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Personalized Boosters after a Computerized Intervention Targeting College Drinking: A Randomized Controlled Trial

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Abstract

Background—Problematic drinking among emerging adult college students is extensive. Computer-delivered interventions (CDIs) have strong appeal because they can be quickly delivered to large numbers of students. Although they are efficacious in the short-term, CDIs are not as efficacious as in-person interventions longer term. The current study examined the utility of emailed boosters containing personalized feedback after a CDI to enhance and extend reductions among emerging adult college drinkers. Sex and age were explored as potential moderators.

Methods—Participants were 537 college students (67.4 % female) aged 18 to 24 years (M age = 19.65, SD = 1.67) who consumed at least one alcoholic drink in the past two weeks. They were randomly assigned to: CDI-only, CDI + booster email, or an assessment-only control condition, and were assessed up to 9 months post-intervention. A booster email with personalized feedback was sent to the CDI + booster email group two weeks after completion of the CDI.

Results—Moderation findings for age revealed that the booster may be an effective means to strengthen and extend intervention effects for emerging adults who are of legal drinking age. However, effects were negligible for underage drinkers. Although the booster effect for the overall sample demonstrated a trend in the expected direction, it failed to reach significance. Booster effects were not significantly moderated by sex. Intervention effects were not moderated by either age or sex.

Conclusions—The present investigation contributes to a limited body of research on boosters to augment main intervention effects in college drinkers. Our study demonstrated that a brief CDI plus a simple email booster with personalized feedback resulted in significant reductions in drinking outcomes for emerging adults of legal drinking age. Efforts to further develop and refine intervention booster strategies represent a promising future direction to minimize harmful drinking among college students.

Keywords

brief alcohol intervention; personalized feedback; boosters; college students; emerging adults

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Problematic drinking among college students is extensive. In 2014, 10.6% of 18- to 25-year old respondents indicated engaging in heavy episodic drinking five or more times in the past month, indicating frequent heavy use (Hingson et al., 2017). Alcohol use among emerging adult college students is associated with unintentional injuries (Hingson et al., 2009) and academic problems such as missed classes and poor grades (Conway & DiPlacido, 2015; Wechsler et al., 1998). Given the pervasive nature of student drinking and the associated problems, many academic institutions administer interventions and preventative programs to curb heavy drinking and reduce associated harms. Computer-delivered programs, in particular, may have strong appeal.

Computer-delivered interventions (CDIs) targeting college drinking have been successful at reducing alcohol consumption compared to assessment-only control conditions (see Elliott et al., 2008). These interventions are popular among colleges as they are relatively inexpensive and can be quickly delivered. In addition, they are easily disseminated to large groups of individuals (e.g., all incoming students, student athletes, members of the Greek system), allowing institutions to adopt a proactive strategy. They can be used to prevent negative consequences before they occur rather than used as reactions to adverse events such as alcohol-related sanctions. A meta-analysis of computer-delivered alcohol interventions for college drinking found that CDIs reduced quantity and frequency of drinking relative to assessment-only controls, with small-to-medium within-group effect sizes at short- and long-term follow-ups (Carey et al., 2009a). However, these effect sizes are often smaller than interventions delivered in-person.

CDIs versus In-Person Interventions

Direct comparisons of in-person interventions to CDIs have been conducted among mandated students (Barnett et al., 2007; Carey et al., 2009b; Carey et al., 2011), heavy drinking students (Butler & Correia, 2009; Murphy et al., 2010; Walters et al., 2009), and general college drinkers (Donohue et al., 2004). In-person interventions and CDIs are fairly comparable in the short-term (up to 3 months; Barnett et al., 2007; Butler & Correia, 2009; Carey et al., 2009b, 2011; Donohue et al., 2004; Murphy et al., 2010; Walters et al., 2009). However, sub-populations may respond better to in-person interventions, such as female mandated students (Carey et al., 2009b, 2011), heavy-drinking African American students (Murphy et al., 2010), or higher risk students (Donohue et al., 2004). In addition, CDIs are not as efficacious as in-person interventions longer-term (Cadigan et al., 2015; Carey et al., 2011; Carey et al., 2012). Finally, a meta-analysis of college drinking interventions indicated that reductions in select indicators were stronger for in-person interventions for intermediate follow-ups (such as peak blood alcohol content [BAC]; Carey et al., 2012).

Boosters

The modest effects for CDIs relative to in-person interventions make students receiving computerized interventions ideal targets for additional materials to increase efficacy. Boosters, or brief, delayed follow-up sessions, may be administered to increase intervention efficacy or prolong the duration of intervention effects. The use of follow-up maintenance or

booster strategies has long been proposed for a spectrum of services/care from prevention efforts (Nation et al., 2003) to behavioral treatments (e.g., Whisman, 1990). Booster sessions serve as an opportunity to prolong the time of intervention contact to help sustain initial therapeutic gains (McKay, 2005). Boosters have been shown to improve the efficacy of select alcohol interventions, including reduced alcohol consumption (Field et al., 2014) as well as reduced alcohol-related problems and related injuries (Longabaugh et al., 2001) among individuals admitted at hospital emergency departments. Boosters also are associated with stronger reductions in drinking for heavy-drinking women (Connors & Walitzer, 2001; Walitzer & Connors, 2007). Finally, boosters have been suggested for at-risk young adult drinkers such as mandated college students in order to prolong brief alcohol intervention effects (White et al., 2007; White et al., 2008).

Boosters for college drinking interventions

While a number of brief intervention studies have incorporated boosters into its design and shown the intervention plus booster to be superior to controls (e. g., Curry et al., 2003; Donovan et al., 2015), few have explicitly examined the incremental impact of the booster component by evaluating the base intervention without the booster. Among studies systematically examining boosters after interventions targeting college drinkers, results are equivocal. Three investigations failed to support the use of boosters in college settings (Caudill et al., 2007; Barnett et al., 2007; Linowski et al., 2016). In a study of fraternity members, the addition of voluntary boosters to a skills-based training did not result in greater intervention effects (Caudill et al., 2007). Study authors suggested it could have been due to the burden of the boosters (in-person, 1.5 hours at 5- and 11-months), as fewer students were willing to attend the study condition with the booster. The other two studies were aimed at mandated students with boosters that were provided at 1-month (Barnett et al., 2007) or 3-months (Linowski et al., 2016), with neither providing support for incremental booster effects. However, through follow-up analysis of Barnett et al.'s (2007) study, it was found that problem severity moderated booster effect, such that among those with a higher number of alcohol-related problems at baseline, those with booster exposure reported fewer heavy drinking days than no booster exposure (Mastroleo et al., 2011).

