*[tiab] OR Film*[tiab] OR Display*[tiab] OR media[tiab] OR Newspaper*[tiab] OR Magazine*[tiab] OR internet[tiab]))**
#8	Search **Advert*[tiab] OR Promot*[tiab] OR Sponsor*[tiab] OR Billboard*[tiab] OR Poster[tiab] OR Posters[tiab] OR branding[tiab] OR social marketing[mh] OR marketing[mh:noexp] OR marketing[tiab] OR commercial[tiab] OR commercials[tiab]**
#7	Search **"Advertising as Topic"[MeSH]**
#6	Search **((((#1) OR #2) OR #3) OR #4) OR #5**
#5	Search **Wine*[tiab] OR Liquor*[tiab] OR Spirits[tiab] OR Beer*[tiab]**
#4	Search **(alcohol*[tiab] AND (drink*[tiab] OR beverage*[tiab] OR intoxicat*[tiab] OR abus*[tiab] OR misus*[tiab] OR risk*[tiab] OR consum*[tiab] OR excess*[tiab] OR problem*[tiab]))**
#3	Search **(drink*[tiab] AND (excess*[tiab] OR heavy[tiab] OR heavily[tiab] OR hazard*[tiab] OR binge[tiab] OR harmful[tiab] OR problem*[tiab]))**
#2	Search **"Alcohol Drinking"[MeSH]**
#1	Search **"Alcohol-Related Disorders"[MeSH]**

Appendix 2. EMBASE search strategy

No.	Query	Results
#1	alcohol abuse'/exp	20.128
#2	alcohol intoxication'/exp	11.57
#3	drinking behavior'/exp	32.649

(Continued)

#4	(drink* NEAR/3 (excess* OR heavy OR heavily OR hazard* OR binge OR harmful OR problem*)):ab,ti	15.566
#5	(alcohol* NEAR/3 (drink* OR beverage* OR intoxicat* OR abus* OR misus* OR risk* OR consum* OR excess* OR problem*)):ab,ti	82.144
#6	wine*:ab,ti OR liquor*:ab,ti OR spirits:ab,ti OR beer*:ab,ti	33.266
#7	#1 OR #2 OR #3 OR #4 OR #5 OR #6	148.236
#8	advertizing'/exp	15.076
#9	advert*:ab,ti OR promot*:ab,ti OR sponsor*:ab,ti OR billboard*:ab,ti OR poster:ab,ti OR posters:ab,ti OR branding:ab,ti OR marketing:ab,ti OR commercial:ab,ti OR commercials:ab,ti	800.025
#10	((ad OR ads OR spot) NEAR/5 (televis* OR tv OR radio OR radios OR movie* OR film* OR display* OR media OR newspaper* OR magazine* OR OR film* OR display* OR media OR newspaper* OR magazine* OR internet)):ab,ti	1.556
#11	social marketing'/exp	2.211
#12	marketing'/de	14.236
#13	#8 OR #9 OR #10 OR #11 OR #12	817.051
#14	policy'/exp OR policy:ab,ti OR policies:ab,ti	192.939
#15	law'/exp	79.431
#16	law:ab,ti OR laws:ab,ti	83.918
#17	limit*:ab,ti OR forbid*:ab,ti OR prohibit*:ab,ti OR interdict*:ab,ti OR regulat*:ab,ti OR reducing:ab,ti OR reduce:ab,ti OR reduced:ab,ti OR reduction*:ab,ti OR restrict*:ab,ti OR ban:ab,ti OR bans:ab,ti OR banned:ab,ti OR banning:ab,ti	4,530,066
#18	#14 OR #15 OR #16 OR #17	4,775,966
#19	#7 AND #13 AND #18	3.424
#20	#7 AND #13 AND #18 AND [humans]/lim AND [embase]/lim	1.569

Appendix 3. Cochrane Library search strategy

No.	Query	Results
#1	MeSH descriptor: [Alcohol-Related Disorders] explode all trees	3159
#2	MeSH descriptor: [Alcohol Drinking] explode all trees	2082
#3	(drink* near (excess* or heavy or heavily or hazard* or binge or harmful or problem*)):ti,ab,kw (Word variations have been searched)	1034

(Continued)

