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REVIEW

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Restricting alcohol marketing to reduce alcohol consumption: A systematic review of the empirical evidence for one of the 'best buys'

Jakob Manthey[1,2] | Britta Jacobsen[1] | Sinja Klinger[1] | Bernd Schulte[1] | Jürgen Rehm[1,3,4,5,6,7]

[1]Center for Interdisciplinary Addiction Research (ZIS), Department of Psychiatry and Psychotherapy, University Medical Center Hamburg-Eppendorf (UKE), Hamburg, Germany

[2]Department of Psychiatry, Medical Faculty, University of Leipzig, Leipzig, Germany

[3]Institute for Mental Health Policy Research and Campbell Family Mental Health Research Institute, Centre for Addiction and Mental Health, Toronto, ON, Canada

[4]Department of Psychiatry, University of Toronto, Toronto, ON, Canada

[5]Dalla Lana School of Public Health, University of Toronto, Toronto, ON, Canada

[6]Faculty of Medicine, Institute of Medical Science, University of Toronto, Toronto, ON, Canada

[7]Program on Substance Abuse, Public Health Agency of Catalonia, Program on Substance Abuse and WHO CC, Public Health Agency of Catalonia, Barcelona, Spain

Correspondence
Jakob Manthey, Center for Interdisciplinary Addiction Research (ZIS), Department of Psychiatry and Psychotherapy, University Medical Center Hamburg-Eppendorf (UKE), Martinistraße 52, 20246 Hamburg, Germany.
Email: j.manthey@uke.de

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Abstract

Background and Aims: Even though a ban of alcohol marketing has been declared a 'best buy' of alcohol control policy, comprehensive systematic reviews on its effectiveness to reduce consumption are lacking. The aim of this paper was to systematically review the evidence for effects of total and partial bans of alcohol marketing on alcohol consumption.

Methods: This descriptive systematic review sought to include all empirical studies that explored how changes in the regulation of alcohol marketing impact on alcohol consumption. The search was conducted between October and December 2022 considering various scientific databases (Web of Science, PsycINFO, MEDLINE, Embase) as well as Google and Google Scholar. The titles and abstracts of a total of 2572 records were screened. Of the 26 studies included in the full text screening, 11 studies were finally included in this review. Changes in consumption in relation to marketing bans were determined based on significance testing in primary studies. Four risk of bias domains (confounding, selection bias, information bias and reporting bias) were assessed.

Results: Seven studies examined changes in marketing restrictions in one location (New Zealand, Thailand, Canadian provinces, Spain, Norway). In the remaining studies, between 17 and 45 locations were studied (mostly high-income countries from Europe and North America). Of the 11 studies identified, six studies reported null findings. Studies reporting lower alcohol consumption following marketing restrictions were of moderate, serious and critical risk of bias. Two studies with low and moderate risk of bias found increasing alcohol consumption post marketing bans. Overall, there was insufficient evidence to conclude that alcohol marketing bans reduce alcohol consumption.

Conclusions: The available empirical evidence does not support the claim of alcohol marketing bans constituting a best buy for reducing alcohol consumption.

KEYWORDS
advertising, alcohol marketing, best buy, control policy, intervention, sponsoring

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INTRODUCTION

The World Health Organization has identified a ban or a comprehensive restriction of alcohol marketing as one of the 'best buys' (i.e. as one intervention that is not only cost-effective, but also feasible for implementation in most countries) for reducing consumption and alcohol-attributable non-communicable disease harm [1]. Marketing is defined here as all elements of the effort to sell commercially produced alcohol. It prominently includes, but is not limited to advertising; marketing, therefore, encompasses the design of the product, the price charged and the place and ease of access. Likewise recognized as part of the modern marketing mix are the people who are in contact with consumers, the processes of delivery (such as online access) and elements of the physical environment the customer experiences [2].

Underlying evidence included an economic modelling study comparing the cost-effectiveness of five interventions (random breath testing, taxation increase, marketing restrictions, sales restrictions and increasing the coverage of brief interventions), which identified marketing restrictions as one of the most cost-effective strategies to reduce alcohol-attributable burden of disease [3]. After the global action plan, various modelling studies corroborated the classification of a ban of marketing as one of the 'best buys' (e.g. Chisholm *et al.*) [4]. Modelling studies often rely on the effect size of a cross-sectional survey in low- and middle-income countries [5] or a panel study of aggregate data from the Organization for Economic Co-operation and Development (OECD) countries [6].

However, any modelling study is only as good as the underlying assumptions, and there seems to be a gap in the evidence on the real-world effectiveness of marketing restrictions. A 2014 Cochrane review found that bans or restrictions of alcohol marketing was not significantly related to reduced consumption, based on a meta-analysis of two Canadian studies conducted in the 1970s [7]. The most recent narrative review of the literature as part of the revision of the book '*Alcohol, No Ordinary Commodity*' [2], classified the evidence for a complete ban of marketing as an overall moderate effect on consumption and attributable problems with some underlying studies, but 'no integrative reviews available or none that include low and middle-income countries'. Moreover, the authors detailed, that the moderate effect was based on indirect evidence from the effects of alcohol advertising on consumption and the evidence of effects of bans on tobacco marketing and advertising on consumption and harms. In fact, several reviews converge on the observation that marketing exposure is linked to the initiation of alcohol use via normalization of this behaviour as part of everyday modern life, in particular among adolescents [8, 9]. Summarizing the evidence from 11 literature reviews and considering the Bradford Hill criteria for causality (among others: strength of association, consistency, temporality and experimental evidence), it has been suggested that the link between marketing exposure and alcohol use is causal [10]. Therefore, the designation of the 'best buy' seems to be based on sufficient evidence that marketing in its various forms is effective in increasing initiation and level of alcohol use, especially among adolescents, plus the assumption that a ban of marketing would remove marketing and, therefore, take away these increases of consumption.

