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The College Drinker's Check-up: Outcomes of Two Randomized Clinical Trials of a Computer-Delivered Intervention[1]

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Abstract

Objective—To evaluate the effectiveness of a computer-delivered intervention (CDI) to reduce heavy drinking and alcohol-related problems in college students in two randomized clinical trials.

Method—In Experiment 1, we randomized 144 students to either the CDI or an *assessment-only control* group with follow-ups at 1 and 12 months. In Experiment 2, we randomized 82 students to either the CDI or a *delayed-assessment control* group with follow-up at 1 month.

Results—*Experiment 1*. Participants in both groups significantly reduced their drinking at both follow-ups. Compared to the control group, the CDI group reduced their drinking significantly more at 1 and 12 months on three drinking measures at $\alpha < .05$. Using a more conservative, Bonferroni-adjusted criterion yielded one significant difference in a measure of heavier drinking at the 1 month follow-up. The mean between-group effect sizes were $d = .34$ and .36 at 1 and 12 months, respectively. *Experiment 2*. Compared to the *delayed assessment* control group, the CDI group significantly reduced (by the Bonferroni-adjusted criterion) their drinking on all consumption measures.

Conclusion—These results support the effectiveness of the CDI with heavy drinking college students when used in a clinical setting. In addition, the significant reductions in typical drinking in the control group in Experiment 1 and not in Experiment 2 combined with comparable baseline characteristics suggests that the control group in Experiment 1 demonstrated assessment reactivity.

Keywords

computer-based intervention; computer-delivered intervention; CDI; problem drinkers; heavy drinking college students; brief motivational intervention; BMI; assessment reactivity

Heavy drinking among college students is a substantial problem associated with many negative consequences. In 2002, the National Institute on Alcohol Abuse and Alcoholism's (NIAAA) Task Force on College Drinking reported that from 1993 to 1999, there were *each year* an estimated 1,400 deaths, 500,000 injuries, more than 600,000 assaults (by another student who had been drinking), 70,000 reported incidents of sexual assault or date rape,

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400,000 incidents of unprotected sex, and more than 100,000 incidents of students being too intoxicated to know if they consented to sex. Academic problems were also common; about 25% of college students reported some alcohol-related consequences from their drinking (e.g., missed classes, poor performance on exams, lower grades overall). Additional negative consequences included drunk driving, vandalism, and involvement with police or campus security. In all, more than 150,000 students developed alcohol-related health problems each year during this period (Hingson, Hereen, & Zakocs, 2002).

A bulletin published in 2007 by the NIAAA reported that rates of binge drinking (defined as 5 or more drinks per occasion for males and 4 or more for females), driving while intoxicated (DWI) and alcohol related deaths had continued to increase since the initial report, while there had been no decline in the number assaults committed by college drinkers on their peers (NIAAA, 2007). Moreover, Hingson, Zha, and Weitzman (2009) reported that among young adults aged 18–24, college students were more likely to engage in these problematic alcohol-related behaviors than same aged peers who were not enrolled in college. Based on these data, it is clear that more needs to be done to address the problem of heavy drinking in college students.

One early response to this need was the development of face-to-face brief motivational interventions (BMIs), such as the Brief Alcohol Screening and Intervention for College Students (BASICS) (Dimeff et al., 1999). The BASICS protocol contains components that consistently demonstrate success in helping college students reduce alcohol consumption and related problems: personalized feedback, normative comparisons and decisional balance exercises (Carey, Scott-Sheldon, Carey & DeMartini, 2007; Larimer & Cronce, 2007). Unfortunately, interventions that contain these components require significant counselor time to implement on an individual basis; thus, a relatively low cost-benefit ratio has constrained their wide-scale implementation.

A cost-effective way to address this problem has emerged with the development of computer-delivered interventions (CDIs). Though relatively uncommon when the Task Force initially began to investigate college drinking, the use and investigation of CDIs has burgeoned over the last decade. When the NIAAA commissioned a review of individual-focused interventions for problematic college drinking, Larimer and Cronce (2007) concluded that significant advances had been made in their use and development across a variety of BMI types (e.g. personalized normative feedback, self-monitoring/self-assessment, BAC skills training).

A number of reviews have made it clear that computers lend themselves well to delivering BMIs for alcohol misuse (Cunningham & van Mierlo, 2009; Hallet, Maycock, Kypri, Howat & McManus, 2009; Bewick, Trusler, Barkham, Hill, Cahill & Mulhern, 2008; Elliot, Carey & Bolles, 2008). Besides their cost-effectiveness, researchers frequently site the flexibility of their design, the capacity for interactivity, the consistency with which they present intervention components, and their ease of use (both for administrators and students) as advantages of CDIs. Given the privacy of this modality, CDIs may appeal to people who might not otherwise seek treatment (Cunningham & van Mierlo, 2009), and also may encourage greater validity of self-report (Elliot, et al., 2008). Finally, with their combination of broad reach and tailored individualization, CDIs fit well with the Task Force's 3-in-1 conception of successful interventions, as they can operate simultaneously to impact individual students, the student body as a whole, and the greater college community (NIAAA, 2007).

As for their effectiveness, a recent meta-analysis of CDIs for heavy drinking college students found "qualified support for the efficacy of CDIs to reduce alcohol use and

problems in college students" (Carey, Scott-Sheldon, Elliott, Bolles, & Carey, 2010, p. 1812). Though often associated with immediate reductions in problematic drinking, and slower developing but long-term reductions in alcohol related problems, improvements vary across outcomes, and average effect sizes, when compared to assessment-only controls, were relatively small (0.09–0.28). Additionally, Carey and colleagues point out that, typically, short-term improvements appear more consistently than do long-term improvements. Thus, the authors contend that the promise of CDIs is tempered by the need to improve their effectiveness, both with dismantling studies and further comparison with other treatment modalities across contexts. They also suggest that in order to sort out these inconsistent results, methodological aspects of CDIs need to be explored, such as assessment reactivity.

The College Drinker's Check-up (CDCU)

We sought to reduce heavy drinking and alcohol related problems in college students by adapting the Drinker's Check-up (DCU), a CDI that we originally developed for older heavy drinkers. And the DCU was based on the original, face-to-face protocol by the same name that was developed by Miller and colleagues (Miller, Sovereign & Krege, 1988). Evidence from a randomized clinical trial (RCT) of the DCU supported the effectiveness of the protocol (Hester, Squires, & Delaney, 2005). Participants in that study reduced their drinking by 45–55% at the 12 month follow-up. The adaptation of the DCU involved revising the assessment and feedback modules to be more appropriate to college age drinkers and the contexts in which they drink (e.g., drinking quantity feedback relative to the norms of their own university).

We developed the CDCU as both a Windows and web-based brief motivational intervention (BMI). We consider it a BMI because it is designed to be administered in one, relatively brief (approximately 35 minute) session. It employs the *FRAMES* elements of effective BMIs (Miller & Sanchez, 1994). FRAMES is an acronym for: *F*eedback is personalized; *A*dvice is given carefully; a *M*enu of options for changing is offered; an *E*mpathic, nonjudgmental tone is used; and there is an emphasis on *S*elf-efficacy.

Assessment Reactivity

We initially planned to conduct a single RCT of the CDCU with follow-ups at 1 and 12 months. In this study the experimental group received the CDCU and the control group was assessment only. During the conduct of the trial however, we observed significant reductions in drinking in the control group at the 1 month follow-up. Consulting with colleagues, we became aware of assessment reactivity data suggesting that some students react to questions about their drinking with less heavy drinking at follow-up. A number of researchers have documented this phenomenon. Carey, Carey, Maisto, and Henson (2006) found that administering the Time Line Follow-Back assessment of drinking reduced consumption at follow-ups out to 12 months. Kypri and colleagues (2006) and McCambridge and Day (2008) found reductions in drinking at follow-up after answering the ten questions in the AUDIT. Walters and colleagues randomized 147 students to either assessment at baseline, 3, 6, and 12 months or delayed assessment at 12 months. At 12 months those who had received the repeated assessments had lower peak blood alcohol concentrations (BACs), lower AUDIT scores, and engaged in more risk reduction behaviors when compared to the delayed assessment group (Walters, Vader, Harris, & Jouriles, 2009). Reacting to assessments in this way, while clinically useful, also presents confounds in research studies. Assessment reactivity can mask the true impact of CDIs when the only control group is assessed at baseline and/or repeatedly. To control for assessment reactivity,

we conducted a second study that compared the CDCU to a *delayed assessment* control group.