Two studies supported booster use with voluntary college drinkers (Braitman & Henson, 2016; Neighbors et al., 2010). Neighbors et al. (2010) found that boosters, in the form of repeated gender-specific normative feedback delivered 4 times over 2 years, led to drinking reductions relative to attention controls, while a single administration of the same feedback did not produce reductions relative to attention controls. Braitman and Henson (2016) demonstrated that a simple personalized email booster delivered at 2-weeks following a CDI resulted in significant drinking changes at 4-weeks follow-up as compared to single session CDI only. Given the promising findings among adult populations (Connors & Walitzer, 2001; Longabaugh et al., 2001; Walitzer & Connors, 2007), further exploration of booster efficacy among college drinkers is warranted. Boosters may be particularly efficacious following CDIs as a strategy to bolster intervention impact.

Booster content

Like Braitman and Henson (2016), the content of the tailored feedback boosters for the current study included descriptive norms, or perceived amount of alcohol consumed by a referent group, and protective behavioral strategies (PBS), or strategies to reduce harm associated with alcohol consumption. These two constructs were chosen because of their ability to be succinctly communicated via mobile technology (such as email or text message) and their demonstrated associations with the alcohol consumption of college drinkers. Descriptive norms are a common intervention component and robust mechanism of change across many studies (see Reid & Carey, 2015 for a review). PBS are a promising potential mechanism of change (Reid & Carey, 2015), with numerous articles demonstrating associations with alcohol use (see Pearson, 2013 and Prince et al., 2013 for reviews; Braitman et al., 2015).

Sex and Age as Potential Moderators

Sex

Sex may be an important factor in intervention and booster efficacy, given the documented sex differences in alcohol consumption. For example, based on the National Survey on Drug Use and Health (NSDUH) data from 2012, women ages 18–25 report lower alcohol consumption than men, including fewer drinking days and drinks per drinking day (White et al., 2015). Moreover, fewer women in this age group report past-month binge drinking (consuming 5+ drinks on the same occasion) and fewer women meet past-year alcohol use disorder criteria than men. Despite these differences, however, an analysis of recent drinking trends suggests that the gap between men and women may be closing (White et al., 2015). Research focusing on gender-related issues and particularly as it relates to intervention efficacy is warranted.

Prior research has found sex differences in alcohol intervention response. A meta-analysis of controlled trials examining interventions for college drinkers found that interventions among samples with higher proportions of women were more successful at reducing alcohol-related problems at short-term follow-up (Carey et al., 2007a). However, sex did not impact intervention short-term efficacy in a meta-analysis of mandated students (Carey et al., 2016). Interestingly, a meta-analysis comparing in-person interventions to CDIs found that samples with higher proportions of women did not respond as strongly to CDIs compared to controls, and that this was true at short-term, intermediate, and long-term follow-ups, but there were no sex differences for in-person intervention response (Carey et al., 2012).

Regarding boosters, among mandated college students, men reacted negatively to a booster (with more drinks per drinking day than the no-booster group), whereas there were no booster-related differences among women (Mastroleo et al., 2011). However, in a study of college drinkers examining multiple administrations of personalized normative feedback compared to single administrations, women who received multiple administrations of the feedback significantly reduced their alcohol-related problems compared to controls, but men did not (Neighbors et al., 2010). Taken together, these findings may indicate that college men and women may respond differently to the current CDI or booster.

Age

Age may also impact the efficacy of the intervention or booster, given its associations with drinking. According to the 2012 NSDUH data, emerging adults under legal drinking age (i.e., ages 18–20) consume larger quantities of alcohol (i.e., more drinks per drinking day) and higher proportions meet alcohol use disorder criteria for the past year than adults of legal drinking age (i.e., ages 21–25; White et al., 2015). Conversely, higher proportions of emerging adults of legal drinking age engaged in binge drinking in the past 30 days than underage drinkers, and among binge drinkers, differences in the number of binge days is negligible across age groups. An examination of binge drinking and high-intensity drinking (i.e., 10+ drinks in a single occasion) revealed the typical developmental trajectory was an increase in frequency from late adolescence to a peak at age 21 to 22, followed by a decrease in frequency thereafter (Patrick et al., 2016). Thus, drinking patterns are clearly different across emerging adulthood based on legal drinking status (i.e., under age versus 21 and above).

Age has not often been explored as a moderator for treatment response among college drinking interventions. However, in a seminal study examining three different types of treatment, Baer et al. (1992) found age by time interactions, where the effect of time on past week alcohol quantity varied by age. For participants who were already 21 at the start of the study, or who never reached 21 during the study duration, there were consistent downward trends across time after treatment. However, participants who reached their 21st birthday during study observation all showed a marked increase on drinking indices during the period of observation, suggesting a change in typical drinking patterns and that intervention impact varied by age status of the drinker. Similarly, although moderation analyses were not performed to statistically examine this, Baer et al. (2001) noted an increase in drinking frequency during year 3 of their study, when the majority of participants turned 21 years of age, across all study conditions. However, they concluded that brief intervention for high-risk college drinkers can achieve long-term benefits, even in the context of these maturational trends. To date, no studies have examined if booster efficacy varies as a function of age.

