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Brief Alcohol Interventions for Adolescents and Young Adults: A Systematic Review and Meta-Analysis

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ABSTRACT

This study reports findings from a meta-analysis summarizing the effectiveness of brief alcohol interventions for adolescents (age 11–18) and young adults (age 19–30). We identified 185 eligible study samples using a comprehensive literature search and synthesized findings using random-effects meta-analyses with robust standard errors. Overall, brief alcohol interventions led to significant reductions in alcohol consumption and alcohol-related problems among adolescents ($\bar{g} = 0.27$ and $\bar{g} = 0.19$) and young adults ($\bar{g} = 0.17$ and $\bar{g} = 0.11$). These effects persisted for up to 1 year after intervention and did not vary across participant demographics, intervention length, or intervention format. However, certain intervention modalities (e.g., motivational interviewing) and components (e.g., decisional balance, goal-setting exercises) were associated with larger effects. We conclude that brief alcohol interventions yield beneficial effects on alcohol-related outcomes for adolescents and young adults that are modest but potentially worthwhile given their brevity and low cost.

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1. Introduction

A sizeable portion of adolescents and young adults engage in heavy episodic consumption of alcohol, and thus put themselves at risk for numerous detrimental consequences related to their physical, mental, and social well-being (Brown et al., 2009). In 2011, for example, the estimated rate of past month binge drinking (five or more drinks on the same occasion for males, four or more for females) was 15% for 16–17 year olds, 31% for 18–20 year olds, and 45% for 21–25 year olds (SAMHSA, 2012). In response, a growing body of research has sought to identify early intervention programs that are effective for preventing or delaying the initiation of alcohol use, or intervening with heavier users before they progress to more problematic levels of use. One approach is a brief intervention, defined broadly here as an intervention aimed at providing motivation for behavior change in a relatively circumscribed time (one to five sessions). Brief interventions are attractive primarily because of their brevity and the varied settings in which they can be conveniently delivered. If effective, they may therefore offer a cost-effective way to address a potentially lethal public health problem (Fleming et al., 2002; Neighbors, Barnett, Rohsenow, Colby, & Monti, 2010; Wutzke, Shiell, Gomel, & Conigrave, 2001).

Prior research reviews have found that brief interventions are indeed generally effective in reducing alcohol consumption among adolescents and young adults (e.g., Carey, Scott-Sheldon, Carey, & DeMartini, 2007; Carey, Scott-Sheldon, Elliott, Bolles, & Carey, 2009; Tait & Hulse, 2003). However, these reviews have not fully explored the characteristics of the participants and interventions that are associated with the strongest intervention effects or the persistence of those effects over time. The accumulating research is ripe for a comprehensive meta-analysis that examines how much, when, for whom, and for how long such interventions are effective in this population—information that can guide future research and aid practitioners planning to implement brief alcohol interventions.

1.1. Brief alcohol interventions for adolescents and young adults

The defining characteristic of a brief intervention is the relatively brief contact time—generally one to five sessions—with a provider such as a physician, nurse, psychologist, counselor, or other service professional. In other regards, these interventions vary considerably, e.g., in length, structure, targets, media communication, underpinning theory, and intervention philosophy (Heather, 1995). Brief interventions are typically not intended to provide a full treatment regimen for individuals with alcohol use disorders but, rather, are designed to motivate and provide resources to participants to help them moderate their alcohol consumption, or, if needed, seek more intensive treatment options. As such, they can be used as universal, selective, or indicated prevention strategies (Barry, 1999). Most brief alcohol interventions include at least one of the following components: a discussion of alcohol consumption, feedback on risk or levels of use, comparisons to local or national norms, information on potential harms, or coping strategies and goal-setting

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plans for dealing with drinking situations. These therapeutic components are most often based on the principles of cognitive behavioral therapy, motivational interviewing (Miller & Rollnick, 1991), the transtheoretical model of behavior change (Prochaska & DiClemente, 1984), or social norms theory (Berkowitz, 2004), all of which emphasize the stimulation of participants' abilities, capacities, and motivations to self-evaluate and self-regulate their behaviors.

Since the launch of the Substance Abuse and Mental Health Services Administration's Screening, Brief Intervention, and Referral to Treatment Initiative in 2003 (SAMHSA, 2014), there has been an expanding body of methodologically rigorous research on brief interventions targeting alcohol and illicit substance use. Prior meta-analyses have shown that brief interventions are capable of reducing alcohol use among adults (Ballesteros, Duffy, Querejeta, Arino, & Gonzalez-Pinto, 2004; Beich, Thorsen, & Rollnick, 2003; Bertholet, Daeppen, Wietlisbach, Fleming, & Burnand, 2005; Bien, Miller, & Tonigan, 1993; Burke, Arkowitz, & Menchola, 2003; Kaner et al., 2007; Poikolainen, 1999; Vasilaki, Hosier, & Cox, 2006; Wilk, Jensen, & Havighurst, 1997). Their effectiveness for younger populations, however, has been less well documented despite the prevalence of excessive alcohol use among youth. The one meta-analysis of which we are aware that focused exclusively on brief interventions for adolescents found a moderate positive benefit for alcohol use outcomes (Cohen's $\overline{d} = 0.28$), but included only 11 studies (Tait & Hulse, 2003). Although several existing meta-analyses have focused on alcohol interventions for college-age students (e.g., Carey et al., 2007; Carey et al., 2009; Fachini, Aliane, Martinez, & Furtado, 2012; Moreira, Smith, & Foxcroft, 2009; Scott-Sheldon, DeMartini, Carey, & Carey, 2009), most of these reviews either included brief interventions among other types of alcohol interventions, or only focused on a specific branded intervention program (e.g., Brief Alcohol Screening and Intervention for College Students [BASICS]). The most comprehensive meta-analysis to date (Carey et al., 2007) focused on 62 studies of individually delivered alcohol interventions for college students and reported positive effects for alcohol outcomes ($\overline{d} =$ 0.17–0.18), although these effects were significantly attenuated over longer follow-up periods.

The large and growing body of empirical research assessing the effectiveness of brief alcohol interventions for adolescents and young adults is now sufficient to support a comprehensive systematic review and meta-analysis that can investigate the sources of variability in outcomes in more depth than has been possible in prior reviews. In particular, the current meta-analysis examines how much, when, for whom, and for how long brief alcohol interventions may be effective in youth populations.

1.2. Sources of variability in the effects of brief alcohol interventions

The growing support for brief alcohol interventions is leading researchers and practitioners to call for research to move beyond questions of whether they work to questions of what makes them work and in which populations and conditions they work best (Cunningham et al., 2009; Nilsen, Kaner, & Babor, 2008). The diversity of brief interventions, for instance, makes it important to investigate the extent to which the effects vary with the characteristics of the interventions themselves. A central question in that regard is just how brief the interventions can be and still be effective. Some researchers have suggested that three to four intervention sessions with a few follow-up calls or brief visits are most effective (Fleming et al., 2002), whereas others have maintained that 1 hour or even 5 minute interventions can be effective, at least with college students (Kulesza, Apperson, Larimer, & Copeland, 2010).

Other intervention characteristics that may moderate treatment effects include the primary intervention modality (e.g., motivational interviewing, cognitive behavioral), specific intervention components (e.g., providing advice, personalized normative feedback, goal-setting exercises), the delivery site (e.g., emergency room, school/university), and the delivery mode (e.g., computerized, in-person) (Bewick et al., 2008; Whitlock, Polen, Green, Orleans, & Klein, 2004; Winters & Leitten, 2007).

Another question is whether brief interventions are more effective for some types of participants than others (Cunningham et al., 2009). Among young populations, a key distinction is between adolescents who are 18 years old and under, for whom alcohol consumption is almost universally illegal, and young adults of college age, many of whom can drink legally and are in the prime years for doing so (SAMHSA, 2012). Gender, race, and baseline levels of alcohol use may also be important individual characteristics associated with intervention effectiveness (Bien et al., 1993; Kaner et al., 2007; Poikolainen, 1999; Walton et al., 2008).

Still another question is whether, given their brevity, brief interventions can produce effects that are sustained. In their meta-analysis of (mostly brief) alcohol interventions for college students, Carey et al. (2007) found that intervention effects were attenuated to non-significance by 27 weeks of follow-up. However, they had limited data with which to investigate this issue, so there is a question as to how generalizable these results are for other populations and across different intervention approaches.