#4	alcohol:ti,ab,kw (Word variations have been searched)	9402
#5	(Wine* or Liquor* or Spirits or Beer*):ti,ab,kw (Word variations have been searched)	867
#6	#1 or #2 or #3 or #4 or #5	10846
#7	MeSH descriptor: [Advertising as Topic] explode all trees	130
#8	MeSH descriptor: [Marketing] this term only	18
#9	(Advert* or Promot* or Sponsor* or Billboard* or Poster or Posters or branding or marketing or commercial or commercials):ti,ab,kw (Word variations have been searched)	18515
#10	((ad or ads or spot) near (Televis* or TV or Radio or Radios or Movie* or Film* or Display* or media or Newspaper* or Magazine* or internet)):ti,ab,kw (Word variations have been searched)	83
#11	#7 or #8 or #9 or #10	18572
#12	MeSH descriptor: [Policy] explode all trees	534
#13	MeSH descriptor: [Health Policy] explode all trees	417
#14	MeSH descriptor: [Legislation as Topic] explode all trees	607
#15	(law or laws):ti,ab,kw (Word variations have been searched)	576
#16	(limit* or ban or bans or banned or banning or forbid* or prohibit* or interdict* or regulat* or reducing or reduce or reduced or reduction* or restrict*):ti,ab,kw (Word variations have been searched)	176183
#17	(policy or policies):ti,ab,kw (Word variations have been searched)	3534
#18	#12 or #13 or #14 or #15 or #16 or #17	179102
#19	#6 and #11 and #18 in Trials	242
#20	#6 and #11 and #18 in Economic Evaluations	4

Appendix 4. PsycINFOsearch strategy

No.	Query
S20	S6 AND S10 AND S19
S19	S11 OR S12 OR S13 OR S14 OR S15 OR S16 OR S17 OR S18
S18	TI policy OR AB policy OR TI policies OR AB policies

(Continued)

S17	TI forbid* OR AB forbid* OR TI prohibit* OR AB prohibit* OR TI interdict* OR AB interdict* OR TI regulat* OR AB regulat* OR TI reducing OR AB reducing OR TI reduce OR AB reduce OR TI reduced OR AB reduced OR TI reduction* OR AB reduction OR TI restrict* OR AB restrict*
S16	TI ban OR AB ban OR TI bans OR AB bans OR TI banned OR AB banned OR TI banning OR AB banning
S15	TI limit* OR AB limit*
S14	TI law OR AB law OR TI laws OR AB laws
S13	SU Legislation as Topic
S12	SU Health Policy
S11	SU Policy
S10	S7 OR S8 OR S9
S9	(TI ad OR AB ad OR TI ads OR AB ads OR TI spot OR AB spot) AND (TI Televis* OR AB Televis* OR TI TV* OR AB TV OR TI Radio OR AB Radio OR TI Radios OR AB Radios OR TI Movie* OR AB Movie* OR TI Film* Or AB Film* OR TI Display* OR AB Display* OR TI media OR AB media OR TI Newspaper* OR AB Newspaper* OR TI Magazine* OR AB Magazine* OR TI internet OR AB Internet)
S8	TI Advert* OR AB Advert* OR TI Promot* OR AB Promot* OR TI Sponsor* OR AB Sponsor OR TI Billboard* OR AB Billboard OR TI Poster OR AB Poster OR TI Posters OR AB Posters OR TI branding OR AB branding OR MJ social marketing OR MJ marketing OR TI marketing OR AB marketing OR TI commercial OR AB commercial OR TI commercials OR AB commercials
S7	SU Advertising as Topic
S6	S1 OR S2 OR S3 OR S4 OR S5
S5	TI Wine* OR AB Wine* OR TI Liquor* OR AB Liquor OR TI Spirits OR AB Spirits OR TI Beer* OR AB Beer*
S4	(TI alcohol* OR AB alcohol*) AND (TI drink* OR AB drink* OR TI beverage* OR AB beverage* OR TI intoxicat* OR AB intoxicat* OR TI abus* OR AB abus OR TI misus* OR AB misus* OR TI risk* OR AB misus* OR TI consum* OR AB consum* OR TI excess* Or AB excess* OR TI problem* OR AB problem*)
S3	(TI drink* OR AB drink*) AND (TI excess* OR AB excess* OR TI heavy OR AB heavy OR TI heavily OR AB heavily OR TI hazard* OR AB hazard* OR TI binge OR AB binge OR TI harmful OR AB harmful OR TI problem* OR AB problem*)
S2	SU Alcohol Drinking
S1	SU Alcohol-Related Disorders

Appendix 5. 'Risk of bias' criteria for RCTs, CCTs and prospective observational studies

Item	Low risk	High risk	Unclear risk
Sequence generation (Selection bias)	Investigators described a random component in the sequence generation process	Investigators described a non-random component in the sequence generation process such as the use of odd or even	Insufficient information to permit judgement of the