The Current Study

The purpose of the current study is to explore 1) the unique impact of boosters containing tailored feedback to strengthen and extend effects of an online intervention targeting college drinking (i.e., control vs. CDI-only vs. CDI + booster email @ 2-weeks), and 2) sex and age as potential moderators of the intervention and booster effects. Given previous short-term longitudinal support of booster efficacy (up to 4 weeks; Braitman & Henson, 2016), we hypothesized that longer-term alcohol consumption (up to 9 months) will be reduced for the CDI + booster email group compared to the CDI-only group. Given that sex and age moderation analyses are exploratory in nature with no strong history in the literature, no specific effects were hypothesized.

Method

Participants

Participants were voluntary undergraduate college students from a mid-size public university in the southeast. Although $n = 561$ students completed the baseline session, 11 students were dropped for not reporting any alcohol consumption in the past two weeks, and an additional 13 were dropped for not meeting age eligibility criteria, as seen in Figure 1. Eligibility criteria included being between 18 to 24 years and consuming at least one alcoholic drink in the past two weeks. There were no other study exclusion criteria. Thus, the current sample consisted of $n = 537$ college students (67.4 % female) with a mean age of 19.65 years ($SD = 1.67$). Descriptive information about the sample demographics can be seen in Table 1.

Study Design

The current study used a randomized controlled trial design to compare a CDI alcohol intervention only (CDI-only), CDI + booster email, and a general health education program (serving as the control condition). Participants were randomly assigned to one of the three condition, stratified by sex, using a 1:1:1 allocation ratio. Follow-up assessments were administered via an online survey at 2, 4, and 6 weeks post-intervention, as well as 3-, 6-, and 9-months. The study was completed once the minimum enrollment target was reached. Study enrollment occurred from January 2013 to December 2013, with the final follow-up assessment completed in September 2014. More details about the trial can be found on the clinicaltrials.gov website (NCT03433794).

Procedure

Participants were recruited using an online participation management system, signing up for specific timeslots in a research lab at a university campus for baseline participation. Upon arrival to the lab, participants were randomized, stratified by sex, to one of three conditions (i.e., control, CDI-only, CDI + booster email). A random number generator in Excel was used by the researcher, restricted to the values of 1 through 3, to accomplish the randomization. Each participant interacted for 60 minutes with their assigned online session (alcohol intervention or general health session). Participants were invited via email to complete the follow-up survey at 2, 4, and 6 weeks post-intervention as well as 3, 6, and 9 months post, with optional text message reminders sent to those who provided mobile phone numbers. Participant flow can be observed in Figure 1. Participants in the booster condition were sent personalized feedback emails approximately two days after their 2-week follow-up survey invitation (i.e., approximately two weeks after the intervention). Participants in other conditions received an email in this same timeframe to standardize contact, thanking them for their participation so far and reminding them of the upcoming follow-up surveys.

Participants received course credit in exchange for their participation in the baseline session. They were paid \$10 for completing the follow-up surveys 2, 4, and 6 weeks post-intervention, and were entered into raffles to win \$25 (3 months post), \$30 (6 months post), or \$50 (9 months post) for completing longer-term follow-up surveys. Finally, participants who completed all surveys were entered into a raffle to win \$200.

Study Conditions

Alcohol intervention (CDI-only)—Participants in the alcohol intervention condition navigated through Alcohol 101 Plus™ for 60 minutes. This is an online intervention, free to institutions and individuals. It is a combination of several intervention components, including alcohol education, personalized feedback, attitude-focused strategies, and skills training. It also included a virtual bar, where participants provide basic information such as sex, weight, and state of residence so that the program can provide tailored information on blood alcohol content (BAC) as well as state regulations regarding legal limits. The program provides updated BACs based upon choices about what to consume (both alcohol and food) and how quickly to consume it, as well as how long their body should take to process the alcohol out of their system. The intervention is highly interactive, with written text, photos, public service announcement videos, personal video testimonials, and video storylines for fictional students where the participant reaches decision points and can choose what the fictional student should do. It is a non-linear environment, where participants choose which sections of the website to explore.

Alcohol intervention plus booster email (CDI + booster email)—In addition to receiving Alcohol 101 Plus™ for 60 minutes, participants in this condition received emailed personalized feedback. Content included sex-specific descriptive normative information (i.e., drinks per week typically consumed by males and females at the same institution), as well as reminders of harm reduction strategies. This included a list of PBS techniques the participant reported using in their last survey, versus techniques not used (i.e., “Don’t forget some of the other strategies you can use to reduce your drinking or minimize harm”). There was also a very brief reminder of why participants might want to engage in harm reduction strategies (i.e., “Remember that if you can reduce your drinking and alcohol-related problems, that can help you avoid some of the common issues associated with alcohol use including declining grades, risky sex, relationship problems, and even legal consequences”). The email was sent from a sex-matched research staff.

Control education session—Participants in the control condition navigated through a general health education session to standardize time in the research lab and contact with research staff. Health Education Answers is an online program developed by Lilly for Better Health®. It offers interactive resources such as health screeners, quizzes, knowledge builders, and self-management tools to help individuals prevent or manage health conditions. The site provides practical tips on general well-being such as healthy eating, physical activity, and stress management, as well as provides information on managing health conditions such as diabetes, heart disease, and depression. Alcohol is not directly addressed in the materials.

Measures

Alcohol consumption—Alcohol consumption was measured using a modified version of the Daily Drinking Questionnaire (DDQ; Collins et al., 1985). Participants indicated, using a grid format, how many standard drinks (e.g., 12-ounce beer, 5-ounce wine, or 1.5 ounce liquor) they consumed each day of the past two weeks, as well as how many hours passed while drinking. This information was used to calculate total number of drinks consumed

during the two-week period (quantity), number of drinking days (frequency), and highest number of drinks on a single day (peak drinks). Using information about weight and sex, an equation by Matthews and Miller (1979) was used to calculate estimated typical blood alcohol concentration (BAC) and highest BAC across the two-week period (peak BAC).

Alcohol-related problems—Problems related to alcohol consumption were assessed using the Young Adult Alcohol Consequences Questionnaire (YAACQ; Read et al., 2006). Participants were presented with a list of 48 items and asked to indicate which ones they experienced in the referenced time period. Problems experienced were summed to create a total problems score. The scale was modified to assess the previous 2 weeks rather than the past year, with good internal consistency (ranged from $\alpha = .88$ to $.90$ across timepoints).