The study reported here used meta-analytic methods to synthesize the empirical findings from brief alcohol intervention research with adolescents (age 11–18) and young adults (age 19–30) with particular emphasis on identifying variables related to differential effects. Specifically, this meta-analysis examined: (1) the overall effects of brief alcohol interventions on adolescent and young adults' alcohol consumption and alcohol-related problems, (2) the variation in effects associated with intervention and participant characteristics, and (3) the persistence of the effects of brief alcohol interventions.

2. Materials and methods

2.1. Inclusion and exclusion criteria

Eligible studies were those focused on brief interventions explicitly aimed at reducing participants' alcohol use or alcohol-related problems. Interventions could target any risk level (universal, selective, or indicated) of participants, as long as they involved five or fewer hours of total contact time and four or fewer weeks between the first and last intervention session (excluding booster sessions). Specifying these criteria for the maximum amount of contact allowable for an intervention to be considered brief was necessary to make reliable decisions about which studies were eligible for inclusion in the review. The criteria used here were guided, first, by the definitions used in other discussions of brief alcohol interventions (e.g., Barry, 1999). We also examined the distributions of contact time and treatment duration values found in the studies included in our meta-analysis of outpatient treatment for adolescents with substance use disorders (Tanner-Smith, Wilson, & Lipsey, 2013). These were bimodal and 5 hours of contact time and 4 weeks duration marked points that generally separated the shorter interventions from more extensive outpatient treatment programs.

Eligible studies had to include comparison conditions of no treatment, a wait-list control, or some form of routine treatment as usual (i.e., services the participants would have received even in the absence of the brief intervention). Studies that compared two types of interventions were not eligible. Eligible participant samples included adolescents and young adults, defined as individuals age 11–25. Samples comprised entirely of undergraduate college students were also eligible even though they may have included students over the age of 25, but no older than age 30. Eligible research designs included randomized controlled designs and controlled quasi-experimental designs that provided enough information to permit estimation of a pretest effect size that could be used (by us

or the primary study authors) to adjust the posttest effect estimates for any initial group differences. Eligible studies were required to assess intervention effects on at least one outcome variable that measured alcohol use or alcohol-related problems (e.g., DUI/DWI). To be applicable to contemporary youth, only studies conducted in 1980 or later were included in the review. There were no geographic or language restrictions on eligibility.

2.2. Search strategy

Using a comprehensive search strategy, we attempted to identify and retrieve the entire population of published and unpublished studies that met the aforementioned inclusion criteria. The following electronic bibliographic databases were searched, current through December 31, 2012: CINAHL, Clinical Trials Register, Dissertation Abstracts International, ERIC, International Bibliography of the Social Sciences, NIH RePORTER, PsycARTICLES, PsycINFO, PubMed, Social Services Abstracts, Sociological Abstracts, and WorldWideScience.org. We also searched the following sources in an attempt to locate gray literature: Australasian Medical Index, Campbell Collaboration Library, Canadian Evaluation Society's Grey Literature Database, Chestnut Health Systems Web site, Cochrane Collaboration CENTRAL, College on Problems of Drug Dependence conference presentations, EPPI-Centre Database of Health Promotion Research, Google Scholar, Index to Theses in Great Britain and Ireland, International Clinical Trials Registry, Joint Meeting on Adolescent Treatment Effectiveness conference presentations, KoreaMed, NIAAA Web site, NTIS, OpenSIGLE, SAMHSA Web site, Social Care Online, and SveMed +. We checked the bibliographies of all screened and eligible studies, as well as the bibliographies of prior narrative reviews and meta-analyses. We also conducted handsearches in *Alcoholism: Clinical and Experimental Research, American Journal on Addictions*, and the *Journal of Substance Abuse Treatment*.

2.3. Screening and coding procedures

Under the supervision of the first author, a team of six master's level research assistants first screened all abstracts and titles resulting from the search to eliminate clearly irrelevant study reports. All six researchers initially screened 500 randomly selected abstracts/titles and discussed disagreements until 100% consensus was reached. The remaining abstracts/titles were screened by one team member, with the first author reviewing all their screening decisions. Any disagreements were discussed until consensus was reached. If there was any ambiguity about the potential eligibility of a report based on the abstract/title, we retrieved the full text report for further review. Full text was retrieved for all study reports that were not judged explicitly ineligible in the initial screening. The same team then used the same procedure to screen full text reports and make final eligibility decisions, with the first author again reviewing all eligibility decisions and consensus used to resolve any disagreements.

All data extraction followed a standardized coding protocol, with data entered directly into a FileMaker Pro database. The coding protocol was similar to those used in our previous meta-analyses (e.g., Wilson, Lipsey, Tanner-Smith, Huang, & Steinka-Fry, 2010), and provided detailed instructions for extracting data related to general study characteristics, participant groups, the interventions, outcome measures, and statistical data needed for effect size calculations (coding protocol available from the authors upon request). Coding information from the eligible study reports was conducted by the same research team after several weeks of training led by the first author. During training, five studies were coded by all the coders, who then convened to resolve coding discrepancies until 100% consensus was attained on all coded variables. After the training period, coding questions were addressed in weekly meetings and decided via consensus with the group. In addition, the first author reviewed all coding and resolved any further discrepancies via consensus with the initial coder.

2.4. Statistical methods

2.4.1. Effect size metric

The intervention effects of interest were represented with standardized mean difference effect sizes (Cohen's d), calculated as the difference between the intervention and control group means on an outcome variable after the end of the intervention divided by the pooled standard deviation. These effect sizes were adjusted with the small-sample correction factor to provide unbiased estimates (g) (Hedges, 1981). All effect sizes were coded so that positive values indicate better outcomes (e.g., lower alcohol consumption, higher abstinence). For binary outcomes (e.g., abstinence), the Cox transformation outlined by Sánchez-Meca, Marín-Martínez, and Chacón-Moscoso (2003) was used to convert log odds ratios into standardized mean difference effect sizes. Effect size and sample size outliers were Winsorized to less extreme values to prevent them from having disproportionate influence on the meta-analysis (Lipsey & Wilson, 2001).[1] When studies reported pretest-adjusted posttest means for both intervention groups (e.g., using ANCOVA or regression methods for adjustment), we used those pretest-adjusted means in the posttest effect size calculations. Otherwise, most studies provided enough information to permit estimation of a pretest effect size for baseline differences between groups, and we then used our own covariate adjustment method to control for pretest differences between groups (described in detail in Section 2.4.4).

The standard errors of the effect size estimates used in the weighting function for the meta-analysis were adjusted for the nesting of participants within clusters (e.g., schools) for studies ($k = 39$) using designs in which clusters were assigned to conditions. In these cases, the standard error of the effect size was multiplied by the square root of the design effect, $SE_{adj} = SE_g * \sqrt{1 + (M-1)*ICC}$, where SE_{adj} is the standard error adjusted for cluster assignment, SE_g is the standard error ignoring clustering, M is the average cluster size, and ICC is the intraclass correlation coefficient for the proportion of between cluster variance (Higgins, Deeks, & Altman, 2008). When cluster-assigned trials did not report the ICC, we used an estimated ICC value of .13, which was the average ICC for all alcohol consumption outcomes across the studies reporting it.[2]

2.4.2. Moderator variables

The following participant characteristics were explored as potential effect size moderators in the analysis: percentage of White youth in the intervention group; average participant age; percentage of males in the intervention group; and whether participants were recruited based on their identification as high-risk drinkers on baseline screening assessments (1 = *high-risk screened/selective sample*; 0 = *universal sample*). The following intervention characteristics were explored as potential effect size moderators: interval between the end of the intervention and posttest measurement; focal modality (21st birthday card, cognitive behavioral/skills training, expectancy challenge, motivational enhancement, psycho-education, personalized feedback/information, cognitive behavioral + motivational enhancement,

[1] Outliers were defined as values falling three times the interquartile range beyond the upper/lower fences of the distributions, and were Winsorized to the upper/lower fence values.

