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Bendtsen Marcus (Orcid ID: 0000-0002-8678-1164)

McCambridge Jim (Orcid ID: 0000-0002-5461-7001)

Text Messaging Interventions for Reducing Alcohol Consumption Among risky drinkers: Systematic Review and Meta-Analysis

Marcus Bendtsen[*,1], Jim McCambridge[2], Katarina Åsberg[1], Preben Bendtsen[1,3]

[1] Department of Health, Medicine and Caring Sciences, Linköping University, Sweden.
[2] Department of Health Sciences, University of York, England.
[3] Department of Medical Specialist, Motala, Sweden.

[*] Corresponding author:
Marcus Bendtsen (marcus.bendtsen@liu.se)
Department of Health, Medicine and Caring Sciences, Division of Society and Health.
Linköping University, 581 83 Linköping, registrator@liu.se, +46 13 28 10 00

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Abstract

Background and Aims: The global growth of mobile phone use has led to new opportunities for health interventions, including through text messaging. We aimed to estimate the effects of text messaging interventions on alcohol consumption among risky drinkers.

Methods: Systematic review and meta-analysis of reports on randomized controlled trials (RCTs) published in English. Searches were done on May 23 2019 in PubMed; PubMed Central; CENTRAL; CDSR; DARE; NHS-EED; Scopus; PsycINFO; PsycARTICLES; CINAHL; and Web of Science. Measurements included number of episodes of heavy drinking per month (HED) and weekly alcohol consumption in grams (WAC). Trials among risky drinkers who were not receiving co-interventions were included in the review (n = 3481, mean 29 years of age, 41% female). Data were extracted from reports and authors were contacted for additional data.

Results: Ten trials were included and all analyses based on random effects models. Primary analyses, including seven trials (n=2528) for HED and five trials (n=2236) for WAC, found that the interventions may reduce self-reported HED (-0.33 episodes per month; 95% CI = -0.79; 0.12) and WAC (-18.62 grams per week; 95% CI = -39.61; 2.38), though both estimates included the null. The GRADE quality of evidence was judged to be low for both HED and WAC, primarily due to risk of attrition and performance bias, heterogeneity, and influence of pilot trials on estimates.

Conclusions: Text messaging alcohol interventions may reduce alcohol consumption compared with no or basic health information; however, there are doubts about the overall quality of the evidence.

Keywords: meta-analysis; telemedicine; text messaging; risky drinking; alcohol consumption; brief interventions

Introduction

Rationale

Alcohol consumption is a leading risk factor for noncommunicable diseases, which are responsible for 70% of deaths globally each year, of which cardiovascular diseases, cancer, respiratory diseases, and diabetes account for over 80% [1]. Alcohol also causes injuries, road traffic accidents, and violence [2]. This means that alcohol consumption continues to be a leading cause of death, with approximately 4.5% of deaths globally attributable to alcohol, and 25% of all deaths in the age group 20-49 years [3]. While there is evidence that suggests that a small amount of alcohol may have a protective effect on myocardial infarction, the overall risk of alcohol consumption outweighs any potential benefit, hence the conclusion that there is no safe dose [4].

mHealth and text messaging

In 2019, it was estimated that 97% of the global population resided in an area with a mobile cellular signal [5]. This global growth of mobile phone subscriptions has led to new opportunities for health promotion, and the field of mobile health (mHealth) has grown substantially over the past decade [6]. Continuous contact with individuals, interactivity, and cost reductions are some of the potential benefits associated with mHealth interventions.

One way that mHealth interventions can be deployed is through text messaging, which is a technology ubiquitous in mobile phones. The technology runs on networks utilizing earlier standards, such as the Global System for Mobile communications (GSM), which are generally more available and cheaper than later standards (3G and 4G). Thus, interventions utilizing text messaging potentially have great reach globally among those who could benefit from health behaviour change.

In alcohol research, text messaging has been used both as a stand-alone intervention and in combination with other digital media such as websites [7–9]. Reviews of mHealth interventions for alcohol, which have included text messaging, have indicated positive yet mixed findings of their efficacy [10,11], however these reviews have had a wide scope and have not included meta-analyses. Thus, direct guidance is limited with respect to the effectiveness of text messaging as a stand-alone alcohol intervention. Therefore, this systematic review and meta-analysis aimed to estimate the effects of text messaging interventions on alcohol consumption among risky drinkers.

Methods

This systematic review and meta-analysis included reports of randomized trials estimating the effects of stand-alone text messaging interventions on alcohol consumption among risky drinkers in comparison to no or basic health information. A review protocol, developed according to PRISMA-P [12], was published in advance of this systematic review [13] (PROSPERO: CRD42019117431, IRRID: PRR1-10.2196/12898), and this report includes the items recommended by the PRISMA statement [14].

Information Sources and search

On May 23 of 2019 we searched PubMed (1982-present), PubMed Central (1989-present), Cochrane Central Register of Controlled Trials (CENTRAL, 1994-present); Cochrane Database of Systematic Reviews (CDSR, 2012-present); Database of Abstracts of Reviews of Effects (DARE, 1997-present); National Health Service Economic Evaluation Database (NHS-EED, 1997-present); Scopus (1969-present); PsycINFO (1983-present); PsycARTICLES (1985-present); Cumulative Index to Nursing and Allied Health Literature (CINAHL, 2000-present); Web of Science (1991-present); International Standard Randomised Controlled Trial Number (ISRCTN, 2006-present) registry; ClinicalTrials.gov (2005-present); and the WHO International Clinical Trials Registry Platform (ICTRP, 2006-present).

Grey literature was sourced from the OpenGrey database (1999-present), and PROSPERO (2012-present) was searched to identify systematic reviews of relevance.

Search strategies can be found in Appendix A.

Eligibility criteria

Only randomized controlled trials (RCTs), including cluster RCTs, evaluating text messaging without co-interventions were eligible for inclusion. We included reports in English and put no restriction on publication date.