Given this summary, we conducted a systematic review of studies evaluating the potential effects of complete or partial bans on marketing on alcohol consumption. Such a review is timely, as there has been accumulating literature on these topics, including literature on effects of digital marketing [11]. Digital marketing has been growing constantly in relative importance as a marketing tool and the industry expects this trend to continue [12]. This form of marketing allows much more personalized and emotional marketing techniques, which have been classified as more effective [11].

Given the importance of the topic and the lack of a recent comprehensive review, we aimed to identify and synthesize the existing evidence on the effects of alcohol marketing and—sponsoring restrictions on consumption.

METHODS

Eligibility criteria

This review was conducted in accordance with Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) guidelines. A literature search was conducted to find all empirical studies with control groups that explored the effects of alcohol marketing regulation changes on alcohol consumptions. The inclusion and exclusion criteria are detailed in Table 1.

Information sources and search strategy

The search was conducted in Web of Science, OVID SP (PsycINFO, MEDLINE, and Embase) in October 2022 and covered all publications since database inception. Individuals, organizations or manufacturers were not contacted as sources. The individual search strings, the date of the search, the number of results and the datasets can be found in Table S1 -. As different databases handle search terms differently, the search strings were adjusted accordingly. An additional search for grey literature from European countries was performed by using Google and Google Scholar in December 2022. Last, we ensured that all studies from a 2014 Cochrane literature review on this matter [7] were considered for inclusion. The search strategy and study protocol were registered in advance on PROSPERO (registration number: CRD42022365297).

Selection process

All search results were uploaded to Covidence (www.covidence.org), which was used for the abstract/title and full text screening process. The titles and abstracts were screened by two independent researchers (B.J. and S.K.). Decisions to include or exclude the screened publications were bound to appropriate criteria (see Table 1). Conflicting decisions were discussed with the project lead (J.M.) and jointly resolved. Overall agreement between the two

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TABLE 1 Inclusion and exclusion criteria of the systematic literature search.

Criteria	Inclusion	Exclusion
Substance	Alcohol consumption	Consequences of alcohol consumption; consumption of other substances
Intervention	Tightening or loosening of regulation regarding alcohol marketing or sponsorship	No change in the regulation of alcohol marketing or sponsorship
Study design	Empirical studies with control group (comparison with a place without (change in) regulation and/or before (change in) regulation)	No control group or empirical data
Sample	No restriction: all ages, genders, etc.	–
Results	Changes in alcohol consumption (per questionnaire or sales data), determined using statistical analyses	–
Publication type	–	Case reports, editorials, other publications (e.g. posters, summaries of conference presentations, commentaries, etc.)
Language	English, German	–
Time period	No restriction	–

reviewers was high (98.4% concordant ratings), but interrater reliability was low (Cohen's κ: 32%). This can be explained by high agreement on the exclusion of publications (in 98.4% of publications that at least one reviewer voted out—both agreed) but several disagreements on the inclusion of publications (in 19.6% of publication that at least one reviewer voted in—both agreed), which could be resolved in subsequent discussions. Full text screening was also performed by two project collaborators (J.M. and S.K.), with high agreement (87.5%), as well as high interrater reliability (Cohen's κ: 73%).

Outcomes

The outcome of interest was alcohol consumption. Specifically, we wanted to know whether changes in marketing or sponsorship regulations were linked to a change in alcohol consumption. We included any measure of alcohol consumption, including self-reported data from surveys as well as sales data reflecting per capita consumption. As both measures have their advantages and disadvantages, they were both included. Therefore, both individual-level and population-based studies were considered.

Other variables

From each study, we obtained information on participant/population, intervention characteristics, the used study design and quantitative findings, all of which are reported in Table 2. Some key information (e.g. countries studied or indicators of uncertainty) were missing or inconsistent in some studies. Missingness or inconsistency of key study-level information was indicated using square brackets in Table 2.

Risk of bias assessment

To assess the risk of bias in (non-randomized) observational studies, the ROBINS-I [23] or the Newcastle-Ottawa-Scale [24] are adequate tools, but they generally do not work well with studies on aggregate data, such as per capita consumption based on sales statistics because the risk of bias related to individual-level data (e.g. loss to follow-up, recall bias, blinding of intervention) do not apply. To overcome the limitations inherent to existing tools, we developed a new approach to evaluate and differentiate the risk of bias of the included observational studies. Our approach covered the four risk of bias domains outlined in the Cochrane handbook, which also structure the ROBINS-I tool: confounding, selection bias, information bias and reporting bias. Using seven items for individual-level (with humans as unit of observation) and five items for aggregate-level studies (with countries or other jurisdictions as unit of observation), the risk of bias was performed by two reviewers (J.M. and J.R.). Inconsistent ratings were resolved in peer-to-peer discussions. The instructions and the ratings are included in Supplementary Material.