[2] Whether a study used a cluster assignment design was uncorrelated with effect size magnitude ($r = -.002$), and only modestly correlated with three moderators of interest (see Appendix C). Sensitivity analyses for all moderator analyses (not shown) that additionally controlled for cluster assignment design yielded results substantively similar to those reported here.

family focused therapy, or other); delivery site (school/university, emergency room, primary care/university health center, or self-administered); format (self-administered and computerized, self-administered but not computerized, individual, group, or family); treatment duration (single session less than 5 minutes, single session of 5–15 minutes, single session longer than 15 minutes, or multi-session); and the presence/absence of the following intervention components (BAC information, alcohol caloric information, decisional balance exercise, general education/information, personalized feedback, gender-specific feedback, goal-setting, providing money/cost information about drinking, or local/national drinking norm referencing).

Methodological and procedural characteristics of studies are often confounded with their substantive features. Therefore, the following methodological characteristics were coded and used in analysis as statistical controls to help disentangle those relationships: study design (randomized vs. controlled quasi-experimental); attrition between pretest and posttest; whether binary data were used to estimate the effect size ($1 = yes$; $0 = no$)[3]; whether the posttest effect size was estimated using author-reported pretest-adjusted posttest means ($1 = yes$; $0 = no$); type of control group (no treatment versus treatment as usual); and pretest differences between intervention and control groups indexed using standardized mean difference effect sizes.

2.4.3. Missing data

There was a modest amount of missing data for five of the moderators and covariates of interest (11% of studies were missing attrition data; 37% pretest effect sizes; 15% average age; 3% gender composition; 24% race composition). For these five covariates, we imputed missing data using an expectation-maximization algorithm (Allison, 2002) so that all cases could be included within any given analysis. Sensitivity analyses (not reported) using listwise deletion to handle the missing moderator and covariate values yielded substantively similar results, so we elected to present results using the imputed values. When primary studies failed to include sufficient statistical information to estimate effect sizes, we contacted the study authors for that information. Overall, we had an excellent response from authors, most of whom provided the needed information. Seventeen studies met all eligibility criteria but did not provide sufficient information to estimate effect sizes, and we were unsuccessful in obtaining that information from study authors. We did not impute missing effect sizes on any outcome variables but, rather, omitted them from any analysis involving those outcomes.

2.4.4. Analytic strategies

Most studies reported multiple measures of alcohol consumption (e.g., frequency of consumption, quantity consumed, blood alcohol concentration), and/or multiple measures of alcohol-related problems (e.g., scores from the Rutgers Alcohol Problem Index, relationship problems, risky sexual behavior, DUI/DWI convictions).[4] We therefore used robust variance estimation in all analyses (Hedges, Tipton, & Johnson, 2010), which permits inclusion of statistically dependent effect sizes (e.g., two different measures of alcohol consumption from the same participant sample), and ensures that studies contributing multiple effect sizes are not given proportionately more weight to the analysis than studies contributing fewer effect sizes.[5] All analyses used inverse variance weighting so that the contribution of each effect size was proportionate to its statistical precision (Hedges & Olkin, 1985; Lipsey & Wilson, 2001). As suggested by Hedges et al. (2010), we used a conservative approach in calculating the weights by applying a weight for each effect size based on the sample size, then dividing that weight by the number of effect sizes in that study for a given outcome type. All analyses used a random-effects model given the presumed heterogeneity in effect sizes, and our desire to generalize findings beyond our analytic sample.

Meta-regression models were used to investigate the overall effects of the interventions, the influence of the intervention and participant characteristics as moderators, and the persistence of effects over time. These models were also used to generate predictions of the mean effect sizes for studies with different intervention characteristics. Because effect sizes can be influenced by the methodological characteristics of the studies, all analyses used effect sizes that were covariate-adjusted to estimate effects at the following values: randomized study design, binary data not used to estimate effect size, effect size adjusted for pretest differences, no-treatment control condition, no pretest differences, and average level of attrition. This was done by predicting the effect sizes from only these variables, retaining the residuals, and adding each residual to the constant predicted from the selected values on the covariates (see Appendix B for estimation model). This conservative technique ensures that any variance in effect sizes associated with differences in method between studies is removed from the analysis of the influence of substantive variables on those effect sizes.

We used contour enhanced funnel plots (Peters, Sutton, Jones, Abrams, & Rushton, 2008) to explore the possibility of bias resulting from omission of small sample size studies with null or negative findings due to selective publication, reporting, or other forms of dissemination biases. None of the funnel plots (not shown, but available from authors upon request) indicated asymmetry, thus providing no clear evidence of small study bias. However, many of the included studies had similar sample sizes and, overall, there was a notable lack of small sample studies regardless of whether they reported negative, null, or positive findings.

All results are presented separately by the age of participant samples (adolescents ages 11–18 versus young adults ages 19–30).[6] Although some results did not differ across age groups, we chose to present them separately given the different legal, social, and developmental norms surrounding alcohol in these groups, and researcher and practitioner interest in age-specific effects. Similarly, we present all results separately for alcohol consumption and alcohol-related problem outcomes, given their conceptual differences.

3. Results

3.1. Literature search

We identified 7593 candidate reports in the literature search; 2467 were duplicates that were dropped from consideration and 2641 were

[3] See Sánchez-Meca et al. (2003) for a discussion on transforming effect sizes into different metrics.

[4] Although some outcomes measures may be more commonly used with certain populations (e.g., blood alcohol concentration measures used in selective or indicated populations of youth), based on our pre-specified eligibility criteria for the systematic review we conceptualized all of these measures as representing the same underlying constructs of alcohol consumption or alcohol related problems, respectively.

[5] The robust standard error technique requires an estimate of the mean correlation (ρ) between all pairs of effect sizes within a study for calculation of the between-study sampling variance estimate, τ^2. In all analyses, we estimated τ^2 with $\rho = .80$; sensitivity analyses (not reported) showed that the findings were robust across different plausible estimates of ρ.

[6] We split group at age 18/19 given that the majority of studies recruited participants from either high school or postsecondary educational settings, and given a specific interest in examining efficacy for adolescents.

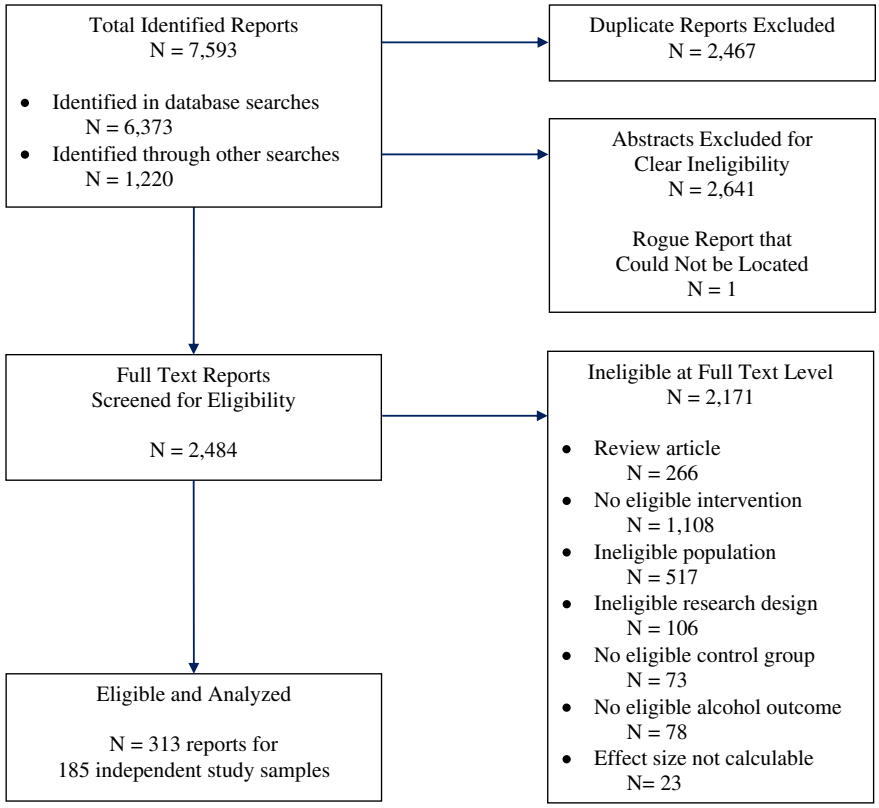


Fig. 1. Study identification flow diagram.

screened as ineligible at the abstract level (see Fig. 1). We were unable to locate one unpublished manuscript (Leffingwell et al., 2007). Of the 2484 articles retrieved in full text, 2171 were deemed ineligible. Seventeen studies (in 23 reports) met all eligibility criteria but did not provide sufficient information to estimate effect sizes, and we were unsuccessful in obtaining that information from study authors.[7] The final meta-analysis reported here was based on 185 studies consisting of independent samples that were reported in 313 documents (see Appendix A for a list of references).