Objectives

The objective of these two RCTs was to evaluate the effectiveness of the CDCU in reducing heavy drinking and alcohol-related problems in college students.

Hypotheses

The experimental groups will show lower levels of drinking and alcohol-related problems relative to the control groups at follow-ups.

General Method

Participants

We sought to recruit heavy drinking college students because they are at the greatest risk for alcohol-related problems. We recruited from a local four-year public university and a community college using display and classified ads in the school newspapers and flyers posted on kiosks around the campuses.

Inclusion/exclusion criteria

Inclusion criteria included: (a) self-identified college student drinkers who meet NIAAA's (2004) criteria for heavy, episodic drinking (i.e. 4 + drinks per occasion for women, 5 + for men, at least once in the last two weeks and an estimated peak BAC of 80mg% or more); and (b) age range of 18–24. The only exclusion criteria were being mandated to an intervention because of an alcohol policy infraction, not having a significant other to corroborate their self-report of drinking, and anticipating not being available for follow-ups. The recruitment period ran from September 2008 to March 2010.

Screening

Screening was a two-step process. The first step was a phone screening conducted when students called to ask about the study. This screening included two questions about drinking, as well as questions about ethnicity, residential status, year in school, and weight. The drinking questions were: "What is the most you've had to drink (in standard drinks, explained) on one occasion in the last 2 weeks?" and "Over how many hours?" We used the BAC calculator in the CDCU program to estimate their peak BAC for that occasion, given the amount consumed in the reported time and considering their weight and gender. If the student met the drinking criteria, we invited him or her to the clinic to complete the screening. Once there, we administered a breathalyzer test to ensure the student was sober. Because heavy drinking college students are at higher risk for mental health problems, we administered the Brief Symptom Inventory (BSI, Derogatis, 2000), to assess psychological distress. The purpose of this was solely to recommend additional counseling and not to screen out distressed students. The cutoff points for this referral were the 63rd percentile or above on either the Global Severity Index or on two of the three subscales. We also queried those who endorsed the item "Thoughts of ending your life" with a response of "a little bit" or a more affirmative response.

We asked potential participants if they would be able and willing to provide the name of a significant other (SO). We informed them that there "was a slight chance" that their SO might be contacted, and that we did this in order to corroborate their self-report of drinking. Though we kept this SO contact information on file, we ultimately did not contact any SOs. We made this requirement to improve the veracity of their self-reports, as has been done in

other studies of this population (Carey et al., Unpublished data, Personal Communication, 10/7/04; Marlatt et al., 1998).

The Research Assistants (RAs) then described the study, presented the informed consent form, and asked if there were any questions. Once the consent form was signed, we enrolled the student in the study, collected demographic and contact data, and randomly assigned the participant to a group. The experimental groups in both experiments were administered the CDCU, which includes both an assessment of and intervention for problem drinking (see below). The control groups in both experiments were administered only the assessment portion of the program, but at different time-points relative to their entrance into the study. While study participants were working on the computer, the RA sat quietly in another part of the room doing paperwork.

Assessment

We used three instruments to measure drinking and related problems at baseline and follow-up. The AUDIT (Babor, Higgins-Biddle, Saunders, & Monteiro, 2001) is a widely used 10-item brief screen for heavy drinking and alcohol problems. Several studies have demonstrated that the AUDIT is effective in identifying problem drinkers in college as well as in broader adult populations (e.g. Chung et al., 2000; Kelly, Donovan, Kinnane & Taylor, 2002). We used the Brief Drinker's Profile (BDP, Miller & Marlatt, 1987) to gather the quantity and frequency of drinking, drug use, and family history of alcohol problems. We incorporated these elements of the BDP into the CDCU as was done with the initial computerized DCU. In that project we found the test-retest reliability of the BDP via computer to be well within acceptable limits (Squires & Hester, 2002). We measured alcohol related problems with a set of 19 questions from the Core Institute's survey of drinking, drug use, and related problems. We call this set of questions the College Students Alcohol Problems (CSAP). The questions are similar to the Rutgers Alcohol Problems Index (RAPI; White & Labouvie, 1989) (e.g., Got into a fight or argument). We chose the CSAP over the RAPI because it has gender and university specific norms that we used in the Feedback module. Presley and colleagues have reported acceptable psychometric properties of the CSAP in their manual for their survey (Presley, Meilman, & Leichliter, 1998).

With the exception of the control group in Experiment 2 (discussed below), we administered the AUDIT, the BDP, and the CSAP at baseline. We gave participants the BDP at all follow-ups and the AUDIT and CSAP at the 12 month follow-up in Experiment 1.

The intervention

The CDCU begins with a screening for heavy drinking using the AUDIT and two questions about the individual's heaviest drinking in the last two weeks. Students are then given personalized feedback, and those who screen positive for heavy drinking are invited to use the rest of the program. (Part of the rationale of the screening process in the experiments was to ensure that participants would screen positive and so be appropriate candidates for the intervention). Once the screening portion of the program is complete, students enter the Look at Your Drinking module, which includes a decisional balance exercise, a comprehensive assessment of drinking and drug use, alcohol-related problems, and risk factors for future alcohol-related problems. The Get Feedback module uses gender- and university-specific norms. Students receive feedback on the quantity and frequency of their drinking compared to their same gender fellow students at their university, BAC feedback, and feedback on how their frequency of alcohol-related problems compares to other, same gender students at their school. The final module, Consider Your Options, extends the initial decisional balance exercise, asking users to rate the level of importance of the "good things" and the "not so good things" about their drinking. It also asks them how ready they are to

change their drinking and takes their readiness into account in helping them develop a plan of action to reduce their drinking and risk for alcohol-related problems.

Follow-ups—Participants were paid $40 for their time and transportation costs to attend each baseline and follow-up session. This payment coupled with the recent challenging economic times may have contributed to our high follow-up rates.

Institutional Review Board—Both studies were approved by our Institutional Review Board at Presbyterian Healthcare Systems, Albuquerque, NM.

Experiment 1

Method

Experimental design—We used a two-group design to compare the CDCU experimental condition to an assessment only control condition, with assessments at three time points: baseline, and 1 and 12 months.

Participant flow and follow-up rates—Figure 1 summarizes the sample sizes at various stages in the Experiment. The follow-up rates at 1 and 12 months were 97% and 90% respectively.

Randomization—We randomized participants by blocks on the basis of gender, year in school, ethnicity (Hispanic, non-Hispanic White, other) and resident status (on or off-campus, Greek).

Planned interventions and timing—Once randomized, we invited participants to sit at a computer. Experimental participants were presented with the CDCU, which provides an overview, screening, and registration before beginning the Look at Your Drinking, Get Feedback, and Consider Your Options modules. For participants in the control group, the RA logged them into the program so that only the assessment module of the CDCU program appeared.

When finished, the RA scheduled the participant's first follow-up for 1 month from baseline assessment, the second follow-up for 12 months after baseline; then the participant was given a check for $40, and thanked for his or her time. We made reminder calls a few days before follow-ups, and resolved any scheduling issues. For these subsequent follow-ups, experimental participants logged themselves onto the program just as returning users would in an actual-use setting; control subjects were again logged on by the RA.