Effect measures

The studies used different analytic approaches and effect measures (e.g. prevalence ratio, coefficients from regressions or time series analyses) for the analysis of the effects of marketing restrictions. To facilitate interpretation of the findings across studies, we extracted the percentage change in the outcome variable (e.g. per capita consumption) associated with the regulatory changes in alcohol marketing. Statistically significant changes are reported in Table 2, whereas the text describes in detail the analyses and the quantitative findings (e.g. CIs or SEs).

Synthesis

The studies and study findings were too heterogeneous to meta-analytically summarize them. The heterogeneity was driven by

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TABLE 2 Summary of studies.

Design/analytic approach	Author(s)	Region/country	Population	Risk of bias	Dependent variable(s)
Repeated cross-sectional survey	Assanangkornchai *et al.* [13]	Thailand	*n* = 88 568 students in the age of 13–17 years	Critical	Prevalence ratio (after/before) for self-reported alcohol use: (a) lifetime, (b) 12-month, (c) 30-day, (d) 30-day binge drinking and (e) 30-day intoxication
Repeated cross-sectional survey with external control	Kypri *et al.* [14]	New Zealand Intervention: Otago Control: Lincoln, Victoria, Waikato	*n* = 4137 university students surveyed in 2005 and 2013	Serious	Changes in self-reported alcohol use in percent: (a) past year, (b) 4-week, (c) 7-day, and (d) recent intoxication
Repeated cross-sectional survey	Leal-López *et al.* [15]	33 countries/regions in Europe and North America [exact list of countries not reported]	*n* = 671 084 adolescents ages 11, 13, 15 surveyed in 2001/02, 2005/06, 2009/10, 2013/14	Serious	Self-reported alcohol use: (a) lifetime, (b) weekly and (c) lifetime drunkenness
Time series analysis	Smart and Cutler [16]	Canada Intervention: British Columbia Control: Ontario	Total population	Serious	Recorded per capita consumption: - Beer - Wine
Time series analysis	Ogborne and Smart [17]	Canada Intervention: Manitoba Control: Alberta	Total population	Serious	Recorded per capita consumption: -Beer
Time series analysis (ARIMA)	Makowsky and Whitehead [18]	Canada Intervention: Saskatchewan Control: New Brunswick	Total population	Serious	Recorded per capita consumption: - Beer, - Wine, - Spirits, - Total
Time series analysis (ARIMA)	Rossow [19]	Norway	Total population	Moderate	Recorded per capita consumption (total)
Time series analysis (regression with linear time trend)	Matrai *et al.* [20]	Spain	Total population	Serious	Recorded per capita consumption (total)
Panel regression	Nelson [21]	45 US states	Total population	Moderate	Recorded per capita consumption: - Beer - Wine - Spirits - Total
Panel regression accounting for endogeneity	Saffer and Dave [6]	20 OECD countries with annual data between 1970 and 1995	Total population	Moderate	Recorded per capita consumption (total)
Panel regression	Nelson [22]	17 OECD countries with annual data between 1975 and 2000	Total population	Low	Recorded per capita consumption (total)

Note: Square brackets indicate missing or inconsistent information.

Abbreviations: ARIMA, autoregressive integrated moving average; BAC, blood alcohol concentration; CI, confidence interval; GLS, generalized least squares; MLDA, minimum legal drinking age; OECD, Organization for Economic Co-operation and Development; TV, television; US, United States of America.

[a]No measure of uncertainty reported.

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TABLE 2 Summary of studies.

Design/analytic approach	Author(s)	Dependent variable(s)	Regulation in alcohol marketing	Further alcohol regulations	Quantitative findings	Summary interpretation
Repeated cross-sectional survey	Assanangkornchai *et al*. [13]	Prevalence ratio (after/before) for self-reported alcohol use: (a) lifetime, (b) 12-month, (c) 30-day, (d) 30-day binge drinking and (e) 30-day intoxication	Ban of alcohol advertisements in 2008	• Increased MLDA • Bans of local sale and consumption in places frequently used by youth	a. Lifetime alcohol use Female: 2.1 (95% CI = 1.8–2.5) Male: 1.4 (95% CI = 1.2–1.5) b. 12-month alcohol use Female: 1.9 (95% CI = 1.6–2.2) Male: 1.2 (95% CI = 1.1–1.3) c. 30-day alcohol use female: 1.7 (95% CI = 1.4–2.1) male: 1.1 (95% CI = 0.9–1.2) d. 30-day binge drinking female: 2.1 (95% CI = 1.6–2.7) male: 1.1 (95% CI = 0.9–1.3) e. 30-day intoxication female: 1.2 (95% CI = 1.0–1.5) male: 0.6 (95% CI = 0.5–0.7)	a. Statistically significant increase in alcohol use (+70%–110%) and binge drinking (+110%) prevalence among female, but not male students b. Statistically significant decrease in intoxication prevalence among male, but not female students
Repeated cross-sectional survey with external control	Kypri *et al*. [14]	Changes in self-reported alcohol use in percent: (a) past year, (b) 4-week, (c) 7-day, and (d) recent intoxication	Ban of alcohol advertising and sponsorship on the university campus in 2009	• Closure of pubs • Sanctioning 'disorderly behaviour' on campus	a. Past-year alcohol use: intervention students: −0.7% (95% CI = −4.4% to 3.0%) Control students: −3.4% (95% CI = −6.5% to −0.2%) b. 4-week alcohol use: intervention students: −3.0% (95% CI = −6.2 to 0.2%) control students: −4.2% (95% CI = −7.3% to −1.0%) c. 7-day alcohol use: intervention students: −4.1% (95% CI = −10.3% to 2.2%) Control students: +1.6% (95% CI = −3.0% to 6.3%) d. Recent intoxication: intervention students: −14.2% (95% CI = −20.3% to −8.1%) Control students: −6.4% (95% CI = −9.9 to −2.8)	a. Statistically significant decrease of past-year and 4-week alcohol use prevalence in control group, but not in intervention group b. No difference in 7-day alcohol use c. Prevalence of intoxication in last 7 days decreased in both control and intervention group with no significant difference after adjustment