3.2. Study characteristics

Table 1 presents descriptive statistics for the studies included in the meta-analysis, shown separately for the 24 adolescent and 161 young adult samples. One-half of the adolescent studies were conducted in the United States versus 81% of the young adult studies. Most studies were randomized controlled trials, published in journal articles, had low to moderate attrition rates, and reported effects 20–24 weeks after the end of the intervention. The most common intervention modalities for adolescents were motivational enhancement/motivational interviewing (MET; 42%) and psycho-educational therapy (PET; 36%). For young adults, MET programs were common (35%) as were those focused on providing personalized feedback about drinking (28%). Most interventions were conducted in high school or university settings. The brief alcohol interventions were notably longer for adolescents than for young adults (100 versus 55 total minutes, 6 versus 3 days covered).

Table 1 also shows the weighted bivariate correlation between each study characteristic and the unadjusted alcohol consumption effect sizes for the adolescent and young adult samples respectively. Most study characteristics had small to modest bivariate correlations with the effect sizes, but several of the method-related characteristics exhibited correlations with effect size magnitude that were large enough to potentially influence results. Given this, all subsequent analyses used these method-related variables (study design, attrition, effect size estimation method, control group type, and pretest differences between groups) to adjust effect sizes to the standard methodological profile described earlier (see Section 2.4.4 and Appendix B).

3.3. Overall effects

3.3.1. Effectiveness for adolescents

The random-effects mean of 134 method-adjusted effect sizes estimated from 24 studies indicated that, on average, adolescents age 11–18 who received brief alcohol interventions reported significantly lower levels of alcohol consumption than those in control conditions ($\bar{g} = 0.27$, 95% CI [0.16, 0.38]). This mean effect size is significantly different from zero, and moderately large. For instance, we can translate this standardized effect size back into the metric of a commonly used measure of alcohol consumption: the number of days alcohol was consumed in the past 30 days using the Alcohol Timeline Followback (TLFB). Using the median from the control groups of studies using the

[7] There was evidence in some of these studies that either the intervention or control group fared better at posttest but effect sizes were still not estimable (e.g., means were presented without standard deviations). We conducted sensitivity analyses (not reported) that made the conservative assumption that all missing effect sizes were equal to zero. The results were substantively similar to those reported here; thus, we are fairly confident that omitting those studies did not bias our results.

Table 1
Characteristics of the studies, participant samples, and interventions in the meta-analysis, by age group.

	Adolescents $k = 24$ $n = 172$		Young adults $k = 161$ $n = 1691$	
	M (SD) % (n)	r_{ES}	M (SD) % (n)	r_{ES}
Study/design characteristics				
U.S. sample; % (n)[a]	50 (12)	−.03	81 (130)	−.03
Journal article publication; % (n)[a]	71 (17)	.09	75 (120)	.04
Randomized controlled trial; % (n)[a]	79 (19)	−.07	90 (145)	−.04
Attrition; M (SD)[b]	0.12 (0.14)	.16	0.23 (0.19)	−.09
Binary data used to estimate ES; % (n)[b]	37 (63)	−.21	7 (117)	−.09
Pretest adjusted ES; % (n)[b]	8 (14)	−.12	14 (241)	.13
Follow-up timing (weeks); M (SD)[b]	24 (14)	−.23	20 (24)	−.14
No treatment control condition; % (n)[b]	39 (67)	.32	59 (990)	.15
Pretest effect size; M (SD)[b]	−0.00 (0.59)	.15	−0.07 (0.37)	.26
Participant characteristics				
Average age; M (SD)[c]	15 (1.5)	.07	20 (1.7)	.14
Percent male composition; M (SD)[c]	.53 (0.17)	.22	0.47 (0.19)	−.00
Percent White composition; M (SD)[c]	0.49 (0.29)	−.33	0.76 (0.18)	.01
High-risk screened sample; % (n)[a]	29 (7)	.19	52 (83)	−.05
Intervention modality; % (n)[c]				
21st birthday card	0	–	8 (20)	−.06
Cognitive behavioral therapy (CBT)	3 (1)	−.03	5 (12)	.00
Motivational enhancement therapy (MET)	42 (14)	.25	35 (91)	.13
CBT + MET	12 (4)	−.14	4 (10)	−.05
Expectancy challenge	3 (1)	−.02	6 (16)	.11
Feedback/information only	0 (0)	–	28 (73)	−.09
Psycho-educational therapy (PET)	36 (12)	−.14	12 (31)	−.09
Intervention Site; % (n)[c]				
Primary care/student health center	9 (3)	−.29	5 (13)	.05
School/university	82 (27)	.19	55 (143)	.09
Self-administered	0 (0)	–	34 (87)	−.12
Emergency room	9 (3)	−.01	2 (6)	−.05
Intervention Format; % (n)[c]				
Subject alone—computerized	6 (2)	−.29	13 (33)	−.04
Subject alone—non-computerized	6 (2)	−.09	27 (70)	−.00
Individual	39 (13)	.26	32 (82)	−.00
Group	42 (14)	−.05	28 (72)	.03
Family	3 (1)	−.06	0 (0)	–
Intervention length; M (SD)[c]				
Total contact time (minutes)	99.9 (80.3)	.19	54.6 (57.2)	.13
Total number of sessions	1.8 (1.2)	.06	1.3 (1.0)	.12
Total days covered	6.2 (9.4)	.03	2.9 (5.4)	.14

Notes. Means and standard deviations shown for continuous measures; percentages and counts shown for dichotomous measures. k = number of studies; n = number of effect sizes. r_{ES} = bivariate correlation with alcohol consumption effect sizes.
[a] Estimates calculated at study level.
[b] Estimates calculated at effect size level.
[c] Estimates calculated at intervention group level.

TLFB outcome measure, this mean effect size of 0.27 translates into a reduction of 1.3 drinking days per month, with adolescents in the intervention groups consuming alcohol an average of 4.9 days in the past month, versus 6.2 days in the past month for those in control groups.

Only eight studies with adolescent samples also reported an alcohol-related problem outcome (e.g., scores on the Rutgers Alcohol Problem Index). Using 38 effect sizes from those 8 studies, the brief alcohol interventions were associated with significantly lower levels of alcohol-related problems among adolescents ($\bar{g}$ = 0.19, 95% CI [0.06, 0.31]). Using Cohen (1988), this mean effect size of 0.19 indicates that brief alcohol interventions produced an 8-percentile improvement on alcohol-related problem outcomes for adolescents, relative to control group participants.

3.3.2. Effectiveness for young adults

One hundred fifty-six studies contributed 1312 method-adjusted effect sizes measuring effects on alcohol consumption among young adults age 19–30. Young adults who received brief alcohol interventions reported significantly lower levels of alcohol consumption than those in control conditions ($\bar{g}$ = 0.17, 95% CI [0.13, 0.20]). This effect is modest in practical terms, equivalent to a 0.8 reduction in drinking days per month, from 6.2 to 5.4 days in the past month (using the alcohol TLFB). However, there was no evidence that this mean effect for young adults was significantly different from that observed for adolescents when tested with a meta-regression model that included a dummy variable for adolescent vs. young adult sample (b = 0.09, 95% CI [−0.01, 0.20]).

Ninety-six studies with young adult samples contributed 379 method-adjusted effect sizes for alcohol-related problem outcomes. These showed a significant beneficial effect on alcohol-related problems ($\bar{g}$ = 0.11, 95% CI [0.08, 0.14]). Using the U3 index, this mean effect size of 0.11 indicates that brief alcohol interventions produced an 4-percentile improvement on alcohol-related problem outcomes for young adults, relative to control group participants. This mean effect size for alcohol-related problems among young adult samples was not significantly different from that observed in the adolescent samples (b = 0.08, 95% CI [−0.03, 0.20]).

3.4. Variability in effects

The between-study variance estimates (τ^2) of the alcohol consumption effect sizes were 0.04 (Q = 46.92) for adolescents and 0.02 (Q = 334.74) for young adults. For effect sizes on the alcohol-related problem outcomes the τ^2 estimates were 0.00 (Q = 7.20) for adolescents and 0.00 (Q = 108.25) for young adults. Despite the homogeneity of these effect sizes evidenced by the small τ^2 and Q values, we examined whether any of the moderators of interest were related the effects of the brief alcohol interventions given our original intent to explore when, for whom, and for how long brief alcohol interventions are effective for adolescents and young adults.

