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# The Effects of Personalized Boosters for a Computerized Intervention Targeting College Student Drinking

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# THE EFFECTS OF PERSONALIZED BOOSTERS FOR A

# COMPUTERIZED INTERVENTION TARGETING COLLEGE

# STUDENT DRINKING

by

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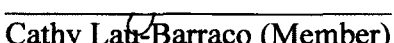Requirements for the Degree of

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# ABSTRACT

## THE EFFECTS OF PERSONALIZED BOOSTERS FOR A COMPUTERIZED INTERVENTION TARGETING COLLEGE STUDENT DRINKING

Abby L. Braitman
Old Dominion University, 2012
Director: James M. Henson

Heavy episodic alcohol use within the college student population is both widespread and problematic (Benton et al., 2004; Core Institute, 2006; Hingson, Zha, & Weitzman, 2009; O'Malley & Johnston, 2002; Perkins, 2002; Singleton, 2007). More than 40% of college students report at least one symptom of alcohol abuse or dependence (Knight et al., 2002). Computerized interventions are widely used because of their advantages over in-person interventions. They are more cost-effective and can quickly deliver tailored individual feedback to more students. Computerized interventions can be administered to large groups of students (e.g., incoming students, athletes, fraternities/sororities). However, a (2007) meta-analysis by Carey and colleagues found that in-person interventions are generally more efficacious than interventions delivered via other mediums.

The current study is a prospective examination of intervention efficacy, the ability of personalized feedback to boost efficacy, and protective behavioral strategies (PBS) as a possible mediator for these relationships. The intervention for the current study, Alcohol 101 Plus™ (Century Council, 2003), incorporates a number of intervention components, including alcohol education, college student drinking norms, skills training, and personalized feedback. The current study sought to improve the efficacy of the online intervention with personalized feedback via email boosters. Content was created

based on a comparison of 2-week data to baseline. Boosters provided personalized feedback based on reported alcohol consumption, alcohol-related problems, and PBS use. They included normative data and emphasized PBS. Data were collected from $N = 233$ college students. Eligibility criteria included drinking 4+ alcoholic drinks within two weeks of the assessment and being between the ages of 18 and 24. Participants were randomized into one of three conditions: 1) control, 2) intervention only, or 3) intervention plus booster. Participants were assessed at baseline (pre-intervention), 2 weeks post, and 4 weeks post. The intervention was administered during the baseline procedure, immediately following assessment. After the 2-week assessment, participants in the intervention-plus-booster condition were sent a booster email.

Piecewise latent growth models revealed no intervention effect among alcohol use indicators or alcohol-related problems. However, knowledge about alcohol and related consequences was significantly increased after the intervention. Interestingly, a significantly indirect effect was found, such that intervention receipt significantly increased growth trajectories for PBS, which in turn was associated with reduced trajectories for alcohol use and related problems. Additionally, the booster emails with personalized feedback had a significant effect. All alcohol use indicators and alcohol-related problems were significantly reduced for those in the experimental booster group. There was limited support for PBS as a mediator of both intervention and booster effects. The implications of these findings are far-reaching, given the prevalence of online interventions targeting college student drinking and the ability of easily-disseminated, cost-effective emails to boost efficacy.

This thesis is dedicated to James John DiOttavio III, my fiancé and best friend. I am grateful for his patience and understanding through this process. I do not know where life will lead me, but I am so happy to experience the adventure with you. Also to my parents, who always supported and encouraged me. They helped me to appreciate early on the value of hard work and academic excellence.

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# CHAPTER 1

# INTRODUCTION

Heavy episodic alcohol use within the college student population is both widespread and problematic (Benton et al., 2004; Core Institute, 2006; Hingson et al., 2009; O'Malley & Johnston, 2002; Perkins, 2002; Singleton, 2007). More than 40% of college students self-report at least one symptom of alcohol abuse or dependence (Knight et al., 2002). There are often many alcohol-related problems associated with frequent alcohol use, ranging from mild (e.g., hangovers, missed classes) to more severe (e.g., DUIs, poor grades, assault, even death; Hingson, Heeren, Winter, & Wechsler, 2005; Hingson et al., 2009; Wechsler et al., 2002).

Computerized interventions targeting alcohol use among college students have been successful at reducing both alcohol consumption and alcohol-related problems (Carey, Scott-Sheldon, Elliott, Bolles, & Carey, 2009). These interventions are very popular among colleges, because they are relatively inexpensive and easily disseminated. For example, over 2,500 institutions use Alcohol 101 Plus, over 550 use e-CHUG, and over 500 use AlcoholEdu® for College, which are three of the most popular computerized interventions (Century Council, 2007; Outside the Classroom, 2010; San Diego State University Research Foundation, 2009; Walters, Miller, & Chiauzzi, 2005). The current project seeks to improve the efficacy of computerized interventions while maintaining the low cost and easy dissemination benefits of this medium.

Brief, delayed follow-up sessions designed to extend the effect of the intervention, called booster sessions, have improved the effect magnitude or duration for interventions targeting smoking cessation, mammograms, caregiver skills, binge eating,

and many other behaviors (e.g., Metz et al., 2007; Schlup, Munsch, Meyer, Margraf, & Wilhelm, 2009; Skinner et al., 2007; Van Camp et al., 2008). Further, they have been used successfully for alcohol use interventions in other populations (Longabaugh et al., 2001) and have been used successfully among college students targeting other behaviors such as physical activity and nutrition (Franko et al., 2008). Despite these successes, prior research has not supported booster efficacy for college student alcohol interventions (Barnett, Murphy, Colby, & Monti, 2007; Caudill et al., 2007). The current project extended research on booster intervention efficacy with computerized interventions targeting college student drinking by improving upon design of the booster session, both in content and delivery method.

In addition to improving and prolonging intervention efficacy, identifying the mechanisms by which interventions effect change is also critical to understanding college student drinking. Researchers have begun to examine protective behavioral strategies (PBS) as a mechanism of change in students who reduce their alcohol consumption and related problems. The results of student use of PBS have been inconsistent, with some studies associating them with reductions in alcohol consumption (Benton, Benton, & Downey, 2006; Martens, Martin, Littlefield, Murphy, & Cimini, 2011; Nguyen, Walters, Wyatt, & DeJong, 2011; Sugarman & Carey, 2007), and others finding consumption is not reduced for those using more PBS (Sugarman & Carey, 2009). However, studies have consistently found that PBS reduce alcohol-related problems (Benton et al., 2006; Benton et al., 2004; Delva et al., 2004; Martens et al., 2011).

The current study investigated if boosters extend the short-lived efficacy of computerized interventions targeting college student alcohol use. And finally, the current

study investigated if PBS functioned as a mediator between computerized experimental manipulations (i.e., intervention status, booster status) and alcohol-related outcomes (i.e., alcohol use and alcohol-related problems).

## Alcohol Use among College Students

Students begin to drink significantly more after they transition into college from high school (Fromme, Corbin, & Kruse, 2008; Nguyen et al., 2011). Screenings during National Alcohol Screening Day revealed that 34-58% of college students engaged in harmful or hazardous drinking[1] (Wallenstein, Pigeon, Kopans, Jacobs, & Aseltine, 2007). In fact, college students are more likely to engage in heavy episodic drinking and to drive under the influence of alcohol as compared to their non-college counterparts of the same age (Hingson, et al., 2009; O'Malley & Johnston, 2002). The proportion of college students engaging in risky drinking behaviors has been somewhat consistent over time (Hingson et al., 2005). From 1999 to 2005, the percentage of college students who reported engaging in episodic drinking in the past month increased from 45% to 50% for students aged 21 to 24 years old and from 39% to 40% for students aged 18 to 20 years old. The percentage of students aged 21 to 24 years old who drove under the influence of alcohol in the past year was fairly constant from 1999 (30%) to 2002 (38%) to 2005 (34%). These statistics remained consistent for students aged 18 to 20 years old (25%, 26%, and 25% respectively). In a 2006 survey of 134 college campuses, 84.1% of students reported drinking alcohol within the past year, and 71.8% of students reported drinking alcohol within the past 30 days (Core Institute, 2006). Male drinkers reported consuming an average of 8.41 drinks per week, whereas female drinkers reported

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[1] "Hazardous drinking" was defined as a score of 8 or higher on the Alcohol Use and Disorders Identification Test (AUDIT; Saunders, Aasland, Babor, Fuente, & Grant, 1993); "harmful drinking" was defined as an AUDIT score of 19 or higher.

consuming an average of 3.62 drinks per week.

**Binge-Drinking**

In addition to increased consumption, frequent binge drinking increased from 19.7% of college students in 1993 to 22.8% in 2001, where frequent binge drinking was defined as three or more occasions of heavy episodic drinking in the previous two weeks (Wechsler et al., 2002). Additionally, 55.0% of students reported engaging in heavy drinking (defined as having five or more drinks in one sitting) within the past two weeks, and 24.0% of students reported engaging in heavy drinking at least three times per week (Core Institute, 2006). Moreover, 31.6% of college students met the criteria for alcohol abuse, and 6.3% met the criteria for alcohol dependency as defined in the Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition (American Psychiatric Association, 1994), with 44.1% of students reporting at least one symptom of either (Knight et al., 2002). For the purposes of the aforementioned studies, heavy episodic drinking, or binge drinking, was defined as 4 or more drinks for women or 5 or more drinks for men within a single occasion (NIAAA, 2002).

**Alcohol-Related Problems**

Because alcohol use is so widespread on college campuses across the United States, alcohol-related problems are prevalent as well, with harm affecting the student drinkers, other students, and even their academic institutions (Perkins, 2002). Between 2001 and 2005, there were approximately 79,000 alcohol-attributable deaths each year in the United States (Centers for Disease Control, 2008). In 2005, there were an estimated 1,357 alcohol-related traffic fatalities for college students aged 18 to 24 years old, and college students are more likely to drive under the influence of alcohol than their non-

college counterparts (Hingson et al., 2009). There were an estimated 468 unintentional injury fatalities unrelated to traffic collisions in 2005 that were due to alcohol use (including alcohol poisoning) for college students aged 18 to 24 years old. This statistic has steadily increased across time from 1999 through 2005 (Hingson et al., 2009).

Whereas some college students experience the severe alcohol-related consequences of physical injury and death, even more college students experience less severe problems. When asked which alcohol-related problems they experienced within the past year, 62.5% of students reported having a hangover, 37.1% reported doing something they later regretted, 32.2% reported getting into an argument or fight, 30.2% reported missing a class, 27.0% reported driving a car under the influence, and 22.1% reported performing poorly on a test or other project (Core Institute, 2006). Students also reported less frequent problems such as being hurt or injured (16.1%), trouble with police or other authorities (13.9%), being taken advantage of sexually (10.1%), taking advantage of another sexually (3.2%), and seriously trying to commit suicide (1.3%). The number of students experiencing alcohol-related problems is on the rise as well. In 1993, 16.6% of students reported experiencing five or more problems within the past 30 days, whereas that number increased to 20.3% in 2001 (Wechsler et al., 2002).

In addition to the above consequences, amount of alcohol consumed is associated with lower grade point averages (GPAs), even after controlling for SAT scores, high school class rank, sex, race, parents' education, parents' income, and athletic status (Singleton, 2007). Moreover, there are a number of costs to academic institutions associated with college student drinking. Besides the aforementioned student grade decline, institutions may incur property damage, student attrition, and legal costs

(Perkins, 2002). Given the pervasiveness of college student drinking and the associated costs to students and institutions, educators and administrators are committed to reducing both alcohol consumption and alcohol-related problems.

## College Student Alcohol Interventions

Educators and administrators originally favored the abstinence approach for addressing college student alcohol use (Beck, 1998). However, the literature has shown that alcohol abstinence programs for college students are largely ineffective, and harm reduction is a much more successful approach for college student drinking (Beck, 1998; Neighbors, Larimer, Lostutter, & Woods, 2006; Witkiewitz & Marlatt, 2006). This is supported by the fact that students who drink heavily and frequently experience a greater proportion of alcohol-related problems than lighter drinkers (Schaus et al., 2009). If students who frequently drink heavily can reduce their drinking to lighter levels, this should have a tremendous impact on alcohol-related problems (Carey et al., 2007; Carey, Scott-Sheldon, et al., 2009; White, 2006).

To better organize evidence of effectiveness and direct future research, the National Advisory Council on Alcohol Abuse and Alcoholism established a Task Force on College Drinking, consisting of educators, alcohol researchers, and students. After examining the literature available, their report recommended alcohol interventions for college drinkers that combine multiple successful components of intervention approaches (NIAAA, 2002). Specifically, they recommend combining motivational interviewing or brief motivational feedback, cognitive-behavioral skills training, and norms clarification, because these strategies have demonstrated repeated success among college students.

A meta-analysis of individual-level alcohol interventions given to college students

revealed that risk reduction interventions were generally efficacious for up to six months (Carey et al., 2007). Participants receiving the interventions immediately reduced their alcohol quantity, frequency of heavy drinking, and peak blood alcohol concentration (BAC). Those reductions were maintained at short-term and intermediate follow-ups in addition to reductions in alcohol-related problems. Over time, the alcohol use reductions diminished (27-195 weeks), but alcohol-related problem reductions were maintained at longer-term follow-ups (Carey et al., 2007).

## Computerized Interventions

Various mediums have been used (e.g., mail, internet, or in-person) in the delivery of brief interventions incorporating feedback and motivational components. These interventions have been generally effective at reducing both drinking and related problems (Neighbors et al., 2006; Walters & Neighbors, 2005; White, 2006). However, in-person interventions have been generally more successful than other mediums (e.g., Carey, Henson, Carey, & Maisto, 2009; Croom et al., 2009; Donohue, Allen, Maurer, Ozols, & DeStefano, 2004; White, Mun, Pugh, & Morgan, 2007). A (2007) meta-analysis by Carey and colleagues found that in-person interventions are generally more efficacious than interventions delivered via other mediums.

Despite these findings, computerized interventions have several advantages over in-person interventions. They are more cost-effective and can quickly deliver tailored individual feedback to more students (Carey, Scott-Sheldon, et al., 2009). Additionally, computerized interventions may be administered as preemptive strategies, whereas in-person interventions rarely are preemptive. In-person interventions can place a strain on expertise, time, and resources if they are to be mass-implemented (Moyer & Finney,

2004). Because they are more expensive and require more resources, colleges are less likely to use in-person interventions unless a student receives an alcohol-related sanction. Therefore, unless they are part of research, students may not receive an in-person intervention until they are referred to treatment for an alcohol-related offense (i.e., already experiencing alcohol-related problems). Conversely, computerized interventions can be administered to large groups of students (e.g., incoming students, athletes, fraternities, and sororities) before they receive sanctions. Additionally, students are accustomed to going online to seek information, indicating that this is a medium with which they are comfortable (Walters et al., 2005). Computerized interventions are comparatively inexpensive and easier to access (Carey, Scott-Sheldon, et al., 2009; Larimer, Cronce, Lee, & Kilmer, 2004).

The intervention for the current study, Alcohol 101 Plus™, incorporates a number of intervention components, including alcohol education, college student drinking norms, skills training, and personalized feedback. Past research has demonstrated that some components may be more effective than others. For example, normative feedback alone results in greater reductions in alcohol use as compared to education alone (Doumas, McKinley, & Book, 2009). However, the combination of components appears to result in the greatest behavior change. Students receiving a combination of motivational interviewing with feedback exhibited significant reductions in drinking compared to students who received only motivational interviewing or only feedback (Walters, Vader, Harris, Field, & Jouriles, 2009). Reviews of the literature found that personalized feedback, particularly when incorporating motivational components and delivered in-person, has been effective at reducing college student alcohol consumption, alcohol-

related problems, or both (Carey et al., 2007; Larimer et al., 2004; Walters & Neighbors, 2005; White, 2006). In addition to incorporating multiple components, Alcohol 101 Plus capitalizes on social learning theory (Bandura, 1977), where participants can observe the behavior of "students" on the virtual campus, and the associated negative consequences of their alcohol use or positive consequences of responsible behavior.

A number of studies support the efficacy of Alcohol 101 (the earlier version of the current program) for reducing alcohol use and/or alcohol-related problems (Barnett et al., 2007; Donohue et al., 2004; Hagman, Clifford, & Noel, 2007; Reis, Riley, Lokman, & Baer, 2000). However, two studies found that Alcohol 101 did not improve outcomes (Lau-Barraco & Dunn, 2008; Sharmer, 2001). There has only been one randomized study examining Alcohol 101 Plus, the newer version of the computerized intervention. Carey, Henson, Carey, and Maisto (2009) found that Alcohol 101 Plus was equally effective as an in-person brief motivational intervention at reducing short-term drinking for male students mandated to treatment. However, female students responded more positively to the in-person intervention.

Generally, computerized interventions have been effective at reducing alcohol consumption compared to control conditions and have been roughly equivalent to other alcohol-targeted content such as alcohol education (Elliott, Carey, & Bolles, 2008). For example, first-year student volunteers who received a computerized intervention reduced their alcohol consumption and alcohol-related problems compared to controls after three months (Doumas & Andersen, 2009). A study comparing an in-person personal feedback intervention, a computerized intervention with identical content, and an assessment-only control condition found that the interventions were equally effective at reducing alcohol

consumption (Butler & Correia, 2009). A meta-analysis of studies including computerized interventions found that they were effective at reducing alcohol consumption and alcohol-related problems compared to control conditions; however, the effect sizes were sometimes smaller than more extensive interventions delivered in-person (Carey, Scott-Sheldon, et al., 2009). These results indicate that students receiving computerized interventions may be ideal targets for additional materials to increase efficacy such as booster sessions.

## Boosters

The use of boosters, or maintenance sessions, as a technique to increase intervention efficacy or prolong the duration of intervention effects is common. It is recommended from the federal level (USDHHS, 1993), and they are considered a key strategy in relapse prevention or intervention maintenance (Sperry, Carlson, & Lewis, 1993). Unfortunately, the efficacy of booster is often not examined. For example, many researchers include boosters in all study conditions, which precludes an examination of booster efficacy (e.g., Monti et al., 2007; Schinke, Schwinn, & Fang, 2010). However, the literature presents a mixed history of booster efficacy.

Whisman reviewed the literature in 1990 and concluded that despite varied results, boosters are generally helpful. He reviewed 26 studies examining boosters for various behavior therapies and found 15 of those studies (58%) significantly improved behavior change. Of the remaining studies, several exhibited a trend in the same direction, but failed to achieve significance.

Since the time of Whisman's (1990) review, evaluations of boosters have revealed positive results in a number of fields relating to behavioral interventions, including dating

violence prevention and reduction (Foshee et al., 2004), assertion training (Baggs & Spence, 1990), parent behavioral training (Van Camp et al., 2008), emotion regulation (Hammond, Westhues, & Hanbidge, 2009), nutrition and physical activity (Franko et al., 2008), couples skills training (Braukhaus, Hahlweg, Kroeger, Groth, & Fehm-Wolfsdorf, 2003), smoking prevention (Dijkstra, Mesters, De Vries, van Breukelen, & Parcel, 1999), and smoking cessation (Metz et al., 2007).

## Booster Implementation

Booster implementation varies greatly in terms of timing after the intervention, number of sessions administered, and medium of communication used. Boosters have consisted of in-person therapy sessions (e.g., Baggs & Spence, 1990; Braukhaus et al., 2003; Van Camp et al., 2008), telephone calls (e.g., Foshee et al., 2004; Metz et al., 2007), interactive websites (e.g., Franko et al., 2008), newsletters (e.g., Foshee et al., 2004), and magazines (e.g., Dijkstra et al., 1999). The timing of the booster session has ranged from days after the original intervention (e.g., Metz et al., 2007), to weeks (e.g., Braukhaus et al., 2003; Franko et al., 2008; Metz et al., 2007), to months (e.g., Baggs & Spence, 1990; Braukhaus et al., 2003; Dijkstra et al., 1999; Van Camp et al., 2008), or even years (e.g., Dijkstra et al., 1999; Foshee et al., 2004; Hammond et al., 2009; Van Camp et al., 2008). Interestingly, timing, number, and medium of sessions seem to be unrelated to efficacy. Longer delays until the booster session are sometimes associated with booster success (e.g., Hammond et al., 2009; Van Camp et al., 2008) and sometimes with booster failure (e.g., Foshee et al., 2004). Single session boosters have met success (e.g., Hammond et al., 2009; Van Camp et al., 2008) and failure (e.g., Franko et al., 2008). Even studies with very similar designs have yielded very different results.

Conflicting data have been obtained from strikingly similar studies. The booster included as part of a dating violence prevention and reduction intervention was found to be completely ineffective (Foshee et al., 2004). However a similar booster targeting smoking prevention was successful (Dijkstra et al., 1999); the smoking rates in the experimental booster group were significantly lower than the rates for those who received only the original intervention. There were a number of similarities between these two studies. The boosters for both studies had similar medium (newspaper versus magazine) and timing (2.5 years versus 9-15 months). In both studies, participants were adolescents in eighth grade at the time of the intervention and were expected to read the booster materials on their own time.

This study comparison suggests that mechanics of booster implementation may not greatly impact booster efficacy. Key factors for booster efficacy are the behavior targeted and the efficacy of the original intervention. If the initial intervention successfully addresses the target behavior and has sustained effects, the booster may not provide any additional value. In the case of the dating violence study, reported physical and sexual dating violence was reduced for everyone who received the intervention, even four years after its administration (Foshee et al., 2004). The booster was unnecessary to extend the length of the effect, because reductions were still present at the latest assessment. These results imply that boosters are unnecessary for interventions with lasting effects, but may add value for target behaviors where interventions yield short-lived results, such as college student drinking.

**Boosters for Alcohol Interventions**

Alcohol use and associated problems are common targets for interventions. These

interventions are often successful, but only for a limited time (Carey et al., 2007; LaBrie et al., 2009). Because the effects of the intervention are short-lived, boosters have the potential to be a valuable addition to an intervention plan (Moyer & Finney, 2004).

Boosters have already been successful in improving the efficacy of some alcohol interventions. Longabaugh and colleagues (2001) examined the efficacy of alcohol interventions administered to individuals admitted at hospital emergency departments. Individuals who met the criteria for eligibility received either standard care or received a brief motivational intervention (BMI) at the hospital targeting alcohol use. Of those who received a BMI, some participants were randomized into receiving a motivational booster 7-10 days after their original hospital visit. All 3 groups (standard care, BMI, and BMI plus booster) had fewer heavy drinking days 12 months after the visit. However, the booster group also reduced alcohol-related problems and alcohol-related injuries. There were no significant group differences on heavy drinking. This demonstrates that boosters can help reduce the harmful effects of alcohol use.