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TABLE 2 (Continued)

Design/analytic approach	Author(s)	Dependent variable(s)	Regulation in alcohol marketing	Further alcohol regulations	Quantitative findings	Summary interpretation
Repeated cross-sectional survey	Leal-López *et al*. [15]	Self-reported alcohol use: (a) lifetime, (b) weekly and (c) lifetime drunkenness	Variations of restrictions in advertising index: a. No restrictions, b. Voluntary self-regulations, c. Partial statutory restrictions d. Complete ban	• MLDA, • Physical availability, • Affordability	a. Lifetime alcohol use: β = −0.11; SE = 0.096 b. Weekly alcohol use: β = −0.17; SE = 0.096 c. Lifetime drunkenness: β = −0.04; SE = 0.087	No statistically significant association of self-reported alcohol consumption with advertising index
Time series analysis	Smart and Cutler [16]	Recorded per capita consumption: - Beer - Wine	Temporary complete marketing ban for alcohol during 1971–1972	None	Quantitative finding reported for beer only: t = 1.206 [a]	No statistically significant change during ban years
Time series analysis	Ogborne and Smart [17]	Recorded per capita consumption: -Beer	Ban of beer advertising (electronic/print) in 1974	None	Changes in beer consumption: • Intervention: t = 2.09 [a] • Control: t = 3.55 [a]	Statistically significant increase of consumption in both intervention (+4.5%) and control province (+5.0%)
Time series analysis (ARIMA)	Makowsky and Whitehead [18]	Recorded per capita consumption: - Beer, - Wine, - Spirits, - Total	Lifting marketing ban in 1983, allowing a. broadcast marketing for beer and wine b. print marketing for beer, wine and spirits	None	Beer consumption change (abrupt) • Intervention: β = 14 890; t = 2.01 • Control: β = −17 880; t = 1.49 Spirits consumption change (abrupt) • Intervention: β = −22 490; t = −3.07 • Control: β = −2465; t = 0.22 Wine consumption change (abrupt) • Intervention: β = 1149; t = 1.09 • Control: β = 1879; t = 2.35 Total consumption change (abrupt) • Intervention: β = −11 110; t = −1.32 • Control: β = 2.26; t = 0.82	• Statistically significant increase of beer (+5%) in intervention • Decrease of spirits (−10%) • Increase of wine in control, but not in intervention • No statistically significant net change in total consumption • No change of total consumption in control province—suggestive of substitution effect caused by partial ban

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TABLE 2 (Continued)

Design/analytic approach	Author(s)	Dependent variable(s)	Regulation in alcohol marketing	Further alcohol regulations	Quantitative findings	Summary interpretation
Time series analysis (ARIMA)	Rossow [19]	Recorded per capita consumption (total)	Complete ban for all media types and beverages in 1975	None	β from ARIMA model = −0.074; SE = 0.023	Statistically significant decrease of consumption (−7.4%)
Time series analysis (regression with linear time trend)	Matrai *et al.* [20]	Recorded per capita consumption (total)	Ban on advertising for alcoholic beverages >20% volume in 1988	Local sales ban in 1990	Correlation coefficient of alcohol per capita consumption and marketing ban: −0.12 (90% CI = −0.17 to −0.07)	Decrease of total recorded per capita consumption (−12%)
Panel regression	Nelson [21]	Recorded per capita consumption: - Beer - Wine - Spirits - Total	a. Billboard ban for spirits b. Price advertisement ban for spirits in 1996	MLDA, state retail monopoly	a. Effect of bans of billboard marketing on total alcohol sales: β = 0.054; SE = 5.90 b. Effect of price advertisement ban on total alcohol sales: β = −0.009; SE = 2.38	a. Billboard ban: statistically significant increase of total consumption (+5%) b. Price advertisement ban: statistically significant decrease of total consumption (−1%)
Panel regression accounting for endogeneity	Saffer and Dave [6]	Recorded per capita consumption (total)	a. Partial ban (e.g. only concerning spirits in TV) b. Complete ban	None	a. Effect of partial bans: β = 0.0367; t = 1.30 b. Effect of complete bans: β = 0.0367; *t* = 0.61	No statistically significant effect of partial or complete bans
Panel regression	Nelson [22]	Recorded per capita consumption (total)	a. Partial ban (concerning spirits in broadcasting media) b. Complete ban	Alcohol policy control index (control of production and wholesale; control of distribution; personal controls; control of marketing; social and environmental controls; national education and prevention programs)	a. Effect of partial bans (GLS model): β = 0.060; t = 5.94 b. Effect of complete bans (GLS model): β = 0.107; t = 14.0	a. Partial ban: increase of consumption (+6%) b. Complete ban: increase of consumption (+11%)

Note: Square brackets indicate missing or inconsistent information.