3.4.1. Variability across intervention characteristics

As is evident in the descriptive summary presented in Table 1, the brief interventions represented in this sample of studies are quite varied in modality, delivery site, format, length, and intervention components. An important question, therefore, is whether any of these characteristics are associated with differential effects on alcohol consumption or alcohol-related problems. Table 2 shows the corresponding predicted mean effect sizes split by age group, outcome type, and intervention characteristic.[8]

[8] Table 2 only includes results for intervention categories with at least two studies (see Table 1). The adjusted effect sizes shown in Table 2 were estimated from meta-regression models that predicted the method-adjusted effect sizes, but also additionally controlled for participant age, gender, race, and baseline alcohol severity. Statistical tests for differences between intervention characteristic categories were estimated using dummy variables in the meta-regression models for each family of intervention characteristic, in turn (i.e., the models did not control for the other intervention characteristics shown in Table 2). There was no evidence of significant differences in effect sizes across any of the intervention characteristic categories shown in Table 2.

Table 2
Predicted mean covariate-adjusted effect sizes and 95% confidence intervals split by intervention characteristic, by age group and outcome type.

	Adolescents		Young adults	
	Alcohol consumption	Alcohol related problems	Alcohol consumption	Alcohol related problems
Modality				
21st birthday card	–	–	0.07 [−0.02, 0.17]$_{8, 36}$	0.04 [−0.07, 0.14]$_{2, 9}$
CBT	–	–	0.13 [0.04, 0.22]$_{8, 67}$	0.10 [0.01, 0.20]$_{4, 15}$
MET	0.33 [0.23, 0.42]$_{12, 71}$	0.30 [0.19, 0.40]$_{4, 26}$	0.20 [0.14, 0.26]$_{68, 623}$	0.17 [0.11, 0.24]$_{52, 182}$
MET/CBT	0.16 [0.03, 0.28]$_{3, 18}$	0.13 [.01, .24]$_{3, 11}$	0.03 [−0.10, 0.17]$_{6, 52}$	0.00 [−0.12, 0.13]$_{6, 20}$
Expectancy challenge	–	–	0.36 [0.10, 0.62]$_{12, 46}$	0.34 [0.08, 0.59]$_{5, 9}$
Feedback/information	–	–	0.20 [0.13, 0.26]$_{54, 372}$	0.17 [0.10, 0.24]$_{30, 99}$
PET	0.28 [0.18, 0.39]$_{8, 39}$	–	0.16 [0.09, 0.23]$_{20, 104}$	0.13 [0.06, 0.20]$_{12, 44}$
Delivery site				
Primary care/health center	0.29 [0.11, 0.48]$_{2, 19}$	–	0.17 [0.05, 0.28]$_{10, 97}$	0.12 [0.01, 0.24]$_{9, 51}$
School/university	0.29 [0.19, 0.39]$_{20, 94}$	0.25 [0.15, 0.35]$_{6, 26}$	0.21 [0.14, 0.28]$_{87, 751}$	0.17 [0.11, 0.23]$_{57, 192}$
Emergency room	0.25 [0.11, 0.38]$_{2, 21}$	0.21 [0.07, 0.34]$_{2, 12}$	0.11 [0.04, 0.19]$_{4, 37}$	0.07 [−0.02, 0.16]$_{3, 19}$
Self-administered	–	–	0.21 [0.15, 0.27]$_{57, 377}$	0.17 [0.10, 0.23]$_{31, 109}$
Format				
Subject alone—comp.	–	–	0.23 [0.14, 0.33]$_{18, 79}$	0.19 [0.09, 0.28]$_{8, 18}$
Subject alone—not comp.	0.29 [0.19, 0.38]$_{2, 10}$	0.24 [0.14, 0.35]$_{2, 7}$	0.20 [0.15, 0.26]$_{54, 383}$	0.16 [0.10, 0.22]$_{30, 108}$
Individual	0.28 [0.19, 0.38]$_{12, 71}$	0.24 [0.13, 0.34]$_{5, 29}$	0.20 [0.14, 0.26]$_{57, 560}$	0.16 [0.09, 0.22]$_{46, 192}$
Group	0.29 [0.17, 0.41]$_{10, 31}$	0.25 [0.13, 0.36]$_{2, 2}$	0.21 [0.11, 0.31]$_{43, 286}$	0.16 [0.08, 0.25]$_{22, 60}$
Length				
Single session, <5 minutes	–	–	0.30 [0.09, 0.51]$_{3, 14}$	0.25 [0.05, 0.46]$_{2, 8}$
Single session, 5–15 minutes	0.27 [0.15, 0.38]$_{2, 19}$	–	0.19 [0.13, 0.26]$_{55, 330}$	0.15 [0.08, 0.23]$_{23, 71}$
Single session, > 15 minutes	0.29 [0.19, 0.39]$_{16, 78}$	0.25 [0.14, 0.36]$_{5, 18}$	0.22 [0.15, 0.28]$_{82, 694}$	0.17 [0.11, 0.24]$_{57, 195}$
Multi-session	0.25 [0.14, 0.35]$_{8, 29}$	0.20 [0.09, 0.31]$_{3, 20}$	0.17 [0.10, 0.25]$_{30, 274}$	0.13 [0.06, 0.20]$_{23, 105}$

Notes: Results of statistical significance tests provided no evidence of differences in mean effect sizes across intervention characteristic categories. Effect sizes adjusted for study method and participant characteristics. 95% confidence intervals estimated with robust standard errors. CBT—cognitive behavioral therapy. MET—motivational enhancement therapy. PET—psychoeducational therapy. MET/CBT—motivational enhancement therapy and cognitive behavioral therapy.
– indicates results not available (fewer than two studies in cell). Subscripts indicate k (number of studies), n (number of effect sizes).

As shown in the top section of Table 2, effects were relatively similar across different intervention modalities for both adolescents and young adults. The notable exceptions to this were 21st birthday card interventions and interventions that combined motivational enhancement and cognitive behavioral therapy components (MET/CBT), neither of which showed evidence of significant beneficial effects in the young adult samples on which they were tested. One limitation of these analyses is the low statistical power for mean effect sizes estimated from such small numbers of studies. Given this, we also estimated mean effect sizes aggregated across the adolescent and young adult samples and outcome types to increase the number of studies and effect sizes contributing to the analysis. Those results were similar, again providing no evidence that either 21st birthday card interventions ($\bar{g} = 0.07$, 95% CI [−0.03, 0.18]) or MET/CBT ($\bar{g} = 0.08$, 95% CI [−0.05, 0.20]) had significant effects.

The mean effects were also quite similar for interventions provided in most of the different delivery sites represented (school/university, primary health care clinics, or self-administered). However, interventions conducted in emergency room settings did not show significant effects on alcohol-related problem outcomes among young adults. Again, because few studies were conducted in emergency room settings (see Table 1), we aggregated results across the age groups. With that larger sample, the mean effect size for alcohol-related problems was still not statistically significant ($\bar{g} = 0.11$, 95% CI [−0.01, 0.22]).

The results shown in Table 2 also indicate that the effects of brief alcohol interventions were similar across different formats (computerized, non-computerized, one-on-one, group). Finally, results in the bottom of Table 2 show that the effects were substantially similar for different amounts of contact time in the adolescent samples, but were more variable across contact time in the young adult samples. Single-session interventions delivered with less than 5 minutes of total contact time (e.g., providing youth with personalized feedback reports to read) showed the largest mean effect on both alcohol consumption ($\bar{g} = 0.30$, 95% CI [0.09, 0.51]) and alcohol-related problem outcomes ($\bar{g} = 0.25$, 95% CI [0.05, 0.46]). However, only a few studies contributed effect sizes to this category, so these results must be interpreted cautiously.