Although boosters have been associated with improved outcomes, we cannot declare that they unequivocally and effectively extend intervention efficacy. It is possible that when boosters are voluntary, this association with improved outcomes is due to other factors such as a higher commitment to change. For example, McCrady, Epstein, and Kahler (2004) administered interventions to men with alcohol problems who were committed to female partners. Participants received either alcohol behavioral couples therapy paired with relapse prevention techniques, alcohol behavioral couples therapy paired with encouraging Alcoholics Anonymous (AA), or alcohol behavioral couples therapy alone. The group receiving relapse prevention techniques learned these

techniques during four booster sessions after the original intervention. Despite the fact that booster session attendance was significantly associated with abstinence within that group, there were no significant differences in alcohol use between conditions 18 months after treatment. It is possible that instead of booster sessions changing alcohol behaviors, those already committed to changing their drinking patterns exhibited this commitment both by maintaining abstinence as well as through their attendance of booster sessions. The booster sessions were a mechanism to demonstrate commitment to change rather than the session impacting the behavior.

Research suggests that individuals may react differently to boosters geared toward improving intervention efficacy. In a study targeting drinking reduction for heavy-drinking women, all women receiving treatment had reduced consumption 18 months later (Connors & Walitzer, 2001). However, boosters to the original treatment (either eight boosters over the course of six months or an additional seven hours of life-skills training during the original intervention or both extensions combined) yielded additional reductions in women who were heavier drinkers at the start of the study. These additional reductions were still observed 30 months after the original intervention (Walitzer & Connors, 2007).

Intervention efficacy also impacts booster efficacy. Interventions that do not influence the target behavior have no effects to extend through the use of booster sessions. For example, substance use prevention was examined among seventh-graders, with the intervention targeting tobacco, alcohol, and marijuana use (Botvin, Baker, Filazzola, & Botvin, 1990). The original intervention was led by either the students' teacher or by their peers. Some schools were randomized to additionally administer ten

booster sessions one year later by the same type of facilitator. Researchers found the students who had peer-led interventions followed by peer-led boosters had the best results for all outcomes (tobacco, alcohol, and marijuana use). Surprisingly, students who had teacher-led interventions followed by boosters fared as poorly as students in the assessment-only condition on most outcomes. This demonstrates how boosters based on ineffective interventions do not necessarily provide additional value.

**College Student Population**

Some populations may be more susceptible to boosters. Alcohol interventions are increasingly popular at the college level. Boosters have also been administered with this population, with mixed results. Unfortunately, some studies suffer from methodological weaknesses that inhibit assessment of booster efficacy, such as weak intervention effects or missing key constructs in assessment.

Barnett and colleagues (2007) examined the efficacy of boosters for both BMIs and computerized interventions. Participants were students mandated to treatment, and the booster was administered one month after the treatment. It consisted of an additional 25 minutes of the original intervention (either BMI or Alcohol 101). Although the number of drinking days was reduced three months after the intervention (and baseline levels were reported pre-sanction drinking), by one year after the intervention drinking had returned to pre-sanction levels and even increased for some outcomes. Booster sessions did not significantly impact outcomes. However, it is possible that the sanction event that mandated students to treatment reduced drinking and related consequences, and so extensions of the interventions were ineffective because the interventions themselves were ineffective. Because both treatment and sanctions were events that

occurred after the data reported at baseline and before the follow-up assessment, it is impossible to disentangle their effects.

Many studies using mandated students do not use assessment-only control groups, precluding an evaluation of the interventions (Barnett & Read, 2005). White, Mun, and Morgan (2008) conducted one of the few randomized-controlled trials with mandated students that included a delayed-treatment control group. Students reduced drinking overall, but there were no group differences at the follow-up assessment, indicating that receiving a sanction is a stronger motivator to change than any intervention given to students. A study by Fromme and Corbin (2004) with a similar design yielded similar results, providing further evidence that mandated students are motivated to reduce their consumption due to the alcohol-related sanction, not interventions. Carey, Henson, Carey, and Maisto (2009) conducted the only study to date whose results support intervention efficacy for mandated students. By assessing both pre-sanction and post-section drinking before the intervention, they were able to extricate the effects of the sanction from their intervention. Although drinking was reduced for both interventions (Alcohol 101 and BMI), they found that only the students who received a brief motivational intervention reduced drinking beyond the effects of the sanction. Based upon the findings of these studies, students are naturally reducing drinking as a result of sanctions, and interventions with these mandated students have limited efficacy. Boosters of interventions given under these conditions may not add much value, as demonstrated by Barnett and colleagues (2007).

Additional studies examining booster sessions for college drinking interventions have suffered from other limitations. In another study where college students received

alcohol-related boosters, alcohol risk-reduction skills training was administered to multiple chapters of a national fraternity (Caudill et al., 2007). Chapters were assigned to one of three conditions: a skills training intervention, the same intervention plus two booster sessions, or assessment only. At the appropriate chapters, 1.5-hour booster sessions were offered at five and eleven months after the original intervention. These sessions were similar to the original intervention. Although the intervention reduced alcohol consumption at six months follow-up, consumption increased to original levels 12-18 months after the intervention. Additionally, boosters were ineffective at reducing alcohol consumption beyond the original intervention. However, booster attendance dwindled as expected with only 79% of participants who attended the original intervention attending at least one booster session. Additionally, only consumption variables were measured; alcohol-related problems were not assessed. It is possible that even though students' consumption returned to original levels, booster recipients were able to maintain reduced risk by modifying their behavior to avoid alcohol-related problems. The current study incorporated booster sessions for select participants in the form of emails reminding them of strategies that can be use to protect themselves from alcohol-related problems. These may help reduce alcohol-related problems with weaker effects on alcohol consumption.

Although booster sessions for interventions targeting college student drinking have not yielded desirable results thus far, the current study improves upon the design and assessment of previous research. The study by Barnett and colleagues (2007) administered interventions and boosters to mandated students. In that case, the effect of the sanction was likely stronger than either the intervention or the booster, yielding non-

significant results. When interventions and boosters were given to members of

fraternities, booster attendance was voluntary and alcohol-related problems were not

assessed (Caudill et al., 2007). In the current study, students were not mandated to

treatment so the intervention may have a stronger effect, and boosters were delivered via

email without requiring them to attend additional appointments with project staff (though

it is possible that students did not read the full email). Additionally, alcohol-related

problems were assessed to capture if students are reducing the harmful effects of alcohol

use without actually reducing use.

## Protective Behavioral Strategies

Although improving and extending intervention efficacy is an important goal in

the current study, it is not sufficient for understanding how interventions effect behavior

change. Determining mechanisms behind why interventions work is also critical to

understanding college student drinking. Protective behavioral strategies (PBS) are

techniques students can use to slow or reduce overall alcohol consumption and related

problems (Sugarman & Carey, 2007). The strategies include selective avoidance of

riskier behaviors (e.g., taking shots of liquor, funneling or shot-gunning beer), strategies

to reduce the impact of alcohol on the body (e.g., eating before and during drinking,

drinking slowly), and alternatives to alcohol use (e.g., finding other ways besides

drinking to reduce stress). The current study examined PBS as a potential mechanism of

change in students who reduce their alcohol consumption and related problems.

Consistent with the harm reduction approach, PBS focuses on drinking reduction

and drinking responsibly, rather than abstinence. Whereas some protective strategies

target abstaining from alcohol (e.g., choosing to participate in enjoyable activities that do

not include alcohol consumption), most strategies are techniques for reducing

consumption (e.g., alternating alcoholic and nonalcoholic beverages, limiting cash before

going out to drink), and thus focus on reducing harm (Sugarman & Carey, 2007). PBS

are consistent with the cognitive-behavioral approach of providing skills to resist social

pressures and reduce harm (Botvin & Wills, 1985). Past research has yielded generally

positive results for PBS, with most studies associating strategy use with reductions in

alcohol consumption (Benton et al., 2006; Benton et al., 2004; Martens, Ferrier, &

Cimini, 2007; Martens et al., 2005; Martens et al., 2008; Martens et al., 2011; Martens,

Pederson, LaBrie, Ferrier, & Cimini, 2007; Nguyen et al., 2011; Ray, Turrisi, Abar, &

Peters, 2009; Sugarman & Carey, 2007), and only one study finding consumption was not

reduced (Sugarman & Carey, 2009).

Studies have consistently associated PBS with reductions in alcohol-related

problems (Benton et al., 2006; Benton et al., 2004; Delva et al., 2004; Martens, Ferrier, et

al., 2007; Martens et al., 2005; Martens et al., 2008; Martens, Pederson, et al., 2007;

Martens et al., 2004; Ray et al., 2009). Interestingly, one study found gender differences

in the relationship between PBS and alcohol-related problems (Delva et al., 2004). For

men, age, race, and Greek affiliation accounted for all shared variance between PBS use

and alcohol-related problems; however, for women, higher PBS use was significantly

associated with reduced alcohol-related problems beyond sociodemographic variables.

Benton and colleagues (2006; 2004) confirmed that PBS serves as a protective factor

against alcohol-related problems by examining the interaction between PBAS and alcohol

use on related problems. There was a significant interaction such that there was a stronger

relationship between alcohol use and related problems among those who used PBS less

frequently and a weaker relationship among those who used PBS more frequently.

There is limited evidence of PBS as a mediating mechanism between known predictors of drinking and either alcohol use or alcohol-related problems. PBS was a mediator between depressive symptoms and alcohol-related problems such that individuals with more symptoms had less PBS use, which was associated with more alcohol-related problems (Martens et al., 2008). PBS was a mediator between social and enhancement drinking movies and the outcomes of alcohol use and alcohol-related problems (Martens, Ferrier, et al., 2007). Higher motive endorsement was associated with lower PBS use, which was associated with greater consumption and alcohol-related problems. PBS was a mediator between age of drinking onset and the outcomes of use and problems such that individuals who started drinking earlier used fewer PBS and thus had increased use and problems (Palmer, Corbin, & Cronce, 2010). Lastly, in the most relevant example, Barnett and colleagues (Barnett et al., 2007) found that PBS was a mediator between BMI receipt and reductions in alcohol volume. PBS did not mediate the relationship between Alcohol 101 and alcohol use; however, that is an outdated version of the current intervention. These studies demonstrate that PBS does sometimes serve as the mediating mechanism between antecedents and alcohol use and alcohol-related problems. The predictors in these studies were associated with the behavior of PBS use, and PBS use was associated with alcohol use and alcohol-related problems. For the current study, I hypothesized that PBS mediated the relationship between experimental manipulations and the outcomes of alcohol use and alcohol-related problems, such that individuals impacted by the interventions (and boosters) engaged in more PBS use, which would lead to reduced alcohol use and alcohol-related problems.

## Hypotheses

For this project, I evaluated the ability of follow-up booster sessions to increase the efficacy of computerized interventions, and examined PBS as a mechanism of change in the outcomes of alcohol use and alcohol-related problems. I proposed the following:

1. Alcohol 101 Plus would be successful in reducing drinking and alcohol-related problems among college students.

    a. Individuals who received the intervention would report less consumption and fewer alcohol-related problems than individuals in the control group two weeks after the intervention.

2. The duration of the effect of an easily-disseminated computerized intervention would be improved by adding follow-up boosters, where efficacy is evidenced by reduced drinking and alcohol-related problems.

    a. Individuals who received boosters would maintain reduced consumption and alcohol-related problems longer than individuals in the intervention-only group.

3. Protective behavioral strategies would mediate the intervention and booster effects on alcohol consumption and problems.

    a. Individuals who received the intervention would use PBS more than individuals in the control group, and more PBS use would be negatively associated with alcohol consumption and alcohol-related problems.

    b. Individuals who received booster emails would use PBS more than individuals who did not, and more PBS use would be negatively associated with alcohol consumption and alcohol-related problems.

# CHAPTER 2

# METHOD

## Participants

Participants were undergraduate college students. Data were collected across four academic semesters, with participation being open only to psychology students for the first two semesters and open to non-psychology students as well as psychology students for the final two semesters. Participants received course credit for baseline participation. For each follow-up survey, students received course credit or entry into a weekly raffle for a $25 gift card. If more than one credit option was available to participants, they were able to select which form of compensation they preferred.

Baseline data were collected from $n = 652$ students, with $n = 84$, $n = 72$, $n = 447$, and $n = 49$ from each semester. Note that the first two semesters were open only to psychology students. The third and fourth semesters were open to students regardless of discipline, hence the increase in participation. Data collection terminated early in the fourth semester to allow time for data analysis. However, $n = 230$ (35.28%) individuals did not meet the criteria of having four or more alcoholic drinks within the prior two weeks, leaving a reduced sample of $n = 422$. Additionally, $n = 15$ (3.56%) participants did not meet the age criteria of 18 to 24 years and were eliminated from the sample. Of the remaining $n = 407$ students in the sample, $n = 15$ (3.69%) did not complete the intervention. Reasons for not completing the intervention were: 1) internet connectivity issues, 2) server problems for the intervention website, and 3) participants voluntarily ending early. Analyses focusing on baseline data only used this sample of $n = 392$. Once they completed the baseline assessment, participants were randomized into one of three

conditions: 1) a control group that received a health education session unrelated to alcohol use (Pos Or Not), 2) an intervention-only group, and 3) an intervention-plus-booster group. After the first two semesters of data collection, assessing intervention efficacy was de-prioritized, thus participants were no longer assigned to the Pos Or Not control group. Within the group of eligible cases who completed the baseline procedure, $n = 159$ (40.6%) participants did not complete any follow-up surveys, resulting in a final sample of $n = 233$ for analyses examining follow-up data. Because data from the two-week follow-up was necessary to generate the booster email, participants assigned to the intervention-plus-booster group who did not complete the first follow-up were unable to receive the booster, and thus were excluded from all follow-up analyses. The flow of participant recruitment, elimination, and random assignment is shown in Figure 1. For students who met eligibility criteria (i.e., four or more alcoholic drinks in the past two weeks, between the ages of 18 and 24, and completed the baseline assessment), the sample was mostly female ($n = 255$; 65.1%), mostly Caucasian or White ($n = 235$; 59.9%) or African-American or Black ($n = 87$; 22.2%), and fairly evenly distributed across class standing with the exception of a small proportion of seniors ($n = 36$; 9.2%). See Table 1 for the full breakdown across demographic information.
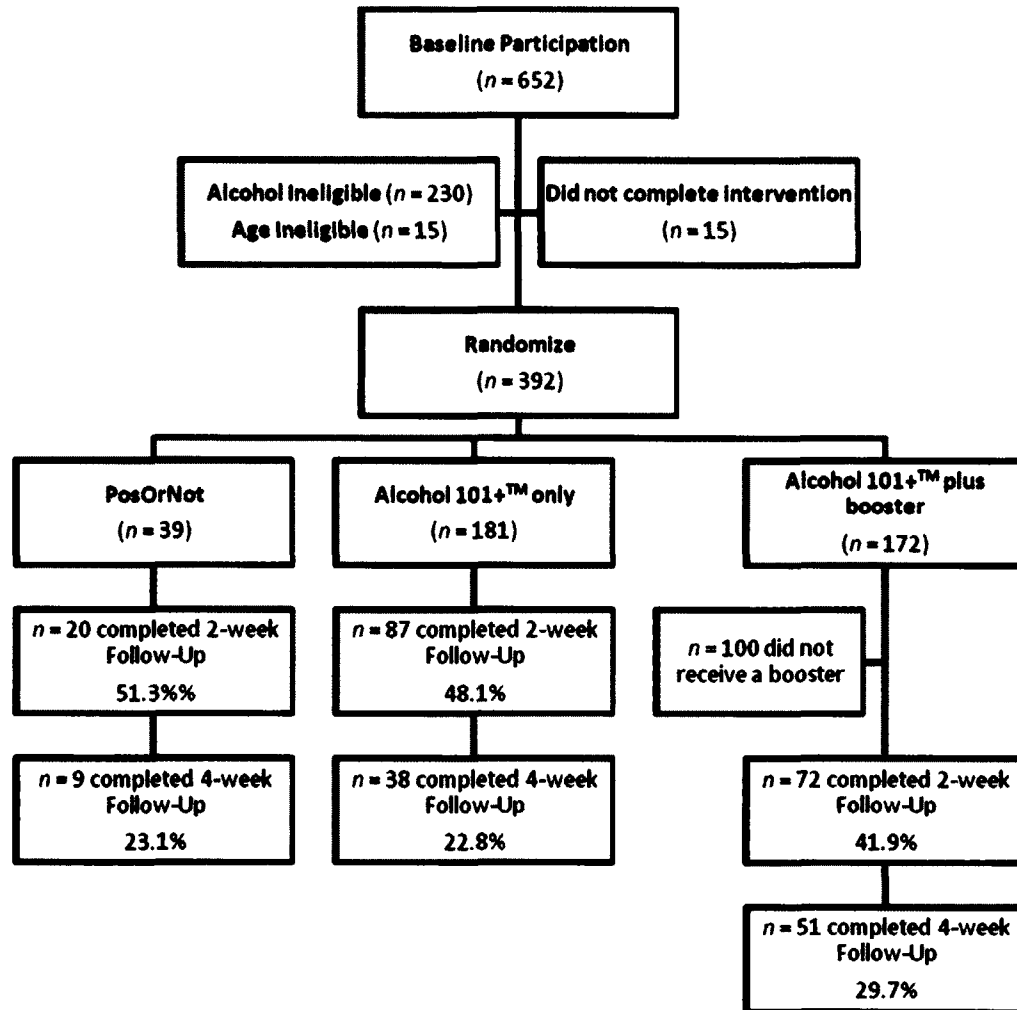
*Figure 1.* Participant flow and numbers from baseline participation through the final follow-up assessment. Note that participants in the Alcohol 101 Plus + booster group who did not complete the 2-week follow-up consequently did not receive the booster and were thus excluded from analyses involving follow-up data.

Table 1

*Participant Characteristic Frequencies*

| Membership Group | *N* | % |
|---|---|---|
| Gender | | |
|     Female | 255 | 65.1 |
|     Male | 136 | 34.7 |
| Race | | |
|     Caucasian or White | 235 | 59.9 |
|     African-American or Black | 87 | 22.2 |
|     Asian or Pacific Islander | 24 | 6.1 |
|     Latino or Latina | 18 | 4.6 |
|     Native American | 2 | 0.5 |
|     Other | 24 | 6.1 |
| Class Standing | | |
|     Freshman | 145 | 37.0 |
|     Sophomore | 114 | 29.1 |
|     Junior | 92 | 23.5 |
|     Senior | 36 | 9.2 |
|     Other | 5 | 1.3 |
| Student Status | | |
|     Full-time | 379 | 96.7 |
|     Part-Time | 10 | 2.6 |
| Residence | | |
|     On-campus dormitory | 172 | 43.9 |
|     On-campus living-learning community | 20 | 5.1 |
|     Off-campus house or apartment | 150 | 38.3 |
|     Greek-affiliated residence (fraternity/sorority) | 1 | 0.3 |
|     With family | 45 | 11.5 |
|     Other | 3 | 0.8 |
| Greek Membership | | |
|     Non-member | 332 | 84.7 |
|     Currently pledging | 16 | 4.1 |
|     Member | 44 | 11.2 |
| Athletic Status | | |
|     Athlete on ODU team | 30 | 7.7 |
|     Not an ODU athlete | 360 | 91.8 |
| Marital Status | | |
|     Single | 282 | 71.9 |
|     In a committed relationship | 93 | 23.7 |
|     Married | 11 | 2.8 |
|     Divorced | 2 | 0.5 |
|     Other | 4 | 1.0 |
| Received Formal Treatment | | |
|     No | 373 | 95.2 |
|     Yes | 18 | 4.6 |

## Materials

### Alcohol 101 Plus™

The majority (90.1%) of participants received Alcohol 101 Plus, an intervention developed by the Century Council (2003). This is an online intervention designed to be implemented to a large number of students either individually or in facilitated discussion groups (e.g., all incoming students, all athletes, all students associated with Greek organizations). The program is a fusion of several components including alcohol education, personalized feedback, attitude-focused strategies, and skills training.

**Intervention navigation.** Alcohol 101 Plus depicts a virtual campus where students can select various campus locations on the map or on the sidebar of the website. There is no avatar or guide, and students can explore the virtual campus at their own pace. Each campus location varies in both the type and method of information presented. Intervention information is disseminated into 1 of 11 campus locations: an administration building, a first-year student residence, an upperclassman student residence, Greek Row, a student union, a kiosk of flyers for commuters, a library, a popular restaurant frequented by athletes, a section on how alcohol affects the brain, a section about D.U.I.'s, and a virtual bar. Some of the virtual locations offer additional options to explore (e.g., a kiosk with flyers they can select, a party with characters they can select, the brain with possible lobes to select).

Users do not follow one linear path; instead, participants choose where on "campus" to start, and different paths can lead to the same information. Mode of information dissemination varies throughout the intervention. In the administration building module the only method of conveying information is through written text and

photos, but other modules contain videos of public service announcements and personal testimonials by real people. Additionally, there are videos of fictional characters portrayed by actors who represent different types of students one might encounter on a real campus.

Participants actively engage in the program rather than passively receive information. The program allows users to follow the fictional characters through typical college situations for which students must decide whether or not to drink, to play drinking games, to drink and drive, to call 911, etc. The characters encounter several decision points along the way, and participants will select one option which then impacts the storyline for that character. Participants may also return to the decision point and select different options to see how the storyline for that character is altered.

**Intervention content.** Alcohol 101 Plus provides an assortment of content, and each campus location contains information or strategies relevant for that location. Topics include information regarding students' own alcohol use, information regarding other students' alcohol use, and possible negative consequences associated with their own use or others. It also presents strategies for how to reduce their drinking, how to stay safe while drinking, and how to be a responsible host. It models how students could refuse drinks or how they could intervene when they see friends behaving irresponsibly.

For example, the administration building contains a number of "flyers" students can select to get information regarding medical emergencies, risky sex avoidance, national rates of college student drinking, phrases they can use to turn down a drink, strategies they can use to pace their drinking, factors that affect the body's absorption of alcohol, physical effects of alcohol use, information about how alcohol affects the brain,

myths about sobering up, a checklist of possible alcohol-related problems they may have experienced (or may in the future), and a link to rules and regulations for alcohol use for their campus. This module contains educational components (e.g., factors affecting absorption of alcohol, how alcohol affects the brain), a normative component (e.g., national college drinking data), and skills components (e.g., how to turn down a drink, how to handle a medical emergency). See Appendix A for a complete list of topics.

**The virtual bar.** At the virtual bar participants provide basic information such as sex, weight, and state of residence so that the program can provide tailored information on blood alcohol concentration (BAC) as well as state regulations regarding legal limits. Participants select type of beverage to consume and how quickly to consume it. The program provides updated BACs based upon their choices as well as how long their body should take to process the alcohol out of their system. The virtual bar is a unique module that allows students to get immediate tailored feedback for possible drinking scenarios.