Abbreviations: ARIMA, autoregressive integrated moving average; BAC, blood alcohol concentration; CI, confidence interval; GLS, generalized least squares; MLDA, minimum legal drinking age; OECD, Organization for Economic Co-operation and Development; TV, television; US, United States of America.

[a]No measure of uncertainty reported.

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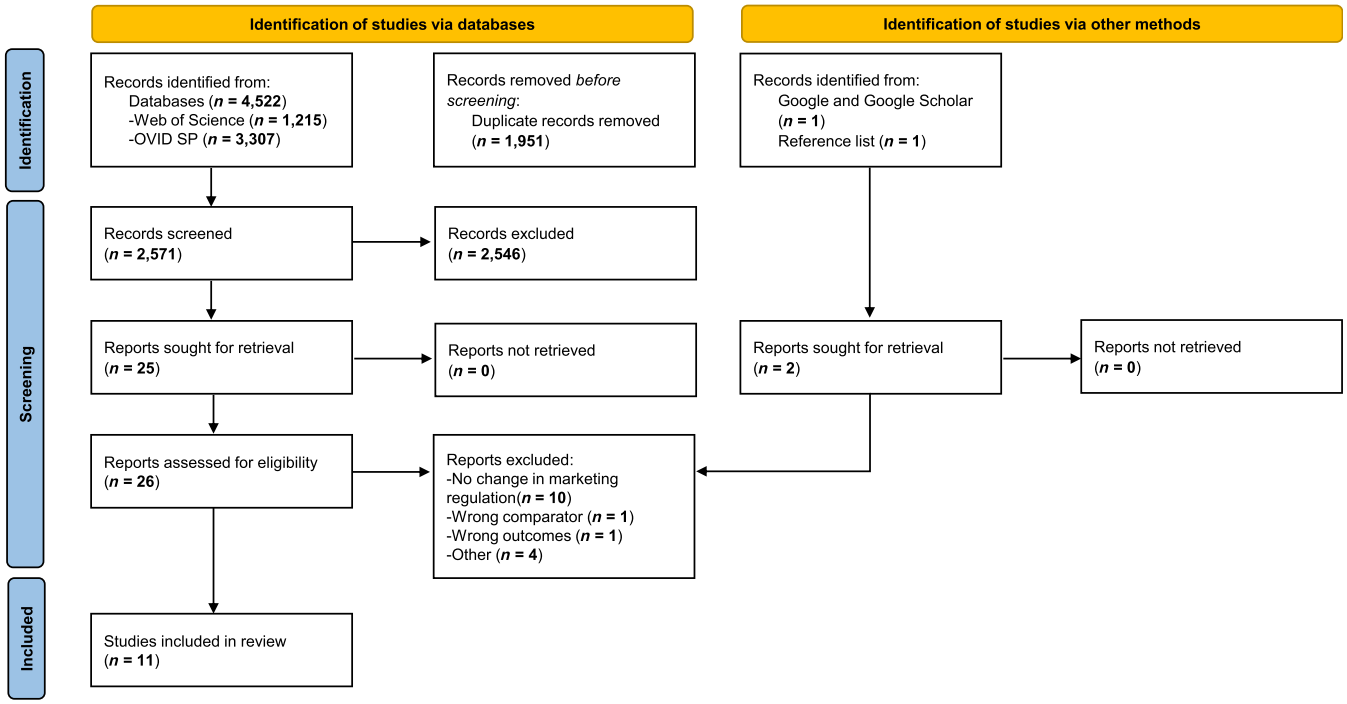

FIGURE 1 Flow chart of literature search.

variations in interventions and the regional coverage (e.g. bans of alcohol advertising and sponsorship on a university campus vs complete nationwide ban), the time, and therefore, the role of digital media (bans were enacted between 1971 and 2009), the direction of the intervention (mostly bans but also ban lifts were studied), study designs (e.g. before–after comparisons vs time series vs panel regression) and outcomes (self-reported prevalence of use vs sales based per capita consumption). Instead, we give a narrative summary of the key findings stratified for the used approaches.

RESULTS

Study selection

After screening *n* = 2571 records, we identified *n* = 26 studies that were considered for inclusion (for the PRISMA flow chart, see Figure 1). One additional study was identified through the additional Google Scholar search [25]. As this industry-related report examined the effects of various marketing restrictions in six countries on per capita consumption without statistical analyses, this report was not included in the final analysis. One other study was identified in the reference list of another study and included [6]. All publications that were excluded during full-text screening are summarized in Table S2 (Data S1).

Finally, 11 studies were included that evaluated changes in alcohol consumption relative to changes in alcohol marketing restrictions (for a summary, see Table 2). In all but one study, the restrictions were applied or lifted across an entire country, state or province. One study examined marketing restrictions on a university campus [14].

Study characteristics

The studies identified in our search were largely based on alcohol restrictions implemented before 2000 and mainly in high-income countries in Europe or North America, but single studies were also conducted in Thailand [13] and New Zealand [14]. Eight of 11 studies evaluated marketing restrictions implemented before the year 2000, whereas the most recent marketing restriction was implemented in 2009 [14].