Table 3 shows mean covariate-adjusted effect sizes for interventions with and without specific therapeutic components (BAC information, alcohol caloric information, decisional balance exercise, general education/information, personalized feedback, gender-specific feedback, goal-setting, providing money/cost information about drinking, or local/national drinking norm referencing).[9] Overall, the effects were relatively similar across different intervention components, but there were some notable exceptions. For adolescents, brief alcohol interventions had significantly larger effects on alcohol consumption if they included decisional balance or goal-setting exercises. For 42 effect sizes from 8 adolescent studies with interventions that included both decisional balance and goal-setting exercises, the estimated mean covariate-adjusted effect size was 0.50 (95% CI [0.18, 0.82], $\tau^2 = 0.10$, $Q = 22.83$), indicating a sizeable effect on alcohol consumption outcomes among adolescents, equivalent to a reduction from 6.2 drinking days in the past month among control participants (using the alcohol TLFB),

[9] The predicted effect sizes shown in Table 3 were estimated from meta-regression models that predicted the method-adjusted effect sizes, but also additionally controlled for participant age, gender, race, and baseline alcohol consumption. Statistical tests for differences between intervention component categories were estimated using dummy variables in the meta-regression models for each intervention components, in turn (i.e., the models did not control for the other intervention components shown in Table 3 due to high inter-correlations between the various intervention components).

Table 3
Predicted mean covariate-adjusted effect sizes and 95% confidence intervals split by presence/absence of intervention components, by age group and outcome type.

	Adolescents		Young adults	
	Alcohol consumption	Alcohol related problems	Alcohol consumption	Alcohol related problems
BAC information				
Yes	0.36 [0.07, 0.65]$_{4, 21}$	–	0.20 [0.14, 0.26]$_{63, 650}$	0.14 [0.08, 0.20]$_{51, 204}$
No	0.43 [0.25, 0.61]$_{20, 113}$	–	0.19 [0.12, 0.26]$_{100, 662}$	0.09 [0.03, 0.15]$_{51, 175}$
Caloric information				
Yes	–	–	0.19 [0.11, 0.27]$_{19, 177}$	0.06 [−0.03, 0.15]$_{13, 48}$
No	–	–	0.19 [0.13, 0.26]$_{141, 1135}$	0.11 [0.06, 0.16]$_{86, 331}$
Decisional balance				
Yes	**0.52 [0.33, 0.71]**$_{9, 45}$	0.46 [0.32, 0.61]$_{5, 32}$	0.26 [0.14, 0.38]$_{32, 284}$	0.15 [0.07, 0.23]$_{26, 77}$
No	**0.30 [0.14, 0.46]**$_{15, 89}$	0.29 [−0.18, 0.75]$_{3, 6}$	0.17 [0.12, 0.22]$_{129, 1028}$	0.10 [0.05, 0.16]$_{74, 302}$
Education/information				
Yes	0.35 [0.15, 0.55]$_{20, 122}$	0.36 [−0.54, 1.26]$_{7, 28}$	0.18 [0.13, 0.24]$_{104, 872}$	0.11 [0.05, 0.16]$_{72, 274}$
No	0.50 [0.34, 0.66]$_{6, 12}$	0.49 [−.010, 1.07]$_{2, 10}$	0.21 [0.12, 0.30]$_{64, 440}$	0.12 [0.05, 0.20]$_{31, 105}$
Feedback, personalized				
Yes	0.39 [0.21, 0.57]$_{12, 76}$	**1.34 [1.17, 1.50]**$_{6, 19}$	0.19 [0.13, 0.26]$_{110, 1030}$	0.12 [0.07, 0.18]$_{76, 307}$
No	0.44 [0.24, 0.64]$_{13, 58}$	**0.03 [−0.04, 0.10]**$_{2, 19}$	0.20 [0.11, 0.29]$_{55, 282}$	0.07 [−0.01, 0.15]$_{27, 72}$
Feedback, gender-specific				
Yes	0.41 [0.13, 0.69]$_{2, 21}$	0.84 [0.52, 1.16]$_{2, 12}$	0.19 [0.11, 0.27]$_{36, 348}$	0.10 [0.02, 0.17]$_{23, 75}$
No	0.41 [0.24, 0.59]$_{22, 113}$	0.38 [0.18, 0.59]$_{6, 26}$	0.19 [0.12, 0.27]$_{128, 964}$	0.12 [0.06, 0.18]$_{77, 304}$
Goal-setting				
Yes	**0.48 [0.30, 0.66]**$_{10, 53}$	0.45 [0.33, 0.57]$_{6, 33}$	0.22 [0.13, 0.31]$_{38, 349}$	0.15 [0.08, 0.21]$_{27, 116}$
No	**0.31 [0.16, 0.46]**$_{14, 81}$	0.21 [−0.21, 0.63]$_{2, 5}$	0.18 [0.12, 0.24]$_{125, 963}$	0.09 [0.03, 0.16]$_{74, 263}$
Money/cost information				
Yes	–	–	0.20 [0.13, 0.27]$_{24, 207}$	0.10 [0.03, 0.18]$_{18, 61}$
No	–	–	0.19 [0.13, 0.26]$_{136, 1105}$	0.11 [0.06, 0.17]$_{82, 318}$
Norm referencing				
Yes	0.43 [0.29, 0.57]$_{8, 57}$	**0.88 [0.71, 1.06]**$_{3, 13}$	0.18 [0.13, 0.24]$_{111, 1015}$	0.11 [0.06, 0.17]$_{76, 306}$
No	0.39 [0.13, 0.65]$_{16, 77}$	**0.34 [0.31, 0.36]**$_{5, 25}$	0.22 [0.11, 0.32]$_{61, 297}$	0.10 [0.01, 0.19]$_{28, 73}$

Notes: Results in bold indicate significant differences between studies with versus without intervention component (within age and outcome categories). Effect sizes adjusted for study method and participant characteristics. 95% confidence intervals estimated with robust standard errors. BAC—blood alcohol concentration. – indicates results not available (fewer than two studies in cell). Subscripts indicate *k* (number of studies), *n* (number of effect sizes).

to 3.9 drinking days among intervention participants. Interventions that included personalized feedback and norm referencing also had significantly larger effects on alcohol-related problems for adolescents, although those results should be interpreted cautiously given the small number of studies contributing effect sizes. Among young adults, however, there was no evidence that any of the intervention components shown were associated with significantly larger or smaller effects on alcohol consumption or alcohol-related problem outcomes, as shown in the right panel of Table 3.

3.4.2. Variability across participants

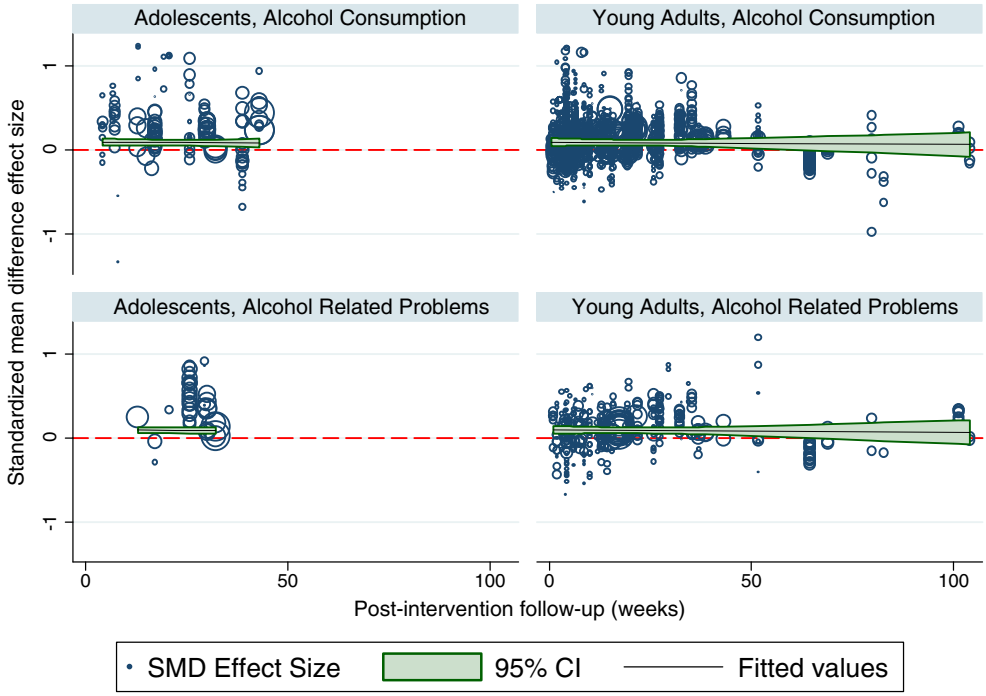
We estimated meta-regression models to examine whether the following characteristics of the participant samples were associated with the magnitude of effects: race composition, average age, gender composition, or whether samples were selected based on high-risk drinking at baseline screening. Results indicated that alcohol consumption outcomes were significantly better for the adolescent samples with fewer White participants, i.e., a greater proportion of minority participants ($b = -0.66$, 95% CI [−0.96, −0.36]). However, of the 24 studies in this analysis, this effect was driven by 4 studies (contributing 12 effect sizes) composed entirely of Hispanic or Mi'kmap Aboriginal Canadian youth who experienced notably larger benefits from the brief interventions ($\bar{g} = 1.03$, 95% CI [0.68, 1.38]). When these four studies were excluded, the proportion of White youth in the intervention group was no longer significantly related to the effect sizes for the alcohol consumption outcomes.