Participants

Trials including risky drinkers (including both harmful and hazardous [15]), identified by a screening tool in any population (eg. students, general population, and primary care patients) were included. No restriction on age was made. Trials which included participants who were obviously receiving care for their alcohol problems, eg. patients in a treatment program, were not included. Trials were excluded if participants were mandated to take part.

Interventions

Interventions consisted of a series of text messages sent to participants' mobile phones over a number of weeks. For an intervention to be included, at least two messages should have been sent per week (on average). The content of the messages should be focused on behaviour change, thus excluding studies where text messages were used only to schedule or remind participants of other activities. Only trials where a text message intervention was the sole intervention were considered; therefore, trials of interventions where text messages were combined with other interventions (eg. therapy or pharmaceutical treatment) were excluded.

Comparators

There were four types of control conditions permitted for inclusion:

Minimal or no contact, including waiting list.

Basic health information on alcohol provided no more than once a week.

Referral to information sources such as websites, or recommended to contact primary healthcare services, with reminders no more than once a week.

Intervention focusing on something other than alcohol consumption, for instance physical activity or smoking.

Outcomes

Trials were included if they planned to report one of two common alcohol consumption outcomes:

Number of episodes of heavy drinking during the past month (heavy episodic drinking, HED).

Weekly alcohol consumption measured in grams or standard units of alcohol (WAC).

Report selection, data collection and risk of bias

MB initially screened the titles and abstracts for duplicates and removed reports that were clearly deemed irrelevant for the objective. Each member of the data collection team (MB, KÅ, PB) independently analysed the full text of the remaining reports and assessed eligibility. The final decision on which reports to include was made through discussion among team members.

A standardized data collection form could not be used due to outcomes being reported with great variety. Instead, MB extracted data from reports and KÅ and PB reviewed and checked the extraction in independent sessions. Authors were contacted for additional data as needed.

The Cochrane Collaboration's tool was used to assess risk of bias in individual trials [16,17]. MB judged each potential source of bias for each report, and then KÅ and PB reviewed these judgments independently. No trials were excluded based on risk of bias, but sensitivity analyses were conducted without those judged to be at high risk of attrition bias. The GRADE framework [18] was used by MB and JM to judge risk of bias across studies and to assess the quality of the body of evidence for each outcome. Trial registration databases and protocols were searched to ensure that trials, and trial outcomes, were reported as planned, supported by Funnel plots and Egger's tests.

Data Items

The following items were extracted from the reports:

Mean and dispersion for HED and WAC.

Number randomized, group sizes, number of follow-up responses, and trial design.

Age, gender, baseline consumption of trial participants.

Number of weeks the intervention lasted, average weekly frequency of text messages, rationale behind content of messages.

The type of control condition used.

The type and source of financial support.

All six clusters were used for the narrative description of the trials (Table 1 and Appendix B), with 1 and 2 used for meta-analyses.

Summary measures

For HED, individuals are typically asked to report the number of times they drank more than a certain number of units of alcohol on the same occasion (country dependent) over the past month, or it may be inferred from timeline follow-back approaches. We converted all data to monthly assessments, converting fixed-response options to numerical measures (eg. Once or twice a week: $(1 + 2)/2 \times 4 = 6$).

For WAC, both a timeline follow-back period approach and a frequency-intensity approach were used, and for both we converted standard drinks data to grams per week for each trial.

Synthesis of results

In trials where outcomes were assessed more than once, we used data from the first post-intervention analysis in the primary meta-analysis. Subgroup analyses were conducted for different time frames: 1-3 months, 4-6 months, and 7+ months also using subsequent follow-up data. Length of follow-up was defined based on time elapsed since randomization.

After designing the protocol, it was found that several included trials were identified as feasibility or pilot trials by their authors. As it became evident that these strongly shaped the synthesized outcomes, we added a stratified analysis of the primary outcomes separating pilot and full-scale trials.

In all meta-analyses, we used random effects models with inverse variance weighting. Heterogeneity was assessed using the I^2 statistic χ^2 tests at the recommended P-value cut-off of .1 [17]. We used R version 3.6.1 with the meta package version 4.9-7 for all analyses.

Results

RECORD selection

The search for records was conducted on May 23 of 2019. Figure 1 shows a PRISMA flow diagram of the record selection process. The search of PubMed (474), PubMed Central (250), CDSR (4), CENTRAL (428), DARE NHS-EED (12), Scopus (219), PsycINFO (149), PsycARTICLES (28), CINAHL (173), Web of Science (646), and OpenGrey (7) yielded a total of 2390 records. Citation searching identified only 2 other candidates.

Among 373 trial registry entries found during the search (ICTRN (102), ClinicalTrials.org (144), ICTRP (100), other (27)), a total of 14 entries were relevant with respect to the

eligibility criteria for this review. Eight of these entries were for the included trials and six items were for ongoing trials.

FIGURE 1

Study characteristics

Of the ten reports included in this review, five were pilot trials and five full-scale trials (as described by the authors). Two reports presented data from the same full-scale trial but for different follow-up intervals [19,20]. A summary of the trials can be found in Table 1, and a summary of data availability can be found in Table 2.

TABLE 1

TABLE 2

Participants were on average 29 years of age and 41% were female. Study populations included emergency department visitors, inpatients, college students, disadvantaged men, and online help seekers. With the exception of online help seekers, recruitment was proactive among populations not primarily seeking help with alcohol consumption. All the interventions consisted of a series of text messages sent over an average of 8 weeks (ranging from 4 to 12). The average frequency of messages sent was approximately 6.3 messages per week.

The text messages were designed to support behaviour change, typically including: self-assessment and feedback on alcohol consumption, information addressing drinking culture in the target population, facts about alcohol, strategies to limit alcohol consumption, motivational content including benefits and consequences, normative feedback, and linking out to additional support. Six of the interventions included a broad set of components ([21–26]), one focused primarily on normative feedback [27], and one intervention (included in three reports) focused on assessment and feedback [20,28,29].