**Pos or Not**

Developed by MTV Networks on Campus, Inc. (2009), Pos or Not is an interactive intervention for college student HIV/AIDS education. Similar to Alcohol 101 Plus, it is an online program accessed via the internet. The program presents users with a picture of an individual accompanied by brief information regarding who they are (e.g., their age, their occupation, a song on their playlist). Based on the picture and the information provided, participants select whether they believe the individual is HIV positive or not. After their selection, the program provides the user with more information regarding the individual (e.g., what that individual wants to share with the world, what they used to think about HIV, if they've been stigmatized). If the individual

was HIV positive the program describes how the individual contracted HIV and how they discovered their positive status. If the individual was HIV negative the program offers strategies for HIV prevention. This is repeated for multiple individuals.

The first section of the Pos or Not intervention dispels stereotypes regarding HIV positive individuals. It is designed to demonstrate that one cannot guess if an individual is HIV positive simply by viewing a picture and seeing some basic information. The second component of the Pos or Not intervention educates students about HIV. After revealing the individual's HIV status, the intervention provides information regarding HIV prevention and protection. After each set of six individuals, the student must select to continue participating in the intervention or to quit. The intervention is not designed to last a particular length of time, though for the purposes of the current study, they were instructed to navigate through the intervention for 60 minutes.

**Protective Behavioral Strategies**

Protective Behavioral Strategy use was assessed using a modified version of Sugarman and Carey's (2007) Protective Behavioral Strategies scale. Participants answered 21 items using a modified 12-point interval rating scale indicating the frequency of strategy use in the previous 2 weeks: *None, 1 time, 2 times, 3 times, 4 times, 5 times, 6 times, 7 times, 8 times, 9 times, 10 times, more than 10 times* (see Appendix B). The original scale used a 6-point rating scale indicating the frequency of strategy use: *none, once, 2-3 times, 4-5 times, 6-10 times,* or *more than 10 times.* The scale consists of 3 dimensions: *Selective Avoidance* (e.g., not participating in drinking games, not doing shots); *Strategies while Drinking* (e.g., eating before and while drinking, limiting cash); and *Alternatives* (e.g., finding other ways besides drinking to reduce stress). Composite

scores were made for each subscale by summing the responses of relevant items; the total score composite was made by summing all items. Internal consistency was adequate for all three subscales across all three timepoints. For *selective avoidance*, $\alpha = .83$, $\alpha = .91$, and $\alpha = .95$ for baseline, time 2, and time 3 respectively. For *strategies while drinking*, $\alpha = .90$, $\alpha = .94$, and $\alpha = .95$ for baseline, time 2, and time 3 respectively. For *alternatives*, $\alpha = .80$, $\alpha = .88$, and $\alpha = .96$ for baseline, time 2, and time 3 respectively. Finally, for *total PBS*, $\alpha = .93$, $\alpha = .95$, and $\alpha = .97$ for baseline, time 2, and time 3, respectively.

Because the response scale reflects number of times the strategy was used, an increase for this raw score could reflect higher PBS use proportionate to frequency of drinking, but could also reflect the same proportionate use of PBS but more drinking episodes. To tease out proportionate PBS use, the raw PBS scores were divided by the number of drinking days, resulting in a score that reflects amount of PBS use controlling for frequency of drinking, where higher scores reflect using PBS more often while drinking, even if not drinking more often.

**Alcohol Use**

Participants' alcohol use was assessed using a modified version of the Daily Drinking Questionnaire (Collins, Parks, & Marlatt, 1985). Participants completed a grid indicating how many drinks they consumed on each day over the past 2 weeks, where a drink is defined as a 12-ounce bottle or can of beer, a 5-ounce glass of wine or wine cooler, a 1.5-ounce shot of hard liquor, such as rum, gin, vodka, or whiskey straight or in a mixed drink, or similar portion of alcohol (Dufour, 2001). They also indicated how many hours passed during each drinking occasion. A total alcohol quantity score was created by summing drinks reported across the grid. Additionally, participants described

their drinking in the past 2 weeks, including how many days they drank to the point of being intoxicated and on how many days they engaged in heavy drinking (i.e., five or more drinks for men and four or more drinks for women; Wechsler, Dowdall, Davenport, & Rimm, 1995). For their heaviest drinking day, participants were also asked how many hours passed during the drinking occasion to determine their BAC (see Appendix C). BAC was estimated using the following formula:

$$\left(\left(drinks/2\right)\times\left(GC/weight\right)\right)-\left(0.016\times hours\right)$$

where *drinks* = number of standard drinks consumed, *hours* = number of hours over which the drinks were consumed, *weight* = weight in pounds, and *GC* = gender constant (9.0 for women, 7.5 for men; Matthews & Miller, 1979).

**Alcohol-Related Problems**

Alcohol-related problems were assessed using the Brief Young Adult Alcohol Consequences Questionnaire (B-YAACQ; Kahler, Strong, & Read, 2005). The B-YAACQ consists of 24 items assessing a single dimension of negative consequences, and respondents indicate with a dichotomous response whether they experienced each consequence within with past 2 weeks (see Appendix D). The consequences listed range from mild (e.g., did embarrassing things or had a hangover) to more severe (e.g., had problems with interpersonal relationships or neglected obligations). Using item response theory (IRT), Kahler, Strong, and Read (2005) demonstrated that items capture a wide range of consequence severity and discriminate sufficiently across participants. The scale performed equally well across men and women.

Kahler, Hustad, Barnett, Strong, and Borsari (2008) demonstrated that the instrument has adequate internal consistency ($\alpha$ = .84-.89) and test-retest reliability (*r* =

.70). They also found that the measure reflected changes in drinking, indicating it would reflect changes to due alcohol interventions. Similar to the study by Kahler and colleagues (2008), I modified the timeframe being assessed. The original scale assesses behaviors over the past year (Kahler et al., 2005); however, it was previously used to assess the previous 30 days (Kahler et al., 2008), and I assessed the previous 2 weeks rather than the past year. The scale score was created by summing the dichotomous items. Internal consistency was adequate across all three timepoints: $\alpha = .82$, $\alpha = .87$, and $\alpha = .85$ for baseline, time 2, and time 3, respectively.

**Alcohol Knowledge**

Knowledge about alcohol use was assessed with 16 items pulled from the Alcohol 101 Plus website. Questions assessed information such as how alcohol affects the body and the brain, legal limits and consequences, and signs of alcohol poisoning. Examples of items include (with correct answers in bold type): "A woman of the same weight as a man can become intoxicated with smaller amounts of alcohol due to: *a) differences in body composition, b) her menstrual cycle, c) she's taking birth control pills, d) she's on a low calorie diet, e) all of the above*;" "What is the legal level for driving while intoxicated in this state, if the person is over 21 years? *a) .02, b) .08, c) .10, d) 1.0, e) 8.0*;" "This section of the brain, _____, controls your ability to reason and problem solving skills. It also controls your judgment and ability to inhibit the expression of behavior. Therefore, drinking excessively will impair your ability to "self check" and maintain self control. *a) cerebellum, b) vestibular system, c) hypothalamus, d) temporal lobe, e) frontal lobes*." A composite score was created by summing the number of correct answers. This scale has not previously been used with research. Internal consistency was

low across time: $\alpha = .43$ for baseline, $\alpha = .49$ for week 2 assessments, and $\alpha = .48$ for week 4 assessments. Further exploration indicated the scale was not unidimensional.

**Boosters**

Booster emails contained normative feedback indicating how many ODU students drink less than the target student (based upon data from our research lab) and reminders of PBS they can use to protect themselves from alcohol-related problems. Additionally, boosters included feedback based upon the previous assessment. If participants reduced their drinking or related problems, the feedback was positive in nature (congratulating them; Appendix F). If participants failed to reduce their drinking or problems, the feedback was negative in nature (urging them to try harder to reduce their drinking and related problems). This tailoring was done to strengthen the message of the email (encouraging reductions in drinking and problems) and to reinforce the impression of a personalized email written by a real person. Emails appeared to come from a research coordinator of the same gender (female: Abby Braitman; male: Edward Johnson).

**Demographics and General Information.**

During the initial assessment, participants reported their age, race, sex, Greek affiliation (i.e., membership in fraternities or sororities), GPA, class standing, student status (full- versus part-time), residential status, relationship status, height, and weight (see Appendix E).

<div align="center">

**Procedure**

</div>

**Initial Assessment**

Participants scheduled their participation time through a computerized participation pool or by signing up during their non-psychology course for an available

timeslot. Participants were also able to sign-up or reschedule by emailing the researcher directly.

Upon arrival, participants were directed to 1 of 20 personal computers. Because data were collected in a computer lab setting, a partition was constructed behind each monitor to minimize the possibility of participants viewing the screens of participants in other rows. Additionally, research assistants provided participants with headphones to minimize disruption from other computers. Participants completed a computerized assessment at the beginning of their first appointment that assessed alcohol use, PBS, alcohol-related problems, alcohol knowledge, and demographics measures. Upon completion of the survey, participants were randomly assigned by gender to one of three possible conditions: a control condition that received a health intervention unrelated to alcohol use (i.e., Pos Or Not), an intervention-only condition that received the Alcohol 101 Plus intervention, or an intervention-plus-booster condition that received the Alcohol 101 Plus intervention plus a personalized booster email after their week-two assessment.

After completing the initial assessment, participants in the experimental groups were then directed to navigate through the Alcohol 101 Plus program for 60 minutes, whereas participants in the control group were asked to navigate through Pos or Not for 60 minutes. As participants navigated through their interventions, research assistants ensured that participants did not go off-task (e.g., viewing other websites or opening other programs). However, they did not monitor or record which sections of the intervention each participant visited. After each participant completed their assigned intervention, they completed a post-test knowledge assessment online. This is in addition to the one they completed during the initial survey.

**Subsequent Assessments**

Approximately two and four weeks after the initial assessment, participants

received an email reminding them that they are eligible for follow-up surveys. This

email included a link to an online follow-up survey that assessed alcohol use, PBS,

alcohol knowledge, and alcohol-related problems for the past two weeks. Participants

were asked to complete that assessment in a timely manner, were informed that the

weekly raffle would be held at 4:00 on Friday so they would need to complete the survey

by that time to be entered, and were reminded that they would be able to complete a

maximum of two follow-up assessments. Two days after the original email, a second

email was sent reminding them to complete the survey if they have not yet done so. At

that time, participants were also contacted by the secondary means of communication

they provided in the initial survey (i.e., alternate email address or text message).

Participants indicated their communication preference in the baseline assessment.

**Boosters**

Approximately one to two days after the second assessment (i.e., two weeks after

the intervention), participants in the experimental booster group received an additional

email that served as a booster to the original intervention. Data from week two were

compared to data from baseline to determine which booster (positive or negative) the

participant should receive. Participants whose scores for alcohol-related problems or

total drinks consumed were reduced at week two received the positively worded email.

Participants whose scores did not decrease for either construct received the negatively

worded email. Participants who did not receive a booster email received a neutral email

thanking them for their participation in the study and reminding them that there would be

another follow-up assessment in approximately two weeks (Appendix F). Regardless of whether students received a booster email with personalized feedback or a neutral email reminding them of the final follow-up assessment, the bottom of the email sent included a link. Participants were asked to confirm receipt of their email by clicking a link at the bottom of the page. This link opened a separate survey in which students were asked to type in their name. This served as a manipulation check to record who read the emails sent to them.

**Confidentiality**

Students provided identifying information for three purposes: (1) to facilitate linking their data across timepoints, (2) to contact and credit participants in non-psychology courses, and (3) to facilitate creating a personal connection for the booster experimental group. To protect participants, files with identifying information were kept only on an encrypted external storage device, and that device was kept in locked storage location when not in use. Only the primary researcher possessed keys to the storage location. Additionally, a certificate of confidentiality from the National Institutes of Health was obtained by the researcher.

# CHAPTER 3

# RESULTS

## Case Elimination

Of the original $n$ = 652 participants, cases were eliminated from the sample for

not meeting the alcohol use criteria of consuming at least four alcoholic drinks within the

prior two weeks ($n$ = 230), or not being between 18 and 24 years of age ($n$ = 15). Cases

were also eliminated for individuals failing to complete the intervention ($n$ = 15). The

remaining cases ($n$ = 392) were from the desired population of college drinkers 18 to 24

years of age.

## Missingness

**Missingness within a timepoint.** Data were examined for missingness. Missing

items within a timepoint were imputed with several exceptions. If more than 20% of

items for a multi-item measure were missing within a case, missing values were not

imputed and those cases were excluded from relevant analyses. Additionally, variables

with nominal response scales (e.g., race, residence, marital status) did not have missing

values imputed. Because alcohol knowledge was assessed with multiple choice items

(nominal in nature) and missingness likely indicated not knowing the answer, missing

items were considered wrong answers and recoded as such. Finally, missingness was not

examined for the B-YAACQ or drinks per day, because missingness for an individual

item would be associated with non-endorsement (i.e., no drinks or no problems

experienced). The number of missing values within each measure are shown in Table 2,

and the results of missingness is indicated (e.g., imputation or not). The number of

values listed as missing for PBS and alcohol knowledge exclude those for whom the

entire measure is considered missing. For single item continuous outcomes with missing

values, all missing data were imputed with the exception of GPA. Because GPA had

such a large proportion of missingness ($n$ = 198; 50.5%), missing values were not

imputed.

Table 2

*Missingness within Timepoint by Measure*

| Measure (and Items) | # of Missing Values | Result of Missingness |
|---|---|---|
| Baseline | | |
| PBS Items (21 items) | 51 | Imputed (no cases >20%) |
| Alcohol Knowledge (15 items)* | 18 | Imputed (no cases >20%) |
| Number of Drinking Days (1 item) | 0 | |
| Days Intoxicated (1 item) | 1 | Imputed |
| Heavy Drinking Days (1 item) | 0 | |
| Drinks on Highest Drinking Day (1 item) | 0 | |
| BAC on Highest Drinking Day (1 item) | 0 | |
| GPA (1 item) | 198 | Not imputed (missing > 50%) |
| Age (1 item) | 0 | |
| | | |
| Follow-ups (both 2-week and 4-week) | | |
| PBS Items (21 items) | 154 | Imputed (except 6 cases >20%) |
| Alcohol Knowledge (15 items)* | 73 | Imputed (except 1 case >20%) |
| Number of Drinking Days (1 item) | 0 | |
| Days Intoxicated (1 item) | 2 | Imputed |
| Heavy Drinking Days (1 item) | 0 | |
| Drinks on Highest Drinking Day (1 item) | 0 | |
| BAC on Highest Drinking Day (1 item) | 0 | |

*Missingness for these items was treated as "wrong answer".

*Imputation*. Many different methods exist for addressing missing data. Methods

such as completer analysis or last-observation-carried-forward are outdated and often

yield biased results (Hedeker & Gibbons, 2006). Popular imputation methods that rely on single imputations such as mean substitution, regression-based imputation, pattern matching, and hot deck imputation all artificially reduce error variance by over-fitting the data (Kline, 2005). In contrast, expectation maximization (EM) imputation maintains the original error rate and does not over-fit the data. It is an iterative method based on maximum-likelihood estimation, and it incorporates adding error to each imputed value based on the original error rate of the data (Keppel & Wickens, 2004; Kline, 2006). EM imputation was used for all missing data replacement.

**Attrition.** It is common for participants to miss entire timepoints in studies incorporating longitudinal data collection. Occasionally, participants will miss a timepoint, but complete a subsequent assessment. Alternatively, once a participant misses a timepoint it is common for them to miss all later assessments. This phenomenon is known as participant attrition. Participants who missed follow-up assessments did not have values imputed, and thus did not contribute data for that assessment period. However, it is important to know why the participant missed the timepoint, or more specifically, if their missingness is related to either observed values or to the unobserved value they would have had at that assessment. Knowing why the data were missed determines what kinds of analyses can be conducted.

*Missing data classification.* One possibility is that missing timepoints are related to the observed value for the data at other timepoints (e.g., participants with missing data at time 2 reported higher levels of drinking at time 1). If participant missingness (attrition) is related to their drinking levels at observed timepoints, the data are considered missing at random (MAR; Rubin, 1976). This was assessed by creating a

dichotomous variable for each follow-up assessment. Participants who had missing data for that timepoint were assigned a score of 1, and participants who had observed data were assigned a score of 0. Associations were examined between these new dichotomous missingness variables and the corresponding data at the first assessment using $t$-tests (for continuous variables) and chi-squares (for categorical variables). This is a simplified form of pattern-mixture modeling (Hedeker & Gibbons, 1997; Little, 1993).

Missingness at either follow-up (40.6% for week two and 68.4% for week four) was not significantly related to alcohol quantity, number of drinking days, number of drinks on highest drinking day, number of days intoxicated, number of heavy drinking days, alcohol-related problems, or knowledge about alcohol, as indicated by $t$-test results displayed in Table 3. However, missingness was significantly related to BAC on highest drinking day for the two-week follow-up and the four-week follow-up. Surprisingly, those who missed the first follow-up had significantly lower BACs ($M = .13$, $SD = 0.10$) than those who completed it ($M = .16$, $SD = 0.12$), $t(388) = 2.00$, $p = .046$, Cohen's d = 0.10. Additionally, those who missed the second follow-up had significantly lower BACs ($M = .14$, $SD = 0.11$) than those who completed it ($M = .16$, $SD = 0.12$), $t(388) = 2.12$, $p = .035$, Cohen's d = 0.11. However, after controlling for gender these differences disappear for both the first follow-up, $F(1, 386) = 2.38$, $p = .124$, partial $\eta^2 = .006$, and the second follow-up, $F(1, 386) = 1.25$, $p = .264$, partial $\eta^2 = .003$.

Table 3

*Relationship of Baseline Values with Attrition at Follow-Ups*

| Baseline Measure | Follow-up | df | t | p |
|---|---|---|---|---|
| Quantity | Week 2 | 390 | -0.18 | .857 |
| | Week 4 | 390 | -0.08 | .939 |
| Alcohol-Related | Week 2 | 390 | -0.14 | .891 |
| Problems | Week 4 | 390 | -0.21 | .835 |
| Number of | Week 2 | 390 | -0.97 | .334 |
| Drinking Days | Week 4 | 390 | 0.48 | .631 |
| Days Intoxicated | Week 2 | 390 | -0.85 | .396 |
| | Week 4 | 390 | -0.22 | .824 |
| Number of | Week 2 | 390 | 0.59 | .558 |
| Heavy Drinking Days | Week 4 | 390 | -0.42 | .674 |
| Number of drinks on | Week 2 | 390 | 1.01 | .315 |
| Highest Drinking Day | Week 4 | 390 | 0.11 | .912 |
| BAC on | Week 2 | 390 | 2.00* | .046 |
| Highest Drinking Day | Week 4 | 390 | 2.12* | .035 |
| Alcohol Knowledge | Week 2 | 390 | -0.26 | .792 |
| | Week 4 | 390 | -1.60 | .111 |
| PBS: Total | Week 2 | 390 | -0.25 | .804 |
| | Week 4 | 390 | 0.09 | .931 |

*Note.* All alcohol consumption outcomes are for the timeframe of the past two weeks. BAC = blood alcohol concentration; PBS = protective behavioral strategies.
*$p < .05$.

The significant relationship between missingness and BAC on highest drinking day indicates that data are MAR. Alternatively, if the missing data had been independent of its own unobserved value and of the observed values at other timepoints, then it would have been considered missing completely at random (MCAR; Rubin, 1976). Structural equation modeling is possible with both MAR and MCAR data with the use of maximum

likelihood estimation, yielding unbiased parameter estimates with sufficient power

(Kline, 2005; Newman, 2009).

If timepoints had been missed because of the unobserved values that would be

recorded at that timepoint (e.g., a participant is ashamed to report their drinking has

increased) above and beyond the relationship to their observed value at an earlier

timepoint, then those data would be classified as missing not at random (MNAR; Rubin,

1976) and would be considered nonignorable. If analyses are conducted with MNAR

data, results are often biased (Hedeker & Gibbons, 2006). There is no definitive test to

assess if data are MNAR as opposed to MAR. However, if I suspected my data were

MNAR, I could have explored these with a series of sensitivity analyses. If these

analyses supported my suspicions, I would have abandoned the proposed analyses and

instead pursued approaches described by Hedeker and Gibbons (2006). This includes

incorporating missingness into the model. However, these analyses greatly reduce power

and are less desirable. Additionally, because of the strong correlations between the same

variable at each timepoint ($r = .57$ to $.71$ for alcohol quantity, $r = .57$ to $.60$ for alcohol-

related problems, and $r = .48$ to $.63$ for number of drinking days), it is likely that the

unobserved outcome would be strongly related to the observed outcome and the

assumption of MAR is sufficient.

## Data Cleaning

Each variable was examined for outliers. Boxplots were chosen over standard

deviations for this examination, because standard deviations are themselves influenced by

extreme cases whereas boxplots are not. Values more than three interquartiles ranges

beyond the center interquartile range were considered extreme scores and were

Windsorized, or reduced (or increased) to a value slightly larger (or smaller) than the most extreme value not identified as an outlier, still maintaining rank among scores (Barnet & Lewis, 1994). For baseline assessments, no outliers were found for days consumed alcohol within the past two weeks, alcohol-related problems, alcohol knowledge, grade point average (GPA), PBS strategies while drinking, PBS alternatives, or PBS total. However, 2 values were Windsorized for days intoxicated out of the past two weeks, 3 values were altered for heavy drinking days out of past two weeks, 1 value was altered for number of drinks on highest drinking day, 1 value was altered for BAC on that highest drinking day, 8 values were altered for sum of drinks consumed across the two weeks, and 2 values were altered for the PBS subscale of selective avoidance.

For the follow-up assessments, no outliers were found for number of days consumed alcohol in the past two week, number of days intoxicated, number of heavy drinking days past two weeks, number of drinks in highest drinking day past two weeks, BAC on highest drinking day, alcohol-related problems, PBS strategies while drinking, PBS alternatives, or PBS total. However, for the two-week follow-up data, 4 outliers were identified for the sum of drinks consumed, and 4 values were altered for PBS selective avoidance. For four-week follow-up data, 1 value was changed for alcohol knowledge. And finally, for the alcohol knowledge post-test immediately following the intervention, 4 values were increased.