(Continued)

	such as the use of random number table, coin tossing, cards or envelope shuffling	date of birth, algorithm based on the day/ date of birth, hospital or clinic record number	sequence generation process
Allocation concealment (Selection bias)	Participants and the investigators enrolling participants cannot foresee assignment, e.g. central allocation; or sequentially numbered, opaque, sealed envelopes	Participants and investigators enrolling participants can foresee upcoming assignment, e.g. an open random allocation schedule (e.g. a list of random numbers); or envelopes were unsealed or non-opaque or not sequentially numbered	Insufficient information to permit judgement of the allocation concealment or the method not described
Blinding *of participants and providers (Performance bias)* *Objective outcomes*	No blinding or incomplete blinding, but the review authors judge that the outcome is not likely to be influenced by lack of blinding Blinding of participants and key study personnel ensured, and unlikely that the blinding could have been broken	No blinding or incomplete blinding, and the outcome is likely to be influenced by lack of blinding Blinding of key study participants and personnel attempted, but likely that the blinding could have been broken, and the outcome is likely to be influenced by lack of blinding	Insufficient information to permit judgement of low or high risk
Blinding *of participants and providers (Performance bias)* *Subjective outcomes*	Blinding of participants and providers and unlikely that the blinding could have been broken	No blinding or incomplete blinding, and the outcome is likely to be influenced by lack of blinding Blinding of key study participants and personnel attempted, but likely that the blinding could have been broken, and the outcome is likely to be influenced by lack of blinding	Insufficient information to permit judgement of low or high risk
Blinding *of outcome assessor (Detection bias)* *Objective outcomes*	No blinding of outcome assessment, but the review authors judge that the outcome measurement is not likely to be influenced by lack of blinding Blinding of outcome assessment ensured, and unlikely that the blinding could have been broken	No blinding of outcome assessment, and the outcome measurement is likely to be influenced by lack of blinding Blinding of outcome assessment, but likely that the blinding could have been broken, and the outcome measurement is likely to be influenced by lack of blinding	Insufficient information to permit judgement of low or high risk
Blinding *of outcome assessor (Detection bias)* *Subjective outcomes*	No blinding of outcome assessment, but the review authors judge that the outcome measurement is not likely to be influenced by lack of blinding Blinding of outcome assessment ensured, and unlikely that the blinding could have been broken	No blinding of outcome assessment, and the outcome measurement is likely to be influenced by lack of blinding Blinding of outcome assessment, but likely that the blinding could have been broken, and the outcome measurement is likely to be influenced by lack of blinding	Insufficient information to permit judgement of low or high risk
Incomplete outcome data	No missing outcome data, reasons for missing outcome data unlikely to be related to true outcome, or missing outcome data balanced in number across groups For dichotomous outcome data, the proportion of missing outcomes compared with observed event risk not enough to have a clinically relevant impact on the intervention effect estimate	Reason for missing outcome data likely to be related to true outcome, with either imbalance in number across groups or reasons for missing data For dichotomous outcome data, the proportion of missing outcomes compared with observed event risk enough to induce clinically relevant bias in intervention effect estimate	Insufficient reporting of attrition or exclusions (e.g. number randomised not stated, no reasons for missing data provided; number of drop out not reported for each group)

(Continued)

	For continuous outcome data, plausible effect size (difference in means or standardised difference in means) among missing outcomes not enough to have a clinically relevant impact on observed effect size Missing data have been imputed using appropriate methods All randomised patients are reported/analysed in the group they were allocated to by randomisation irrespective of non-compliance and co-interventions (intention to treat)	For continuous outcome data, plausible effect size (difference in means or standardised difference in means) among missing outcomes enough to induce clinically relevant bias in observed effect size 'As-treated' analysis done with substantial departure of the intervention received from that assigned at randomisation	
Selective reporting	A protocol is available which clearly states the primary outcome as the same as in the final trial report The study protocol is not available but it is clear that the published reports include all expected outcomes, including those that were pre-specified (convincing text of this nature may be uncommon)	The primary outcome differs between the protocol and final trial report One or more reported primary outcomes were not pre-specified (unless clear justification for their reporting is provided, such as an unexpected adverse effect) One or more outcomes of interest in the review are reported incompletely so that they cannot be entered in a meta-analysis The study report fails to include results for a key outcome that would be expected to have been reported for such a study	No trial protocol is available or there is insufficient reporting to determine if selective reporting is present
Free of other bias: *Comparability of cohorts for baseline characteristics and outcome measures on the basis of the design or analysis*	Exposed and non exposed individuals are matched in the design for most important confounding factors Authors demonstrated balance between group for the confounders Analysis are adjusted for most important confounding factors and imbalance Randomised controlled trial	No matching or no adjustment for most important confounding factor	No information about comparability of cohort
Free of other bias: selection of the non-exposed cohort	The sample has been drawn from the same community as the exposed cohort Randomised controlled trial	The sample has been drawn from a different source	No description of the derivation of the non-exposed cohort
Free of other bias: protection against contamination	Allocation was by community, institution or practice and it is unlikely that the control group received the intervention Randomised controlled trial	It is likely that the control group received the intervention	It is possible that communication between intervention and control groups could have occurred
Ascertainment of exposure	Information in the study was obtained from a secure record (e.g. clinical records or structured interview) Randomised controlled trial	Self report	No description