For a longer description of each trial and intervention, please see Appendix B.

Results of individual TRIALS and synthesis of results

Pooled results with respect to HED are presented in Figure 2 and for WAC in Figure 3 (with stratified analyses separating pilot from full-scale trials). A risk of bias summary for each outcome is presented in Figures 4 and 5, details can be found in Appendix C. Subgroup analyses of different follow-up intervals can be found in Appendix D. Here we present findings for each outcome, taking into consideration effect size estimates, risk of bias, and overall quality of the presented body of evidence (in accordance with the GRADE framework [18]).

FIGURE 2

FIGURE 3

FIGURE 4

FIGURE 5

The prevalence of risky drinking, a planned secondary outcome, was not studied consistently with our protocol to enable meta-analysis to be undertaken; only two trials (Sharpe 2018 [24] and Crombie 2018 [22]) measured prevalence of risky drinking following criteria specified by authors in the reports (Sharpe 2018 [24] used AUDIT-C $\geq$ 3 for females and $\geq$ 4 for males, and Crombie 2018 [22] used $\geq$ 3 episodes of heavy drinking per month).

Heavy episodic drinking

The primary meta-analysis of HED found a weighted mean difference of -0.33 episodes per month (95% CI = -0.79; 0.12) in favour of the text messaging interventions. When removing pilot trials from the primary analysis, the overall effect size for HED was lower (-0.21 episodes per month, 95% CI = -0.59; 0.16). In both cases, the confidence intervals suggest that effect sizes may be more than twice as large as estimated, but they also include zero, thus we cannot rule out null findings.

The GRADE quality of the body of evidence for HED was judged to be low. First, small pilot trials shaped the overall outcome estimates. Second, high risk of performance bias due to lack of blinding was prevalent in all trials except Muench 2017 [26]. Third, risk of attrition bias was high in the full-scale trials by Suffoletto 2014 [29] and Thomas 2018 [25], which together have a weight of 41% in the primary analysis of HED. A sensitivity analysis removing these two trials resulted in a similar overall effect estimate (-0.28 episodes per month, 95% CI = -0.96; 0.39).

HED was not reported as planned in Bock 2016 [23] (ClinicalTrials.gov NCT02507115), which warrants some concern about publication bias. However, as the trial included few participants it was not judged to impact the overall quality of evidence for HED. A Funnel plot (Figure 6) and Egger's test (P-value = 0.049) revealed marginally statistically significant asymmetry, but we did not judge this to warrant further downgrade of the quality of evidence.

The subgroup analyses of different follow-up intervals (Appendix D) revealed no apparent reduction of effect over time: -0.43 episodes per month (95% CI =-0.99; 0.13) at 1-3 months, -0.39 episodes per month (95% CI = -1.03; 0.24) at 4-6 months, and -0.36 episodes per month (95% CI = -1.09; 0.36) at 7+ months. The GRADE quality of the body of evidence for 4-6 and 7+ month follow-up intervals was considered low, since all included trials in these subgroups had high or unclear risk of bias due to incomplete data, however no pilot trials were included.

FIGURE 6

Weekly alcohol consumption

The primary meta-analysis of WAC found a weighted mean difference of -18.62 grams per week (95% CI= -39.61; 2.38), in favour of the text messaging interventions. When removing pilot trials, the overall effect size for WAC was lower (-8.91 grams per week, 95% CI = -29.25; 11.43). As was the case for HED, confidence intervals suggest that null findings cannot be ruled out, however nor can estimates more than twice as large.

The GRADE quality of the body of evidence for WAC was judged to be low. First, pilot trials strongly shaped overall effect estimates and heterogeneity was evident when analysing WAC (I^2 = 73%, P-value < .01). Second, there was high risk of performance bias prevalent in all trials except Muench 2017 [26]. Third, risk of attrition bias was judged to be high for Suffoletto 2014 [29]. Removing this trial in a sensitivity analysis revealed similar effect estimates (-18.97 grams per week, 95% CI = -48.95; 11.02), being more reliant on pilot trials. A Funnel plot (Figure 7) and Egger's test (P-value = 0.39) revealed no evidence of asymmetry.

Subgroup analyses of WAC for different follow-up intervals (Appendix D) revealed some modest reductions of effect over time: -23.45 grams per week (95% CI =-48.72; 1.83) at 1-3 months, -15.71 grams per week (95% CI = -31.10; -0.31) at 4-6 months, and -18.75 grams per week (95% CI = -36.38; -1.12) at 7+ months. All included trials in these subgroups had high or unclear risk of bias due to incomplete data, however no pilot trials were included, thus the GRADE quality of body of evidence for these subgroup analyses were judged to be low.

FIGURE 7

Discussion

Summary of evidence

The meta-analyses in this review provided low quality evidence of text messaging interventions reducing HED by 0.33 episodes per month, and low quality evidence of text messaging interventions reducing WAC by 18.62 grams per week. As the confidence intervals did not rule out null findings, any possible benefits are likely to be small and imprecisely estimated. Concerns about attrition and performance bias, heterogeneity, and the degree to which pilot trials shaped effect estimates were reasons for downgrading the quality of evidence using GRADE.

Comparison to previous literature

There have been no published meta-analyses of text messaging alcohol interventions to which we could directly compare the findings herein. However, two recent meta-analyses with a broader scope, including a diverse set of digital alcohol interventions, are informative.

An individual patient data meta-analysis (IPDMA) of digital interventions included 19 trials of both guided and unguided interventions in non-student populations [30]. The overall analysis suggested that the unguided interventions reduced WAC (-32.30 grams per week, 95% CI = -58.80; -5.90). There was evidence of heterogeneity among the included trials (I^2 = 55.5%, P-value < .001), and as here, outcomes were self-reported.