Study designs varied between repeated cross-sectional surveys, time series analyses and panel regressions, mainly with sales-based per capita consumption as outcome variable (see Table 2). The first set of studies investigated marketing restrictions in single locations [13, 14, 16–20], whereas some also considered external control conditions, either statistically [14] or descriptively [16–18]. The second set of studies investigated data from between 17 and 45 locations while using self-reported [15] or per capita consumption [6, 21, 22] as outcome.

Studies examining marketing restrictions in one location

Of all seven studies that evaluated marketing restrictions in one location, only two studies reported decreased alcohol consumption following the enactment of a complete [19] or partial marketing ban [20]. Using time series analyses, the complete ban enacted in Norway in 1975 was linked to an immediate 7% reduction of per capita consumption [19], whereas the partial ban (for higher strength

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products only) enacted in Spain in 1988 was correlated with a 12% decrease in per capita consumption [20]. Although the Norwegian study accounted for time variations in real alcohol prices, the Spanish study considered changes in socio-demographic composition. However, an insufficient description of the statistical approach and the unjustified use of 90% CIs in the Spanish study attenuate confidence in these estimates.

Of the remaining five natural experiments, three were conducted in Canada. The first studied a temporary complete marketing ban during 1971 and 1972 in British Colombia [16]; the second a lift of an advertising ban in Saskatchewan [18]; and the third a partial ban in Manitoba enacted in 1974 [17]. Using time series analyses, two studies found no statistically significant change in total consumption [18] as well as beer and wine consumption [16]. In the third study from Manitoba, small statistically significant increases in beer sales of similar magnitude (~5%) were observed both in the intervention and the control province (Alberta) [17]. Of note, evidence of a possible substitution effect could be observed in the study conducted in Saskatchewan [18], where advertising bans were lifted. Here, broadcast marketing for beer, but not for spirits was allowed, which was followed by statistically significant increased beer sales, but decreased spirit sales. This pattern was not observed in a control province (New Brunswick).

In the two remaining natural experiments, the marketing restrictions were analysed in combination together with other restrictive measures (e.g. raising minimum legal drinking age), which were implemented at the same time or close to the marketing restriction [13, 14]. In the first study that evaluated a ban of marketing on a college campus in New Zealand in 2009, survey data was collected 4 years prior and 4 years post the restrictions [14]. The findings suggest a statistically significant decline in the prevalence of alcohol consumption only among college students at colleges where marketing regulations remained unchanged. This was true for past-year and 4-week prevalence, whereas 7-day difference did not change in either group. Moreover, prevalence of recent intoxication declined with statistical significance among students from all colleges, with greater declines on intervention compared to control colleges. However, the difference in change between sites did not remain statistically significant when accounting for differences in respondent characteristics. Therefore, the marketing restriction, as well as the other measures, was not found to have an impact on students' drinking behaviour in this study. In the last study of natural experiments, the ban of alcohol advertisement enacted in Thailand in 2008 was evaluated using survey data from 2007 and 2016 [13]. In addition to the marketing restriction, the minimum legal drinking age was also raised from 18 to 20 years during this period, in addition to the enactment of sale bans for areas frequented by youth. The analyses suggest that alcohol use has generally increased with statistical significance between 2007 and 2016 among female, but not among male high school students (mean age: 15 years). For example, the prevalence ratios, calculated as the prevalence in 2016 divided by the prevalence in 2007, was 1.9 (95% CI = 1.6–2.2) for 12-month alcohol use prevalence and 2.1 (95% CI = 1.6–2.7) for 30-day binge drinking prevalence among women; for men, the ratios were 1.2 (95% CI = 1.1–1.3) and 1.1 (95% CI = 0.9–1.3), respectively [13]. As female high school students started from a lower level of alcohol use, the gender gap almost closed during this period. The aim of reducing alcohol use among adolescents could not be achieved according to the study authors.

Studies examining marketing restrictions in more than one location

Four studies used similar study designs, analysing either individual-level survey data [15] or sales-derived per capita consumption data [6, 21, 22] from multiple jurisdictions (mostly high-income countries) over a period of 13 to 26 years.

In a repeated cross-sectional study using data from 33 countries in North America and Europe, the correlation between alcohol advertising restrictions and self-reported alcohol consumption among adolescents was examined, adjusting for the impact of other alcohol control policies [15]. Using four waves of data between 2001 and 2014, no statistically significant association between advertising restrictions and lifetime and weekly alcohol consumption, as well as lifetime drunkenness were found when adjusting for socio-demographics and other alcohol control policy measures, such as alcohol availability.

Based on data from 45 United States states between 1982 and 1997, Nelson estimated the impact of two different marketing restrictions on beverage-specific sales: (a) billboard marketing bans for spirits; and (b) bans of price advertising for spirits [21]. This study compared states with and without billboard marketing bans (no temporal change) and evaluated the removal of a price advertising ban following a court ruling in 1996. Adjusting for state differences in socio-demographic variables, alcohol prices, alcohol retail monopolies and minimum legal drinking age, the findings suggest that the link between total alcohol sales and bans of billboard marketing or price advertising was not statistically significant.