Demographics—Self-reported demographic information (e.g., age, sex, race, ethnicity, year in school, relationship status, and affiliation with the Greek system) was collected. To assess age as a potential moderator of intervention and booster efficacy, and given the documented changes in drinking trajectories prior to versus after turning 21 (Patrick et al., 2016; White et al., 2015), a dichotomous version of the variable was created to reflect being of legal drinking age (i.e., 21+; $n = 151$, 28.1%, coded as 1) versus underage ($n = 386$, 71.9%, coded as 0). Sex was dichotomously coded as 0 = *female* ($n = 362$, 67.4%), 1 = *male* ($n = 175$, 32.6%).

Analysis Approach

Analyses for alcohol consumption were conducted using curve-of-factors latent growth modeling (Duncan & Duncan, 1996; McArdle, 1988; see Figure 2). Alcohol consumption at each timepoint was operationalized as a latent variable defined by quantity, frequency, peak drinks, typical BAC, and peak BAC. As indicated in Figure 2, loadings for each item were constrained to equality across time so that latent variables were consistently defined. The quantity indicator was used to scale the latent factor; as such, consumption is referred to as “drinks” throughout, even though the latent factor represents drinks (i.e., drinking quantity, peak number of drinks), days (i.e., drinking frequency), and concentrations (i.e., typical BAC, peak BAC). Analyses for alcohol-related problems were conducted on observed composite scores for total problems reported. To capture problems after controlling for consumption, number of drinks consumed (quantity) was included as a time-varying covariate (e.g., problems at Week 2 controlling for quantity at Week 2).

To operationalize growth over time, intercepts and slopes were fitted to latent factors (for consumption) or observed scores (for problems) in a hybrid of piecewise and linear growth (also seen in Figure 2). Using a piecewise approach, loadings were specified as 0 or 1 for Slope 1 to represent growth to Week 2, capturing any potential intervention effect. Similarly, loadings were specified as 0 or 1 for Slope 2 to represent growth from Week 2 to Week 4, capturing any potential booster effect. Finally, growth from Week 4 through Month 9 was represented as a linear effect for Slope 3, with loadings reflecting time elapsed in 2-week units (i.e., 1.0, 4.3, 10.6, 16.9 assuming a typical month is equal to 4.2 weeks), to assess maintenance over time. Condition was captured using two observed variables: intervention (0 = *control general health session* [representing the control group], 1 = *alcohol intervention*

[representing both CDI-only and CDI + booster email]) and post-intervention booster (0 = *control email* [for CDI-only], 1 = *personalized feedback email* [for CDI + booster email]).

To assess potential moderation for effects associated with alcohol consumption, multigroup models were conducted for each moderator (i.e., sex and age). The measurement and structural paths are identical to the main model (see Figure 2). An unconstrained model was conducted where all measurement-related parameters were constrained to equality across both groups, as were any covariate-related structural parameters. Structural parameters related to condition (i.e., the effect of intervention and booster on latent intercepts and slopes) were freely estimated separately for each group (i.e., by sex and by age). Two companion models were conducted for each outcome that were nearly identical to this first model, except for the constraint of a single parameter. In the first companion model testing moderation of the booster effect, the effect of booster (CDI + booster email compared to CDI-only) on slope 2 (i.e., growth immediately following the booster) was constrained to equality across both groups. In the second companion model testing moderation of the intervention effect, the effect of intervention (control group compared to other two groups combined) on slope 1 (i.e., growth immediately following the intervention) was constrained to equality across both groups. A significant likelihood ratio test based on the chi-square values for both models would indicate significant misfit by constraining the parameter to equality, or that the effect of booster on slope 2 growth (or effect of intervention on slope 1 growth) is significantly different across groups (i.e., moderation). Due to convergence issues with models assessing sex as a moderator (i.e., the smaller proportion of the male group combined with the sparseness of later follow-up data) and the focus on booster effects for the hypotheses, the moderation models were conducted using only the first three follow-up assessments (i.e., through week 6).

Sex was included as a covariate in all models (except the models where it was included as the grouping variable)¹. All models were conducted in Mplus (version 8; Muthén & Muthén, 1998–2017) using expectation maximization estimation. Parameters were estimated using all available data, assuming data were missing-at-random. To account for small departures in normality, bootstrapping with 1,000 replications was used, and 95% bias-corrected bootstrap confidence intervals (BCCIs) were used to determine significance, where BCCIs that did not contain zero were considered significant.

Power analyses were conducted using Mplus (version 6.1) and SAS® software (version 9.2; SAS Institute Inc., 2010) based on a method described on the Mplus website (Muthén & Muthén, 2010). These power estimates were calculated for each effect [(i.e., intervention effect, booster effect, and moderation)] based on a meta-analysis of previous research using computerized interventions (Carey et al., 2009a) and an estimated small-to-medium effect for the booster. After incorporating attrition rates from the same population (Braitman & Henson, 2016), it was calculated 116 participants per condition were necessary to obtain a power level of .80, for a total of 348 participants. To be conservative (i.e., allow for a smaller

¹We also ran the main analyses for overall consumption and problems controlling for both sex and age. We found that age was not a significant predictor, and that the general pattern of findings was unchanged. We present only the findings controlling for sex.

booster effect size or higher attrition levels), data were collected from additional participants by not terminating recruitment until the end of the relevant semester.

Results

Preliminary Analyses

The data were examined for outliers, with extreme cases being reduced to non-extreme values (while maintaining rank) when detected. Outliers were detected for quantity (5.0%), peak drinks (1.7%), typical BAC (1.3%), and peak BAC (1.5%). Missingness on any follow-up survey was not significantly associated with any variables of interest for the current study (i.e., alcohol consumption), nor any demographic characteristics (i.e., sex, race, ethnicity, year in school, Greek status); in addition, missingness on follow-up surveys did not vary by condition assignment. One-way ANOVAs indicated conditional equivalence at baseline across all study variables (p 's > .05). Means and standard errors for all variables at each timepoint are shown in Table 2, separated by condition.