A Cochrane review also took a broader view of digital interventions and was last updated in 2017 [31]. A total of 42 trials were included in the analyses of WAC, and revealed an effect of -22.84 grams per week (95% CI = -15.36; -30.31) in favour of the interventions. Heterogeneity was again marked (I^2 = 77.6%, P-value < .0001), and sensitivity analyses removing trials with high risk of performance bias due to lack of blinding halved the estimates. Analyses of HED, including 15 trials, suggested an effect of -0.24 episodes per month (95% CI = -0.35; -0.13) in favour of the interventions.

The effect sizes found in the IPDMA were somewhat higher than in this study and the Cochrane review. The reduction in HED found in this review, particularly in full-scale trials, is very similar to the findings in the Cochrane review. There is thus some consistency across these studies, both with respect to the substantive findings, and in the limitations of the literatures reviewed.

Limitations

The issue of variability of outcomes in brief alcohol intervention research is well established [31,32]. As an indication of the magnitude of the problem, the ORBITAL project [33], which aims to produce a core outcome set for brief alcohol interventions, identified 2641 different outcomes used, measured in approximately 1560 different ways, in 405 trials of brief alcohol interventions [32]. Due to this variability, we were not able to include all trials in both outcome analyses, clearly a limitation of this review. While the ORBITAL project is not yet complete, we recommend researchers consider adhering to the core outcome set to ensure that the synthesis of results from trials can be done efficiently.

Risk of bias due to lack of blinding of study participants was regarded as high in all included trials. Even trials that used non-alcohol related text messages as a control condition cannot claim blinding, as participants were aware of the nature of the study. Blinding of participants was unlikely an option in the included trials, however future trials may consider using different designs which reduce the likelihood of bias. For example, in a factorial design, effects of the components of an intervention could be estimated, allowing participants to be informed that everyone will receive the intervention but that different versions of the intervention is being tested.

The uncertainties due to lack of blinding are also related to another form of bias that is highly relevant to this literature yet does not feature directly in the tool used; the outcomes are both self-reported. It is plausible that bias may be differential by randomization arm, due to intervention participants downplaying the extent of their drinking for social desirability reasons more so than control participants [34–36]. Studies in alcohol treatment contexts find self-report to be valid [37], though data in brief intervention trials give cause for concern [38]. There is a need for further study of this important issue, though it is worth noting that dedicated online alcohol studies have not identified such problems [39,40], and objective measures of alcohol consumption are not sufficiently available to overcome reliance on self-report.

The eligibility criteria required some homogeneity with respect to intervention delivery, ie. a series of text messages were sent over several weeks. However, this did not place restrictions on intervention content. Including interventions with different content in meta-analyses may be viewed as a strength, as it allows for effect sizes to be computed which are marginalized over different content, however such marginalization arguments should be tempered as there was a limited number of trials included here.

Prevalence of risky drinking was a planned secondary outcome which was not possible to analyse, as it was scarcely reported. We decided to not request this data from authors of the included trials as it was a secondary outcome, and this decision should be considered a limitation of this review. Likewise, planned subgroup analyses with respect to age and gender could not be conducted due to data not being available. Additional planned sensitivity above those already reported were not necessary, eg. no cluster RCTs were included and all reports included intention-to-treat data. Finally, it was planned that two team members independently would extract data from studies, however due to the variety of ways outcome measures were reported, our standardized form could not be used. Instead, MB extracted data and KÅ and PB independently checked the extracted data.

Conclusions

Implications for practice

The effect estimates for HED found in this meta-analysis corresponds to approximately one less episode of heavy drinking every three months. Such an effect is small but not trivial at the population level. WAC estimates similarly identify a small reduction which could nonetheless potentially have a meaningful effect on population level incidence of cardiovascular and other noncommunicable diseases [3,4]. Helping individuals to reduce their alcohol consumption is important to do, and it should be noted that trials are mostly concerned with group level estimates (as is this review), and they may be masking important heterogenous effects of interventions in sub-groups [41].

An important factor when considering the synthesized effect sizes is that, with the exception of Muench 2017 [26], the included trials all proactively recruited participants by offering participation to individuals not primarily seeking help with alcohol. Additionally, the interventions were unguided and relied on widely available and cheap technology. Thus, small effect sizes may be indicative of potential benefit if they are free from bias.

Finally, and beyond the primary analyses, the absence of clear attenuation in effects over time is somewhat surprising; whilst this is clearly what is anticipated for brief interventions [42], it is hypothetically possible that repeated exposure to text messages over time facilitates more enduring effects.

Implications for research

When comparing the findings of this study of text messaging interventions with those on digital interventions more broadly, it is noteworthy that estimated effects are similar across reviews. From a research perspective this leaves unanswered questions with respect to

heterogeneity in effect estimates, and also underlines concern regarding bias in such trials stemming from the lack of blinding of participants.

Only one of the trials included in this review, Muench 2017 [26], was judged to be at low risk of performance bias since participants were with some certainty blinded. A similar lack of blinding of participants was also evident in the IPDMA discussed earlier [30], with only one out of the 19 included trials judged to have a low risk of performance bias. Likewise, few of the included trials in the aforementioned Cochrane review [31] (13 trials, 23%) were judged to have a low risk of performance bias, all others were judged to be at high risk due to non-blinding of participants.

Similar effect sizes in the three different reviews may be due to research artefacts rather than intervention effects. It should therefore be emphasised, that not only is there a need for more full-scale trials of text messaging interventions to better interrogate possible benefit, but future trials could seek to implement blinding [43] in such a way that information given to participants at the time of study entry does not allow participants to become aware of their allocated condition or the precise nature of the study and their role in it.