Power analyses and sample sizes—Sample sizes were arrived at by determining the power to detect the anticipated between-group treatment effects. Several prior studies of feedback interventions for college student drinking (Agostinelli, Brown, & Miller, 1995; Collins, Carey, & Sliwinski, 2002; Larimer et al., 2001; Marlatt et al., 1998; Murphy et al., 2001; Neighbors, Larimer, & Lewis, 2004), as well as our prior research with the DCU (Hester et al., 2005), provided estimates of the magnitude of the treatment effect. Average effect size across these studies ranged from .20 to 1.00 yielding a mean between-group effect size of d = .46. Planned inclusion of baseline drinking variables as covariates in analyses allowed for a somewhat increased noncentrality parameter in power analyses. Although Hester et al. (2005) obtained correlations between pre and post levels of drinking averaging .64, given the emphasis on binge drinking measures in the current study, we used an estimated correlation of only .45. This implied that a power of .8 could be achieved by using 61 subjects per group. Allowing for 15% attrition over the 12 months of the study, we determined that an initial sample size of 72 per group would be sufficient to achieve a power

of .8 using α = .05 for the primary test of the anticipated between-group difference on one of our primary dependent variables covarying the pre-treatment assessment on that variable.

Data analysis plan—Given the inconsistency in alcohol consumption over various episodes of binge drinking, we anticipated that the correlation over time in measures of heavy drinking would be low, and likely less than the .5 cutoff where repeated measures analyses of variance (ANOVAs) become less powerful than ignoring pre levels. (In fact, such pre-post correlations averaged only .36 in the current study.) Analyses of covariance (ANCOVAs) in such circumstances are clearly preferred as being more sensitive than these alternative methods of testing for group differences (Maxwell & Delaney, 2004, p. 446). Thus, the primary planned analyses of the hypothesized group differences were ANCOVAs covarying the pre-treatment assessment on that variable. We examined four dependent variables in both studies: Standard Drinks per Week, Peak BAC in a Typical Week, Average Number of Drinks in two Heavy Episodes in the prior month, and Average Peak BAC in those two Heavy Episodes, with two additional variables, AUDIT, and CSAP being available only at the 12-month follow-up of study 1. We used Bonferroni adjusted alpha levels of .05/4 = .0125 or .05/6 = .0083. We also wanted to confirm that overall participants were reducing their drinking over time. Thus, secondary analyses of the significance of overall change were also conducted using repeated measures ANOVAs to test for the main effect of Time.

Results

Participants—Table 1 summarizes the demographic characteristics of the participants. There were no significant differences between the samples in the two conditions on any of the demographic variables or on the mean values on the dependent variables at baseline.

There were no interactions of the treatment conditions with the stratification factors of gender, year in school, ethnicity, or residential status, thus analyses are reported only for the primary independent variable of treatment condition, CDCU vs. control. To conserve space and to facilitate comparison of results from the two studies, plots of means from both studies are shown on the same figure for a given dependent variable, with the CDCU group results indicated by dashed lines and the Control group results indicated by solid lines.

Results of primary analyses—As seen in Figures 3–6 (results for Experiment 1 are indicated by the square symbols in the figures), the reductions in drinking and alcohol-related problems in the CDCU group tended to be greater than that in the assessment only control group. At 1 month, three of the four ANCOVA tests of the treatment effect reached an unadjusted α of .05, but were not significant using the more conservative Bonferroni-adjusted criterion of .0125. The one exception was the test of the average peak BAC on two heavier occasions in the previous month (Av. BAC Heavier) at the 1 month follow-up which met the stricter criterion (see Table 2).

At the 12 month follow-up, three of six tests again yielded *p* values below an unadjusted α of .05: Drinks per Week, Average number of drinks in two heavy episodes, and Average Peak BAC in those heavy episodes. The Peak BAC Typical Week variable, the AUDIT (which measures both consumption and alcohol-related problems), and the CSAP which measures alcohol-related problems, yielded trends favoring the experimental group with effect sizes (*d*) of .31–.33 but not significant p values. No tests were significant at the Bonferroni adjusted α = .0083. Results on all variables tended to favor the CDCU group. Between-group effect sizes (Cohen's *d*) averaged .34 at 1 month and .36 at 12 months, with all effect sizes being at least in the small to medium range except for Drinks per Week at 1 month.

We also conducted secondary analyses of the change over time. These tests were consistently highly significant statistically, F's > 10, p's < .001, on all dependent variables, both in terms of change from baseline to the average of the follow-ups and also in terms of the continued improvement from the 1 month to the 12 month follow-up. (This was also true when change from baseline to the average of the follow-ups was tested within the treatment and control groups separately; similarly, separate within-group tests of continued improvement from 1 month to 12 months were still significant but yielded F's > 5, p's < .03). The within-subject effect size, averaging across the four consistently available dependent variables, was d = .484 for change from baseline to 1 month (d = .638 for treatment, d = .355 for control), and an additional d = .313 for change from 1 to 12 months (d = .326 for treatment, d = .304 for control). The overall reduction in drinking at 12 months amounted to 33% to 46% of baseline levels. The effect size of overall change from baseline to 12 month, averaging across all six dependent variables, was d = .725 (d = .899 for treatment, d = .579 for control). Individual differences in amount of improvement from baseline to post-treatment were significantly related to baseline values on drinking variables in both groups, with the mean correlation being .54 in the control group and .71 in the treatment group. Heavier drinkers reduced their drinking more at follow-ups.

Discussion

These outcome data provide modest support for our hypothesis that participants in the experimental group would show lower levels of drinking and alcohol-related problems relative to the control group at follow-ups. While three between-group tests were significant at .05 at each follow-up, using the Bonferroni-adjusted criterion which accounts for multiple tests of treatment effect, only one measure of heavy drinking at the 1 month follow-up showed a significant between group effect. On the other hand, the between-group effect sizes averaged .35 over all measures and follow-up times, which exceeds the average effect size range of 0.09–0.28 for CDIs compared to assessment only control groups reported by Carey et al, (2010). This modest support for the effectiveness of the CDCU needs to be considered in the context of an unanticipated finding: participants in the control group substantially reduced their drinking at follow-ups. One could argue that the control group was demonstrating a natural reduction in drinking over time, a "maturation" effect seen in college students. Comments by participants in the control group, however, led us to suspect assessment reactivity was involved. "I never realized how much I was drinking" or "I never added it all up before" were typical spontaneous comments made by participants in the control group as they went through their assessments on the computer and the RA sat quietly in the background. These comments and the recent research on assessment reactivity led us to control for it in a second experiment.

Experiment 2

Method

Experimental design—In this experiment we compared the CDCU treatment group to a *delayed assessment* control condition, with assessments at baseline (CDCU group only) and at 1-month (both groups). Based on the outcomes in Experiment 1 we also simplified the randomization in Experiment 2, randomizing only on gender and resident status.

Procedures were generally the same as in Experiment 1 through the 1 month follow-up. The major difference was that we used a delayed assessment or retrospective pretest (Pratt, McGuigan, & Katzev, 2000) with the control group. Once participants in this group signed the informed consent form the RA thanked them for their participation and scheduled their 1 month follow-up but did not ask them about their baseline level of drinking and alcohol related problems. At their 1 month follow-up we first assessed their baseline drinking for the

month prior to enrolling in the study (and alcohol-related problems in the previous year) and then assessed their drinking in the month between enrollment and their follow-up.

Participant Flow & Follow-ups—Figure 2 summarizes the sample sizes at various stages in the Experiment. Follow-up at 1 month was 42 for the CDCU group and 39 for the control group, a follow-up rate of 98%. None were excluded from the analyses.

Results

Participants—Table 3 summarizes the demographic characteristics of the participants in Experiment 2. There were no significant differences between the samples in the two conditions on any of the demographic variables or on the mean values on the dependent variables at baseline.