Alcohol Consumption

Findings for the effect of intervention and booster are shown in Table 3. Note that when listed as an outcome (italic, flush left), “intercept” refers to baseline levels of overall consumption. There was not a significant effect for intervention or booster at baseline, indicating equivalence across conditions on consumption. When listed as a parameter for each slope (not italic, indented), intercept refers to the mean of that slope when all predictors are zero (i.e., for the control group). Thus, the intercept for Slope 1 indicated that the control group significantly reduced their consumption by about 2 “drinks” by Week 2, $b = -2.200$, $\beta = -0.232$. The intercept for Slope 2 showed a negligible, non-significant continued decline for the control group through week 4, $b = -0.549$, $\beta = -0.051$. Finally, the intercept for Slope 3 indicated an essentially flat maintenance trajectory for the control group through month 9, $b = -0.032$, $\beta = -0.038$.

Also seen in Table 3, although receiving the intervention was associated with an additional reduction in Slope 1, $b = -1.696$, $\beta = -0.085$, this reduction was not significant. Similarly, although receiving the booster was associated with an additional reduction in Slope 2, $b = -0.944$, $\beta = -0.042$, this reduction also was not significant. Finally, neither receiving the intervention nor receiving the booster was associated with changes in the Slope 3 trajectory. These trends can be seen in panel *a* of Figure 3. Both the CDI-only condition and CDI + booster email condition demonstrated stronger declines in growth from baseline to week 2. Similarly, the CDI + booster email condition showed a decline in growth from week 2 to week 3, whereas the CDI-only condition showed a slight increase back in the direction of pre-intervention levels. However, none of these differences in trajectories were significant. The trajectories through month 9 were fairly similar, where the CDI + booster email condition continued to have slightly lower consumption, but not significantly so.

Alcohol-Related Problems

As seen in the right half of Table 3, there was a significant effect for intervention on baseline alcohol-related problems, indicating those who were randomized to receive the intervention

experienced significantly more problems at baseline (failed randomization). Regarding change in growth to week 2, the intercept for Slope 1 indicated that the control group had a non-significant decrease in problems, controlling for alcohol quantity, by week 2, $b = -0.729$, $\beta = -0.205$. Similar to the model for consumption, the intercept for Slope 2 showed a non-significant decline for the control group's problems, controlling for alcohol quantity, through week 4, $b = -0.297$, $\beta = -0.132$. As before, the intercept for Slope 3 indicated an essentially flat maintenance trajectory for the control group through month 9, $b = 0.003$, $\beta = 0.013$.

A similar pattern of results for intervention and booster effects was observed for problems as for consumption. Also seen in the right half of Table 3, although receiving the intervention was associated with an additional reduction in Slope 1 for problems, $b = -1.867$, $\beta = -0.251$, this reduction was not significant. Similarly, although receiving the booster was associated with an additional reduction in Slope 2 for problems, $b = -0.560$, $\beta = -0.109$, this reduction also was not significant. Finally, neither receiving the intervention nor receiving the booster was associated with changes in the problems Slope 3 trajectory. These trends can be seen in panel *b* of Figure 3. As with consumption, both the CDI-only condition and CDI + booster email condition demonstrated stronger declines in problem growth from baseline to week 2. Similarly, the CDI + booster email condition showed a decline in growth from week 2 to week 3, whereas the CDI-only condition showed a plateau. However, none of these differences in trajectories were significant. The trajectories through month 9 were fairly similar, where the CDI + booster email condition continued to have slightly lower problems, but not significantly so.

Moderation

Age moderating consumption—A likelihood ratio test indicated significant booster effect moderation by age for alcohol consumption, $\chi^2(1) = 4.70$, $p = .030$, where the effect of booster receipt on immediate growth (Slope 2) for alcohol consumption was significantly different for those who were of legal drinking age as compared to underage participants. For participants under the legal drinking age (i.e., 18 to 20 years), the booster was not significantly associated with Slope 2 growth, $b = 0.453$, $\beta = 0.022$, $p = .791$. However, for participants 21 to 24 years of age (i.e., legal drinking age), receiving the booster was associated with significantly stronger reductions in drinking for Slope 2, $b = -5.904$, $\beta = -0.289$, $p = .031$, by almost 6 “drinks” in the metric of the consumption latent variable. Age did not significantly moderate the intervention effect of the CDI for alcohol consumption, $\chi^2(1) = 0.06$, $p = .806$.

Age moderating problems—A likelihood ratio test also indicated significant booster effect moderation by age for alcohol-related problems, controlling for consumption, $\chi^2(1) = 4.77$, $p = .029$, where the effect of booster receipt on immediate growth (Slope 2) for problems was significantly different across age groups. For participants under the legal drinking age, the booster was not significantly associated with Slope 2 growth, $b = 0.549$, $\beta = 0.084$, $p = .444$. However, for participants 21 to 24 years of age, receiving the booster was associated with significantly stronger reductions in problems for Slope 2, $b = -3.125$, $\beta = -0.342$, $p = .029$, after controlling for consumption levels; this is equivalent to a reduction of

about three problems. Similar to consumption, age did not significantly moderate the intervention effect of CDI for alcohol-related problems, $\chi^2(1) = 2.13, p = .144$.

To follow up these significant moderation findings for consumption and problems by age, we explored differences at baseline for those under versus over the legal drinking age. As shown in Table 4, there were no significant differences at baseline for quantity, peak drinks, or problems. However, students of legal drinking age did drink significantly more often, and had significantly lower BACs (both for a typical day and their peak drinking day).

Sex moderating consumption and problems—Likelihood ratio tests indicated there was no significant booster effect moderation by sex for consumption, $\chi^2(1) = 0.01, p = .913$, or for problems, $\chi^2(1) = 0.04, p = .838$. Similarly, sex did not significantly moderate the intervention effect for CDI for alcohol consumption, $\chi^2(1) = 0.00, p = .999$, nor for alcohol-related problems, $\chi^2(1) = 0.08, p = .784$. Taken together, these findings indicate neither the effect of intervention nor booster on these outcomes significantly varied for men as compared to women.