Acknowledgments

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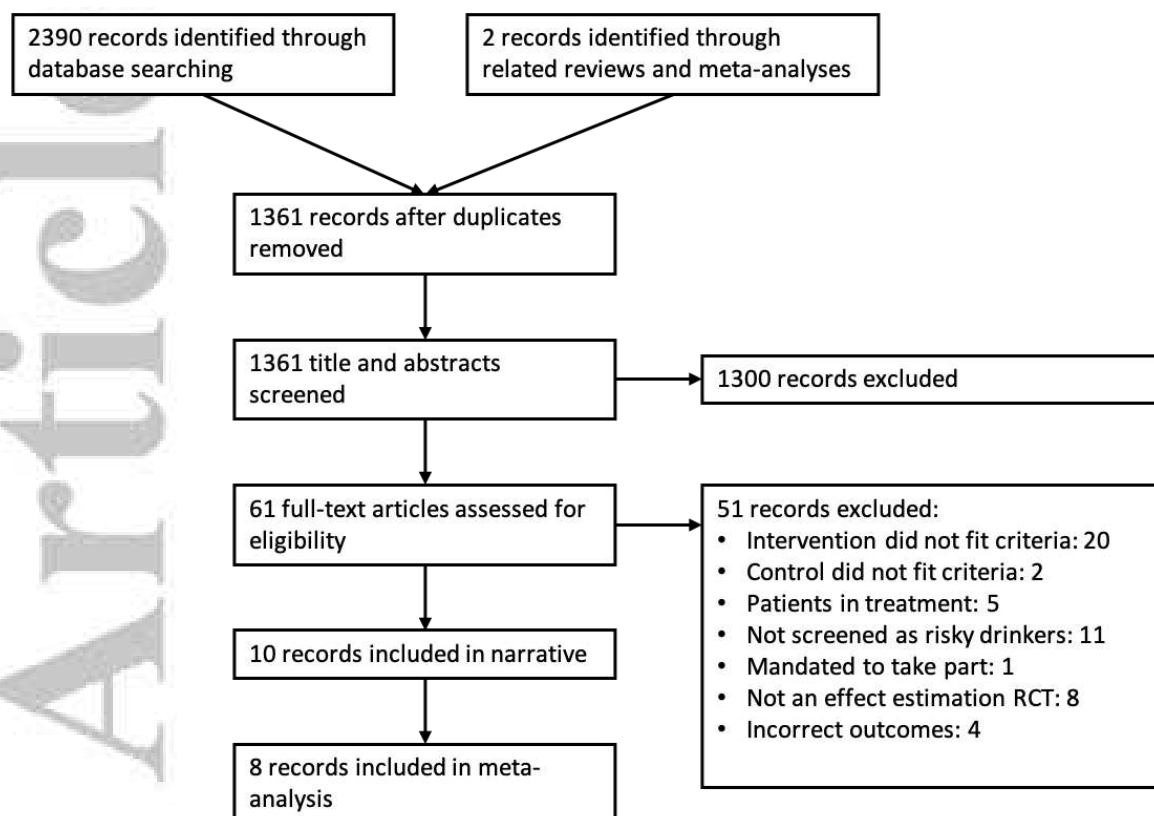
Figure 1 – PRISMA flow diagram of record selection process.

Study	Experimental Total	Mean	SD	Control Total	Mean	SD	Mean Difference	MD	95%-CI	Weight
Pilot										
Suffoletto 2012	14	2.50	3.30	25	4.98	5.07		-2.48	[-5.44; 0.48]	2.2%
Muench 2017	118	7.88	7.87	28	11.56	9.92		-3.68	[-7.10; -0.27]	1.7%
Merrill 2018	34	3.29	2.75	34	3.47	2.42		-0.18	[-1.41; 1.05]	10.1%
Random effects model	166			87				-1.68	[-3.91; 0.56]	14.0%
Heterogeneity: $I^2 = 60\%$, $\tau^2 = 2.3279$, $p = 0.08$										
Full-scale										
Suffoletto 2014	290	3.10	4.30	308	3.91	4.39		-0.81	[-1.51; -0.11]	20.5%
Crombie 2018 (6-month)	366	3.82	5.40	371	3.85	5.40		-0.03	[-0.81; 0.75]	18.3%
Sharpe 2018 (3-month)	262	1.85	3.23	272	1.82	2.67		0.03	[-0.47; 0.53]	26.7%
Thomas 2018	226	4.30	3.54	180	4.46	3.62		-0.16	[-0.86; 0.54]	20.4%
Random effects model	1144			1131				-0.21	[-0.59; 0.16]	86.0%
Heterogeneity: $I^2 = 24\%$, $\tau^2 = 0.0360$, $p = 0.27$										
Random effects model	**1310**			**1218**				**-0.33**	**[-0.79; 0.12]**	**100.0%**
Heterogeneity: $I^2 = 41\%$, $\tau^2 = 0.1348$, $p = 0.12$										
Residual heterogeneity: $I^2 = 44\%$, $p = 0.11$										