Results of primary analyses—Results for the four dependent variables in Experiment 2 (indicated by the triangle and circle markers in Figures 3–6) consistently indicated advantages of the CDCU treatment group over the delayed assessment control group, which were clearly significant using either unadjusted or Bonferroni-adjusted α levels. ANCOVA tests of the treatment effect were highly significant for all four dependent variables, with the average between-group effect size (.82) being large (see Table 3). As in Experiment 1, secondary analyses indicated a highly significant improvement over time on all dependent variables ($p < .01$), with the mean within-group effect size over all variables being $d = .128$ for the control group and $d = .605$ for the CDCU group. However, in contrast to Experiment 1, the delayed assessment control group showed no improvement from baseline to 1 month post on Drinks per Week or on Peak BAC in a Typical Week (F's < 1). Although the delayed assessment control group did reduce their mean drinks and peak BAC for the average of two heavier episodes ($F(1,38)= 6.10$, $p = .018$, and $F(1,38)= 8.76$, $p = .005$, respectively), the improvement on every drinking measure was significantly greater in the CDCU condition than in the delayed assessment control group ($p < .01$).

Alcohol-related problems were not assessed at follow-up in this experiment because the time frame for the CSAP and the AUDIT is the previous 12 months.

Results of analyses comparing control groups—Secondary analyses were conducted comparing particular results from the two studies. As suggested by the rationale for Experiment 2, we anticipated that the results from the Control groups in the two studies might differ. In fact, although there were no significant differences at baseline between the two Control groups, ANCOVAs indicated that the typical drinking outcomes at the 1 month follow-up were significantly different across the two studies at an unadjusted α of .05 both for Drinks per Week, $F(1, 113) = 7.67$, $p = .007$, and for Peak BAC in a Typical Week, $F(1, 113) =4.13$, $p = .045$. In contrast, for the two measures of average drinking in heavier episodes, ANCOVAs did not approach significance, $p > .5$. On all four variables, the direction of the difference was the same, namely, the control group in Experiment 1 had lower levels of drinking at the one-month follow-up than the control group in Experiment 2. Follow-up tests of change from baseline to 1 month follow-up indicated that the reason for the group differences in the ANCOVA was that in Experiment 1 the control group's decline was highly significant statistically on all 4 dependent variables ($p < .001$), whereas in Experiment 2, the control group did not improve from baseline to the 1 month follow-up on Drinks per Week or Peak BAC in a Typical Week ($p > .5$) but some improvement was seen on the two measures of heavier drinking episodes ($p < .05$).

Discussion

These outcome data provide strong support for our hypothesis that participants in the experimental group would show lower levels of drinking relative to the control group at follow-up. While the control group did reduce its heavier drinking, which might be attributed either to "maturation" and/or the Hawthorne effect of the anticipated assessment, the experimental group reduced its drinking to a significantly greater extent on all measures. The between-group effect sizes in Experiment 2 are some of the largest in the literature, with the average effect size of .82 being in the large range. These reductions are clinically as well as statistically meaningful. BAC is a measure of the intensity of a person's level of intoxication, and a person's peak BAC is one of the greatest risk factors for alcohol-related problems on any particular drinking occasion. The participants in the CDCU group reduced their peak BAC in a typical week by 42%, from 198mg% to 114mg%. They also reduced their heavier episode peak BACs by 47%, from 267mg% to 139mg%. While they are still at risk with BACs of 114—139mg%, their level of risk is substantially less.

Summary and Concluding Discussion

The primary goal of these two studies was to evaluate the effectiveness of the CDCU in reducing heavy drinking and alcohol-related problems in heavy drinking college students. Overall, these outcome data provide support for our hypothesis that participants who received the CDCU would reduce their drinking more than participants in the control groups at follow-ups. We find it interesting that the magnitude of change in these heavy drinking college students is comparable to that found in our study of the Drinker's Check-up (DCU), a CDI for older adults (Hester, Squires, & Delaney, 2005). The reduction in drinking in that study was 45–55% at 12 months and that group showed a continued reduction in drinking from the 8 week to the 12 month follow-up similar to that in our Experiment 1.

So what could account for the clinically meaningful reductions in drinking? Without dismantling studies, we can only offer speculations. First, the safest assumption to make may be that the CDCU's effectiveness derives in no small part from its implementation of personalized normative feedback (PNF), which has consistently been found to alter normative perceptions, and is frequently associated with reductions in subsequent drinking behavior (Moreira, Smith & Foxcroft, 2009; Walters, Vader, & Harris, 2007; Larimer & Cronce, 2007). Tailored feedback has been found to promote behavior change across a variety of college drinking interventions, and was further enhanced here with the use of gender-specific as well as university-specific norms (Lewis & Neighbors, 2006).

Second, the program incorporates the FRAMES elements found in face-to-face BMIs. While a computer program cannot provide complex empathic responses, it can set an empathic and nonjudgmental tone. As with Motivational Interviewing, one's choice of words is important. During the development of the CDCU Bill Miller consulted with us on this aspect of the program. Third, the CDCU contains two decisional balance exercises, another common element in face-to-face BMIs. The first exercise precedes the assessments and may reduce defensiveness. That exercise is revisited in the third module, Consider Your Options, and extended in an attempt to get the student to think more deeply about his or her drinking.

Fourth, the Consider Your Options module measures the student's readiness to change and takes that into account in taking them through the process of setting up a plan for changing their drinking. For instance, if the student is not at all ready to change, the program recommends considering the steps that follow (setting up a change plan) to be hypothetical and something the student could refer to in the future should he or she change his or her mind about changing. Future research (discussed below) can hopefully disaggregate the impact of these elements. Fifth, it is also possible that the CDCU engages the student in

thinking longer and more deeply about his or her drinking and that this improves outcomes. That would be consistent with Jouriles and colleagues' finding in which recalling or re-reading personalized feedback immediately following the eCHUG CDI improved short-term outcomes (Jouriles et al., 2010).

Finally, the secondary analyses comparing the control groups in the two studies suggests the role that assessment reactivity may have had in Experiment 1 in reducing typical drinking. Although participants were not randomly assigned to *experiments*, there was no evidence of important pre-existing differences between the control groups in the two studies. The significant differences in typical drinking in these control groups at 1 month but not at baseline can be most parsimoniously explained by assessment reactivity. The reductions in *heavier* drinking in the two control groups at 1 month could be a function of "maturation," regression to the mean, the Hawthorne effect, or some combination of the three.

Assessment reactivity is considered differently depending on whether it occurs in the context of clinical research or treatment. Assessment reactivity is a "problem" when it obscures the interpretation and/or validity of clinical trials of protocols. Some have argued this may have occurred during project MATCH, wherein assessment reactivity masked the true impact of the experimental intervention by reducing the between-group differences (Clifford & Maisto, 2000; Epstein et al., 2004). It can also make the intervention appear more effective than it might otherwise be without the benefit of the assessment effect.

On the other hand, many have argued that assessment itself may entail therapeutic benefit either as a prelude (e.g., Bien, Miller & Tonigan, 1993) or an adjunct to treatment (Finn & Tonsager, 1997). Given the success of BMIs, there has been renewed interest among alcohol researchers in investigating assessment reactivity as a useful clinical tool. Recent studies have examined the effects of assessment *as an intervention*. They include studies of the effect on alcohol consumption simply from answering the questions in the AUDIT (Kypri et al., 2006; McCambridge & Day, 2008; Walters et al., 2009); investigations of the relationship between assessment and other aspects of treatment (Maisto, Clifford, & Davis, 2007; Carey et al., 2006; Epstein et al., 2004); and, research exploring whether some methods of assessment are more effective at changing drinking than others (Clifford, Maisto, & Davis, 2007).