## Normality

Because bias-corrected bootstrapped confidence intervals are used to assess significance for the main analyses, the assumption of normality is not essential. However, whereas normal distributions are not necessary for the main analyses, a number

of *t*-tests and ANOVAs were conducted to explore missingness and determine covariates, so the assumption of normality needed to be met for those analyses. Histograms as well as skewness and kurtosis estimates were examined for each continuous variable to assess normality. Histograms did not display any highly non-normal distributions (e.g., bimodal data), nor did estimates indicate extreme skewness or kurtosis.

Bivariate normality was also examined. Scatterplots were created for each continuous predictor with each dependent variable (e.g., PBS dimension with alcohol outcomes). No distinctly non-linear relationships were observed, so no variable transformations were necessary. Means and standard deviations for alcohol-related measures can be seen across time and by assignment in Table 4. Additionally, Figure 2 illustrates the means for alcohol quantity, and Figure 3 illustrates the means for alcohol-related problems, with error bars representing the standard error of the mean.

Table 4

*Means and Standard Deviations for Alcohol-Related Measures by Assignment*

| Measure | Min | Max | Baseline | | Week 2 | | Week 4 | |
|---|---|---|---|---|---|---|---|---|
| | | | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| Alcohol Quantity | | | | | | | | |
| Pos Or Not | 0.0 | 77.00 | 19.33 | 16.24 | 14.85 | 14.53 | 9.78 | 9.78 |
| Alc101 only | 0.0 | 86.60 | 25.79 | 21.65 | 18.61 | 18.09 | 17.18 | 15.18 |
| Alc101+bstr | 0.0 | 86.20 | 19.75 | 15.84 | 13.18 | 13.81 | 10.26 | 13.13 |
| Alcohol-Related Problems | | | | | | | | |
| Pos Or Not | 0.0 | 20.0 | 5.49 | 4.07 | 4.10 | 3.85 | 2.44 | 3.00 |
| Alc101 only | 0.0 | 21.0 | 6.09 | 4.29 | 4.56 | 4.98 | 3.45 | 3.73 |
| Alc101+bstr | 0.0 | 21.0 | 4.69 | 3.79 | 2.77 | 3.09 | 1.99 | 3.01 |
| Number of Drinking Days | | | | | | | | |
| Pos Or Not | 0.0 | 10.0 | 3.95 | 1.75 | 3.20 | 2.42 | 2.56 | 2.30 |
| Alc101 only | 0.0 | 14.0 | 4.50 | 2.26 | 3.28 | 2.31 | 3.63 | 2.95 |
| Alc101+bstr | 0.0 | 14.0 | 3.74 | 2.17 | 2.77 | 1.99 | 2.09 | 2.17 |

Table 4 (continued)

| Measure | Min | Max | Baseline | | Week 2 | | Week 4 | |
|---------|-----|-----|----------|----|--------|----|--------|----|
| | | | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| **Days Intoxicated** | | | | | | | | |
| Pos Or Not | 0.0 | 6.0 | 2.13 | 1.26 | 1.65 | 1.76 | 1.44 | 1.88 |
| Alc101 only | 0.0 | 9.0 | 2.41 | 2.02 | 1.78 | 1.81 | 1.42 | 1.37 |
| Alc101+bstr | 0.0 | 8.5 | 1.97 | 1.74 | 1.23 | 1.40 | 1.11 | 1.52 |
| **Heavy Drinking Days** | | | | | | | | |
| Pos Or Not | 0.0 | 6.0 | 2.26 | 1.37 | 1.96 | 1.70 | 1.11 | 1.62 |
| Alc101 only | 0.0 | 11.0 | 3.02 | 2.38 | 2.14 | 2.15 | 2.37 | 2.17 |
| Alc101+bstr | 0.0 | 11.0 | 2.50 | 2.13 | 1.67 | 1.91 | 1.22 | 1.77 |
| **Number of Drinks on Highest Drinking Day** | | | | | | | | |
| Pos Or Not | 0.0 | 18.0 | 7.18 | 3.26 | 5.30 | 3.56 | 3.67 | 3.64 |
| Alc101 only | 0.0 | 27.0 | 7.85 | 4.70 | 6.14 | 4.75 | 6.26 | 4.81 |
| Alc101+bstr | 0.0 | 28.0 | 6.94 | 4.03 | 5.40 | 3.88 | 3.81 | 4.09 |
| **BAC on Highest Drinking Day** | | | | | | | | |
| Pos Or Not | 0.00 | 0.38 | 0.15 | 0.09 | 0.12 | 0.10 | 0.10 | 0.12 |
| Alc101 only | 0.00 | 0.51 | 0.15 | 0.11 | 0.11 | 0.10 | 0.13 | 0.12 |
| Alc101+bstr | 0.00 | 0.58 | 0.14 | 0.11 | 0.10 | 0.09 | 0.07 | 0.09 |
| **PBS: Selective Avoidance** | | | | | | | | |
| Pos Or Not | 0.0 | 69.0 | 18.41 | 16.38 | 17.85 | 18.40 | 13.00 | 13.59 |
| Alc101 only | 0.0 | 66.0 | 14.82 | 14.71 | 10.36 | 13.52 | 8.26 | 11.50 |
| Alc101+bstr | 0.0 | 71.2 | 15.17 | 14.07 | 13.55 | 17.11 | 13.68 | 19.75 |
| **PBS: Strategies While Drinking** | | | | | | | | |
| Pos Or Not | 0.0 | 110.0 | 41.64 | 24.52 | 25.67 | 23.34 | 17.78 | 19.22 |
| Alc101 only | 0.0 | 110.0 | 35.47 | 23.82 | 22.45 | 23.90 | 16.87 | 20.25 |
| Alc101+bstr | 0.0 | 110.0 | 34.34 | 25.38 | 23.13 | 25.75 | 21.24 | 27.91 |
| **PBS: Alternatives** | | | | | | | | |
| Pos Or Not | 0.0 | 44.0 | 21.54 | 12.95 | 19.00 | 13.59 | 18.44 | 12.89 |
| Alc101 only | 0.0 | 44.0 | 18.23 | 10.79 | 14.13 | 11.53 | 12.25 | 9.81 |
| Alc101+bstr | 0.0 | 44.0 | 19.00 | 12.35 | 13.83 | 12.59 | 12.72 | 12.40 |
| **PBS: Total** | | | | | | | | |
| Pos Or Not | 0.0 | 231.0 | 81.80 | 48.14 | 62.77 | 46.31 | 49.22 | 37.48 |
| Alc101 only | 0.0 | 199.0 | 68.53 | 43.24 | 46.95 | 43.50 | 37.38 | 37.46 |
| Alc101+bstr | 0.0 | 222.0 | 68.55 | 45.17 | 50.65 | 50.58 | 47.64 | 56.40 |
| **Knowledge** | | | | | | | | |
| Pos Or Not | 6.0 | 15.0 | 10.05 | 2.06 | 10.65 | 2.06 | 11.22 | 1.56 |
| Alc101 only | 3.0 | 16.0 | 10.31 | 2.10 | 12.01 | 2.15 | 12.45 | 1.98 |
| Alc101+bstr | 2.0 | 16.0 | 9.93 | 2.07 | 11.74 | 2.05 | 12.25 | 1.90 |

*Note.* All outcomes are for the timeframe of the past two weeks. Alc101 = Alcohol 101 Plus; bstr = personalized booster email; BAC = blood alcohol concentration; PBS = Protective Behavioral Strategies.
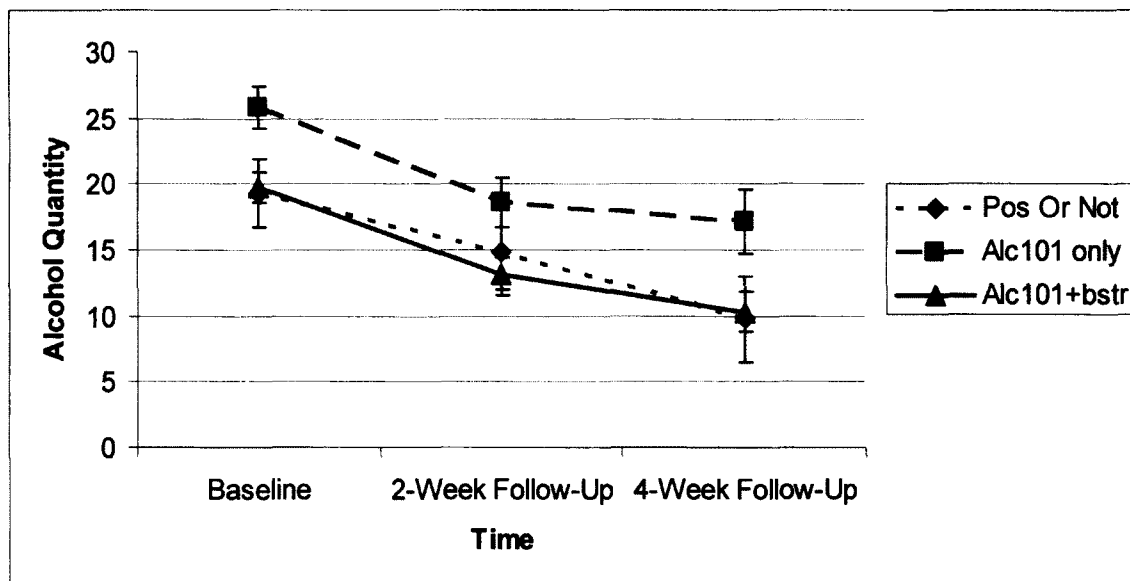
*Figure 2.* Mean alcohol quantity across time by assignment. Alc101 = Alcohol 101 Plus; Alc101+bster = Alcohol 101 Plus combined with booster. Note that error bars represent the standard error of the mean for that group at that timepoint.
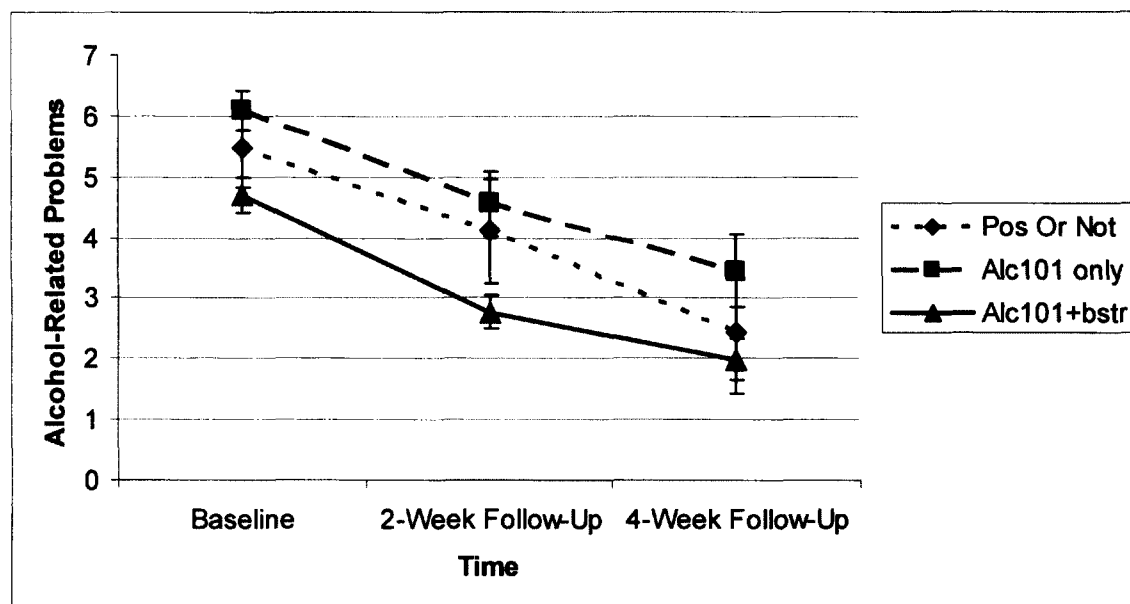


*Figure 3.* Mean Alcohol-Related Problems across Time by Assignment. Alc101 = Alcohol 101 Plus; Alc101+bster = Alcohol 101 Plus combined with booster. Note that error bars represent the standard error of the mean for that group at that timepoint.

**Covariates**

In order to clearly explicate the effects of the intervention and boosters from additional outcome variance, known influences on alcohol use were examined for their predictive value, and potential covariates that significantly predicted outcomes in the current data were controlled for in analyses. A list of potential covariates was created from demographic variables and from the literature. Students who live on campus tend to drink more than students who live off campus, especially with family (O'Hare, 1990; White, Fleming, Kim, Catalano, & McMorris, 2008), whereas living in a residential learning community on campus serves as a protective factor for drinking (Cranford et al., 2009) so residence type was explored as a potential covariate. Greek affiliation (i.e., being in a fraternity or sorority) is also a strong predictor of alcohol use and related problems in previous research (Knight et al., 2002; Scott-Sheldon, Carey, & Carey, 2008; Wechsler et al., 2002). Additionally, fraternity and sorority members are more likely to be diagnosed with alcohol abuse or dependence (Knight et al., 2002). In addition, demographic and academic variables such as sex, race, age, marital status, GPA, year in school, student status (i.e., full-time versus part-time), and history of formal treatment for alcohol use were explored.

Correlations (for continuous variables) and ANOVAs (for categorical variables) were conducted exploring the association of possible covariates with the main outcomes of quantity of alcohol and alcohol-related problems. One structural equation model was conducted for the time-varying covariate of spring break, with parameter estimates constrained to equality to assess the overall effect of spring break. Only covariates that had significant predictive value were included in later analyses; alpha was set at .10 for

these analyses to ensure any relevant covariates were included. Note that "significance" refers to $\alpha = .05$ for all other analyses, and the more liberal alpha level was only used for decisions of covariate inclusion. For the continuous variables, GPA was significantly associated with alcohol quantity, $r(192) = .132, p = .067$, but not alcohol-related problems, $r(192) = .011, p = .876$. Age was not significantly associated with alcohol quantity, $r(390) = -.059, p = .246$, or alcohol-related problems, $r(390) = -.051, p = .314$. The time-varying covariate of spring break did not significantly predict alcohol quantity, unstandardized $b = 1.49$, 95% CI [-3.93, 6.62], $ns$, or alcohol-related problems, $b = 0.85$, 95% CI [-0.70, 2.68], $ns$. As seen in Table 5, residence, Greek membership (i.e., fraternities and sororities), race, gender, and history of formal treatment for alcohol use were significant predictors, whereas student status, class (i.e., year in school), athletic status, and marital status were not. All significant categorical covariates were then dummy coded, with the largest group receiving a code of $0$. Multinomial variables were collapsed into two groups, with the discrepancy in outcome means determining group membership. The final coding for gender was $1 = male, 0 = female$; Greek affiliation: $1 = current member or pledging, 0 = not a member$; residence: $1 = Greek-affiliated residence$ (e.g., fraternity house)[2], $0 = all else$; race: $1 = African-American or Black, 0 = all other races$; formal alcohol treatment: $1 = received formal treatment, 0 = did not receive formal treatment$.

To examine multicollinearity, all possible covariates were included in a simultaneous regression predicting each alcohol outcome at baseline. This was repeated

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[2] Because residence may have been strongly related to Greek affiliation (with members living in associated houses), both covariates were included simultaneously to predict alcohol quantity and alcohol-related problems. Both covariates were still significant predictors of alcohol-related problems. It seems living in a Greek-affiliated residence significantly impacts problems above and beyond Greek membership alone.

with intervention status and booster status included as predictors as well. For all

analyses, tolerance ranged between .898 and .971, indicating high proportions of each

variable are independent of the other predictors, or that multicollinearity is very low

(Cohen, Cohen, West, & Aiken, 2003). For all analyses with covariates, the model was

first analyzed including race, gender, residence, Greek membership, GPA, and history of

formal treatment; covariates that did not significantly ($\alpha$ = .10) predict any outcome

slopes were eliminated, and the models were re-analyzed with the remaining covariates.

Table 5

*Impact of Potential Covariates on Alcohol Quantity and Related Problems at Baseline*

| Covariate | Outcome | df | F | p |
|---|---|---|---|---|
| Residence | Alcohol Quantity | 5, 385 | *3.37** | *.005* |
|  | Problems | 5, 385 | 1.54 | .178 |
| Student Status | Alcohol Quantity | 1, 387 | 1.00 | .318 |
|  | Problems | 1, 387 | 0.74 | .392 |
| Greek | Alcohol Quantity | 2, 389 | 2.94[†] | .054 |
|  | Problems | 2, 389 | 2.58[†] | .077 |
| Race | Alcohol Quantity | 5, 384 | *5.90** | *.000* |
|  | Problems | 5, 384 | 1.87[†] | .099 |
| Class | Alcohol Quantity | 4, 387 | 0.12 | .976 |
|  | Problems | 4, 387 | 0.95 | .434 |
| Athlete | Alcohol Quantity | 1, 388 | 1.26 | .262 |
|  | Problems | 1, 388 | 0.20 | .657 |
| Gender | Alcohol Quantity | 1, 389 | *24.06** | *.000* |
|  | Problems | 1, 389 | 1.64 | .201 |
| Marital Status | Alcohol Quantity | 4, 387 | 1.14 | .339 |
|  | Problems | 4, 387 | 0.20 | .939 |
| Treatment | Alcohol Quantity | 1, 389 | 3.01[†] | .084 |
|  | Problems | 1, 389 | *8.94** | *.003* |

*Note.* Significance at the .05 level is denoted with bold, italic text, whereas significance at the .10 level is denoted with bold text. *$p$ < .05, [†] < .10.

**Baseline Equivalence**

Baseline equivalence in outcomes across group assignment was also examined.

As shown in Table 6, a series of ANOVAs revealed that group assignment (i.e., *Pos or*

*Not control*, *Alcohol 101 Plus only*, or *Alcohol 101 Plus and booster*) was significantly

related to the continuous outcomes at baseline of alcohol quantity, number of drinking

days, alcohol-related problems, and number of heavy drinking days. It was not related to

number of days intoxicated, highest number of drinks, BAC on highest drinking day,

knowledge, or any dimensions of PBS. Specific group means can be found in Table 4.

These differences at baseline are accounted for in hypothesis-testing models by including

the effect of group assignment on intercepts for the outcomes, and allowing these

intercepts to correlate with growth.

Table 6

*Baseline Equivalence across Assignment*

| Outcome | df | F | p |
| --- | --- | --- | --- |
| Alcohol Quantity | 2, 389 | 5.17* | .006 |
| Number of Drinking Days | 2, 389 | 3.98* | .019 |
| Alcohol-Related Problems | 2, 389 | 5.27* | .005 |
| Number of Days Intoxicated | 2, 389 | 2.56 | .079 |
| Number of Heavy Drinking Days | 2, 389 | 3.39* | .035 |
| Highest Number of Drinks | 2, 389 | 2.04 | .131 |
| BAC on Highest Drinking Day | 2, 389 | 0.48 | .616 |
| Alcohol Knowledge | 2, 389 | 1.49 | .228 |
| PBS: Selective Avoidance | 2, 389 | 0.99 | .374 |
| PBS: Strategies while Drinking | 2, 389 | 1.41 | .246 |
| PBS: Alternatives | 2, 389 | 1.29 | .277 |
| PBS: Total | 2, 389 | 1.55 | .213 |

## Analysis Choice

Data were analyzed using piecewise growth models conducted within the larger framework of structural equation modeling (SEM) using Mplus (version 6.1; Muthén & Muthén, 1998-2010). Although it was possible to conduct select analyses using simpler models and software (e.g., repeated measures ANOVA or split-plot ANOVA in SPSS), I chose to use a SEM framework for multiple reasons: 1) the third hypothesis involves examining mediation which is streamlined through SEM, especially when including covariates; 2) because the third hypothesis is tested through SEM, testing all hypotheses through SEM facilitates comparisons across analyses; and 3) most outcomes are operationalized in an intuitive metric (e.g., number of drinks or number if drinking days), and parameter estimates in the SEM framework allow interpretation of these effects in terms of number of drinks or number of drinking days (as compared to ANOVA or ANCOVA where the metric is not incorporated in the estimates).

Additionally, all analyses were bootstrapped with $n = 1000$ replications. Bootstrapping facilitates the creation of empirical confidence intervals which can be used to test significance without assuming a normal underlying distribution. For each parameter, $n = 1000$ estimates are created; the confidence intervals mark the middle 95% (or other percent associated with chosen alpha) of these estimates. Intervals not containing zero are considered significant at the chosen alpha level. All significance tests reported from the SEM framework relied on the use of 95% bias-corrected confidence intervals (for $p < .05$), or 99% bias-corrected bootstrapped confidence intervals (for $p < .01$), where the bias-correction is an adjustment for bias in the central tendency of the parameter estimate (MacKinnon, Lockwood, & Williams, 2004).

Despite the use of a SEM framework, model fit is not presented for any analyses conducted for two reasons: 1) the current hypotheses are focused on specific relationships (e.g., does the intervention impact growth in problems, or does PBS growth impact problem growth?), not an overarching theory of alcohol use; and 2) many models were just-identified, meaning that the number of estimated parameters equaled the number of available degrees of freedom; consequently, fit indices would falsely indicate perfect fit (Kline, 2010).

## Hypothesis 1: The Intervention Effect

To test Hypothesis 1 that Alcohol 101 Plus reduced alcohol use and alcohol-related problems, I assessed the model depicted in Figure 4. This model was conducted for each alcohol outcome as well as on overall consumption (a latent variable with individual use variables as indicators) to test if the growth of alcohol outcomes from baseline to week two is significantly impacted by the intervention. A piecewise growth model was conducted, with factor loadings fixed to 1 for the intercept and set to 0 (for baseline) and 1 (for week two) for the slope factor. Intervention status was coded as 1 = *received the Alcohol 101 Plus intervention*, 0 = *did not receive intervention* (e.g., navigated Pos or Not). This model was run for each alcohol use outcome, both with and without relevant covariates.

The hypothesis that alcohol use and related problems would be reduced after receiving the Alcohol 101 Plus intervention was assessed with the path marked with "H1" in Figure 4. A significant negative path would support the hypothesis and indicate that the trajectories for students who received the intervention were significantly lower (indicating drinking less, having fewer drinking days, or experiencing fewer alcohol-

related problems) than students who did not receive the intervention. This model includes all experimental conditions. The parameter estimates for this path for each alcohol outcome model are shown in Table 7, which indicates that the intervention did not significantly impact growth trajectories for alcohol quantity, alcohol-related problems, number of drinking days, number of days intoxicated, highest number of drinks, or BAC on highest drinking day. However, the growth slope for knowledge about alcohol was significantly higher and the growth slope for number of heavy drinking days was significantly lower for those who received the Alcohol 101 Plus intervention.