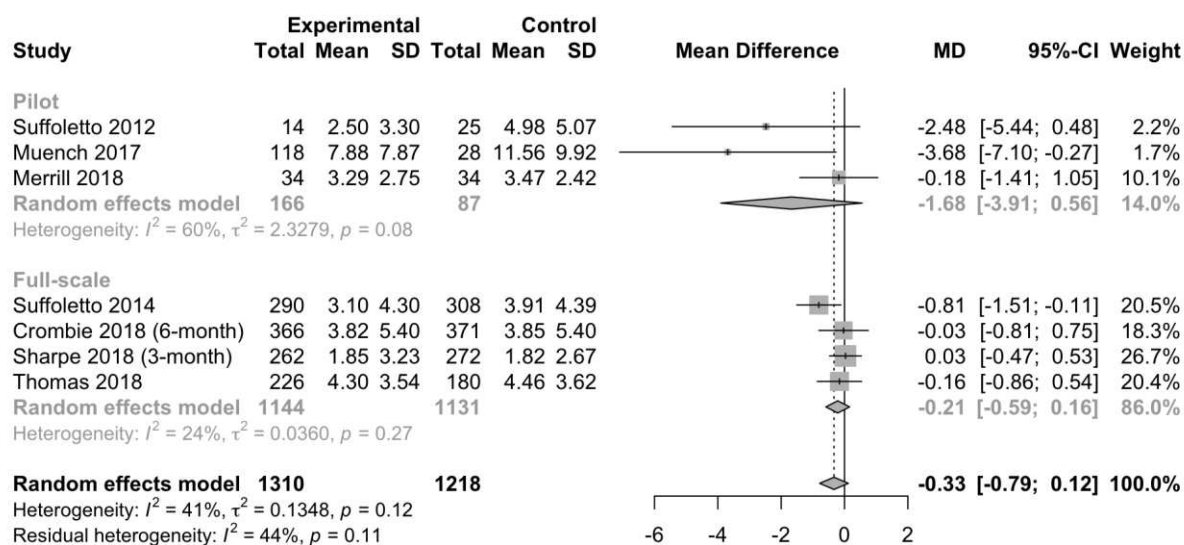Mean Difference axis: -6 -4 -2 0 2

Figure 2 - Results from individual trials and overall effect estimate with respect to number of heavy drinking episodes per month. Primary planned analysis which includes the first post intervention follow-up interval from each included trial (SD – standard deviation, MD – mean difference, CI – confidence interval). Stratified by pilot and full-scale trials.

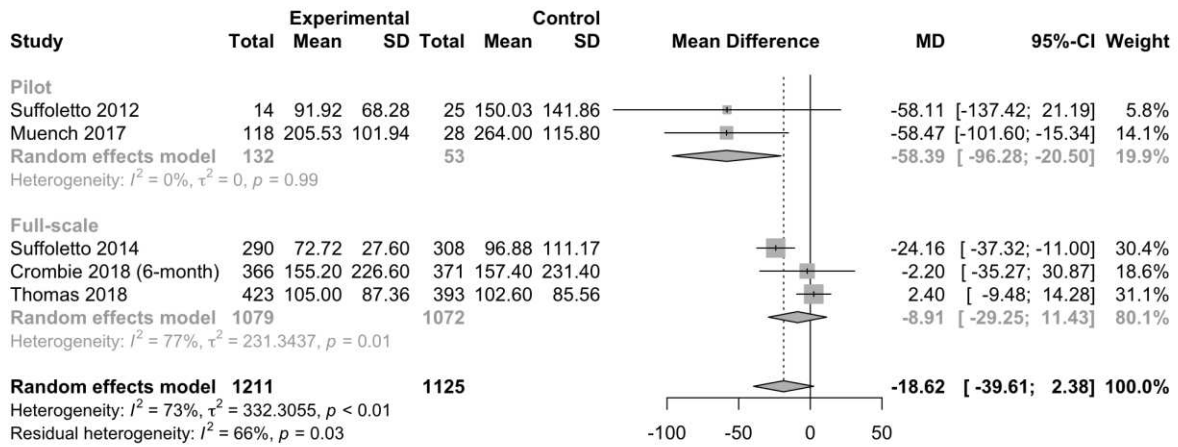| Study	Total	Experimental Mean	SD	Total	Control Mean	SD	Mean Difference	MD	95%-CI	Weight
Pilot										
Suffoletto 2012	14	91.92	68.28	25	150.03	141.86		-58.11	[-137.42; 21.19]	5.8%
Muench 2017	118	205.53	101.94	28	264.00	115.80		-58.47	[-101.60; -15.34]	14.1%
Random effects model	132			53				-58.39	[-96.28; -20.50]	19.9%
Heterogeneity: $I^2 = 0\%$, $\tau^2 = 0$, $p = 0.99$										
Full-scale										
Suffoletto 2014	290	72.72	27.60	308	96.88	111.17		-24.16	[-37.32; -11.00]	30.4%
Crombie 2018 (6-month)	366	155.20	226.60	371	157.40	231.40		-2.20	[-35.27; 30.87]	18.6%
Thomas 2018	423	105.00	87.36	393	102.60	85.56		2.40	[-9.48; 14.28]	31.1%
Random effects model	1079			1072				-8.91	[-29.25; 11.43]	80.1%
Heterogeneity: $I^2 = 77\%$, $\tau^2 = 231.3437$, $p = 0.01$										
Random effects model	**1211**			**1125**				**-18.62**	**[-39.61; 2.38]**	**100.0%**
Heterogeneity: $I^2 = 73\%$, $\tau^2 = 332.3055$, $p < 0.01$										
Residual heterogeneity: $I^2 = 66\%$, $p = 0.03$										

(x-axis: -100 -50 0 50)