Discussion

The current study explored the utility of emailed boosters containing personalized feedback after an online intervention targeting college drinking. Importantly, moderation findings for age revealed that the booster may be an effective means to strengthen and extend intervention effects for emerging adults who are of legal drinking age. However, effects were weaker than expected for the general student population. The booster effect for the overall sample demonstrated a trend in the expected direction, but failed to reach significance, with effects being negligible for underage drinkers. Booster effects were not significantly moderated by sex. Intervention effects were not moderated by either age or sex.

The current findings for students of legal drinking age replicated short-term effects from a similar booster (Braitman & Henson, 2016). In addition, the current examination incorporated a longer term investigation (up to 9 months rather than 4 weeks). As seen in Figure 3, reductions in alcohol consumption continued through month 9. The current study used general student emerging adult drinkers (i.e., at least one alcoholic drink in the past two weeks), similar to other studies that demonstrated booster efficacy (e.g., student volunteers who consumed at least four drinks in the past two weeks, between the ages of 18 and 24 [Braitman & Henson, 2016]; incoming freshmen who consumed at least five/four drinks for men/women on one or more occasions during the past month [Neighbors et al., 2010]). The three investigations that did not demonstrate booster efficacy in college settings focused on fraternity members (Caudill et al., 2007) or students mandated to treatment (Barnett et al., 2007; Linowski et al., 2016), typically reporting much higher drinks per drinking day.

Our findings suggest that college students could be more receptive to booster content shortly after the original intervention, when they may still be more motivated to control their drinking and the booster serves as a simple reminder of how they compare to other college students and how they might cut back. Similar to Braitman and Henson (2016), the current emailed booster was sent two weeks after the original intervention, capitalizing on a

potential desire to cut back that has not yet extinguished from the original intervention. Other studies that did not demonstrate booster efficacy had a longer time gap between the base intervention and subsequent booster delivery (i.e., 1 month [Barnett et al., 2007] or 3 months post-intervention [Linowski et al., 2016], or even 5 or 11 months post-intervention [Caudill et al., 2007]). The timing of the booster message may be a critical component for reaching students when they are most receptive to change. Further research is needed to identify optimal timing of booster implementation. One approach may be to systematically manipulate the latency of booster delivery to identify the delivery timing associated with the greatest drinking reductions over time.

Along these same lines, the booster may be very timely for college drinkers of legal drinking age. Given that emerging adults age 21 and older consume less alcohol and have lower proportions meeting alcohol use disorder criteria than underage emerging adults (White et al., 2015), and the observed decline in the frequency of binge drinking and high intensity drinking after ages 21 to 22 (Patrick et al., 2016), it may be that the booster is accelerating and strengthening a naturally occurring process. Emerging adults of legal drinking age may be beginning to mature out of risky drinking patterns, and the booster strengthens this process. Although mean trajectories for all three conditions demonstrated reductions in alcohol consumption and related problems throughout the course of the study, after receiving the emailed booster feedback, participants of legal drinking age who received the booster after the intervention demonstrated an additional reduction of almost 6 drinks and over 3 problems beyond reductions demonstrated by participants of legal drinking age who did not receive the booster after the intervention. This demonstrates the booster contributed further reductions beyond maturational processes, with this group reducing both drinking, and drinking-related harm even after controlling for consumption levels. These findings are particularly impactful in light of research supporting emerging adults of legal drinking age to remain at-risk for binge drinking (White et al., 2015). To date, this study is the first to establish age as a moderator of booster efficacy.

Interestingly, sex was not a moderator of either booster or intervention efficacy, for either alcohol consumption or related problems. Much of the research establishing sex differences in treatment response were meta-analyses examining the proportions of women in samples (e.g., Carey et al., 2007a, 2012, 2016). Among studies directly assessing sex differences in treatment response, there has been conflicting evidence, with some studies finding stronger treatment response for women (e.g., Murphy et al., 2004, 2010), or that women respond more strongly to in-person interventions than computerized (e.g., Carey et al., 2009b), or that women respond more strongly to repeated feedback (Neighbors et al., 2010). However, the current findings join a larger body of literature demonstrating no sex differences in treatment response for college drinking (e.g., Borsari & Carey, 2000; Carey et al., 2007b; Larimer et al., 2007; Marlatt et al., 1998; Murphy et al., 2012; Walters et al., 2009).

Finally, intervention effects in the current study were relatively weak, demonstrating a trend of an additional reduction of 1.7 drinks (i.e., 3.9 drinks for intervention recipients rather than 2.2 for control participants), but failing to reach significance. A similar trend was seen for problems, with an additional reduction of 1.9 problems, but again failing to reach significance. The intervention effect in the current study may have been weakened by use of

a general health intervention for control participants; a focus on improving diet and exercise or general health could potentially lead to reductions in heavy drinking as well (see Leasure et al., 2015 for a discussion).

The findings of the current investigation should be interpreted in light of several study limitations. Results were based on participants' retrospective reports of their own alcohol-related behaviors, which may be susceptible to recall or reporting bias. Recall bias was minimized by asking participants to only recall the past two weeks; however, a prospective method such as daily diary design may lend more validity to future research. Alternatively, alcohol consumption may be verified by the use of transdermal alcohol assessment (Simons et al., 2015) or by collecting collateral reports (Borsari & Muellerleile, 2009). In addition, the current study focused on emerging adult college drinkers, including relatively light drinkers consuming at least one drink containing alcohol in the past two weeks. Generalizations beyond this population of interest should be made with caution. Moreover, emails were not tracked, therefore there was no way of knowing if booster recipients actually opened or read their emails, precluding the ability to assess fidelity. Finally, the attrition rate is a limitation for the current study, and is likely linked to the compensation strategy. Study retention likely was hindered by two relevant factors. First, switching compensation type from baseline (course credit) to follow-ups (monetary payment) may have reduced the incentive to participate for those motivated by course credit. Second, switching from guaranteed payments for the short-term follow-ups (\$10 each) to raffle entry for the long-term follow-ups likely contributed to our attrition rate.