Whatever the contribution of assessment reactivity to within-group change, the between-group differences in Experiment 1 show that there is more to CDIs than assessment reactivity. The magnitude of the advantage of those receiving the CDCU over the assessment only control group corresponded to an overall between-group effect size of $d = .35$, averaging across all dependent variables and across the two follow-ups.

Limitations

Our study has a number of limitations. First, we recruited students through advertising and those who responded may have been more ready to change their drinking than heavy drinkers who did not respond. Second, the data are based on self-report—the validity of the self-report, however, was improved by requiring the name of an SO to corroborate their self-report of drinking, assuring participants of confidentiality of their responses, and verifying their sobriety before collecting any data. Third, in Experiment 2 we measured the delayed assessment control group's baseline level of drinking at the 1 month follow-up. Although we took steps to be clear about which period of drinking we were asking about (baseline vs. 1 month.), participants' recall and self-report of their baseline drinking conceivably may have been influenced by their current (i.e. 1 month.) drinking; however, the lack of differences between the reports of baseline drinking in the contemporaneous (Experiment 1) and delayed (Experiment 2) reports of baseline drinking provides support for the validity of the

retrospective pretest. One could also argue that our randomization procedures themselves would have resulted in an equal distribution of drinking levels between the two groups in Experiment 2. Fourth, participants were assessed in a clinical setting, our offices, rather than allowing them to use the CDCU online at their leisure (e.g. their dorm room or apartment). The outcomes may not have been as robust if participants were multi-tasking (e.g. texting, web surfing, watching youtube) while at the same time going through the CDCU. For this reason we consider these studies to be effectiveness studies of the CDCU in clinical settings but efficacy studies of the CDCU in non-clinical settings.

External Validity

Two aspects of the study contribute to its external validity. First, the study sample was diverse, included a large proportion of women and community college as well as four-year university students. Second, the software program used in this study is the same that will be available to future users of the programs. A computer-based intervention, by its nature, interacts with users in the same way over time; there is no "drift" from the protocol.

Future Research

We need a better understanding of two aspects of CDIs for college students: the elements of our interventions and how we can maximize assessment reactivity to improve outcomes. Dismantling studies are needed to examine the relative contributions of decisional balance exercises, using university and gender specific personalized feedback about both drinking and alcohol-related problems, exercises that engage the student in thinking about his or her drinking, and making plans for changing. With respect to assessment reactivity, we need to add delayed assessment control groups to control for its confounding impact on the interpretation of results. Solomon (1949) recommended this approach over 50 years ago but it does not seem to be used often. Assessment reactivity itself may be as important as the other elements of our interventions in promoting change. Experimentation with the wording or phrasing of questions or the prelude to the questions themselves may also prove fruitful. As Moos noted in his commentary on recent reactivity research, “we need to understand the context and mechanisms of reactivity in order to minimize it when we want to obtain a ‘true’ estimate of a construct of interest, but also to maximize it when we want to enhance desired behavior change in response to treatment interventions” (Moos, 2008, p. 250). With heavy drinking and alcohol-related problems on the increase (Hingson, 2010), we need to continue to find more effective ways to intervene.

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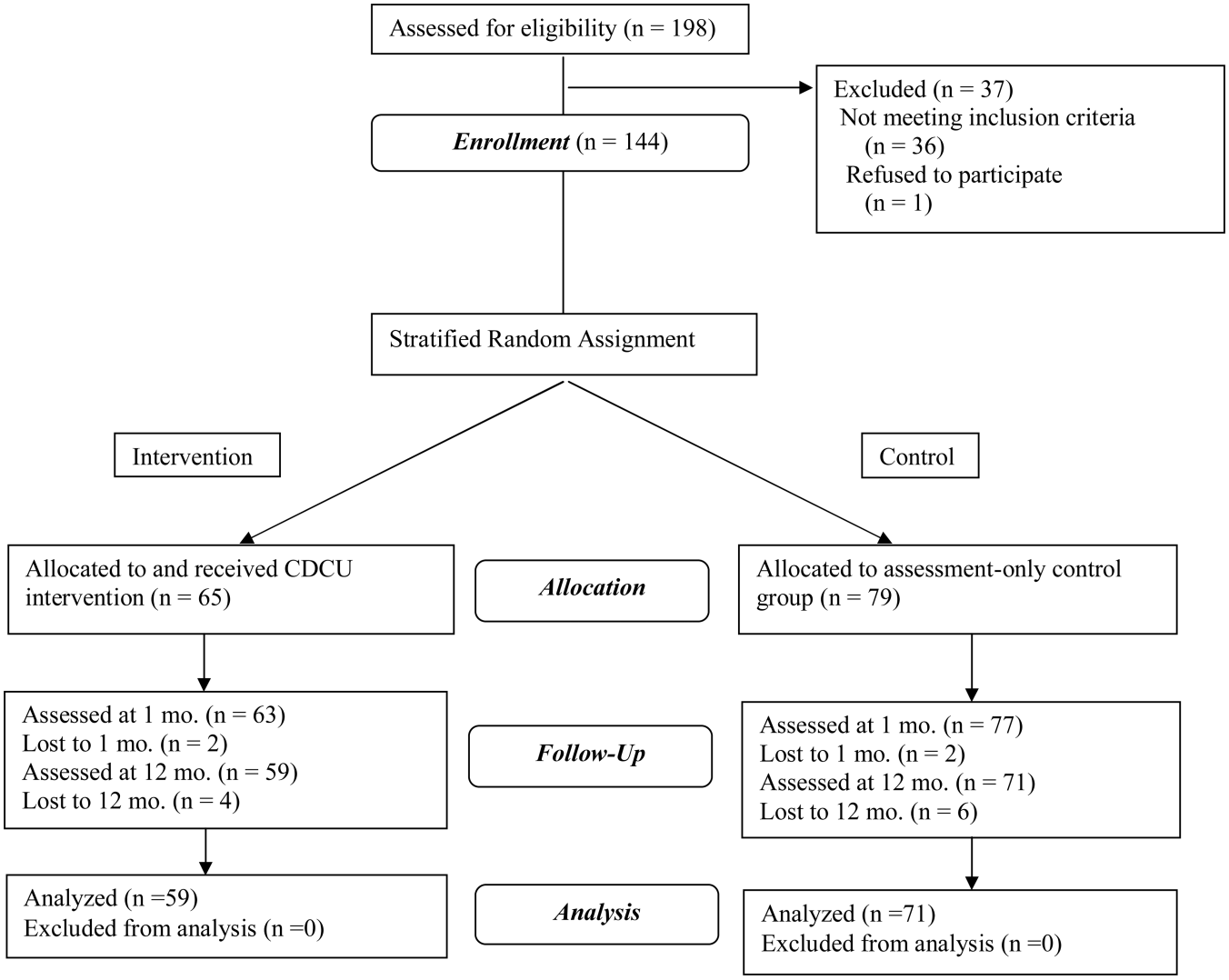


Figure 1.
Flowchart of participant flow and follow-up, Experiment 1.