The intervention effect was also tested on overall alcohol consumption using a multiple indicator growth model. As shown in Figure 5, a latent variable was constructed for each timepoint, with each alcohol consumption variable at that timepoint as an indicator of the factor. The factor loadings were fixed to 1 for the alcohol quantity indicators, and the factor loadings for each of the other outcomes were constrained to equality across timepoints, as were their intercepts. The constrained factor loadings are indicated with "a" through "e" in the figure. A piecewise growth model was added, with factor loadings fixed at 1 for the intercept and fixed at 0 (for baseline) and 1 (for week two) for the slope factor. Similar to the model for individual outcomes, the effect of the intervention on the growth slope was assessed with the path marked "H1," which was not significant as indicated by bias-corrected bootstrapped 95% confidence intervals.
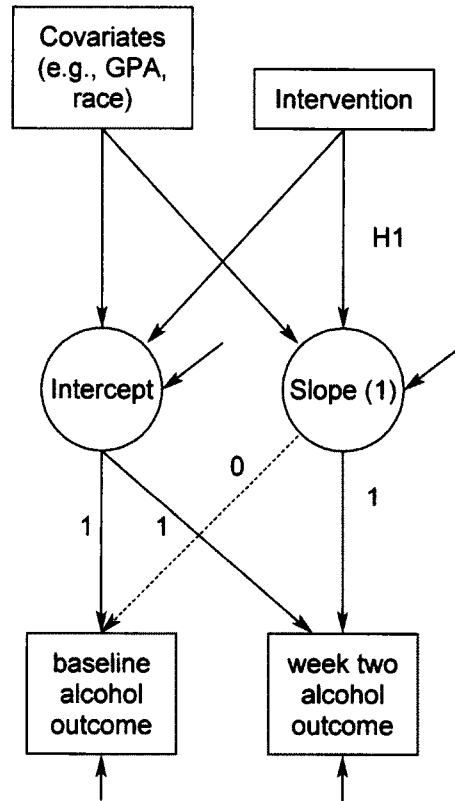
*Figure 4.* Piecewise growth model assessing Hypothesis 1: The intervention effect

(specific outcomes modeled separately). Intervention was coded as *0 = Pos Or Not*

*control, 1 = Alcohol 101 Plus.*
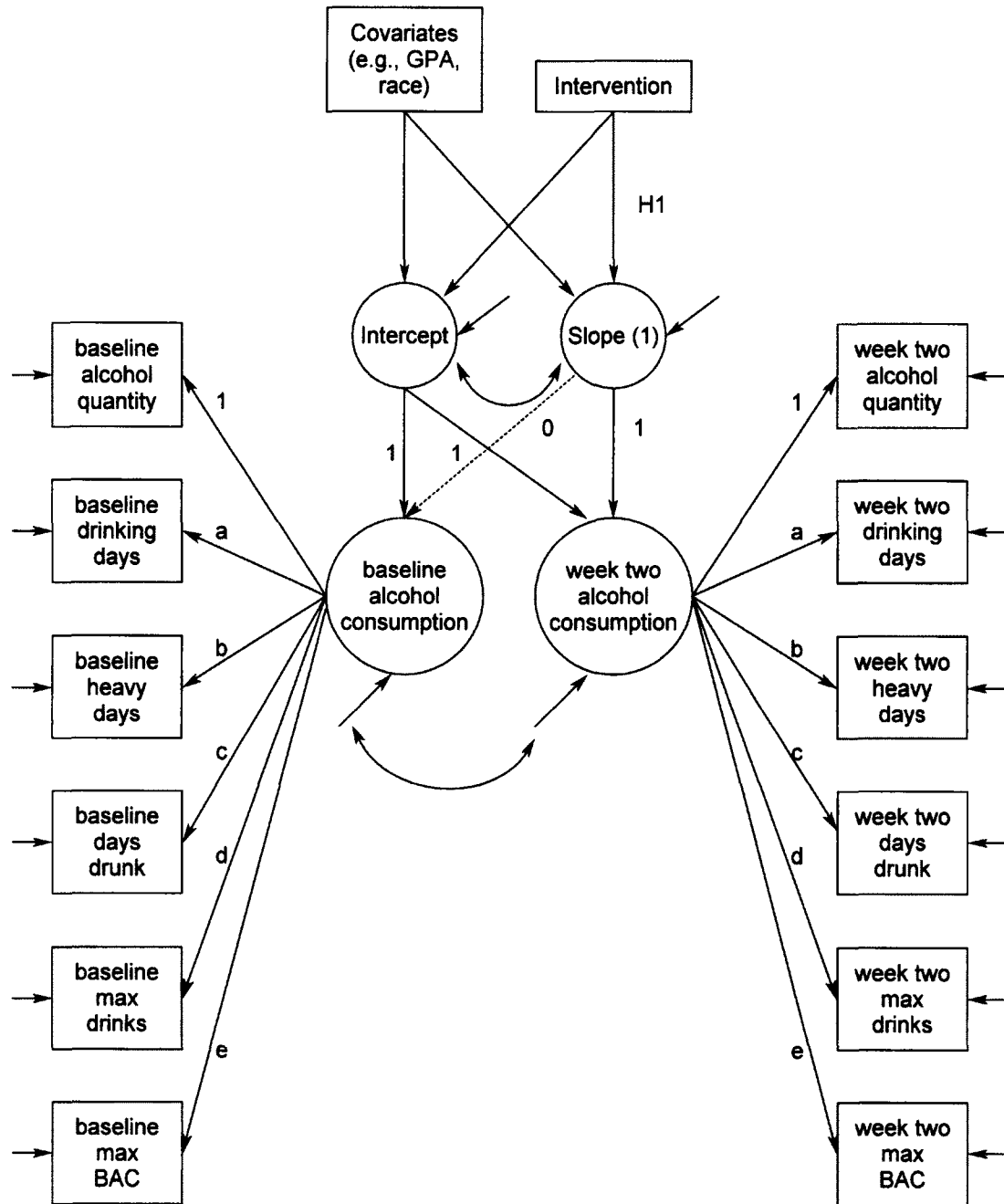
*Figure 5.* Multiple indicator piecewise growth model assessing Hypothesis 1: The

intervention effect (modeling overall consumption). Intervention was coded as *0 = Pos*

*Or Not control, 1 = Alcohol 101 Plus.* Factor loadings with matching letters (i.e.,

matching outcome indicators) were constrained to equality.

Table 7

*Hypothesis 1: The Effect of Alcohol 101 Plus on Growth to Week Two*

| Outcome | *b* | β | 95% CI | *p* |
|---|---|---|---|---|
| Overall Consumption | -2.973 | -0.265 | -7.666, 2.086 | *ns* |
|   Alcohol Quantity | -3.500 | -0.269 | -7.497, 0.503 | *ns* |
|   Number of Drinking Days | -0.315 | -0.139 | -1.238, 0.576 | *ns* |
|   Number of Heavy Drinking Days | -0.656* | -0.296 | -1.358, -0.029 | < .05 |
|   Number of Days Intoxicated | -0.498 | -0.319 | -1.105, 0.038 | *ns* |
|   Highest Number of Drinks | 0.508 | 0.130 | -0.920, 2.450 | *ns* |
|   BAC on Highest Drinking Day | -0.003 | -0.025 | -0.044, 0.052 | *ns* |
| Alcohol-Related Problems | -0.586 | -0.157 | -2.330, 1.533 | *ns* |
| Alcohol Knowledge | 1.155** | 0.519 | 0.410, 1.825 | < .01 |

*Note*. 95% CI = 95% bias-corrected bootstrapped confidence intervals with *n* = 1000. *p* < .01 indicates that zero was not included in the 99% bias-corrected bootstrapped confidence interval. Also, β represents the parameter coefficient when the outcome variable is standardized, but not the predictor, therefore it is the number of standard deviations the outcome variable is expected to change for those who received the intervention.

Table 8

*Hypothesis 1: The Effect of Alcohol 101 Plus on Growth to Week Two with Covariates*

| Outcome | b | β | 95% CI | p |
|---|---|---|---|---|
| Overall Consumption | -2.511 | -0.223 | -7.079, 2.568 | ns |
| Covariates included: gender, race, Greek, residence | | | | |
| Alcohol Quantity | -0.337 | -0.024 | -6.918, 5.294 | ns |
| Covariates included: gender, race, Greek, residence, GPA, and treatment | | | | |
| Number of Drinking Days | -0.337 | -0.148 | -1.229, 0.558 | ns |
| Covariates included: race, Greek, and residence | | | | |
| Number of Heavy Drinking Days | -0.300 | -0.135 | -1.141, 0.404 | ns |
| Covariates included: residence, GPA, and treatment | | | | |
| Number of Days Intoxicated | 0.269 | 0.168 | -0.286, 0.942 | ns |
| Covariates included: race, residence, and GPA | | | | |
| Highest Number of Drinks | 0.461 | 0.118 | -1.310, 2.431 | ns |
| Covariates included: gender, race, residence | | | | |
| BAC on Highest Drinking Day | 0.002 | 0.018 | -0.045, 0.058 | ns |
| Covariates included: race, Greek, and residence | | | | |
| Alcohol-Related Problems | 0.755 | 0.203 | -1.536, 3.443 | ns |
| Covariates included: residence and GPA | | | | |
| Alcohol Knowledge | 1.243** | 0.559 | 0.490, 1.921 | < .01 |
| Covariates included: gender, residence, and treatment | | | | |

*Note.* 95% CI = 95% bias-corrected bootstrapped confidence intervals with $n$ = 1000. Greek = Greek affiliation, GPA = grade point average, treatment = formal treatment for alcohol use, and BAC = blood alcohol concentration. $p$ < .01 indicates that zero was not included in the 99% bias-corrected bootstrapped confidence interval. Also, β represents the parameter coefficient when the outcome variable is standardized, but not the predictor, therefore it is the number of standard deviations the outcome variable is expected to change for those who received the intervention.

As seen in Table 8, these results were consistent after controlling for demographic and academic covariates, with alcohol knowledge significantly increasing even after controlling for the effects of relevant covariates, and no other alcohol outcome growth

slopes being impacted by intervention receipt. The difference for the growth slope for heavy drinking days is no longer significant.

**Post-Test for Alcohol Knowledge**

In addition to the assessment at the beginning of the data collection session, participants completed a post-intervention knowledge survey after completing their assignment intervention (identical to the measure at the beginning of the session). A 2x2 split-plot ANOVA including the factors of time (pre- and post-intervention) and assignment (*Pos Or Not control* versus *Alcohol 101 Plus*) indicated a significant time by assignment interaction, $F(1, 380) = 32.52, p < .001$, partial $\eta^2 = .079$. Simple main effects indicated that there was a significant increase in knowledge for the *Alcohol 101 Plus* group ($M_{difference} = 2.04, SE = 0.11, p < .001$), but not for the *Post Or Not control* group ($M_{difference} = 0.12, SE = .32, p = .699$). The Alcohol 101 Plus intervention did immediately increase in knowledge about alcohol compared to those who completed a health intervention unrelated to alcohol use.

**Hypothesis 2: The Booster Effect**

To test Hypothesis 2 that receiving a booster would reduce growth trajectories for alcohol use and alcohol-related problems as compared to students in the intervention only condition, the model depicted in Figure 6 was assessed. As before, this model was conducted for each alcohol outcome, and tested if the growth in the alcohol outcome from week two to week four was significantly impacted by receiving a booster email. This model included data from two conditions: Alcohol 101 Plus only and intervention plus personalized booster. Booster receipt was coded as 1 = *received a booster*, 0 = *did not receive a booster*. A significant negative coefficient for the line marked with "H2"

would indicate a significant reduction in the growth trajectory for outcomes, the expected effect. As seen in Table 9, growth was significantly reduced for number of drinking days, number of heavy drinking days, highest number of drinks, and BAC on highest drinking day for those who received the booster email. Alcohol quantity, number of days intoxicated, alcohol-related problems, and knowledge about alcohol were not significantly impacted by booster receipt.

As with the first hypothesis, the booster effect was also tested on overall alcohol consumption using a multiple indicator growth model. As shown in Figure 7, a latent variable was constructed for each timepoint, with each alcohol consumption variable at that timepoint as an indicator of the factor. As before, the factor loadings were fixed at 1 for alcohol quantity and constrained across time for other outcomes, and a piecewise growth model was added assessing growth from week two to week four. Similar to the model for individual outcomes, the effect of the booster on the growth slope was assessed with the path marked "H2," which was significant as indicated by bias-corrected bootstrapped 95% confidence intervals. Receiving the booster was associated with a significantly lowered growth trajectory for overall alcohol consumption from week two to week four.

As seen in Table 10, after controlling for relevant demographic and academic factors, results were consistent for most alcohol use measures (i.e., alcohol quantity, number of drinking days, number of heavy drinking days, number of drinks consumed on highest drinking day, BAC on highest drinking day, alcohol-related problems, and alcohol knowledge), indicating that booster receipt reduced alcohol use growth above and beyond the effects of academic and demographic variables. The exception to this pattern

was overall consumption which became non-significant once relevant covariates were

included in the model.
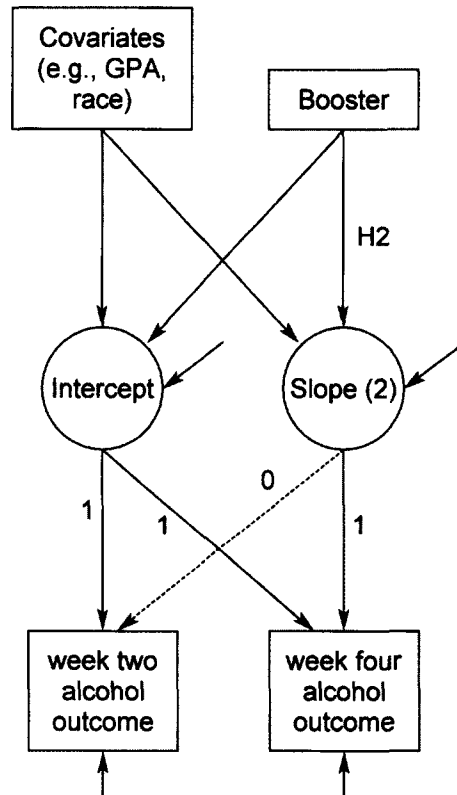


*Figure 6.* Piecewise growth model assessing Hypothesis 2: The booster effect (specific

outcomes modeled separately). Booster was coded as *0 = did not receive, 1 = received.*
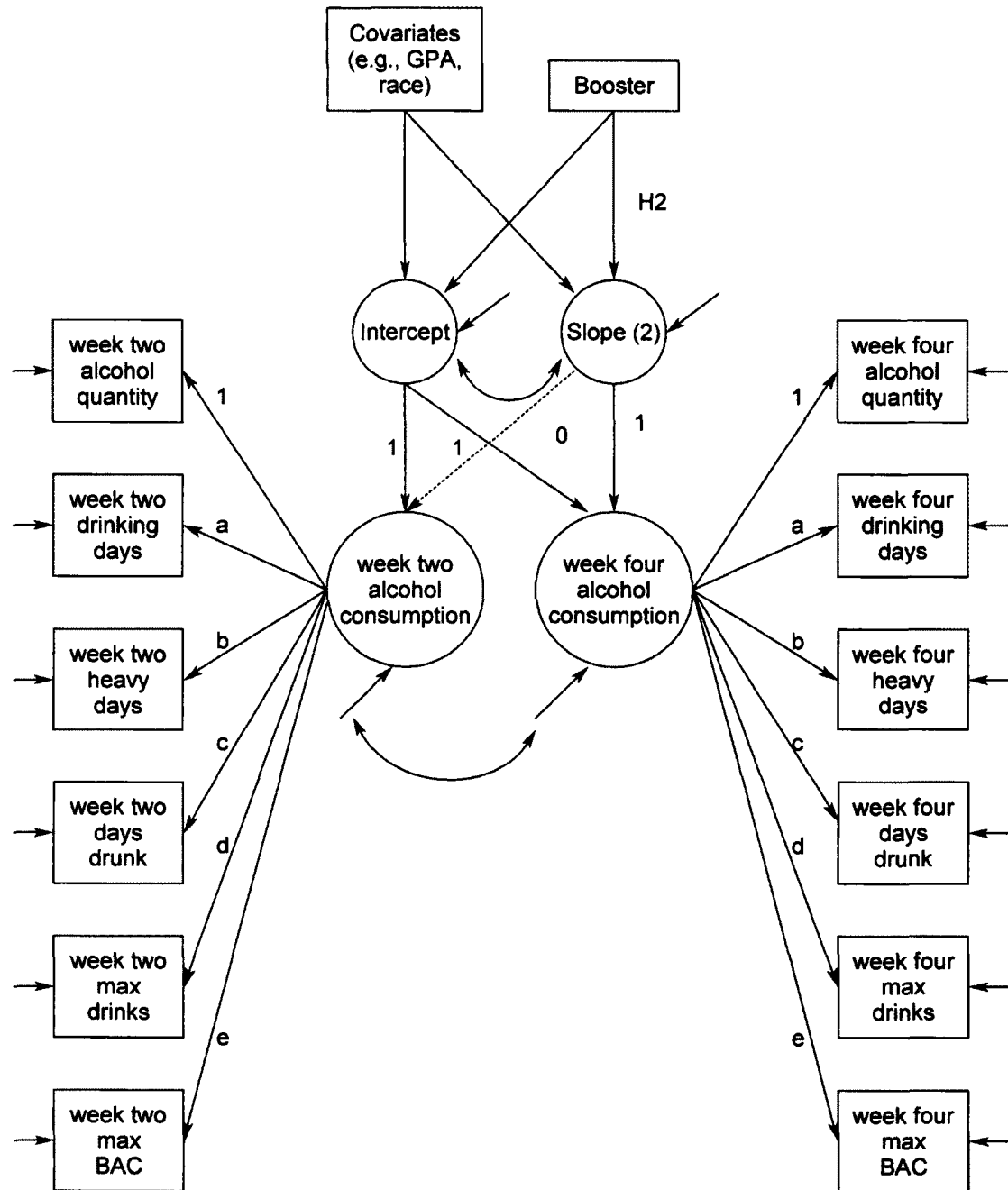
*Figure 7.* Multiple indicator piecewise growth model assessing Hypothesis 2: The

booster effect (modeling overall consumption). BAC = blood alcohol concentration.

Booster was coded as *0 = did not receive, 1 = received*. Factor loadings with matching

letters (i.e., matching outcome indicators) were constrained to equality.

Table 9

*Hypothesis 2: The Effect of the Personalized Booster on Growth to Week Four*

| Outcome | $b$ | $\beta$ | 95% CI | $p$ |
|---|---|---|---|---|
| Overall Consumption | -5.532* | -0.553 | -10.303, -0.704 | < .05 |
| Alcohol Quantity | -3.535 | -0.312 | -8.191, 1.192 | *ns* |
| Number of Drinking Days | -1.035* | -0.503 | -2.038, -0.215 | < .05 |
| Number of Heavy Drinking Days | -0.843* | -0.460 | -1.597, -0.149 | < .05 |
| Number of Days Intoxicated | -0.041 | -0.030 | -0.549, 0.472 | *ns* |
| Highest Number of Drinks | -2.362** | -0.614 | -3.795, -0.745 | < .01 |
| BAC on Highest Drinking Day | -0.068** | -0.696 | -0.107, -0.028 | < .01 |
| Alcohol-Related Problems | -0.521 | -0.155 | -1.813, 0.822 | *ns* |
| Alcohol Knowledge | 0.221 | 0.138 | -0.394, 0.809 | *ns* |

*Note.* 95% CI = 95% bias-corrected bootstrapped confidence intervals with $n$ = 1000. $p$ < .01 indicates that zero was not included in the 99% bias-corrected bootstrapped confidence interval. Also, $\beta$ represents the parameter coefficient when the outcome variable is standardized, but not the predictor, therefore it is the number of standard deviations the outcome variable is expected to change for those who received the booster.

Table 10

*Hypothesis 2: The Effect of the Personalized Booster on Growth to Week Four with Covariates*

| Outcome | b | β | 95% CI | p |
|---|---|---|---|---|
| Overall Consumption | -6.620 | -0.618 | -14.798, 0.609 | ns |
| Covariates included: gender, race, residence, and GPA | | | | |
| Alcohol Quantity | -6.569 | -0.537 | -14.009, 0.729 | ns |
| Covariates included: race, Greek, residence, and GPA | | | | |
| Number of Drinking Days | -1.026* | -0.500 | -2.037, -0.177 | < .05 |
| Covariates included: race, Greek, and residence | | | | |
| Number of Heavy Drinking Days | -0.748* | -0.412 | -1.581, -0.096 | < .05 |
| Covariates included: race, residence, and treatment | | | | |
| Number of Days Intoxicated | -0.039 | -0.029 | -0.587, 0.486 | ns |
| Covariates included: race, and residence | | | | |
| Highest Number of Drinks | -2.399** | -0.623 | -3.762, -0.949 | < .01 |
| Covariates included: race, Greek, and residence | | | | |
| BAC on Highest Drinking Day | -0.069** | -0.676 | -0.104, -0.031 | < .01 |
| Covariates included: gender, race, and Greek | | | | |
| Alcohol-Related Problems | -0.618 | -0.183 | -1.979, 0.695 | ns |
| Covariates included: race, residence and GPA | | | | |
| Alcohol Knowledge | 0.084 | 0.052 | -0.551, 0.704 | ns |
| Covariates included: race, Greek, residence, and treatment | | | | |

Note. 95% CI = 95% bias-corrected bootstrapped confidence intervals with $n$ = 1000. Greek = Greek affiliation, GPA = grade point average, treatment = formal treatment for alcohol use, and BAC = blood alcohol concentration. $p < .01$ indicates that zero was not included in the 99% bias-corrected bootstrapped confidence interval. Also, β represents the parameter coefficient when the outcome variable is standardized, but not the predictor, therefore it is the number of standard deviations the outcome variable is expected to change for those who received the booster.

## Hypothesis 3: PBS as a Mediator

Hypothesis 3 posits that PBS is a mediator for each of the experimental

manipulations. Mediation refers to a theorized relationship among variables. Mediators,

like outcome variables, are impacted by the predictor, but are theorized to be an agent of

change on the outcome variable (Baron & Kenny, 1986). The total effect refers to the relationship between the predictor of interest and the outcome measure, including all shared relationships with other variables (or not controlling for them). The direct effect is the relationship between the predictor of interest and the outcome measure after controlling for other variables that may also affect the outcome measure. The indirect effect is the effect a predictor has on an outcome as it works through a mediator. Ultimately, mediation is a test of the indirect effect.