The last two studies analysed partially overlapping data. In the paper by Saffer and Dave [6], data from 20 OECD countries between 1970 and 1995 was analysed, whereas Nelson included data from 17 OECD countries between 1975 and 2000 [22]. In both papers, it was differentiated between partial and complete marketing bans. Partial marketing bans included restrictions that only concerned one type of media (e.g. only television [TV]) or only one beverage type (e.g. only beer). In the first analysis, Saffer and Dave [6] used structural equations that allowed modelling the endogeneity of marketing bans. Specifically, the models did assume that bans were to some degree driven by declining public support for restrictions, reflected in declining per capita consumption (e.g. bans are no longer necessary and, therefore, lifted). Accounting for endogeneity, in addition to alcohol prices and an alcohol culture variable, the findings suggest that partial and complete alcohol bans were related to reduced total alcohol sales, using a level of statistical significance of =10%. However, if systematic country differences were accounted for by inclusion of country dummies, the coefficients turned positive and insignificant. In the second

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analysis, Nelson criticized the approach by Saffer on numerous grounds [22]. First, marketing bans are not the result of changes in public attitudes, but according to Nelson, should be treated as exogenous variables. Second, evaluations of the impact of advertising bans need to consider the influence of other control policies, which he specifies with an alcohol control index that covers control of production and retail, as well as prevention programs. The findings suggest that both partial and complete alcohol marketing bans are positively associated with total alcohol sales (both results statistically significant).

Risk of bias in studies

The risk of bias of each study is listed in Table 2 and further detailed in Table S2. According to our rating system, one and three studies were of low and moderate risk of bias, respectively. Six studies had a serious risk of bias and a single study had a critical risk of bias.

In the single study with critical risk of bias, a simple pre-post comparison without any control for confounding was conducted [13]. The serious risk of bias was mainly because of lacking external controls [16–18, 20] and lack of control for time-varying confounders [14, 16–18], for example, wage trends, affordability of alcoholic beverages or implementation of other alcohol control policies. One study with serious risk of bias lacked information on the marketing restrictions evaluated and the countries included for analysis [15]. Moderate risk of bias was assigned one study because of lack of external control [19] and two studies because interventions were insufficiently described [6, 21]. Only one study failed in none of the four risk of bias dimensions [22].

Considering only the four studies with low or moderate risk of bias would not change the overall picture: one study found a reduction [19], one study reported null findings [5] and two studies indicate that marketing bans are linked to increased consumption [21, 22]. Importantly, none of those four studies evaluated marketing restrictions implemented after the year 2000.

DISCUSSION

Abstract

We identified 11 studies that have investigated how marketing restrictions are linked to alcohol consumption. The high heterogeneity between the included studies in terms of time, intervention, study design, population, regional coverage and analytic approach inhibited a meta-analytic summary. Most studies report null findings, suggestive of no impact [6, 14–18]. Reduced consumption following the implementation of marketing restrictions was reported in three studies with moderate [19], serious [20] and critical risk of bias [13]. In two studies with moderate [21] and low [22] risk of bias, marketing restrictions were linked to increased consumption. Overall, there is insufficient evidence to conclude that alcohol marketing restrictions constitute an effective tool to reduce alcohol consumption. Accordingly, marketing bans may not be considered a best buy.

Limitations

First, we did not contact industry representatives to include the evidence they may have; therefore, we acknowledge the residual risk of not having included every relevant study. However, our search was based on major scientific databases and also included a grey literature search in European countries, which resulted in identifying all studies included in a 2014 Cochrane review [7] and several other works, including the study cited by many modelling studies [6]. Accordingly, we believe to have captured the very essence of the literature. Second, the approaches and analyses reported in the included studies varied greatly, partially reflecting the scientific advances in the field since 1976—the year in which the first study was published. Third, some studies have not provided enough information on the implemented marketing restrictions. For example, in the analyses of Leal-López *et al.* [15] there is no information on the names of countries included nor on the specific marketing restrictions in each country. Therefore, it is possible that this study examined the relationship between various marketing restrictions and alcohol use, rather than evaluating changes in alcohol marketing restrictions within one or more countries. Fourth, it needs to be emphasized that the interventions studied were mostly enacted before the year 2000, many even before the year 1990—including two of three studies suggestive of desired effects [19, 20]. This is a key limitation because marketing bans enacted more than 30 years ago may have different effects today—in a market environment that is increasingly digital rather than physical. Fifth, we did not find any study that evaluated a ban on alcohol sponsoring. Sixth, the literature identified mostly describes marketing restrictions in few high-income countries and an additional grey literature search among European countries yielded no additional results. Accordingly, the limited understanding of the effects of marketing bans is even more restricted for low- and middle-income countries.

Implications

The findings of this review may be considered to contrast the claim that bans on alcohol marketing constitute an effective means to reduce alcohol consumption. This review did not find sufficient evidence to support this claim, but it also did not find sufficient evidence to reject it either. The essence of this work is that this claim is not based on sufficient empirical direct evidence. Clearly, there is a large body of indirect evidence, that is, exposure to marketing bans is linked to increased likelihood of drinking, especially among youth [8, 9]. Likewise, there is some experimental evidence available linking marketing exposure to increased alcohol use among young adults [7]. Accordingly, we would agree with the notation that the association between exposure to alcohol marketing and underage drinking is causal [10].