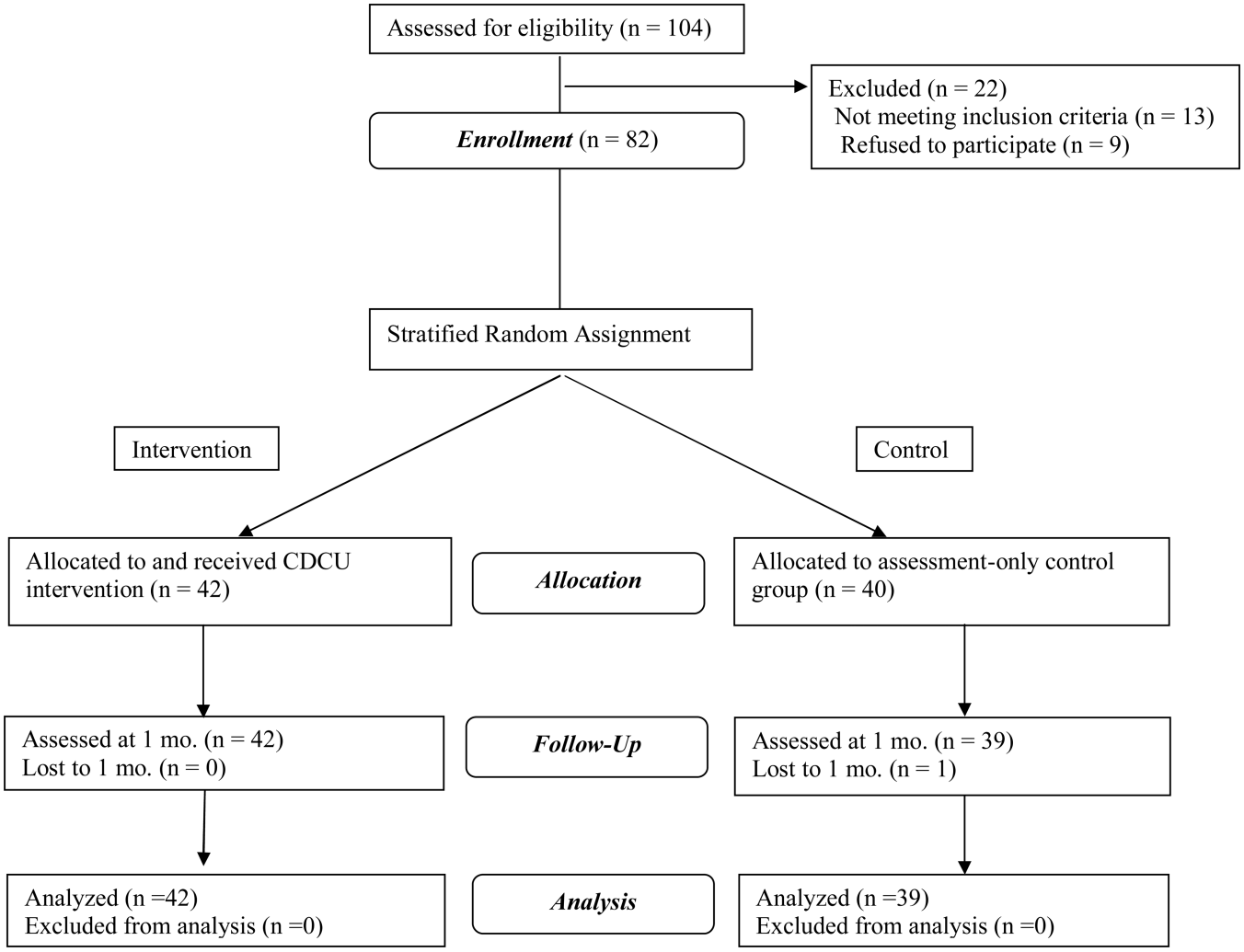
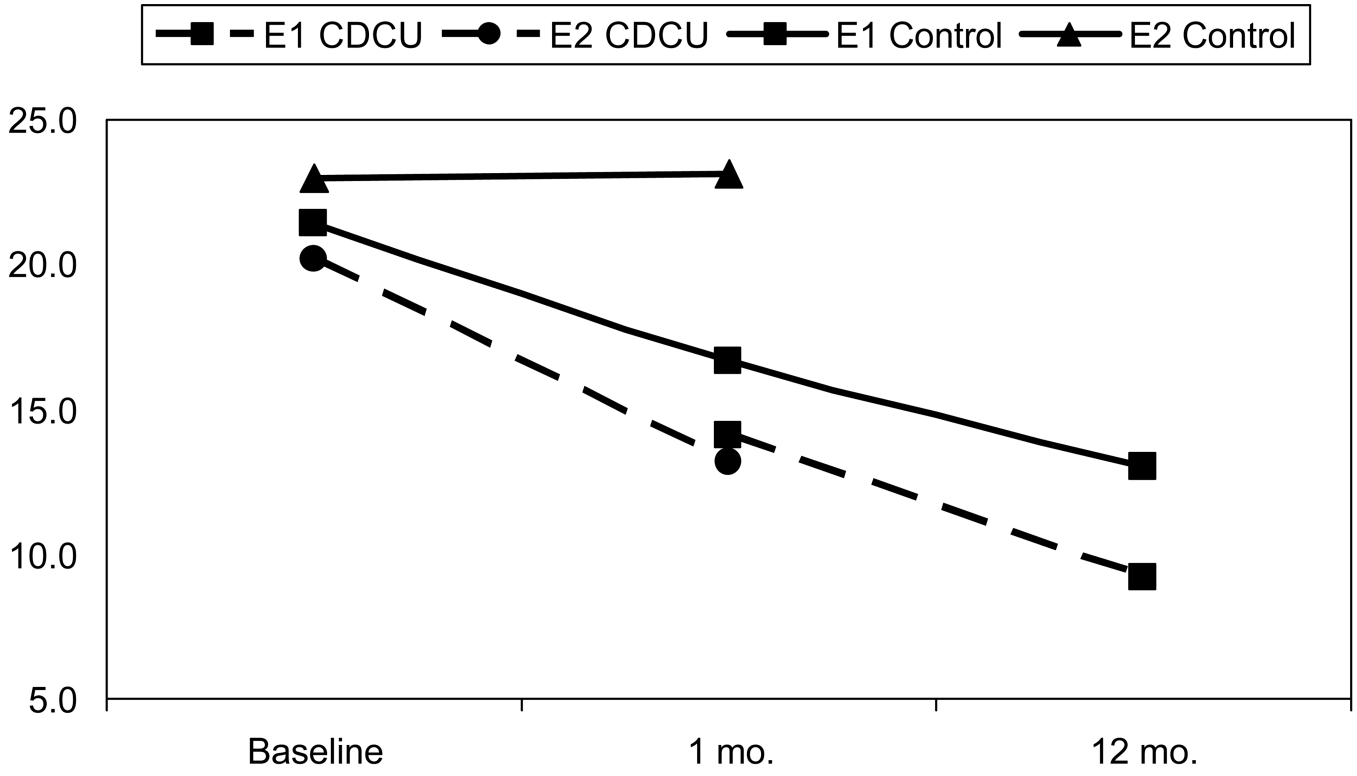
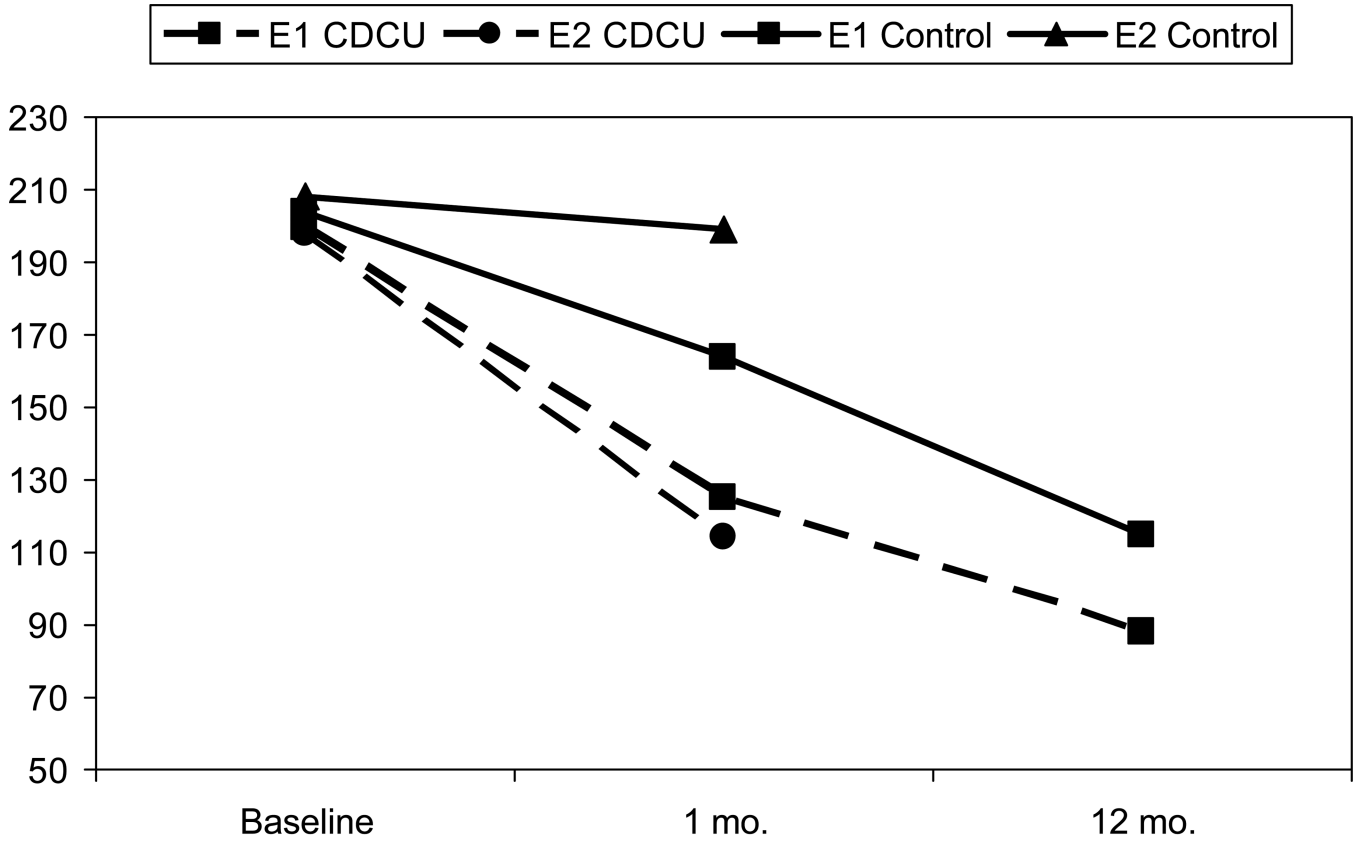