The Baron and Kenny (1986) approach to mediation involves multiple steps and has been very popular for years. However, it is possible to test mediation simultaneously through SEM. This also allows for more complicated models such as the inclusion of covariates, multiple mediators, and longitudinal data such as for the current study. In the current study, the predictors are experimentally manipulated, so the mediator, PBS, is a variable that could be influenced as a result of our experimental manipulation. In this case, I posited that (a) the participants who receive interventions would use more PBS than those who do not, and (b) the participants who received booster emails would use more PBS than those who do not. I also expected that participants who used more PBS would have lower levels of alcohol use and alcohol-related problems. Hypotheses 1 and 2 investigated the total effects for experimental manipulations on alcohol use outcome. I also expected PBS would at least partly explain the relationship such that the experimental manipulations influenced PBS, and PBS would be significantly associated with the outcomes. Those are the indirect effects assessed in the models described below.

## PBS dimensions

Previous studies assessing PBS as a mediator have varied in terms of how PBS was represented in the model. PBS was sometimes treated as a single score (e.g., Palmer et al., 2010), sometimes separate analyzes were conducted for each subscale of PBS (e.g., Sugarman & Carey, 2007, 2009), and sometimes subscale scores were treated as indicators for a latent variable (e.g., Martens, Ferrier, et al., 2007; Martens et al., 2008). Sugarman and Carey (2007, 2009) conducted separate analyses for each dimension of PBS, because within their sample each dimension had a different relationship with alcohol use. For the current study, I examined the relationship each raw score dimension has with the outcome variables. Consistent with the findings of Sugarman and Carey (2007, 2009), the correlations in Table 11 indicate that the relationships with each outcome differ across dimensions of PBS (e.g., different valence of effects and differing in levels of significance).

However, I also examined the relationship each dimension has with the outcome variables using the new proportionate PBS scores (raw scores divided by number of drinking days). The updated correlations are shown in Table 12. As expected, controlling for the number of drinking days changed the differential functioning of PBS dimensions such that relationships were consistent across dimensions for alcohol quantity, alcohol-related problems, number of days intoxicated, number of heavy drinking days, and max number of drinks on highest drinking day, both in valence of effect as well as level of significance. Dimensions of PBS had consistent valence, but differing levels of significance in their relationships with BAC on highest drinking day and alcohol-related knowledge. This is markedly different from the initial correlations.

This change in associations may be most pronounced for the drinking outcome of number of heavy drinking days, which had a significant positive relationship with PBS: strategies while drinking, $r(231) = .20, p = .004$, a negative but non-significant relationship with PBS: alternatives, $r(231) = -.06, p = .408$, and no relationship with PBS: selective avoidance, $r(231) = .00, p = 982$. This resulted in a positive but non-significant overall relationship with PBS: total, $r(231) = .10, p = .167$ . However, after scaling raw PBS scores with frequency of drinking, these relationships all became significantly negative, with $r(231) = -.23, p = 001$ for PBS: selective avoidance, $r(231) = -.17, p = .011$ for PBS: strategies while drinking, $r(231) = -.34, p < .001$ for PBS: alternatives, and $r(231) = -.25$, $p < .001$ for overall PBS: total. The PBS: total score now truly represents associations across all sub-scales. Based on similar findings across all drinking outcomes, the proportionate total PBS score was used for all analyses involving PBS.

Table 11

*Correlations among Raw PBS Scores and Alcohol Use Outcomes*

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. PBS: avoid | - | .70* | .58* | .86* | -.09 | -.14* | -.12 | .00 | -.09 | -.03 | -.13 | .03 |
| 2. PBS: SWD | | - | .57* | .93* | .16* | .05 | .12 | .20* | .12 | .15* | .01 | .19* |
| 3. PBS: alternatives | | | - | .77* | -.03 | -.10 | -.13 | -.06 | .05 | .01 | .01 | .01 |
| 4. PBS: total | | | | - | .05 | -.04 | -.01 | .10 | .05 | .07 | -.03 | .12 |
| 5. Alcohol Quantity | | | | | - | .50* | .77* | .72* | .77* | .62* | .19* | .70* |
| 6. Alcohol-Related Problems | | | | | | - | .55* | .44* | .41* | .44* | -.07 | .35* |
| 7. Number of Days Intoxicated | | | | | | | - | .76* | .61* | .53* | .11 | .57* |
| 8. Number of Heavy Drinking Days | | | | | | | | - | .60* | .51* | .15* | .58* |
| 9. Number of Drinks on Heaviest Drinking Day | | | | | | | | | - | .80* | .21* | .40* |
| 10. Heaviest Drinking Day BAC | | | | | | | | | | - | .14 | .33* |
| 11. Alcohol Knowledge | | | | | | | | | | | - | .13 |
| 12. Number of Drinking Days | | | | | | | | | | | | - |

*Note.* PBS = protective behavioral strategies; avoid = selective avoidance; SWD = strategies while drinking; drunk = number of days intoxicated; BAC = blood alcohol concentration.

Table 12

*Correlations among Proportionate PBS Scores and Alcohol Use Outcomes*

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. PBS: avoid | - | .81* | .71* | .93* | -.30* | -.12 | -.28* | -.23* | -.23* | -.16* | -.15* |
| 2. PBS: SWD | | - | .61* | .94* | -.24* | -.09 | -.21* | -.17* | -.13* | -.07 | -.04 |
| 3. PBS: alternatives | | | - | .82* | -.39* | -.11 | -.36* | -.34* | -.21* | -.18* | -.12 |
| 4. PBS: total | | | | - | -.33* | -.11 | -.30* | -.25* | -.20* | -.14* | -.10 |
| 5. Alcohol Quantity | | | | | - | .48* | .77* | .72* | .77* | .62* | .20* |
| 6. Alcohol-Related Problems | | | | | | - | .53* | .42* | .40* | .40* | -.04 |
| 7. Number of Days Intoxicated | | | | | | | - | .76* | .61* | .53* | .12 |
| 8. Number of Heavy Drinking Days | | | | | | | | - | .59* | .50* | .15* |
| 9. Number of Drinks on Heaviest Drinking Day | | | | | | | | | - | .80* | .22* |
| 10. Heaviest Drinking Day BAC | | | | | | | | | | - | .14* |
| 11. Alcohol Knowledge | | | | | | | | | | | - |

*Note.* PBS = protective behavioral strategies; avoid = selective avoidance; SWD = strategies while drinking; drunk = number of days intoxicated; BAC = blood alcohol concentration. Number of drinking days was removed as an outcome from this table due to its inclusion in the proportionate PBS score.

**PBS as a Mediator of the Intervention Effect**

Consistent with Hypothesis 3a, I posited that participants increase PBS use after receiving the intervention, and this increased use is associated with reductions in the outcome for that same time period. Although total effects were not observed for the intervention's impact on growth slopes of alcohol outcomes (except for knowledge), indirect effects were still explored, such that intervention receipt would impact proportionate PBS use, and PBS would impact alcohol use and related problems. These indirect effects would still be of substantive interest; they just would not be mediating a

significant total effect (Preacher & Hayes, 2004). These relationships were assessed using the model shown in Figure 7. The significance of the indirect effect (the combination of lines marked with H3a in the model) was assessed using bias-corrected percentile bootstrapped confidence intervals created by using a bootstrapping procedure with 1000 replications. This method is superior to the traditional Sobel test (Sobel, 1982), because the sampling distribution of the indirect effect often is not normal, which is a requirement of the Sobel test; however, by bootstrapping the model, I created an empirical sampling distribution for my indirect effect allowing me to use bias-corrected empirical confidence intervals, thus eliminating the assumption of a normal distribution (MacKinnon et al., 2004; Preacher & Hayes, 2004, 2008). Participants from all conditions were included in the assessment of the model depicted in Figure 7. Similar to assessing the main effect for the intervention, intervention status was coded as *0 = Pos Or Not control, 1 = Alcohol 101 Plus*. Because the booster was not yet issued, both experimental groups are included in the "Alcohol 101 Plus" group for this analysis. Factor loadings were still fixed to 1 for both intercepts and 0 and 1 for growth slopes. As before, the intercept and slope factors were allowed to correlate. For this model, the two intercept factors (for PBS and for the alcohol outcome) were allowed to correlate.

As with the first two hypotheses, the indirect effect was also tested on overall alcohol consumption using a multiple indicator growth model. As shown in Figure 9, a latent variable was constructed for each timepoint, with each alcohol consumption variable at that timepoint as an indicator of the factor. As before, the factor loadings were fixed at 1 for alcohol quantity and constrained across time for other outcomes, and a piecewise growth model was added assessing growth from baseline to week two.
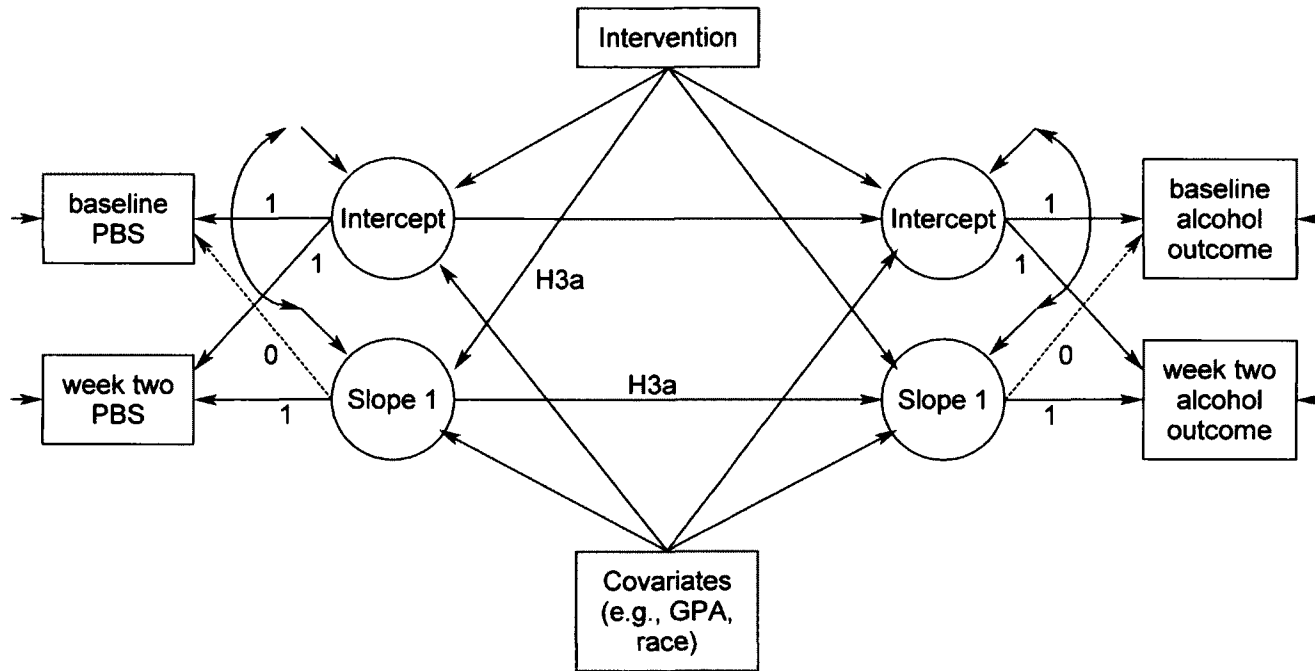
*Figure 8.* Piecewise growth model assessing Hypothesis 3a: PBS as a mediator of the intervention effect (specific outcomes modeled separately). Although not depicted in the figure, intercepts were allowed to correlate in the model (i.e., the PBS intercept and the alcohol outcome intercept). Intervention was coded as *0 = Pos Or Not control, 1 = Alcohol 101 Plus.*
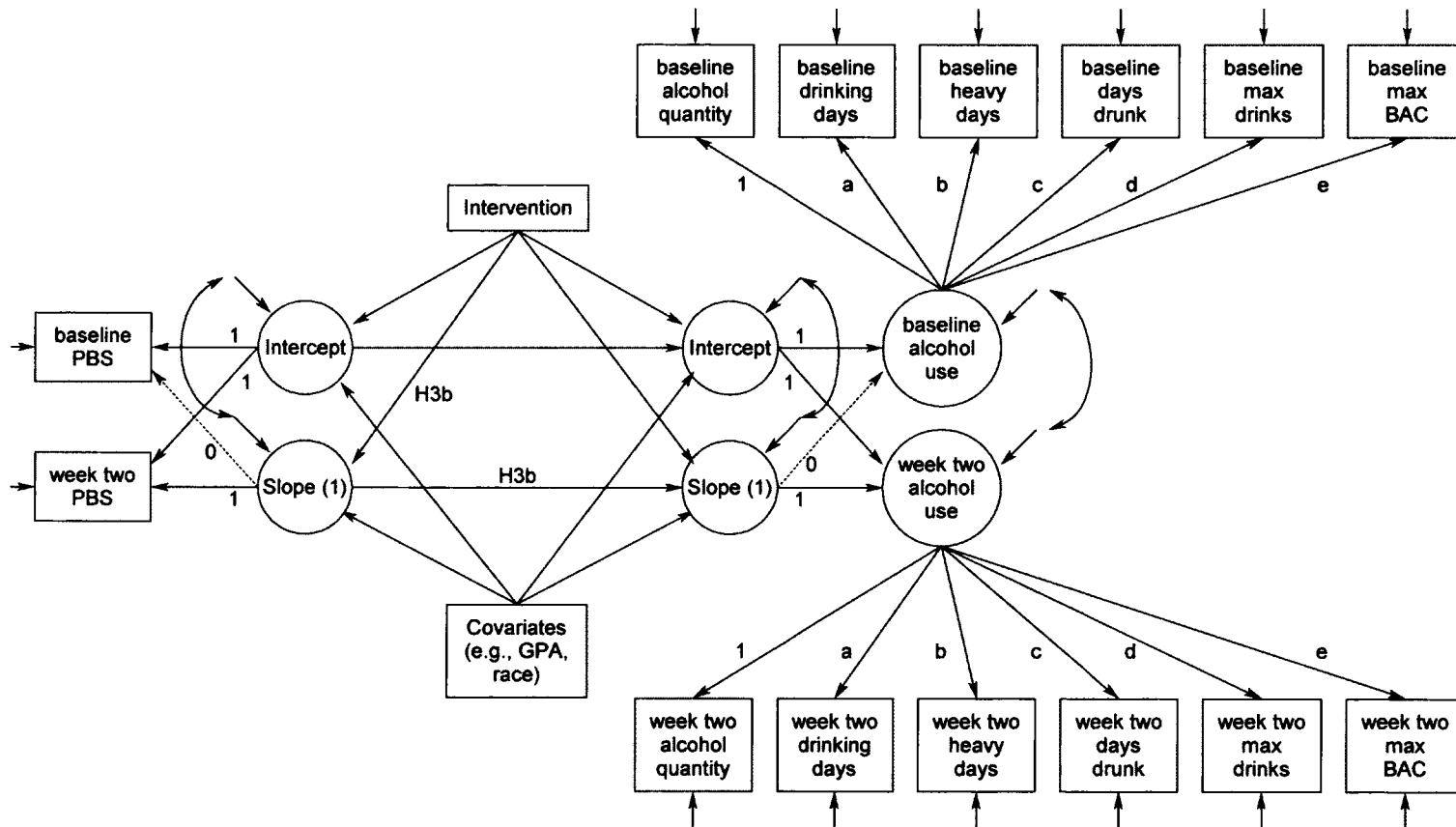
*Figure 9.* Multiple indicator piecewise growth model assessing Hypothesis 3a: PBS as a mediator of the intervention effect (modeling overall consumption). PBS = protective behavioral strategies, BAC = blood alcohol concentration. Although not depicted in the figure, intercepts were allowed to correlate in the model (i.e., the PBS intercept and the alcohol consumption intercept). Intervention was coded as *0 = Pos or Not control, 1 = Alcohol 101 Plus*. Factor loadings with matching letters (i.e., matching outcome indicators) were constrained to equality.

Consistent with Hypothesis 3a, the indirect effect (a combination of the two lines marked H3a) was significant for overall consumption, alcohol quantity, number of heavy drinking days, number of days intoxicated, and alcohol knowledge, as indicated by bias-corrected bootstrapped 95% confidence intervals. Receiving the intervention was associated with increased growth in proportionate PBS use, which in turn was associated with significantly lowered alcohol consumption growth and increased alcohol knowledge growth from baseline to week two. Specific values for the indirect effects, the two parameters that contribute to the indirect effects, and the direct effects can be found in Table 13. The indirect effect was not significant for number of drinks on highest drinking day, BAC on highest drinking day, or alcohol-related problems.

Of the models with significant indirect effects, all outcomes have non-significant direct effects with the exception of alcohol knowledge. This would normally indicate full mediation; however, in this case there was no significant total effect to mediate. The only significant direct effect observed was for alcohol knowledge, which was also the only significant total effect from Hypothesis 1. This indicates that the booster receipt is associated with increased growth in knowledge and increased growth in proportionate PBS use, and that PBS growth does not account for all increased growth in knowledge.

For most models (i.e., overall consumption, specific indicators of alcohol use, alcohol-related problems, and alcohol knowledge), the intervention significantly increased the growth slope of proportionate PBS use compared to those who completed the control health education session. The impact of PBS on alcohol use varied. Increased PBS growth significantly reduced the growth of alcohol outcomes for overall consumption, number of heavy drinking days, and number of days intoxicated.

Consistent with non-significant indirect effects, the impact of PBS growth on the growth of alcohol outcomes was not significant for number of drinks on highest drinking day, BAC on highest drinking day, and alcohol-related problems.

Table 13

*Hypothesis 3a: PBS as a Mediator of the Intervention Effect*

| Outcome | $b$ | $\beta$ | 95% CI | $p$ |
|---|---|---|---|---|
| **Overall Consumption** | | | | |
| Direct Effect | -1.887 | -0.053 | -6.902, 3.141 | *ns* |
| Intervention, PBS slope | 10.429* | 0.142 | 0.052, 28.847 | < .05 |
| PBS slope, outcome slope | -0.105** | -0.217 | -0.201, -0.053 | < .01 |
| Indirect Effect | -1.099* | -0.031 | -3.303, -0.043 | < .05 |
| **Alcohol Quantity** | | | | |
| Direct Effect | -2.465 | -0.060 | -6.916, 1.744 | *ns* |
| Intervention, PBS slope | 10.715* | 0.146 | 0.275, 28.986 | < .05 |
| PBS slope, outcome slope | -0.097** | -0.172 | -0.195, -0.050 | < .01 |
| Indirect Effect | -1.035* | -0.025 | -3.243, -0.102 | < .05 |
| **Number of Heavy Drinking Days** | | | | |
| Direct Effect | -0.480 | -0.069 | -1.197, 0.203 | *ns* |
| Intervention, PBS slope | 11.177* | 0.151 | 0.851, 29.501 | < .05 |
| PBS slope, outcome slope | -0.016** | -0.168 | -0.027, -0.004 | < .01 |
| Indirect Effect | -0.176* | -0.025 | -0.593, -0.022 | < .05 |
| **Number of Days Intoxicated** | | | | |
| Direct Effect | -0.419 | -0.084 | -1.053, 0.224 | *ns* |
| Intervention, PBS slope | 11.177* | 0.152 | 0.998, 29.881 | < .05 |
| PBS slope, outcome slope | -0.007* | -0.105 | -0.016, -0.011 | < .05 |
| Indirect Effect | -0.079* | -0.016 | -0.290, -0.008 | < .05 |
| **Number of Drinks on Highest Drinking Day** | | | | |
| Direct Effect | 0.613 | 0.049 | -0.861, 2.463 | *ns* |
| Intervention, PBS slope | 11.297* | 0.154 | 1.106, 29.785 | < .05 |
| PBS slope, outcome slope | -0.009 | -0.055 | -0.031, 0.015 | *ns* |
| Indirect Effect | -0.105 | -0.008 | -0.594, 0.088 | *ns* |
| **BAC on Highest Drinking Day** | | | | |
| Direct Effect | -0.001 | -0.004 | -0.042, 0.055 | *ns* |
| Intervention, PBS slope | 11.425* | 0.155 | 0.957, 29.805 | < .05 |
| PBS slope, outcome slope | 0.000 | -0.027 | -0.001, 0.000 | *ns* |
| Indirect Effect | -0.001 | -0.004 | -0.011, 0.003 | *ns* |

Table 13 continued

| Outcome | $b$ | $\beta$ | 95% CI | $p$ |
|---|---|---|---|---|
| Alcohol-Related Problems | | | | |
| Direct Effect | -0.469 | -0.040 | -2.364, 1.615 | *ns* |
| Intervention, PBS slope | 11.346* | 0.154 | 1.113, 29.778 | < .05 |
| PBS slope, outcome slope | -0.010 | -0.064 | -0.026, 0.011 | *ns* |
| Indirect Effect | -0.117 | -0.010 | -0.566, 0.046 | *ns* |
| Alcohol Knowledge | | | | |
| Direct Effect | 1.025* | 0.147 | 0.233, 1.787 | < .05 |
| Intervention, PBS slope | 11.420* | 0.155 | 1.307, 29.843 | < .05 |
| PBS slope, outcome slope | 0.011 | 0.120 | -0.001, 0.029 | *ns* |
| Indirect Effect | 0.130* | 0.019 | 0.004, 0.468 | < .05 |

*Note.* 95% CI = 95% bias-corrected bootstrapped confidence intervals with $n$ = 1000. $p <$ .01 indicates that zero was not included in the 99% bias-corrected bootstrapped confidence interval. Also, to facilitate comparison across effects, $\beta$ for this table represents the value when both $X$ and $Y$ are standardized, regardless of whether $X$ is dichotomously coded.

As shown in Table 14, results of analyses controlling for relevant demographic and academic factors were consistent with the original analyses for overall consumption, alcohol quantity, number of heavy drinking days, number of days intoxicated, number of drinks on highest drinking day, BAC on highest drinking day, and alcohol knowledge. For alcohol-related problems, the indirect effect of intervention through proportionate PBS growth became significant after controlling for relevant covariates. Covariates included in the analysis are not listed in Table 14 because they were identical to the covariates listed in Table 8 above[3].

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[3] For the overall consumption model, the covariates of Greek status and residence were removed because of convergence issues.