Figure 3 - Results from individual trials and overall effect estimate with respect to grams of alcohol per week. Primary planned analysis which includes the first post intervention follow-up interval from each included trial (SD – standard deviation, MD – mean difference, CI – confidence interval). Stratified by pilot and full-scale trials.
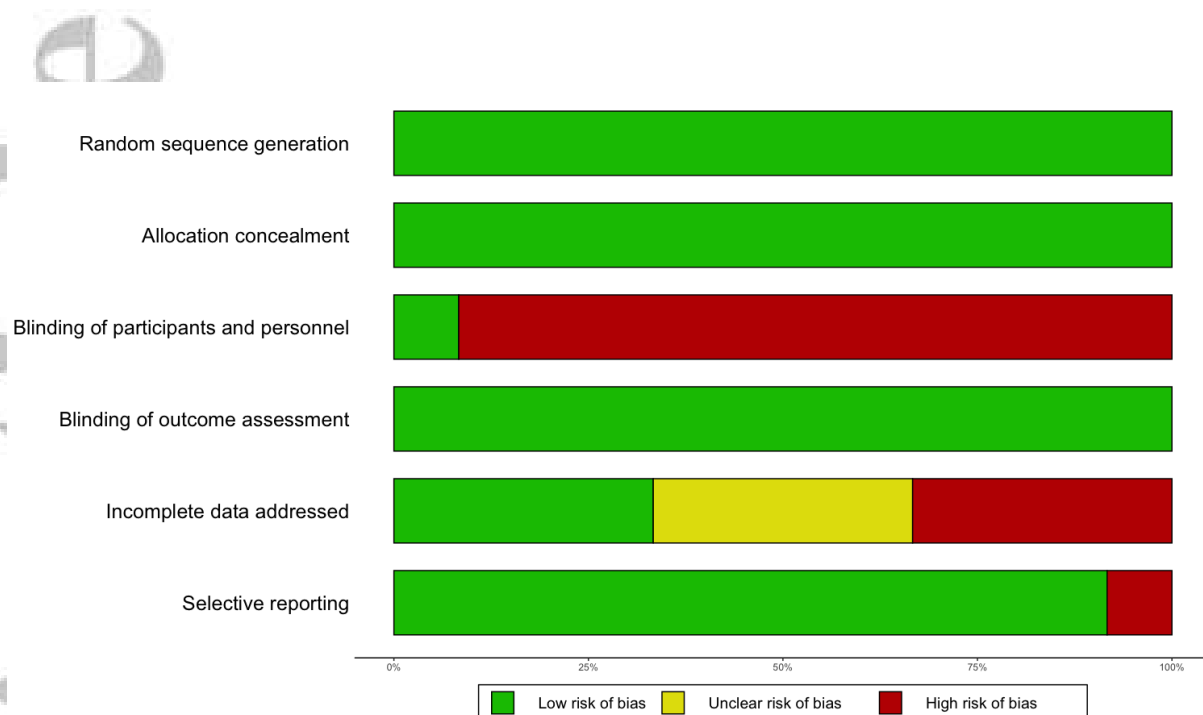
Figure 4 - Risk of bias summary: risk of bias broken down for each criterion across all included trials.

Figure 5 - Risk of bias plot: risk of bias broken down for each criterion and each included trial.
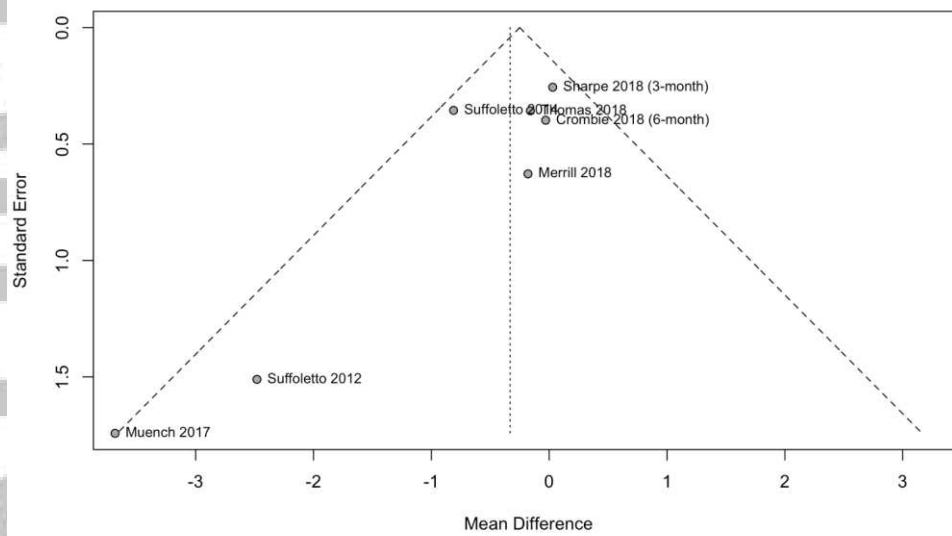
Figure 6 – Funnel plot investigating publication bias of heavy episodic drinking. Egger's test revealed marginally statistically significant asymmetry (P-value = 0.049).
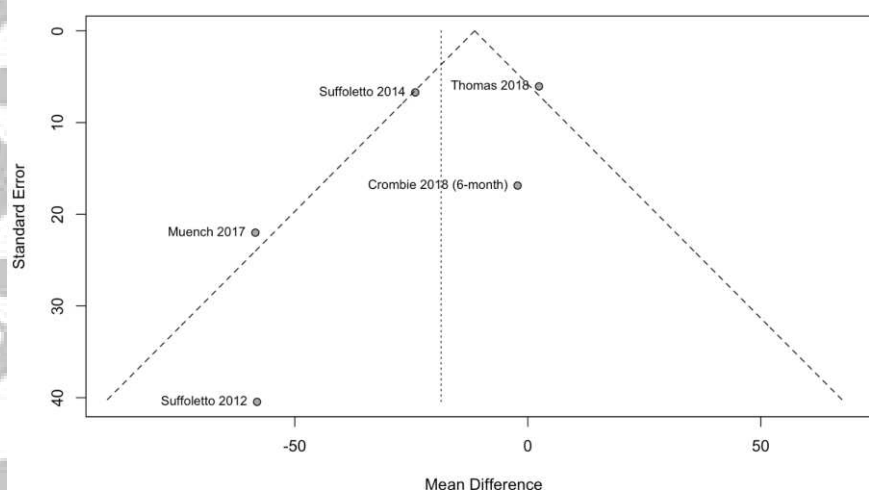
Figure 7 – Funnel plot investigating publication bias of weekly alcohol consumption. Egger's test revealed no statistically significant asymmetry (P-value = 0.39).

Table 1 - Summary of included trials evaluating the effect of text messaging interventions on alcohol consumption outcomes.