Future research should explore the utility of an emailed feedback booster after a computerized intervention with more empirical support. Moreover, in light of the findings of the current study and other short-term boosters compared to ineffective boosters with longer wait times, future research should examine the ideal timing of booster delivery. Is it better to capitalize on maintaining the initial drive to curb drinking, or is it better to deliver later to prevent the extinction of effects? Along these same lines, future research should explore motivation or readiness to change prior to the intervention, after the intervention, and after the booster. It is possible effective boosters are delivered during a window of receptivity after the initial intervention. Replications would also be beneficial with specific populations that have not responded to boosters in the past (e.g., mandated students [Barnett et al., 2007; Linowski et al., 2016], members of Greek life [Caudill et al., 2007] etc.) or that are underexamined, with literature only recently supporting intervention approaches (e.g., non-student emerging adults [Lau-Barraco et al., 2018]). In addition, booster content should be methodically examined in future research, as the small number of studies implementing boosters have used various approaches (e.g., succinct reminders of personalized normative feedback, sometimes paired with harm reduction skills, or more time with the original intervention) delivered across various modalities.

Overall, the present investigation contributed to a limited body of research on the utility of intervention boosters to augment main intervention effects in college drinkers. Our study demonstrated that a brief computer-delivered alcohol intervention plus a simple email booster with personalized feedback resulted in significant reductions in drinking outcomes for emerging adult college students of legal drinking age. Research efforts to further develop

and refine intervention booster strategies represent a promising future direction to minimize harmful drinking among college students.

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Figure 1.
CONSORT participant flow diagram. CDI = computer delivered intervention for college drinking.

Figure 2. Piecewise-linear hybrid latent growth models for overall consumption (a curve-of-factors model). Consumption factor loadings with matching letters (i.e., matching outcome indicators) were constrained to equality. Growth slope loadings were set to 1 unless otherwise specified (i.e., for Intercept, Slope 1, and Slope 2).

Figure 3.

Trajectories of alcohol consumption (panel *a*) and alcohol-related problems controlling for consumption (panel *b*) by condition. CDI = computer delivered intervention. Note that Control = general health intervention only, CDI-Only = alcohol CDI only, CDI + booster email = alcohol CDI plus personalized feedback booster email, w2 = week 2, w4 = week 4, w6 = week 6, m3 = month 3, m6 = month 6, m9 = month 9.

Table 1

Demographic Information for the Sample

	<i>n</i>	%
<i>Sex</i>		
Female	362	67.4
Male	175	32.6
<i>Hispanic</i>		
Yes	55	10.2
No	479	89.2
<i>Race</i>		
White or Caucasian	262	48.8
Black or African-American	201	37.4
Asian or Pacific Islander	25	4.7
Native American	4	0.7
Other	34	6.3
<i>Class Standing</i>		
Freshman	234	43.6
Sophomore	131	24.4
Junior	106	19.7
Senior	62	11.5
<i>Marital Status</i>		
Single	394	73.4
Committed Relationship	128	23.8
Married	7	1.3
Divorced	1	0.2
Other	7	1.3

Note. Group membership may not sum to $n = 537$ (100%) due to participants choosing not to respond to select items.

Table 2

Descriptive Statistics for Study Variables over Time by Condition

	Baseline		Week 2		Week 4		Week 6		Month 3		Month 6		Month 9	
	M	(SE)	M	(SE)	M	(SE)	M	(SE)	M	(SE)	M	(SE)	M	(SE)
<i>Quantity</i>														
Control	16.93	(1.10)	14.21	(1.26)	13.20	(1.22)	12.17	(1.46)	12.07	(1.50)	14.55	(2.14)	12.55	(1.85)
CDI-only	17.99	(1.28)	12.50	(1.29)	12.59	(1.69)	12.25	(1.81)	13.77	(2.27)	12.62	(2.11)	11.27	(2.26)
CDI + booster	18.04	(1.22)	15.42	(1.54)	13.70	(1.71)	15.88	(2.14)	14.92	(1.78)	16.10	(2.69)	12.23	(2.80)
Total	17.64	(0.69)	14.03	(0.79)	13.15	(0.89)	13.34	(1.04)	13.61	(1.09)	14.35	(1.32)	11.99	(1.30)
<i>Frequency</i>														
Control	3.97	(0.17)	3.37	(0.24)	3.14	(0.25)	2.97	(0.27)	3.10	(0.31)	3.15	(0.39)	3.37	(0.49)
CDI-only	3.80	(0.18)	2.96	(0.25)	2.57	(0.27)	2.51	(0.30)	3.00	(0.41)	2.80	(0.40)	2.85	(0.44)
CDI + booster	3.98	(0.18)	3.35	(0.25)	2.96	(0.29)	3.05	(0.32)	3.19	(0.32)	3.18	(0.40)	2.67	(0.50)
Total	3.92	(0.10)	3.23	(0.14)	2.89	(0.15)	2.84	(0.17)	3.10	(0.20)	3.04	(0.23)	2.99	(0.27)
<i>Peak drinks</i>														
Control	5.80	(0.28)	4.99	(0.36)	4.90	(0.35)	4.55	(0.41)	4.29	(0.47)	4.98	(0.61)	4.57	(0.41)
CDI-only	6.35	(0.31)	5.00	(0.38)	5.07	(0.48)	4.85	(0.56)	5.08	(0.61)	4.29	(0.63)	4.08	(0.63)
CDI + booster	6.14	(0.28)	5.27	(0.43)	5.07	(0.49)	5.38	(0.58)	5.22	(0.52)	4.90	(0.62)	4.31	(0.86)
Total	6.09	(0.17)	5.08	(0.22)	5.01	(0.25)	4.91	(0.30)	4.87	(0.31)	4.73	(0.36)	4.32	(0.36)
<i>Typical BAC</i>														
Control	0.07	(0.00)	0.06	(0.00)	0.06	(0.01)	0.06	(0.01)	0.05	(0.01)	0.06	(0.01)	0.06	(0.01)
CDI-only	0.07	(0.00)	0.06	(0.01)	0.07	(0.01)	0.06	(0.01)	0.06	(0.01)	0.05	(0.01)	0.05	(0.01)
CDI + booster	0.07	(0.00)	0.06	(0.01)	0.06	(0.01)	0.06	(0.01)	0.06	(0.01)	0.06	(0.01)	0.05	(0.01)
Total	0.07	(0.00)	0.06	(0.00)	0.06	(0.00)	0.06	(0.00)	0.06	(0.00)	0.06	(0.01)	0.05	(0.00)
<i>Peak BAC</i>														
Control	0.10	(0.01)	0.09	(0.01)	0.09	(0.01)	0.09	(0.01)	0.08	(0.01)	0.10	(0.01)	0.09	(0.01)
CDI-only	0.11	(0.01)	0.10	(0.01)	0.11	(0.01)	0.10	(0.01)	0.10	(0.01)	0.09	(0.02)	0.08	(0.01)
CDI + booster	0.12	(0.01)	0.10	(0.01)	0.10	(0.01)	0.10	(0.01)	0.10	(0.01)	0.09	(0.01)	0.08	(0.01)
Total	0.11	(0.00)	0.10	(0.00)	0.10	(0.01)	0.10	(0.01)	0.09	(0.01)	0.09	(0.01)	0.08	(0.01)
<i>Problems</i>														
Control	6.66	(0.41)	4.05	(0.45)	4.49	(0.57)	3.93	(0.48)	3.91	(0.55)	3.38	(0.61)	3.61	(0.65)