Table 14

*Hypothesis 3a: PBS as a Mediator of the Intervention Effect with Covariates*

| Outcome | b | β | 95% CI | p |
|---|---|---|---|---|
| Overall Consumption | | | | |
| Direct Effect | -2.065 | -0.087 | -6.976, 2.970 | *ns* |
| Intervention, PBS slope | 10.648* | 0.145 | 0.003, 28.771 | < .05 |
| PBS slope, outcome slope | -0.103** | -0.316 | -0.205, -0.048 | < .01 |
| Indirect Effect | -1.093* | -0.046 | -3.275, -0.037 | < .05 |
| | | | | |
| Alcohol Quantity | | | | |
| Direct Effect | -1.721 | 0.042 | -6.431, 2.377 | *ns* |
| Intervention, PBS slope | 10.203* | 0.139 | -0.241, 28.370 | *ns* |
| PBS slope, outcome slope | -0.092** | -0.163 | -0.187, -0.045 | < .01 |
| Indirect Effect | -0.936* | -0.023 | -2.918, -0.017 | < .05 |
| | | | | |
| Number of Heavy Drinking Days | | | | |
| Direct Effect | -0.501 | -0.072 | -1.204, 0.173 | *ns* |
| Intervention, PBS slope | 10.813* | 0.147 | 0.836, 29.721 | < .05 |
| PBS slope, outcome slope | -0.014* | -0.152 | -0.025, -0.002 | < .05 |
| Indirect Effect | -0.157* | -0.022 | -0.593, -0.012 | < .05 |
| | | | | |
| Number of Days Intoxicated | | | | |
| Direct Effect | -0.354 | -0.071 | -1.032, 0.285 | *ns* |
| Intervention, PBS slope | 11.114* | 0.151 | 0.566, 29.421 | < .05 |
| PBS slope, outcome slope | -0.007* | -0.101 | -0.016, 0.000 | < .05 |
| Indirect Effect | -0.076* | -0.015 | -0.293, -0.002 | < .05 |
| | | | | |
| Number of Drinks on Highest Drinking Day | | | | |
| Direct Effect | 0.741 | 0.060 | -0.780, 2.574 | *ns* |
| Intervention, PBS slope | 10.758* | 0.146 | 0.420, 29.167 | < .05 |
| PBS slope, outcome slope | -0.008 | -0.046 | -0.030, 0.020 | *ns* |
| Indirect Effect | -0.083 | -0.007 | -0.615, 0.119 | *ns* |
| | | | | |
| BAC on Highest Drinking Day | | | | |
| Direct Effect | 0.002 | 0.007 | -0.037, 0.054 | *ns* |
| Intervention, PBS slope | 11.071* | 0.151 | 0.650, 29.419 | < .05 |
| PBS slope, outcome slope | 0.000 | -0.029 | -0.001, 0.001 | *ns* |
| Indirect Effect | -0.001 | -0.004 | -0.011, 0.004 | *ns* |
| | | | | |
| Alcohol-Related Problems | | | | |
| Direct Effect | 1.191 | 0.111 | -1.222, 4.088 | *ns* |
| Intervention, PBS slope | 15.783* | 0.267 | -0.062, 42.538 | *ns* |
| PBS slope, outcome slope | -0.028 | -0.152 | -0.064, 0.004 | *ns* |
| Indirect Effect | -0.436* | -0.040 | -1.865, -0.001 | < .05 |

Table 14 continued

| Outcome | $b$ | $\beta$ | 95% CI | $p$ |
|---|---|---|---|---|
| Alcohol Knowledge | | | | |
| Direct Effect | 1.157** | 0.166 | 0.3376, 1.900 | < .01 |
| Intervention, PBS slope | 9.975 | 0.136 | -0.014, 29.364 | *ns* |
| PBS slope, outcome slope | 0.013* | 0.132 | 0.002, 0.031 | < .05 |
| Indirect Effect | 0.125* | 0.018 | 0.000, 0.502 | < .05 |

*Note.* 95% CI = 95% bias-corrected bootstrapped confidence intervals with $n$ = 1000. $p$ < .01 indicates that zero was not included in the 99% bias-corrected bootstrapped confidence interval. To facilitate comparison, $\beta$ for this table represents the value when both $X$ and $Y$ are standardized, regardless of whether $X$ is dichotomously coded.

## PBS as a Mediator of the Booster Effect

Hypothesis 3b states that PBS is a mediator for the booster effect. I posit that after receiving the booster, participants increased PBS use, and this increased use is associated with reductions in the outcome for that same time period. These relationships were assessed using the model shown in Figure 10. Participants from two conditions were included in the assessment: Alcohol 101 Plus only, and intervention plus personalized booster. Factor loadings were still fixed to 1 for both intercepts, and 0 and 1 for both growth slopes. Similarly, the intercept and slope factors were allowed to correlate, as were two intercept factors (for PBS and for the alcohol outcome).

As with all other hypotheses, PBS as a mediator of the booster effect was tested on overall alcohol consumption using a multiple indicator growth model. Figure 11 shows how latent variables were constructed for each timepoint, with each alcohol consumption variable at that timepoint as an indicator of the factor. The factor loadings were fixed at 1 for alcohol quantity and constrained across time for other outcomes, and a piecewise growth model was added assessing growth from week two to week four.
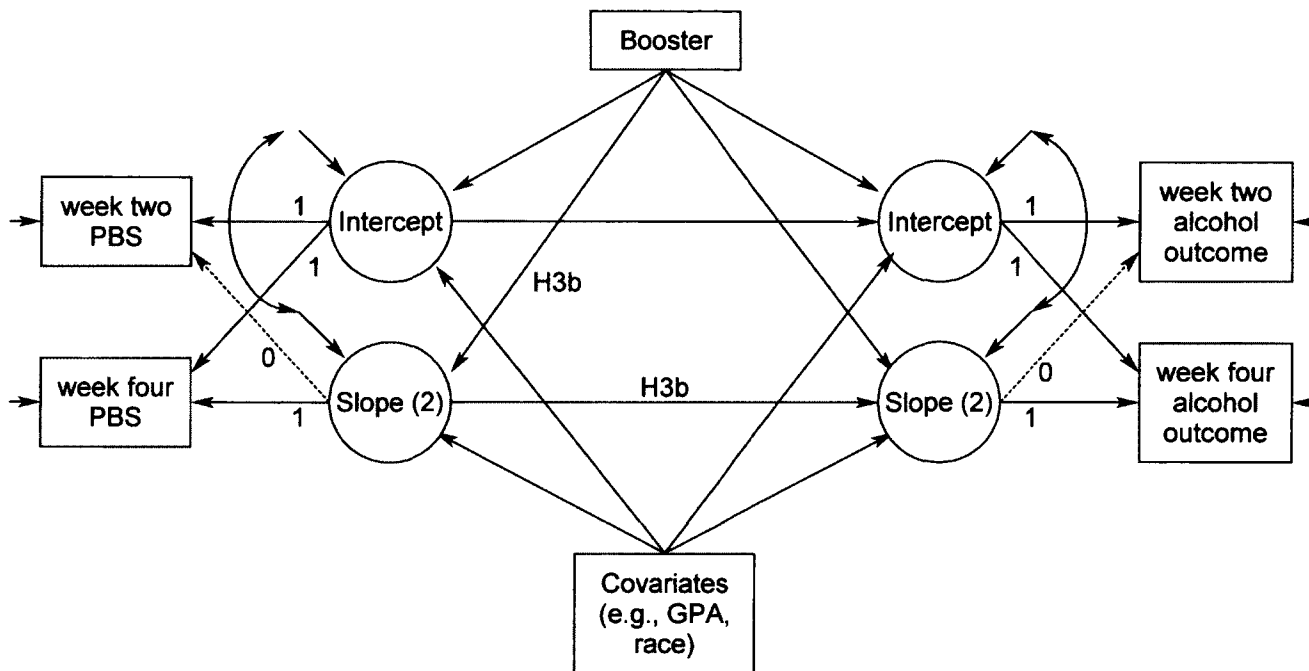
*Figure 10.* Piecewise growth model assessing Hypothesis 3b: PBS as a mediator of the booster effect (specific outcomes modeled separately). Although not depicted in the figure, intercepts were allowed to correlate in the model (i.e., the PBS intercept and the alcohol outcome intercept). Booster was coded as *0 = did not receive, 1 = received.*
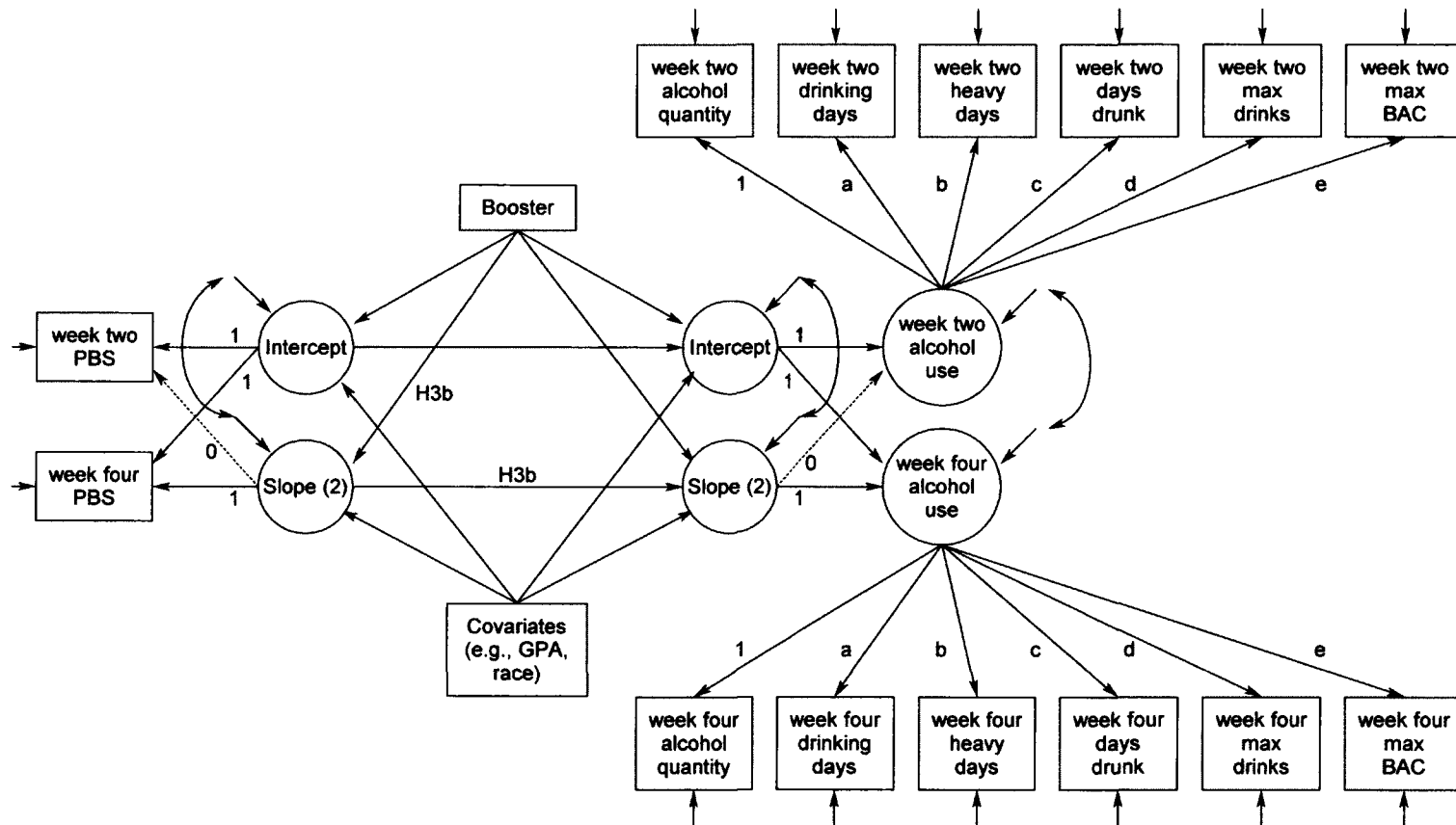
*Figure 11.* Multiple indicator piecewise growth model assessing Hypothesis 3b: PBS as a mediator of the booster effect (modeling overall consumption). PBS = protective behavioral strategies, BAC = blood alcohol concentration. Although not depicted in the figure, intercepts were allowed to correlate in the model (i.e., the PBS intercept and the alcohol consumption intercept). Booster was coded as *0 = did not receive, 1 = received.* Factor loadings with matching letters (i.e., matching outcome indicators) were constrained to equality.

As seen in Table 15, there was a significant indirect effect for alcohol quantity and alcohol-related problems with the direct effect becoming non-significant, indicating full mediation. The booster significantly increased the growth of proportionate PBS use, and PBS growth significantly reduced the alcohol growth slope and problems growth slope. However, these were the only outcomes where the booster effect was significantly mediated by PBS use. As expected, booster receipt significantly increased the proportionate PBS use growth slope in the models for all alcohol outcomes. Unfortunately, this increase in proportionate PBS use was not significantly related to the growth of the outcome variables for number of drinking days, number of heavy drinking days, number of days intoxicated, and number of drinks and associated BAC on highest drinking day. Consequently, there was not a significant indirect effect for overall consumption.

The results of the same analyses conducted with relevant demographic and academic factors are seen in Table 16. Both indirect effects became non-significant after controlling for relevant demographic and academic variables. Results are somewhat consistent for quantity and problems, with PBS growth being impacted by booster receipt. Unfortunately, this increased PBS use was not associated with changes in alcohol use growth. For the remaining outcomes, neither half of the indirect effect was observed (i.e., booster receipt did not impact PBS growth, and PBS growth did not impact outcome growth). The total effect for booster receipt on number of heavy drinking days was significant. Interestingly, the direct effect for number of heavy drinking days became non-significant after including PBS in the model, despite the lack

of significance for the indirect effect. Covariates included in the analysis are not listed in

Table 16 because they identical to the covariates listed in Table 10 above.

Table 15

*Hypothesis 3b: PBS as a Mediator of the Booster Effect*

| Outcome | b | β | 95% CI | p |
|---|---|---|---|---|
| Overall Consumption | | | | |
| Direct Effect | -4.091 | -0.202 | -10.084, 0.788 | *ns* |
| Booster, PBS slope | 9.800* | 0.285 | 1.600, 18.518 | < .05 |
| PBS slope, outcome slope | -0.142 | -0.241 | -0.318, 0.019 | *ns* |
| Indirect Effect | -1.387 | -0.069 | -4.578, 0.016 | *ns* |
| Alcohol Quantity | | | | |
| Direct Effect | -1.816 | -0.079 | -7.307, 2.971 | *ns* |
| Booster, PBS slope | 9.975* | 0.290 | 1.778, 19.135 | < .05 |
| PBS slope, outcome slope | -0.168** | -0.253 | -0.323, -0.042 | < .01 |
| Indirect Effect | -1.679* | -0.073 | -4.749, -0.181 | < .05 |
| Number of Heavy Drinking Days | | | | |
| Direct Effect | -0.692 | -0.186 | -1.660, 0.079 | *ns* |
| Booster, PBS slope | 9.488* | 0.277 | 1.415, 18.344 | < .05 |
| PBS slope, outcome slope | -0.016 | -0.148 | -0.039, 0.005 | *ns* |
| Indirect Effect | -0.152 | -0.041 | -0.524, 0.020 | *ns* |
| Number of Days Intoxicated | | | | |
| Direct Effect | 0.052 | 0.019 | -0.560, 0.618 | *ns* |
| Booster, PBS slope | 9.691* | 0.282 | 1.182, 18.951 | < .05 |
| PBS slope, outcome slope | -0.009 | -0.114 | -0.027, 0.011 | *ns* |
| Indirect Effect | -0.088 | -0.032 | -0.406, 0.055 | *ns* |
| Number of Drinks on Highest Drinking Day | | | | |
| Direct Effect | -2.299* | -0.297 | -4.135, -0.477 | < .05 |
| Booster, PBS slope | 9.340* | 0.273 | 0.899, 18.325 | < .05 |
| PBS slope, outcome slope | -0.007 | -0.032 | -0.075, 0.047 | *ns* |
| Indirect Effect | -0.068 | -0.009 | -1.013, 0.322 | *ns* |
| BAC on Highest Drinking Day | | | | |
| Direct Effect | -0.068** | -0.345 | -0.117, -0.025 | < .01 |
| Booster, PBS slope | 9.253* | 0.271 | 1.108, 18.601 | < .05 |
| PBS slope, outcome slope | -0.000 | -0.009 | -0.001, 0.001 | *ns* |
| Indirect Effect | -0.001 | -0.003 | -0.020, 0.010 | *ns* |

Table 15 continued

| Outcome | b | β | 95% CI | p |
|---|---|---|---|---|
| **Alcohol-Related Problems** | | | | |
| Direct Effect | -0.147 | -0.022 | -1.444, 1.070 | *ns* |
| Booster, PBS slope | 10.043* | 0.292 | 1.793, 18.953 | < .05 |
| PBS slope, outcome slope | -0.037* | -0.186 | -0.072, -0.006 | < .05 |
| Indirect Effect | -0.369* | -0.054 | -1.065, -0.036 | < .05 |
| **Alcohol Knowledge** | | | | |
| Direct Effect | 0.214 | 0.066 | -0.451, 0.882 | *ns* |
| Booster, PBS slope | 9.263* | 0.271 | 1.125, 18.404 | < .05 |
| PBS slope, outcome slope | 0.000 | 0.001 | -0.020, 0.023 | *ns* |
| Indirect Effect | 0.001 | 0.000 | -0.205, 0.248 | *ns* |

*Note.* 95% CI = 95% bias-corrected bootstrapped confidence intervals with $n = 1000$. $p <$ .01 indicates that zero was not included in the 99% bias-corrected bootstrapped confidence interval. Also, to facilitate comparison across effects, β for this table represents the value when both $X$ and $Y$ are standardized, regardless of whether $X$ is dichotomously coded.

Table 16

*Hypothesis 3b: PBS as a Mediator of the Booster Effect with Covariates*

| Outcome | b | β | 95% CI | p |
|---|---|---|---|---|
| **Overall Consumption** | | | | |
| Direct Effect | -3.902 | -0.193 | -10.164, 0.915 | *ns* |
| Booster, PBS slope | 9.312* | 0.272 | 0.707, 18.652 | < .05 |
| PBS slope, outcome slope | -0.142 | -0.241 | -0.328, 0.032 | *ns* |
| Indirect Effect | -1.324 | -0.065 | -4.553, 0.111 | *ns* |
| **Alcohol Quantity** | | | | |
| Direct Effect | -1.768 | -0.078 | -7.070, 2.640 | *ns* |
| Booster, PBS slope | 8.854 | 0.259 | -0.400, 18.153 | *ns* |
| PBS slope, outcome slope | -0.161 | -0.243 | -0.341, 0.001 | *ns* |
| Indirect Effect | -1.425 | -0.063 | -4.831, 0.081 | *ns* |
| **Number of Heavy Drinking Days** | | | | |
| Direct Effect | -0.607 | -0.164 | -1.512, 0.084 | *ns* |
| Booster, PBS slope | 9.107 | 0.267 | -0.051, 18.263 | *ns* |
| PBS slope, outcome slope | -0.017 | -0.152 | -0.040, 0.007 | *ns* |
| Indirect Effect | -0.151 | -0.041 | -0.561, 0.022 | *ns* |

Table 16 continued

| Outcome | b | β | 95% CI | p |
|---|---|---|---|---|
| **Number of Days Intoxicated** | | | | |
| Direct Effect | 0.046 | 0.017 | -0.569, 0.610 | *ns* |
| Booster, PBS slope | 8.822 | 0.259 | -0.129, 18.381 | *ns* |
| PBS slope, outcome slope | -0.009 | -0.113 | -0.028, 0.010 | *ns* |
| Indirect Effect | -0.080 | -0.029 | -0.393, 0.046 | *ns* |
| **Highest Number of Drinks** | | | | |
| Direct Effect | -2.342* | -0.302 | -4.126, -0.524 | < .05 |
| Booster, PBS slope | 8.458 | 0.248 | -0.795, 17.586 | *ns* |
| PBS slope, outcome slope | -0.008 | -0.034 | -0.087, 0.046 | *ns* |
| Indirect Effect | -0.066 | -0.008 | -1.220, 0.373 | *ns* |
| **BAC on Highest Drinking Day** | | | | |
| Direct Effect | -0.067** | -0.339 | -0.115, -0.025 | < .01 |
| Booster, PBS slope | 8.738 | 0.256 | -0.537, 18.122 | *ns* |
| PBS slope, outcome slope | 0.000 | 0.003 | -0.002, 0.002 | *ns* |
| Indirect Effect | 0.000 | 0.001 | -0.029, 0.003 | *ns* |
| **Alcohol-Related Problems** | | | | |
| Direct Effect | 1.723 | 0.234 | -0.942, 4.589 | *ns* |
| Booster, PBS slope | 12.590* | 0.421 | 1.105, 25.613 | < .05 |
| PBS slope, outcome slope | -0.082 | -0.332 | -0.213, 0.056 | *ns* |
| Indirect Effect | -1.031 | -0.140 | -4.443, 0.105 | *ns* |
| **Alcohol Knowledge** | | | | |
| Direct Effect | 0.114 | 0.035 | -0.575, 0.737 | *ns* |
| Booster, PBS slope | 9.311* | 0.269 | 0.109, 18.841 | < .05 |
| PBS slope, outcome slope | -0.004 | -0.046 | -0.023, 0.020 | *ns* |
| Indirect Effect | -0.040 | -0.012 | -0.323, 0.159 | *ns* |

*Note.* 95% CI = 95% bias-corrected bootstrapped confidence intervals with $n = 1000$. $p < .01$ indicates that zero was not included in the 99% bias-corrected bootstrapped confidence interval. Also, to facilitate comparison across effects, β for this table represents the value when both $X$ and $Y$ are standardized, regardless of whether $X$ is dichotomously coded.

Hypotheses were tested with 68 individual analyses described above. Table 17

summarizes findings for each hypothesis, listing whether each analysis was significant

(supporting the hypothesis) or not significant (not supporting the hypothesis). Results are

listed both with and without covariates.

Table 17

*Summary of Hypothesis Support or Non-Support across Alcohol Outcomes*

| Outcome | Hyp1 | Hyp2 | Hyp3a | Hyp3b |
|---|---|---|---|---|
| Overall Consumption | *ns / ns* | sig / *ns* | sig / sig | *ns / ns* |
| Alcohol Quantity | *ns / ns* | *ns / ns* | sig / sig | sig / *ns* |
| Number of Drinking Days | *ns / ns* | sig / sig | --- / --- | --- / --- |
| Number of Heavy Drinking Days | sig / *ns* | sig / sig | sig / sig | *ns / ns* |
| Number of Days Intoxicated | *ns / ns* | *ns / ns* | sig / sig | *ns / ns* |
| Highest Number of Drinks | *ns / ns* | sig / sig | *ns / ns* | *ns / ns* |
| BAC on Highest Drinking Day | *ns / ns* | sig / sig | *ns / ns* | *ns / ns* |
| Alcohol-Related Problems | *ns / ns* | *ns / ns* | *ns* / sig | sig / *ns* |
| Alcohol Knowledge | sig / sig | *ns / ns* | sig / sig | *ns / ns* |

*Note.* Sig = significant; *ns* = not significant. The finding before the "/" corresponds to the analysis without covariates, and the finding after the "/" corresponds to the analysis after including covariates. Hyp1 = hypothesis 1 (the intervention effect); Hyp2 = hypothesis 2 (the booster effect); Hyp3a = first half of hypothesis 3 (the indirect intervention effect through PBS); Hyp3b = second half of hypothesis 3 (the indirect booster effect through PBS). Number of drinking days was omitted from analyses involving PBS as a predictor or mediator due to its incorporation in the proportionate PBS variable.