Source / Methods	Participants[a]	Interventions	Controls	Outcomes[b] (follow-up rate)
Suffoletto 2012 [28] Pilot randomized controlled trial	Young adults recruited from emergency departments. Intervention n=15 Control n=30 Mean age: 21 y Female: 64% Mean HED past month: 5.2	12-week program with number of messages varying depending on input from participant. Pre-weekend planning and post-weekend assessment through text messages with real-time feedback.	Post-weekend assessment message only for 12-weeks (n=15) No messages (n=15)	3-month: HED (86.7%) WAC[c] (86.7%)
Crombie 2013 [21] Pilot randomized controlled trial	Disadvantaged men reached through community outreach program Intervention n=34 Control n=33 Mean age: 34.2 y[d] Females: 0% Mean HED past month: 5.88	36 messages over 28 days. Tailored to the target group and constructed to take advantage of the conventional pattern of heavy weekend drinking.	34 messages over 28 days. General health information not mentioning alcohol.	3-month: HED (96%) WAC (96%)
Suffoletto 2014 [29] Suffoletto 2015 [20] Randomized controlled trial	Young adults recruited from emergency departments. Intervention n=384 Control n=381 Mean age: 22 y Female: 65.4% Mean AUDIT-C score: 6.3	12-week program with number of messages varying depending on input from participant. Pre-weekend planning and post-weekend assessment through text messages with	Post-weekend assessment message only for 12-weeks (n=196) No messages (n=185)	3-month: HED (78.2%) WAC[c] (78.2%) 6-month: HED (63.5%) WAC[c] (63.5%) 9-month: HED (54.9%) WAC[c] (54.9%)

		real-time feedback.		
Bock 2016 [23] Pilot randomized controlled trial	Community college students recruited by posted flyer Intervention n=31 Control n=29 Mean age: 21.8 y Females: 61.7% Mean HED past two weeks: 3.91	36 messages sent over 6 weeks. Facts about alcohol, strategies to limit alcohol use and alcohol-related-risks, and motivational messages.	36 messages sent over 6 weeks. General motivational content without reference to alcohol or harm reduction.	6-week (93.3%) 12-week (88.3%) HED/WAC not available in report.
Muench 2017 [26] Pilot randomized controlled trial	Online help seekers Intervention n=127 Control n=30 Mean age: 43.2 y Female: 74.9% Mean HED per week: 3.4 HED	85 messages over 12 weeks + weekly assessment. Four different versions: Loss-framed Gain-framed Statically tailored Tailored adaptive	Weekly assessment only	3-month: HED (83%) WAC (83%)
Crombie 2018 [22] Randomized controlled trial	Disadvantaged men reached through community outreach program Intervention n=411 Control n=414 Mean age: 34.6 y Females: 0% Mean HED past month: 6.58	112 messages over 12 weeks. Tailored to the target group and the drinking culture of disadvantaged young men.	89 messages over 12 weeks. General health information not mentioning alcohol.	6-month: HED (89.3%) WAC (89.3%) 15-month: HED (85.6%) WAC (85.6%)

Merrill 2018 [27] Pilot randomized controlled trial	College students invited by email Intervention n=34 Control n=34 Mean age: 19 y Female: 70.6% Mean HED past month: 3.77	28 messages over 28 days. Feedback based on descriptive norms (what others do) and injunctive norms (what others approve of).	28 messages over 28 days. Text messages with fun facts unrelated to alcohol.	1-month: HED (100%) WAC[c] (100%)
Sharpe 2018 [24] Randomized controlled trial	Inpatients admitted for injury. Intervention n=299 Control n=299 Mean age: 34 y Female: 28.6% Mean AUDIT-C score: 6.85	16 messages over 4 weeks. Feedback and reflection on drinking, recommendation to cut down and linked to existing services, tips and strategies, support and encouraging content.	One message acknowledging participation in the trial and indicated they would be contacted in three months.	3-month: HED[c] (89.3%) 6-month: HED[c] (82.8%) 12-month: HED[c] (72.1%) WAC not available
Thomas 2018 [25] Randomized controlled trial	University and college students invited through email. Intervention n=460 Control n=436 Mean age: 25.4 y Female: 56.8% Mean WAC: 165.6 grams of alcohol	62 messages sent over 6 weeks. Messages throughout the week to support behavior change, post-weekend assessment through texting with feedback.	Recommended to assess their drinking at website, no further contact.	3-month: HED (45.3%) WAC (91.1%)

[a] y = years
[b] HED = heavy episodic drinking, WC = weekly consumption
[c] Made available by request from corresponding author
[d] Converted from categorical (25-29 n=20, 30-34 n=11, 35-39 n=18, 40-44 n=15)

Table 2 - Data availability from the trials included in the systematic review.

Source	HED	WAC	Comment
Suffoletto 2012 [28]	In report	By request	Two arms fit the criteria for control which were combined using weighted means.
Crombie 2013 [21]	Not available	Not available	Standard deviations of outcome measures were not available. We decided against imputing standard deviations as it was a pilot trial with few participants, thus the actual sample standard deviations could potentially be very different from those reported in other included trials.
Suffoletto 2014 [29]	In report	By request	Two arms fit the criteria for control which were combined using weighted means.
Suffoletto 2015 [20]	In report	By request	Two arms fit the criteria for control which were combined using weighted means. Data were not included in the primary meta-analyses as they came from the same trial participants as in Suffoletto 2014 [29], thus records would not be independent for statistical analysis purposes.
Bock 2016 [23]	Not available	Not available	
Muench 2017 [26]	In report	In report	There were four intervention arms and one control arm. The intervention arms were combined using weighted means.
Crombie 2018 [22]	In report	In report	Data from the 6-month follow-up was included in the primary meta-analyses.
Merrill 2018 [27]	In report	By request	WAC was made available by request but could not be used without additional data not available at the time of analysis.
Sharpe 2018 [24]	By request	Not available	Data on the third item of the AUDIT-C questionnaire was made available by request. As was planned in the protocol, categorical answers were converted to numeric: Never = 0, Less than monthly = 0.5, Monthly = 1, Weekly = 4, Daily or almost daily = 22.5. Data from the 3-month follow-up was included in the primary meta-analyses.
Thomas 2018 [25]	In report	In report	As was planned in the protocol, categorical answers for HED were converted to numeric: Never = 0, Less than monthly = 0.5, Monthly = 1, 2-3 times per month = 2.5, Once or twice a week = 6, 3 times or more per week = 14.