Within the adolescent and young adult samples, the age of the participants was not associated with the intervention effects for alcohol consumption outcomes. However, among adolescent samples, the effects on alcohol-related problems were significantly smaller in older samples ($b = -0.11$, 95% CI [−0.17, −0.05]). There was no evidence that the gender composition of the adolescent samples (percentage of males) was associated with the effects on alcohol consumption ($b = -0.06$, 95% CI [−0.79, 0.67]) or on alcohol-related problems ($b = 0.66$, 95% CI [−0.20, 1.53]); nor for young adults' alcohol consumption ($b = 0.06$, 95% CI [−0.21, 0.33]) or alcohol-related problems ($b = -0.03$, 95% CI [−0.21, 0.16]). Very few studies were composed of all male or female samples, but restricting analyses to those studies also showed little differential effectiveness across gender. For instance, the mean method-adjusted effect size for alcohol consumption for young adult female samples was 0.28 (95% CI [0.14, 0.41]) versus 0.34 (95% CI [0.05, 0.63)] for young adult male samples.

Finally, there was no consistent evidence that brief alcohol interventions were differentially effective for high-risk screened participants. The one exception to this was for alcohol-related problem outcomes among adolescent samples, whereby high-risk screened participants experienced larger beneficial intervention effects than those that were not selected for interventions based on their baseline alcohol consumption ($b = 0.49$, 95% CI [0.27, 0.72]). Adolescents already exhibiting heavy or hazardous alcohol consumption patterns may exhibit larger intervention effects given that they have more room for improvement after the intervention (relative to universal prevention programs where most participants may consume alcohol relatively infrequently).

3.4.3. Persistence of effects over time

For the adolescent samples, longer intervals between the brief interventions and follow-up measures of the outcomes were not associated with effects on alcohol consumption ($b = -0.005$, 95% CI [−0.01, 0.002]) nor with the effects on alcohol-related problems ($b = 0.0005$, 95% CI [−0.01, 0.02]). It is important to note, however,



Notes: Effect sizes adjusted for study method characteristics and shown proportionate to random-effects inverse variance weights. Fitted values and confidence intervals from meta-regression with robust variance estimates. Dashed line shown at null value of zero.

Fig. 2. Scatter plots of method-adjusted effect sizes and posttest follow-up timing, by age group and outcome type.

that none of the adolescent studies reported follow-up results longer than 1-year after the end of the intervention. Several young adult samples included longer follow-up periods, up to 4 years post-intervention. For alcohol-related problems, the follow-up period for those young adult samples was not related to the effect sizes ($b = -0.001$, 95% CI [−0.002, 0.001]). However, longer follow-up intervals (in weeks) were associated with smaller effects on alcohol consumption among the young adults ($b = -0.003$, 95% CI [−0.004, −0.001]). For example, the mean effect size for alcohol consumption among young adults was 0.22 at 1-week follow-up ($k = 15$, $n = 66$, 95% CI [0.17, 0.27]) and 0.08 at 24-month follow-up ($k = 3$, $n = 24$, 95% CI [−0.11, 0.28]).

Fig. 2 shows results from the meta-regression models that examined the persistence of effects over time, split by age group and outcome type. Each effect size is shown proportionate to its weight in the analysis (larger circles represent higher weight, larger sample size studies) and the fitted regression line with its 95% confidence interval is imposed on the graph. A dashed line is shown at the null effect size value of zero (i.e., indicating no difference between the outcomes for the brief intervention and control group). As shown in the upper right panel of Fig. 2, the effects of brief alcohol interventions were attenuated to non-significance at the 2-year follow-up point for alcohol consumption among young adults. Although there was no observed attenuation of effects over time for adolescents, only relatively short follow-up intervals were used in the adolescent studies. If only follow-up intervals of less than 1 year post-intervention are examined for the alcohol consumption effects in the young adult samples (equivalent to the follow-up intervals in the adolescent samples), the results also indicate no significant relationship between the effect sizes and the length of follow-up intervals. The available evidence thus indicates no significant decrease in the effects of the brief interventions on alcohol consumption for up to 1 year after the intervention.

4. Discussion

This meta-analysis synthesized findings from 185 experimental and quasi-experimental independent study samples that examined the effects of brief alcohol interventions on alcohol-related outcomes for adolescents and young adults who were not seeking treatment. Overall, brief alcohol interventions with up to 5 hours of total contact time were associated with statistically significant post-intervention reductions in alcohol consumption and alcohol-related problem outcomes. These effects were modest for adolescents—equivalent to 0.27 and 0.19 standard deviation reductions in alcohol use and alcohol-related problems respectively. Although smaller in magnitude, the benefits for young adults were also positive and significant—0.17 and 0.11 standard deviation reductions in alcohol use and alcohol-related problems respectively. Overall, these results indicated that youth receiving brief alcohol interventions reduced their alcohol consumption between 1.0 and 1.3 drinking days per month (relative to control participants, who reported an average of 6.2 drinking days per month at baseline).

These effect estimates are of the same order of magnitude as those reported in previous meta-analyses examining similar interventions for youth (e.g., Jensen et al., 2011; Scott-Sheldon et al., 2009; Tait & Hulse, 2003). The primary strengths of this meta-analysis are the large number of studies included and the application of statistical techniques that permit inclusion of multiple effect sizes from each study. These combined to produce a rich data set that allowed exploration of the variability in effects rather than being restricted to estimating overall mean effects. The results yielded several findings with implications for both research and practice.

One such finding is that, despite their brevity, the effects of brief alcohol interventions persist for up to 1 year after the end of the interventions. Few studies have followed samples longer than that, but those that do indicate that the effects may dissipate by 2 years after the interventions (at least among young adults). Furthermore, although the effects were generally similar across different participant demographic profiles and various intervention formats, certain treatment modalities and therapeutic components were associated with larger or smaller effects. In particular, there were no statistically significant effects for the MET/CBT combinations and 21st birthday card interventions evaluated in these research studies. The 21st birthday card interventions typically involved mailing college students a postcard or flyer with a birthday message and safe drinking tips or information about the dangers of binge drinking. One possible explanation for the lack of effects from this type of intervention could be poor implementation fidelity—many of the studies did not monitor the number of students who actually received the birthday card, nor how many read it. The source of the null findings for the MET/CBT interventions, on the other hand, is especially puzzling in light of the positive effects of MET and CBT by themselves. Perhaps combining these two components in a circumscribed time to provide a brief intervention dilutes the beneficial effects each has alone (see Bell, Marcus, & Goodlad, 2013, for a recent meta-analysis of component treatment studies).

Although the primary goal of this meta-analysis was to identify any features of brief interventions that are associated with differential effects, another important contribution for any meta-analysis is to guide future research. This can be accomplished not only by highlighting gaps in the evidence, but also by providing suggestions for new intervention strategies that build upon the characteristics most strongly associated with positive outcomes that can be identified in the currently available research. We therefore have used the results from this meta-analysis to create a hypothetical profile of characteristics associated with the most and least effective brief alcohol interventions for youth. Based on predicted values from the regression models developed during our analysis, the interventions for adolescents that would potentially yield the largest benefit on alcohol consumption would use MET in a single session of more than 15 minutes delivered on a high school campus. That intervention would include decisional balance, goal-setting, and norm referencing as therapeutic components and would not include BAC information, basic education/information, or personalized feedback. Our regression model predicts that this hypothetical intervention profile would produce a mean effect size for alcohol outcomes of 0.39, equivalent to a reduction from 6.2 to 4.4 drinking days in the past month (using the TLFB for alcohol consumption). This is in contrast to the hypothetical "worst" intervention profile for adolescents, which the regression model indicates would be MET/CBT conducted in multiple sessions, delivered individually in an emergency room, and with therapeutic components that include BAC information, basic education, and personalized feedback, and which do not include decisional balance, goal-setting or norm referencing. This worst-case hypothetical profile yields a predicted effect size for alcohol consumption among adolescents of 0.10, equivalent to a reduction from 6.2 to 5.7 drinking days in the past month.