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However, this does not necessarily imply that a marketing ban results in an immediate reduction of alcohol use—as investigated in the studies included in this review. There are several barriers for an immediate effect to occur and to be detected, which we outline in the following paragraphs.

Most importantly, a ban of alcohol marketing needs to be comprehensive and cover all beverage types and all forms of media. Partial bans that affect only certain beverages or only some types of media are expected to be ineffective. One of the included studies suggested that lifting a marketing ban for beer and wine resulted in increased beer and decreased spirit sales, therefore, a substitution with zero net effects [18]. Another study showed that a ban on TV marketing was followed by a surge in marketing expenses for sponsoring [26]. Accordingly, a ban needs to be comprehensive and cover all types of beverages and media to avoid substitution effects and be effective.

Another barrier for the effectiveness of a marketing ban is the implementation and enforcement. This becomes increasingly important in a digital environment, in which a great deal of industry spending for alcohol marketing occurs (e.g. ~40% in Germany in 2021) [27]. The growing importance of digital marketing has been acknowledged by researchers [11], but real-world lessons are scarce. In Norway, the complete marketing ban is circumvented by producers using editorials of news magazine for disseminating information on their products to the public [28]. In Finland, digital alcohol marketing was banned in 2015, followed by a reduced number of social media activities by alcohol brands, but not in their interaction with customers [29]. In Lithuania, a comprehensive marketing ban including social media came into effect in 2018. Using data from Facebook and Instagram collected in 2021, infringements of the law can be observed on social media accounts of alcohol producers, while influencers act in grey zones (e.g. product placement) [30]. These examples highlight not only the complexity of marketing bans, but also the need to monitor the effects and boundaries of such bans.

In addition to the challenges associated with drafting and enforcing a marketing ban bill, there are also barriers to evaluating such an intervention. Major determinants for robustness of the empirical findings are (a) the reliability of the outcome measured; and (b) the methods used. For (a), the use of self-reported alcohol use allows the observation of possible effects in different population groupings, for example, among adolescents. However, self-reported alcohol use in surveys is subject to measurement errors [31] and sampling biases [32]. Given these limitations, per capita alcohol consumption is considered the gold standard for monitoring purposes [33]. However, per capita consumption data is insensitive if changes occur in subgroups only. Assuming that alcohol marketing would have an impact on youth drinking behaviour only, analyses using per capita consumption data would have little chances to detect even large effects as youth make up only a fraction of total consumption in most high-income countries. For (b), the study design should control for alternative explanations, including secular trends and time varying confounders, such as changing affordability. Importantly, only few studies have considered these factors. Another important aspect concerns endogeneity, which has been addressed differently in two of the included studies [6, 22] and was ignored in the remaining studies. It has been argued that marketing bans may be the result of changing attitudes in the population, which are driven by trends in alcohol consumption. If a marketing ban is not an exogenous variable, the analyses clearly need to account for this possible bias. Possibly, marketing bans may constitute exogenous variables in some, but not all countries where they have been enacted and further research should be conducted to shed more light on this aspect.

Although the available empirical evidence currently does not support the claim that alcohol marketing bans constitute an effective intervention for curbing alcohol use, this does not mean that respective restrictions are irrelevant for strengthening public health. In fact, marketing bans are simple measures with low implementation costs (not accounting for possible losses for the marketing industry) and based on the indirect evidence, it can be assumed that marketing restrictions are beneficial for the health of youth in the long run. Further, in absence of alcohol marketing, public health messages may have a larger reach and beneficial impact on alcohol health literacy.

Finally, for alcohol marketing bans to be termed a 'best buy', we would expect more direct real-world evidence that unequivocally demonstrates reduced consumption and/or improvements of health outcomes, as done for pricing policies and availability restrictions (e.g. Kilian *et al.*) [34]. Until evidence is available that can prove so, we recommend (1) to not claim that marketing bans constitute a 'best buy'; and (2) to avoid estimating the impact of alcohol marketing on consumption and health outcomes using modelling studies (e.g. Chisholm *et al.*) [4].

Conclusions

Based on our assessment of the literature, we conclude that there is insufficient evidence to claim that alcohol marketing bans are a best buy to reduce alcohol consumption.

AUTHOR CONTRIBUTIONS

Conceptualization: Jakob Manthey and Jürgen Rehm. *Methodology*: Jakob Manthey. *Software*: N/A. *Validation*: Jakob Manthey. *Formal analysis*: Jakob Manthey. *Investigation*: All authors. *Resources*: N/A. *Data curation*: N/A. *Writing—original draft*: Jakob Manthey. *Writing—review and editing*: All authors. *Visualization*: Britta Jacobsen. *Supervision*: Jürgen Rehm. *Project administration*: Jakob Manthey. *Funding acquisition*: Jakob Manthey and Jürgen Rehm.

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DECLARATION OF INTEREST

Unrelated to the present work, J.M. has worked as consultant for public health agencies and has received honoraria for presentations/workshops/manuscripts funded by various public health agencies.

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DATA AVAILABILITY STATEMENT

All data generated or analysed during this literature review are included in this published article.

REGISTRATION AND PROTOCOL

This review was registered in advance on PROSPERO (registration number: CRD42022365297).

ORCID

Jakob Manthey https://orcid.org/0000-0003-1231-3760
Jürgen Rehm https://orcid.org/0000-0001-5665-0385

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