Figure 2.
Flowchart of participant flow and follow-up, Experiment 2.



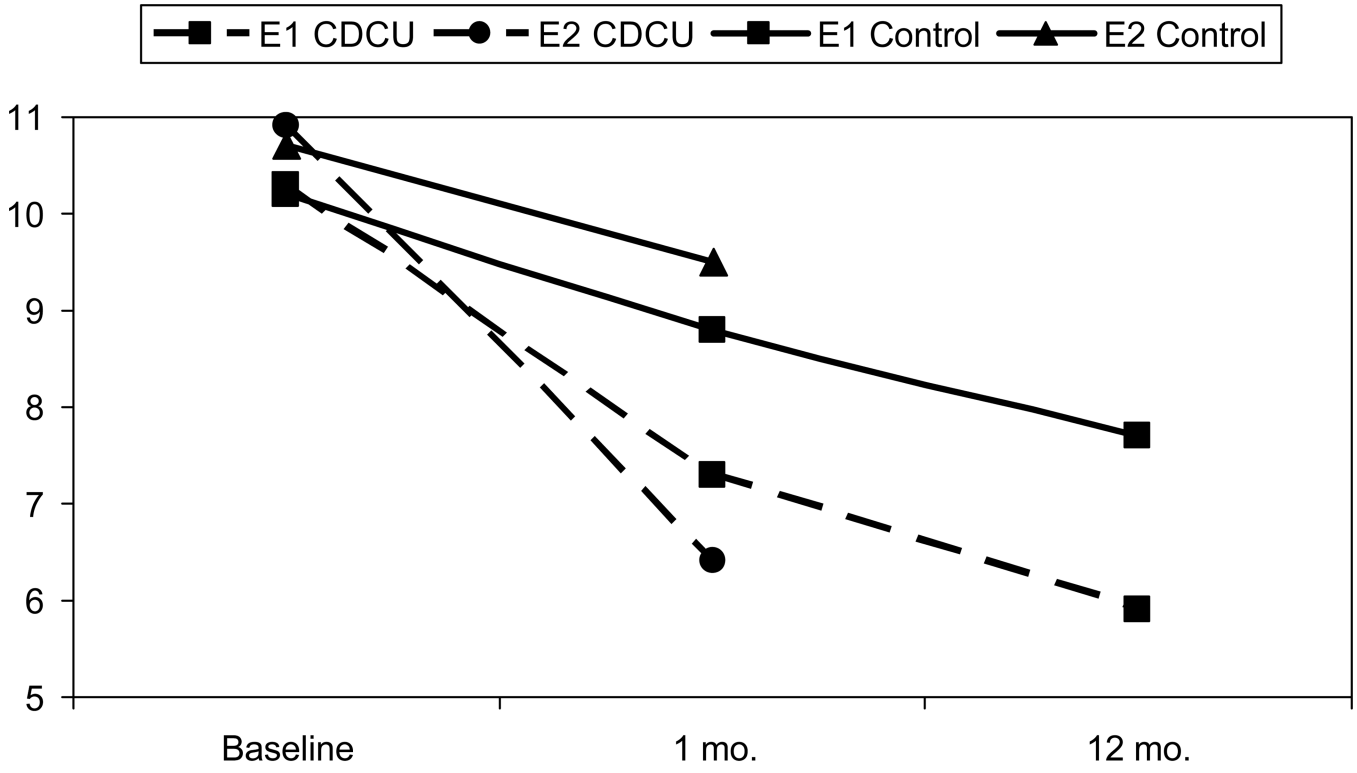
	Baseline		1 month		12 months	
Condition	M	SD	M	SD	M	SD
E1 CDCU	20.2	13.5	14.1	14.5	9.2	8.5
E2 CDCU	20.2	20.5	13.2	21.9		
E1 Control	21.4	12.7	16.7	11.4	13.0	11.8
E2 Control	23.0	21.8	23.1	30.2		

Figure 3.
Means for Standard Drinks in a Typical Week. Squares denote Experiment 1, circles and triangles are used for Experiment 2. Solid lines denote Control conditions, dashed lines denote Treatment conditions. The assessment of Baseline drinking in the Experiment 2 Control condition was by a retrospective pretest.