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	Baseline		Week 2		Week 4		Week 6		Month 3		Month 6		Month 9	
	M	(SE)	M	(SE)	M	(SE)	M	(SE)	M	(SE)	M	(SE)	M	(SE)
CDI-only	7.16	(0.46)	4.63	(0.52)	4.15	(0.59)	3.95	(0.57)	4.38	(0.74)	3.44	(0.73)	2.62	(0.67)
CDI + booster	7.01	(0.47)	5.06	(0.58)	3.70	(0.56)	4.14	(0.61)	4.51	(0.59)	3.39	(0.62)	3.10	(0.77)
Total	6.94	(0.26)	4.57	(0.30)	4.13	(0.33)	4.00	(0.32)	4.27	(0.36)	3.41	(0.38)	3.11	(0.40)

Note. CDI = alcohol computer delivered intervention, booster = emailed booster with personalized feedback, BAC = blood alcohol concentration. Each cell represents means with standard errors in parentheses.

Table 3

Intervention and Booster Effects on Alcohol Consumption and Related Problems

Outcome	Alcohol Consumption			Alcohol-Related Problems		
	<i>b</i>	β	95% BCCI	<i>b</i>	β	95% BCCI
<i>Intercept (Baseline Factor)</i>						
Intercept	14.476*	0.935	[0.81, 1.08]	2.535*	0.637	[0.28, 1.01]
Intervention	1.684	0.064	[−0.04, 0.17]	2.444*	0.294	[0.08, 0.50]
Booster	−0.180	−0.007	[−0.10, 0.09]	−0.383	−0.042	[−0.27, 0.23]
<i>Slope 1: Growth from Baseline to Week 2</i>						
Intercept	−2.200*	−0.232	[−0.45, −0.05]	−0.729	−0.205	[−0.57, 0.18]
Intervention	−1.696	−0.085	[−0.22, 0.06]	−1.867	−0.251	[−0.50, 0.003]
Booster	−0.090	−0.004	[−0.13, 0.13]	0.192	0.023	[−0.25, 0.32]
<i>Slope 2: Growth from Week 2 to Week 4</i>						
Intercept	−0.549	−0.051	[−0.25, 0.15]	−0.297	−0.132	[−0.68, 0.33]
Intervention	1.015	0.045	[−0.10, 0.18]	0.289	0.062	[−0.20, 0.31]
Booster	−0.944	−0.042	[−0.18, 0.10]	−0.560	−0.109	[−0.41, 0.23]
<i>Slope 3: Growth from Week 4 to Month 9</i>						
Intercept	−0.032	−0.038	[−0.26, 0.18]	0.003	0.013	[−0.52, 0.50]
Intervention	−0.164	−0.092	[−0.26, 0.08]	−0.061	−0.151	[−0.41, 0.11]
Booster	0.025	0.014	[−0.15, 0.21]	−0.024	−0.055	[−0.37, 0.17]

Note. 95% BCCI = 95% bias-corrected bootstrap confidence intervals for standardized estimates. Significant effects at the $p < .05$ level (as evidenced by BCCIs not containing zero) are indicated with bold text.

* $p < .05$

Table 4
Baseline Alcohol Consumption and Related Problems by Legal Drinking Age Status

	<i>M</i>	(<i>SD</i>)	<i>df</i>	<i>t</i>	<i>p</i>	95% CI		Cohen's <i>d</i>	
<i>Quantity</i>			535	-1.06	.290	-4.65	1.39		-0.103
Under 21	17.19	15.76							
21 or older	18.81	16.62							
<i>Frequency</i>			223.5	-3.52*	.001	-1.40	-0.39		-0.411
Under 21	3.67	2.18							
21 or older	4.56	2.82							
<i>Peak drinks</i>			535	0.54	.592	-0.53	0.93		0.051
Under 21	6.15	3.95							
21 or older	5.95	3.75							
<i>Typical BAC</i>			535	4.23*	<.001	0.01	0.03		0.387
Under 21	0.08	0.06							
21 or older	0.05	0.05							
<i>Peak BAC</i>			535	2.60*	.010	0.01	0.04		0.243
Under 21	0.12	0.09							
21 or older	0.10	0.08							
<i>Problems</i>			535	0.34	.732	-0.93	1.33		0.033
Under 21	6.99	5.93							
21 or older	6.79	6.19							

Note. LL = lower limit, UL = upper limit.

* $p < .05$