Using the same regression modeling technique, we projected the hypothetical intervention profiles for young adults predicted to produce the largest and smallest effects based on the existing research. Those results suggest that the strongest program for young adults would be a self-administered computerized expectancy challenge intervention conducted on a university campus. It would include BAC information, decisional balance, goal-setting, and money/cost information as therapeutic components and would not include basic education/information or norm referencing. This profile predicts that the intervention would produce a mean effect size of 0.59, equivalent to a reduction from 6.2 to 3.5 drinking days in the past month. In contrast, the worst-case scenario profile for young adults is a self-administered, multi-session MET/CBT intervention delivered in an emergency room. That profile predicts a negative, but non-significant mean effect size of −0.14, equivalent to an increase from 6.2 to 6.8 drinking days in the past month. Of course, it is critical to note that these best and worst case profiles are simulated and do not necessarily represent any actual interventions represented in the available research. Nor can we assume that the effects of brief interventions with these profiles would actually produce the predicted effects. Nonetheless, this exercise does suggest some possible brief intervention configurations that may be worth investigating in future research.

One limitation of the current meta-analysis was its necessary reliance on the information that is reported by the authors of the primary research studies that were included. The limited reporting of the details of the respective interventions inherently limits the range and depth of characteristics available for analyzing the variability in brief alcohol intervention effects. For instance, the personnel used to deliver brief alcohol interventions vary widely in terms of their background training, education level, and therapeutic expertise; e.g., undergraduate peers trained by research staff (Mayhew et al., 2010), trained professional clinicians (Borsari et al., 2012), and computerized self-administered interventions (Butler & Correia, 2009). Unfortunately, such characteristics of the personnel delivering the interventions are so inconsistently reported that we were unable to examine their relationship to the effectiveness of the brief alcohol interventions. As a practical matter, however, knowing about any influence of the characteristics of the delivery personnel on the intervention effects would be important for anyone implementing a brief alcohol intervention.

Another limitation, stemming in this case from the nature of the research that has actually been conducted rather than insufficient reporting, was our inability to examine the long-term persistence of effects, given that few primary studies reported follow-ups beyond 12-months (particularly in the adolescent samples). Although expecting large, persistent effects may be overly ambitious for brief interventions that are often delivered in a single-session, understanding the persistence of these effects is nonetheless important. Even modest reductions in alcohol consumption may have lasting benefits for youth during these critical developmental periods where progression to alcohol use disorders begins.

Despite these limitations, findings from the current meta-analysis provide compelling evidence that brief alcohol interventions can yield beneficial effects on alcohol consumption and alcohol-related problems in non-treatment seeking populations of adolescents and young adults. Although the magnitude of the effects is generally modest, the brevity and low cost of these interventions allow them to be applied on a relatively large scale where they may add incrementally to the influences that deter risky drinking among youth. To optimize the beneficial effects, providers may want to be guided by the factors this study found to be associated with the most positive effects on alcohol consumption. In particular, these research results indicate that the site, modality, and delivery characteristics of brief alcohol interventions may influence their effectiveness with adolescent and young adult populations.

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Appendix A. References to study reports providing effect sizes included in the meta-analysis

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Appendix B. Unstandardized coefficients and 95% confidence intervals from meta-regression model used to create method-adjusted effect sizes

	B		95% CI
Main effects			
Adolescent sample (vs. young adult)	−0.02		[−0.22, 0.19]
Alcohol consumption outcome (vs. alcohol-related problem)	0.05	*	[0.01, 0.09]
Randomized controlled trial (vs. quasi-experimental)	−0.03		[−0.16, 0.09]
Attrition	−0.16	*	[−0.29, −0.04]
Effect size estimated from binary data (vs. not)	−0.16	*	[−0.27, −0.05]
Effect size estimated with pretest adjusted data (vs. not)	0.06		[−0.02, 0.14]
No treatment control condition (vs. treatment as usual)	0.05		[−0.00, 0.11]
Pretest effect size	0.27	*	[0.10, 0.44]
Interactions with age of sample			
Adolescent sample × attrition	0.38		[−0.57, 1.34]
Adolescent sample × pretest adjusted effect size data	−0.19		[−0.39, 0.02]
Intercept	0.14	*	[0.01, 0.28]
Residual between-studies variance τ^2	0.02		
Number of studies (*k*)	185		
Number of effect sizes (*n*)	1863		

Notes: Unstandardized coefficients and 95% confidence intervals estimated with meta-regression model using robust standard errors. Multiplicative interaction terms were used for all study method moderators that had bivariate correlations with alcohol consumption effect sizes that were in opposite directions (positive/negative) for adolescent and young adult samples (see Table 1). *$p < .05$.

Appendix C. Bivariate correlations between study, participant, and intervention characteristics

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.
1. U.S. sample	1.0																						
2. RCT	.13	1.0																					
3. Follow-up timing	−.07	.00	1.0																				
4. No treatment control	−.09	−.05	.00	1.0																			
5. Cluster assignment	.04	−.17	−.04	−.10	1.0																		
6. High-risk sample	.02	−.02	.19	.17	−.45	1.0																	
7. Average age	−.16	−.06	−.11	.12	−.13	.20	1.0																
8. Percent male	−.15	−.16	.16	.05	−.12	.13	.03	1.0															
9. Percent White	.09	.05	.07	.25	.03	.16	.05	.06	1.0														
10. 21st birthday card	.07	.04	−.13	−.08	−.04	−.18	.11	−.08	.02	1.0													
11. CBT	.00	−.05	.01	.09	.18	−.20	.03	.07	.04	−.03	1.0												
12. MET	.04	−.05	.10	.06	−.04	.32	−.06	.02	.05	−.15	−.21	1.0											
13. CBT + MET	.09	.01	.01	.00	−.07	−.12	−.08	−.02	−.08	−.04	−.05	−.23	1.0										
14. Expectancy challenge	.04	−.06	−.10	−.05	.07	−.10	.07	.15	.03	−.03	−.04	−.17	−.04	1.0									
15. Feedback only	−.20	.14	.03	.02	−.18	.13	.19	−.03	−.04	−.09	−.13	−.56	−.14	−.10	1.0								
16. PET	.09	−.09	−.12	−.10	.22	−.30	−.22	−.05	−.03	−.05	−.07	−.32	−.08	−.06	−.19	1.0							
17. Primary care	−.20	.01	.12	−.26	−.09	.13	.10	.03	.10	−.05	−.07	−.02	.02	−.06	.03	.06	1.0						
18. School/univ.	.19	−.19	−.03	.28	.19	.01	−.09	−.04	.15	−.18	.13	.37	.06	.15	−.45	−.12	−.36	1.0					
19. Self-admin.	−.02	.16	−.08	−.04	−.08	−.16	.13	−.10	−.07	.26	−.06	−.49	−.14	−.10	.52	.16	−.19	−.68	1.0				
20. Emergency room	.05	.06	.10	−.25	−.09	.18	−.11	.11	−.30	−.04	−.05	.14	.15	−.04	−.13	−.08	−.07	−.26	−.13	1.0			
21. Subject alone (comp.)	.02	.07	−.10	−.06	−.08	−.13	.04	−.09	.01	.59	.01	−.21	−.06	−.01	.01	.03	.05	−.21	.25	−.06	1.0		
22. Subject alone (non-comp.)	−.15	.17	−.13	.04	−.10	−.07	.10	−.02	−.03	−.08	−.10	−.51	−.06	−.02	.60	.16	.02	−.55	.62	−.09	−.16	1.0	
23. Individual	.12	−.01	.25	.08	−.35	.51	−.03	.16	.08	−.14	−.16	.68	−.09	−.16	−.35	−.24	.03	.30	−.43	.20	−.23	−.56	1.0
24. Group	.03	−.18	−.12	−.10	.56	−.45	−.08	−.13	−.08	−.08	.31	−.15	.21	.23	−.22	.09	−.08	.35	−.30	−.11	−.13	−.31	−.46

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