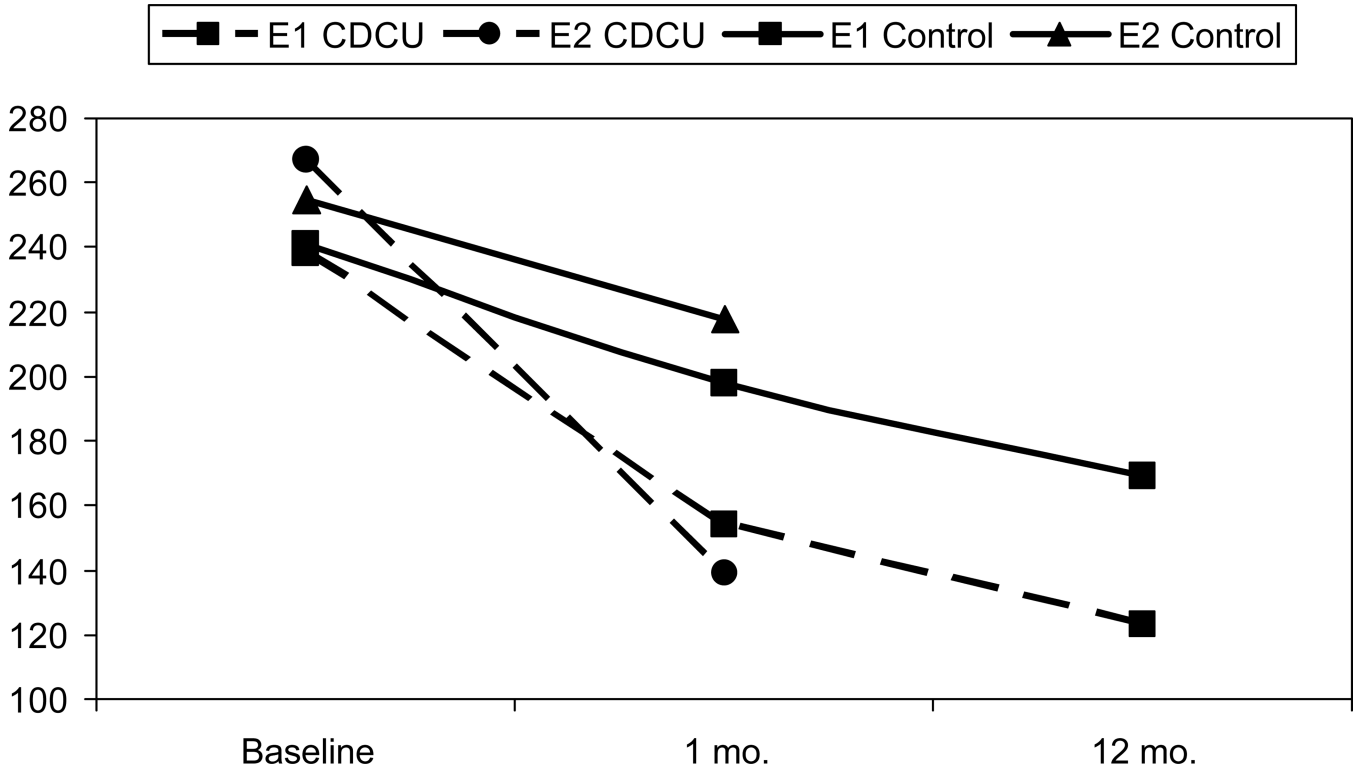
	Baseline		1 month		12 months	
Condition	M	SD	M	SD	M	SD
E1 CDCU	200	103	125	105	88	83
E2 CDCU	198	132	114	121		
E1 Control	204	125	164	106	115	91
E2 Control	208	112	199	156		

Figure 4.
Means for Peak BAC in a Typical Week. Squares denote Experiment 1, circles and triangles are used for Experiment 2. Solid lines denote Control conditions, dashed lines denote Treatment conditions. The assessment of Baseline drinking in the Experiment 2 Control condition was by a retrospective pretest.



	Baseline		1 month		12 months	
Condition	M	SD	M	SD	M	SD
E1 CDCU	10.3	4.9	7.3	4.0	5.9	4.1
E2 CDCU	10.9	7.2	6.4	4.8		
E1 Control	10.2	3.7	8.8	4.3	7.7	5.2
E2 Control	10.7	6.2	9.5	7.0		

Figure 5.
Mean number of drinks in two heavier episodes in previous month. Squares denote Experiment 1, circles and triangles are used for Experiment 2. Solid lines denote Control conditions, dashed lines denote Treatment conditions. The assessment of Baseline drinking in the Experiment 2 Control condition was by a retrospective pretest.



	Baseline		1 month		12 months	
Condition	M	SD	M	SD	M	SD
E1 CDCU	238	119	154	100	123	105
E2 CDCU	267	217	139	145		
E1 Control	241	108	198	107	169	122
E2 Control	255	132	218	140		

Figure 6.
Mean in two heavier episodes in previous month. Squares denote Experiment 1, circles and triangles are used for Experiment 2. Solid lines denote Control conditions, dashed lines denote Treatment conditions. The assessment of Baseline drinking in the Experiment 2 Control condition was by a retrospective pretest.

Table 1

Demographics of Participants in Experiment 1.

		Control		Treatment	
Variable		**Frequency**	**Percentage**	**Frequency**	**Percentage**
Gender	Male	49	62%	41	63%
	Female	30	38%	24	37%
Year in School	1	23	29%	18	28%
	2	17	22%	17	26%
	3	21	27%	17	26%
	4	13	16%	10	15%
	5	5	6%	3	5%
Residence	Dorm	18	23%	12	18%
	Greek	10	13%	7	11%
	Off-campus	51	65%	46	71%
Race/ Ethnicity	Asian American	1	1%		
	Black	6	8%	2	3%
	Hawaiian/Pacific Islander			1	1%
	Mixed race	6	8%	4	6%
	Native American			3	5%
	Non-Hispanic White	40	51%	40	62%
	Hispanic/Latino	26	33%	15	23%
Varsity Athlete	Yes	5	6%	2	3%
	No	74	94%	63	97%
		Mean	SD	Mean	SD
Age		20.29	1.63	20.51	1.80

Table 2

Statistical Tests of Treatment Effect Covarying the Baseline Value of the Variable and Between-group Effect Sizes (with 95% Confidence Intervals) in Experiment 1: CDCU vs. Assessment Only Control Group

Dependent Variable	1 Month Post				12 Months Post			
	F(1,137)	*p*	*d*	95% C.I.	*F*(1,127)	*p*	*d*	95% C.I.
Drinks per Week	0.41	.522	.11	−.22, .44	4.12	.044	.36	.01, .71
Peak BAC Typical Week	5.88	.017	.41	.08, .75	3.24	.074	.32	−.03, .66
Av Number Drinks Heavier[a]	5.82	.017	.41	.07, .74	5.46	.021	.41	.06, .76
Av BAC Heavier[b]	6.85	.010	.44	.11, .78	5.21	.024	.40	.05, .75
AUDIT scores					3.38	.068	.33	−.03, .67
CSAP scores					2.96	.088	.31	−.04, .66

Note. Exact confidence intervals for effect sizes were computed using methods described by Odgaard and Fowler (2010).

[a] Average Number of Drinks in two Heavy Episodes in the prior month.

[b] Average Peak BAC in those two Heavy Episodes in the prior month.

Table 3

Demographics of Participants in Experiment 2.

		Control		Treatment	
Variable		**Frequency**	**Percentage**	**Frequency**	**Percentage**
Gender	Male	23	58%	23	55%
	Female	17	42%	19	45%
Year in School	1	18	45%	14	33%
	2	7	18%	9	21%
	3	5	12%	10	24%
	4	8	20%	8	19%
	5	2	5%	1	2%
Residence	Dorm	13	32%	13	31%
	Greek	4	10%	3	7%
	Off-campus	23	58%	26	62%
Race/ Ethnicity	Asian American			1	2%
	Black	1	3%	1	2%
	Hawaiian/Pacific Islander				
	Mixed race	4	10%	3	7%
	Native American	1	3%	1	2%
	Non-Hispanic White	17	43%	21	50%
	Hispanic/Latino	16	40%	15	36%
Varsity Athlete	Yes	3	8%	2	5%
	No	37	92%	40	95%
		Mean	SD	Mean	SD
Age		20.28	2.09	20.02	1.52

Table 4

Statistical Tests of Treatment Effect Covarying the Baseline Value of the Variable and Between-group Effect Sizes (with 95% Confidence Intervals) in Experiment 2: CDCU vs. Delayed Assessment Control Group

	1 Month Post			
Dependent Variable	***F*(1,78)**	***p***	***d***	**95% C.I.**
Drinks per Week	7.38	.008	.60	.16, 1.05
Peak BAC Typical Week	12.85	.001	.80	.34, 1.25
Av Number Drinks Heavier[a]	16.65	.001	.91	.45, 1.36
Av BAC Heavier[b]	19.12	.001	.97	.51, 1.43

Note. Exact confidence intervals for effect sizes were computed using methods described by Odgaard and Fowler (2010).

[a] Average Number of Drinks in two Heavy Episodes in the prior month.

[b] Average Peak BAC in those two Heavy Episodes in the prior month.