Appendix 6. 'Risk of bias' criteria for ITS studies

Item	Low risk	High risk	Unclear risk
Was the intervention independent of other changes?	Compelling arguments that the intervention occurred independently of other changes over time and the outcome was not influenced by other confounding variables/historic events during study period. If events/variables identified, note what they are	The intervention was not independent of other changes in time	Insufficient information to permit judgement of low or high risk
Was the shape of the intervention effect prespecified?	Point of analysis is the point of intervention OR a rational explanation for the shape of intervention effect was given by the author(s). Where appropriate, this should include an explanation if the point of analysis is NOT the point of intervention	It is clear that the shape of the intervention was not prespecified	Insufficient information to permit judgement of low or high risk
Was the intervention unlikely to affect data collection?	The intervention itself was unlikely to affect data collection (e.g. sources and methods of data collection were the same before and after the intervention)	The intervention itself was likely to affect data collection (e.g. any change in source or method of data collection reported)	Insufficient information to permit judgement of low or high risk
Was knowledge of the allocated interventions adequately prevented during the study?	The authors state explicitly that the primary outcome variables were assessed blindly, or the outcomes are objective, e.g. length of hospital stay. Primary outcomes are those variables that correspond to the primary hypothesis or question as defined by the authors	If the outcomes were not assessed blindly	Insufficient information to permit judgement of low or high risk
Were incomplete outcome data adequately addressed? *(If some primary outcomes were assessed blindly or affected by missing data and others were not, each primary outcome can be scored separately)*	Missing outcome measures were unlikely to bias the results (e.g. the proportion of missing data was similar in the pre- and post-intervention periods or the proportion of missing data was less than the effect size, i.e. unlikely to overturn the study result)	Missing outcome data were likely to bias the results. Do not assume 100% follow up unless stated explicitly)	Insufficient information to permit judgement of low or high risk
Was the study free from selective outcome reporting?	There is no evidence that outcomes were selectively reported (e.g. all relevant outcomes in the methods section are reported in the results section)	If some important outcomes are subsequently omitted from the results	Insufficient information to permit judgement of low or high risk
Was the study free from other risks of bias?	There is no evidence of other risks of bias, e.g. should consider if seasonality is an issue (i.e. if January to June comprises the pre-intervention period and July to December the post, could the 'seasons' have caused a spurious effect)	There is evidence that other risks of bias exist, such as seasonality	Insufficient information to permit judgement of low or high risk

Appendix 7. May 2014 search: records retrieved per database

Database	No. of records retrieved	Potentially eligible	Included	Date searched
PubMed	432	0	0	2014-05-28
EMBASE	319	0	0	2014-05-27
CENTRAL	55	0	0	2014-05-28
NHS Economic Evaluations Database	0	0	0	2014-05-28
Cochrane Drug and Alcohol Specialised Register	0	0	0	2014-05-28

Appendix 8. Results of searches of economic and marketing databases

Database	URL	Search term	No. of records retrieved	Potentially eligible	Included	Date searched
AgEcon	ageconsearch.umn.edu/	alcohol	154	1	0	2013-10-16
Business Source Premier	EBSCOHost	alcohol advertising	654	16	0	2013-10-18
ETOH databases on the National Institute of Health Alcohol and Alcohol Problems database (1972 to 2003)	http://etoh.niaaa.nih.gov/	ban; restriction	29; 134	0;1	0;0	2013-10-22
The Chartered Institute of Marketing (UK-based)	http://library.cim.co.uk/icswpd/exec/icswppro.dll	alcohol	237	3	0 (2 reviews)	2013-10-22
Association for Consumer Research	http://www.acrwebsite.org/search/search-conference-proceedings.aspx	alcohol	560	3	0	2013-10-22

Appendix 9. Results of conferences and manual report archives searched

Conference	URL	Search term	No. of records retrieved	Potentially eligible	Included	Date searched
International Health Economics Association	www.ssrn.com	alcohol	699	20	0	2013-06-03

(Continued)

Research Society on Alcoholism	www.rsoa.org	No proceedings published. Oral and poster presentations are published in the journal, *Alcoholism: Clinical and Experimental Research* and should therefore have been identified in the journal database searches				
International Society for Biomedical Research in Alcoholism	www.isbra.com/	We were not able to obtain access to this and no response to email requests was received				
Kettil Bruun Society	http://www.kettilbruun.org	The contact person responded that the Society is in the process of archiving conference papers and there is no means to search electronically at the current time. The 2013 symposium was manually searched				2013-10-21
39th Annual Alcohol Epidemiology Symposium of the Kettil Bruun Society, Kampala, Uganda, 3 - 7 June, 2013	Manual search of conference abstract book	-	143	2	0	2013-10-24
International Network on Brief Interventions for Alcohol Problems (INEBRIA)	http://www.inebria.net/Du14/html/en/dir1338/index.html	INEBRIA contact person responded that INEBRIA conferences do not cover alcohol advertising				2013-10-22
Vietnam Alcohol Policy Workshop 2009	http://www.icap.org/	Manual handsearch	0	0	0	2013-10-18
ICAP Africa Region Conference 2008	http://www.icap.org/	Manual handsearch	0	0	0	2013-10-18
ICAP Asia-Pacific Alcohol Forum 2008	http://www.icap.org/	Manual handsearch	0	0	0	2013-10-18
The Foundation for Alcohol research	http://www.abmrf.org/meetings_conferences.asp	The Foundation supports the Research Society on Alcoholism. No proceedings published. Oral and poster presentations are published in the journal, *Alcoholism: Clinical and Experimental Research* and should therefore have been identified in the journal database searches. See above under Research Society on Alcoholism				
European Advertising Standards Alliance	http://www.easa-alliance.org/	EASA does not have a database of meeting abstracts but provided relevant articles and papers for consideration				

WHAT'S NEW

Date	Event	Description
10 November 2014	Amended	Correction of an error in abstract and PLS

HISTORY

Protocol first published: Issue 9, 2013
Review first published: Issue 11, 2014

Date	Event	Description
31 July 2014	Feedback has been incorporated	After referee and an update of the search we have incorporated all comments and findings from the updated search.

CONTRIBUTIONS OF AUTHORS

The study was commissioned by the Alcohol and Drug Abuse Research Unit and the South African Cochrane Centre of the South Medical Research Council. NS co-ordinated the author team. NS and DCP extracted data and JEA served as the arbiter. NS inputted data and conducted analysis, and DCP and JEA checked them. CDHP and JV regularly reviewed results and provided guidance in the interpretation of results and recommendations for ensuring the comprehensiveness of the review. MJ provided expertise in marketing. All authors contributed to the interpretation of the results. NS wrote the initial draft of the review and all authors contributed to writing the final draft.

DECLARATIONS OF INTEREST

NS, DCP, JV, TK and MJ declare no conflicts of interest.

JEA is a member of the WHO Working Group on Alcohol Taxation and Pricing. This working group is involved in drafting a technical resource on alcohol pricing and taxation policies and guidelines on how best to implement such policies.

CDHP is a member of the WHO Expert Panel on Drug Dependence and Alcohol Problems and a board member of the Global Alcohol Policy Alliance, a network whose mission is to reduce alcohol-related harm worldwide by promoting science-based policies independent of commercial interests.

SOURCES OF SUPPORT

Internal sources

- Alcohol and other Drug Research Unit, Medical Research Council, South Africa.

  The Unit commissioned the study in partnership with the South African Cochrane Centre and co-funded the lead author of the review.
- South African Cochrane Centre, Medical Research Council, South Africa.

  The Unit commissioned the study in partnership with the Alcohol and other Drug Research Unit and co-funded the lead author of the review.

External sources

- No sources of support supplied

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

The review followed the protocol as it was published, with no differences in methodology.

INDEX TERMS

Medical Subject Headings (MeSH)

Advertising [*methods] [statistics & numerical data]; Alcohol Drinking [epidemiology] [*prevention & control]; Interrupted Time Series Analysis; Motion Pictures [*statistics & numerical data]; Randomized Controlled Trials as Topic

MeSH check words

Adolescent; Adult; Humans; Male; Young Adult