# CHAPTER 4

# DISCUSSION

The current study was a prospective examination of college student drinking and related problems. The purpose of the current study was to attempt to replicate previous results in the literature indicating that Alcohol 101 Plus is efficacious at reducing alcohol use and related problems in college students (Hypothesis 1), to assess the efficacy of a new booster technique of sending personalized feedback to students via email (Hypothesis 2), and to explore PBS as a mediator of both effects (Hypothesis 3). I was unable to replicate the findings of previous researchers as the current data indicate that the Alcohol 101 Plus intervention was not effective at reducing growth trajectories for alcohol use or related problems, though it did increase participants' knowledge about alcohol. Thus, Hypothesis 1 was not supported. The personalized feedback booster delivered via email, however, was successful at reducing many alcohol use outcomes, so Hypothesis 2 was supported for alcohol use. Interestingly, though there was not a total effect for the intervention, there was a significant indirect effect with PBS as the mechanism of change for several alcohol use outcomes. These findings partially support Hypothesis 3a. And finally, PBS was a significant mediator for the relationship between booster receipt and alcohol quantity as well as alcohol-related problems. These findings partially support Hypothesis 3b.

## Hypothesis 1: The Intervention Effect

The effect of the Alcohol 101 Plus intervention was non-significant in predicting growth across all outcomes of alcohol use (i.e., overall consumption, alcohol quantity, number of drinking days, number of heavy drinking days, number of days intoxicated,

number of drinks consumed on highest drinking day, and BAC on highest drinking day) as well as alcohol-related problems. This was true both with and without controlling for relevant demographic and academic factors. Whereas most previous studies evaluated the efficacy of this intervention's predecessor (Alcohol 101) rather than the current intervention, the current findings are contrary to several studies in the literature (Barnett et al., 2007; Donohue et al., 2004; Hagman et al., 2007; Reis et al., 2000), though consistent with others (Lau-Barraco & Dunn, 2008; Sharmer, 2001). To my knowledge, only one other study assessed the current version of Alcohol 101 Plus. Carey, Henson, Carey, and Maisto (2009) found that Alcohol 101 Plus was equally effective as an in-person brief intervention at reducing short-term drinking for male students mandated to treatment. However, the current student population consisted of volunteers rather than those mandated to treatment; perhaps the intervention has more impact with individuals who had a recent alcohol sanction.

This is not to say that alcohol use and related problems did not change after baseline study participation, but rather that the type of session (i.e., HIV-focused control session versus alcohol-focused intervention) did not impact growth. Examination of the growth slope intercepts reveal that overall alcohol consumption did significantly decrease from baseline to week two, $b = -6.10$, $\beta = -1.42$, 95% CI [-10.93, -1.09], even after controlling for gender, race, Greek status, and residence, $b = -7.54$, $\beta = -2.53$, 95% CI [-10.31, -5.01]. Additionally, alcohol-related problems decreased as well, $b = -1.21$, $\beta = -0.56$, 95% CI [-2.61, 0.37], though not significantly so. However, after controlling for GPA and residence, the reduction in problems became significant, $b = -2.35$, $\beta = -1.02$, 95% CI [-4.09, -0.96], with participants experiencing approximately two fewer alcohol-

related problems than in the two weeks prior to the intervention. This indicates that participants reduced both alcohol consumption and alcohol-related problems immediately after participating in the baseline session of the study. This may be due to the assessment rather than to either health education session. Students completed a number of questionnaires, including reporting their drinking, their knowledge of the consequences of alcohol use, and problems they experienced related to their alcohol use. Increasing awareness of their current consumption and making salient the connection between their alcohol use and problems they have experienced may be driving the reductions in subsequent alcohol use and related problems. This assessment-only effect has been observed in several previous studies (e.g., Hustad, Barnett, Borsari, & Jackson, 2010; Kivlahan, Marlatt, Fromme, Coppel, & Williams, 1990; McCambridge & Day, 2008; McCambridge & Strang, 2005). So it may not be that the intervention is ineffective, but rather not more effective than the Hawthorne Effect.

Moreover, though participation in the intervention did not impact alcohol use or related problems, knowledge about alcohol was significantly increased both immediately post intervention and two weeks later, compared to those in the control condition. This increased knowledge may have implications for alcohol use after the erosion of any assessment effects. Follow-up for the current study was relatively short, and it is possible that the initial reduction in use and problems may erode for those in the control group, but be maintained for those who received the intervention.

**Hypothesis 2: The Booster Effect**

Personalized feedback provided via email significantly impacted the trajectory of several alcohol use outcomes with the exception of alcohol quantity, number of days intoxicated, and alcohol-related problems. Outcomes associated with typical use (e.g., alcohol quantity) were not impacted, but outcomes associated with higher risk use (e.g., number of heavy drinking days, highest number of drinks, and BAC on highest drinking day) were influenced by the booster email. The trajectory for participants who received the booster email was reduced by almost four drinks in overall consumption, one fewer drinking day, and two fewer drinks on their highest drinking day. Participants also significantly reduced their BAC on their highest drinking day and reduced the number of heavy drinking days. These findings were consistent even controlling for demographic and academic factors, with the exception of overall consumption. Though the strength of the parameter estimate for overall consumption actually increased in comparison ($b$ = -5.532, $\beta$ = -0.553 without covariates; versus $b$ = -6.620, $\beta$ = -0.618 after controlling for gender, race, residence, and GPA), the bias-corrected, bootstrapped confidence intervals increased in width, indicating more variability in growth trajectories after controlling for these factors. Therefore, the booster may have been more effective for some participants than others; the next step in this program of research may be to explore moderators of this feedback booster effect to identify for whom the booster is most efficacious. Possible moderators could be participant characteristics not explored, drinking trajectories, and absorption of the booster content. Booster content absorption may be associated with which participants took the time to confirm receipt of the booster email (discussed below).

The findings of the current study are consistent with previous research. Multiple reviews of the literature conclude that personalized feedback provided to college students has generally been effective at reducing alcohol use and related problems (Carey et al., 2007; Larimer & Cronce, 2002, 2007; Walters & Neighbors, 2005). The feedback is often combined with other forms of intervention (e.g., motivational interviewing), but is still effective when delivered as a stand-alone procedure. The current feedback was easily generated using basic software (i.e., survey software, SPSS, and Microsoft Office). The significant findings of the current study combined with the easy dissemination and cost effectiveness of emailed feedback has promising clinical implications. The ease of use and low cost will likely be popular among academic institutions currently employing the use of computerized interventions targeting drinking, including over 2,500 institutions using Alcohol 101 Plus, over 550 using e-CHUG, and over 500 using AlcoholEdu® for College, three of the most popular computerized interventions (Century Council, 2007; Outside the Classroom, 2010; San Diego State University Research Foundation, 2009; Walters et al., 2005). However, more temporally distant follow-up assessments are needed to evaluate the longer-term impact of the feedback.

### Hypothesis 3: PBS as a Mediator

One interesting finding in the current data that was not hypothesized was the change in associations with alcohol measures after rescaling the measure for PBS use. In raw score form, each dimension functioned differently in its association with alcohol use, with strategies designed to be beneficial (e.g., alternating alcoholic and non-alcohol drinks) unexpectedly relating to higher levels of use or problems. There was no difference in raw score form between an individual who drank three times and used PBS

all three times, from an individual who drank 14 times but used PBS only three times. Scaling PBS use with drinking frequency allowed the assessment of how PBS use proportionate to the number of drinking occasions was related to increases or decreases in alcohol outcomes, and yielded the expected pattern of higher levels of proportionate PBS use across all dimensions being associated with lower levels of alcohol use.

The mediation models exploring the effects of intervention receipt on growth trajectories for proportionate PBS use, and PBS growth on the growth trajectories for alcohol outcomes revealed consistent associations between intervention receipt and increases in PBS growth from baseline to week two. However, whereas proportionate PBS use was strongly correlated with alcohol outcomes at baseline, many of the parameter estimates between PBS growth and alcohol outcome growth were non-significant. This implies that cross-sectional associations did not carry over into prediction of growth. However, despite these non-significant paths, the indirect effect was still significant for most alcohol use indicators, excluding alcohol-related problems. So although the expected intervention effect was not observed, there still was an indirect influence on reduced consumption through increased PBS use.

Because the booster significantly reduced the growth trajectories for most alcohol use indicators, the significant indirect effect observed for alcohol quantity and related problems is considered to be a true mediation effect. As expected, booster receipt increased growth for proportionate PBS use, which in turn decreased the growth trajectory for alcohol quantity and related problems, leaving the direct effect between booster receipt and alcohol growth non-significant Interestingly, whereas the total effects for booster receipt on growth for most alcohol outcomes were significant, the booster

positively impacted PBS growth as expected, and cross-sectional associations between proportionate PBS use and alcohol outcomes were significant, the growth in PBS was not significantly associated with growth in alcohol outcomes for number of heavy drinking days, number of days intoxicated, and number of drinks and associated BAC on highest drinking day. It may be that PBS use and booster receipt shared too much predictive variance, causing both the effect of PBS growth on alcohol use growth and the direct effect of booster receipt on alcohol use growth to become non-significant.

What was observed in both sets of indirect effects analyses was the ability of the intervention and the booster email to consistently impact the growth trajectories of PBS use. This indicates that PBS use can be influenced, which has considerable implications for intervention research targeting college student drinking. Education about possible strategies that could be implemented and skills training to effectively use them could be components of effective interventions.

## Limitations

Although the current study had many promising findings, including the ability of personalized feedback boosters to reduce drinking, the indirect effects of interventions and feedback on PBS, and PBS in turn on alcohol outcomes, and the ability of interventions and feedback to manipulate boosters, there were also several limitations that should be addressed.

Although there were a total of three assessments, they were very temporally close (i.e., only two weeks apart). The effects observed were only verified for the short-term (up to four week), and we do not know the duration of the effects. It is possible that they will not last much longer than the assessment period, and could erode within the span of a

single month. Future research should expand on the current study by assessing intermediate (i.e., 1-3 month) and longer-term (6+ months) effects as well.

Another limitation of the current study was the abysmal rate of attrition (40.6% for week two and 68.4% for week four). Whereas many longitudinal studies are able to provide financial compensation for every participant, the current study relied on course credit and raffles. So rather than guaranteed payment, participants were offered only the chance to win for their additional participation. Additionally, many non-psychology students did not have the possibility of receiving additional course compensation for additional participation and were offered only the possibility of entering the weekly raffle. If the course compensation was their incentive to participate in the baseline procedure, the lack of additional credit may have discouraged their follow-up participation. Another possibility is that the timeline for assessments may have caused the surveys to seem more tedious than they otherwise would have been. Completing identical questionnaires two weeks apart may feel more tiresome than completing them months apart. Additionally, the initial survey completed during baseline was longer than subsequent assessments. Participants were informed that the follow-up surveys were shorter than the initial assessment, but may not have realized how short, and may have felt the first survey was too long. Additionally, whereas the computerized nature of the intervention and survey is considered a benefit to the institution due to the comparatively low strain on resources, the computerized nature of the study may have weakened participants' perceived connection to the research and to the study, reducing follow-up rates compared to studies with in-person interventions. Finally, there is anecdotal evidence (i.e., unsolicited comments after participation) that whereas some participants

found the interactive, computerized intervention to be engaging and interesting, others found it boring or tedious. Participants who disliked the baseline procedure may not have been willing to complete the follow-up assessments.

To combat these high attrition rates, future research should offer guaranteed monetary compensation in exchange for participation, should emphasize the brief nature of the follow-up surveys compared to the original survey, and should have more temporally distant follow-up sessions to prevent fatigue or boredom associated with re-taking the same survey. Additionally, the perceived connection between the participant and the researcher or study should be strengthened by increasing the interaction between the researcher and participants during the baseline study, running smaller groups of participants simultaneously (e.g., 2 or 3 instead of 20), and having the same researcher who interacted with them at baseline then follow-up with the participant for follow-up sessions. Finally, participants who do not complete their follow-up session immediately after receiving the invitation could receive a phone call from their assigned research coordinator rather than an email or text message. This may further strengthen the connection between the participant and the study, reducing overall attrition.

Another limitation of the current study was the very low internal consistency rating for the alcohol knowledge scale. It was chosen because it was part of the Alcohol 101 Plus package, and so its questions directly assessed knowledge that could have been gained by completing the intervention. No prior reliability or validity information was available on the scale. Though alcohol knowledge as represented by the scale score was significantly increased by intervention receipt, the scale has low internal consistency and further exploration indicated it was multidimensional. Thus, a single scale score does not

appropriately represent what the scale assesses. A more reliable and valid measure should be used in future studies.

Finally, the procedure for the current study provided careful control over the environmental conditions for the intervention. The baseline procedure always took place in a computer or research lab. Distractions were minimized by reserving the room only for that task, providing partitions to prevent participants from seeing one another's computer screens, providing headphones to minimize noise contamination from other participants, and having research assistants keep participants on task (e.g., not on other websites or using their mobile devices). Although this level of control contributed to the validity of the study, there is no guarantee that this distraction-free environment would be available if the intervention were administered on a larger scale. Additionally, the follow-up surveys and the personalized feedback were all online, providing no level of control over the environment. Participants may have been distracted while completing follow-up assessments or reading their personalized feedback.

**Confirmation of Booster Receipt**

Email was chosen as the medium for booster implementation in part because of the minimal burden on recipients. Requiring in-person booster sessions would not only require additional resources of the academic community, but would also reduce adherence among students. However, email has other disadvantages. Participants were asked to confirm receipt of their booster email by clicking a link at the bottom of the email. This link opened a separate window in which students were asked to type in their name. The same link was provided to recipients of the neutral email reminding them that there would be one last assessment in two weeks. Out of the 72 individuals that received

a booster email, only 39 (54.2%) confirmed booster receipt by clicking the link at the bottom of the email page. This rate is very low. However, only 9 people who received only the Alcohol 101 Plus intervention (6.4% of those who completed any follow-ups) clicked on their link in their neutral email without feedback, and 1 person in the control group (5% of those who completed any follow-ups) clicked on the link in their neutral email. Comparatively, significantly more people acknowledged receipt of their email in the booster group than in the groups that received a neutral email, $\chi^2(2) = 69.81, p < .001$.

After observing the low confirmation rates after the first year of data collection, a question was added to the final follow-up survey to assess if the booster email was received. Participants were asked if they received an email with personalized feedback from their research coordinator. Out of the 72 individuals in the booster group, 48 participated in the study after that question was added. Within that group, 43 (89.6%) participants indicated that they did receive an email with personalized feedback. However, in the intervention-only group, 26 out of 43 students (60.5%) indicated that they also received an email with personalized feedback. This is a high rate of false positives, indicating perhaps the question wasn't understood, or was quickly read so that participants missed the text about feedback. However, a significantly higher proportion of individuals in the booster group indicated that they received an email with personalized feedback than individuals in the intervention-only group, $\chi^2(1) = 10.49, p = .001$. The different proportions across groups support that the booster emails were being read with more frequency than the normal emails, but the generally low confirmation rates and the high number of false positive indicators are a limitation to any conclusions drawn.

A better method of ensuring booster delivery needs to be identified. Read receipts can be disabled, and links may be de-activated automatically unless enabled. Perhaps emails should be re-sent until the recipient confirms receipt. The booster email would be sent every day to the given email address until the recipient acknowledges receipt. Alternatively, text message reminders could be sent to participants' phones, asking them to read the booster and confirm its receipt. Or both methods could be employed simultaneously.

## Clinical Implications

The findings from the current study have a number of clinical implications for college student drinking and related problems. Failure to observe an intervention effect implies that the desired effect of reduced consumption and problems may not be observed at the over 2,500 institutions currently using Alcohol 101 Plus (Century Council, 2007). However, reduced consumption was observed across all conditions, so perhaps simple assessment may reduce use and related problems for students at academic institutions. Knowledge about alcohol was increased by the intervention, so it is possible the intervention may be more effective after the assessment effect deteriorates.

Additionally, the observed efficacy of the personalized booster delivered via email has positive clinical implications. This booster design has less cost to the institution than in-person visits, has a minimal time burden on both staff and students, and can reach more students than in-person booster sessions. It is a very efficient way to potentially reduce alcohol consumption among the student body. Although a reduction in alcohol-related problems was not observed in the current data, it is possible there is a delayed impact on problems after continued reduced consumption.

Finally, the ability of PBS to be manipulated and its consistent associations with reduced consumption and problems may indicate a new component to be incorporated into existing interventions. The current study focused only on introducing possible strategies to students. PBS education alone can impact alcohol consumption and related problems, and skills training relating to PBS could potentially have a larger impact.

## Future Directions

The findings of the current study are very promising, but future research should expand on this topic before widespread adoption of the procedure. Replicating the study with a longer timeline would assess the duration of the effects observed, and if they are sustainable across longer periods of time. Reductions in drinking lasting only four weeks may not seem a worthwhile use of resources, so evaluating the duration of reductions would be very informative. Replications of the study should also use a better assessment of alcohol knowledge with established reliability and validity, and should attempt to reduce attrition rates. Longer follow-ups may naturally improve follow-up rates if participants are getting bored completing the same survey so close together. Guaranteed monetary compensation may also improve follow-up rates. A better method of ensuring booster receipt should also be developed.

Additionally, the eligibility criteria for the current study required only four or more alcoholic drinks within the past two weeks. This resulted in a sample of college student drinkers, not necessarily heavy drinkers. Future research should place additional restrictions on eligibility criteria such as including only students who engage in heavy, episodic drinking or only students who experienced alcohol-related problems in the desired time range to increase the ability to detect effects by increasing the sensitivity of

the scales used. Specifically, this may result in significance effects for number of heavy drinking days, number of days intoxicated, and alcohol-related problems. These were all frequency/count measures that were endorsed as zero by several participants who would not be considered heavy drinkers.

In addition to improved replications, future research should explore moderators of both the booster effect and the intervention effect. Efficacy may depend on personality characteristics, or some components may be more salient for those with particular drinking profiles (e.g., episodic drinkers versus consistently heavy drinkers). Moderation exploration may help us better understand who the intervention and the booster most benefit, and why indirect effects were observed for the intervention without the presence of a significant total effect for some outcomes.

Finally, the current study demonstrated that it is possible to manipulate PBS use, and confirmed that higher proportionate use of PBS is associated with lower alcohol consumption and related problems. Future studies should examine if education about PBS, skills training, or a combination of the two is most effective at increasing proportionate PBS, and if those changes can translate to sustained reductions in alcohol use or related problems.

## Conclusion

Data from the current study failed to support the first hypothesis that participation in the Alcohol 101 Plus intervention would result in significantly reduced growth trajectories for alcohol use and related problems. However, alcohol knowledge was significantly increased, and alcohol use and related problems significantly decreased for everyone in the study, indicating that a Hawthorne Effect may be at play. It possible that

longer-term follow-ups would eventually reveal an intervention effect after the assessment effect dissipates. Interestingly, though there was not a total effect for the intervention, there was a significant indirect effect with proportionate PBS use as the mechanism of change.

Furthermore, an easily generated booster email providing personalized feedback did significantly reduce the growth trajectory for alcohol use. The implications of this finding are far-reaching, given the prevalence of online interventions targeting college student drinking, and the ability of easily-disseminated, cost-effective emails to boost efficacy. Moreover, PBS was a significant mediator for the relationship between booster receipt and many alcohol use outcomes. However, the relationship was not observed for related problems.

Using drinking frequency as a scaling factor changed the differential associations of PBS use dimensions with alcohol outcomes, such that higher proportionate use of all PBS dimensions was associated with lower rates of consumption and related problems. Additionally, the ability of experimental manipulations to impact proportionate use of PBS has potential clinical implication for intervention development and modification.

Although there were several limitations to the current study, the findings are nonetheless promising. Not only does the current study support the use of personalized feedback boosters to reduce alcohol consumption and related problems, but it also highlights practical methodological issues related to the assessment of PBS and the overall importance of PBS as a target for alcohol interventions (including its manipulability). Future research should attempt to replicate the current findings with more persistent procedures for maintaining participation rates, should assess possible

moderators of the intervention and booster effects to determine who most benefits, and

should explore education and skills training related to PBS as enhancements to other

intervention procedures.

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# APPENDIX A

## ALCOHOL 101 PLUS CONTENT

1. Administration Building
   a. The Wellness Center
      i. Handling a medical emergency (danger signs, what to do, what not to do)
      ii. "Top 10" ways to turn down a drink (things to say when offered)
      iii. How much is too much? (a list of alcohol-related problems drinkers may experience)
      iv. Nausea (how alcohol irritates your digestive system)
      v. Food effects on alcohol processing
      vi. How does intoxication occur? (describing the physical process)
      vii. The brain (link to the brain module)
      viii. Activity (staying active to drink less and self-pace)
      ix. Time (describing absorption rates, self-pacing, tips/symptoms of when you're most susceptible to the effects of alcohol)
      x. Hangover (description of symptoms of hangover and how it occurs)
      xi. Sobering up (dispelling myths about coffee, exercise, and cold showers)
      xii. Does everyone do it? (national college student drinking norms)
      xiii. Sex (tips for avoiding risky situations, sex differences in alcohol processing)
      xiv. Prescription drugs (how they can interact with alcohol, impact on BAC)
      xv. Mixing (why to mix your own)
   b. Office of the Dean
      i. Campus rules and regulations (outside link)
      ii. Truth & consequences (links to videos of "students" from other campus locations: athlete, two first year students, Greek, D.U.I.)

2. First Year Residence
   a. Christy decides if she wants to drink in the dorm (video with decision point)
      i. Yes ⇒ gets caught ⇒ campus rules and regulations (outside link)
      ii. No ⇒ others drinks
   b. National college student drinking norms
   c. "Top 10 ways" to turn down a drink

3. Greek Row
   a. Mike decides if they should throw a party for everyone with alcohol, a party for everyone without alcohol, or a party with alcohol for those of legal age (video with decision point)
   b. Responsible hosting tips
   c. Designated drivers, campus shuttles
   d. Campus rules and regulations (outside link)
   e. How to have fun without alcohol

      f.   Consequences of letting underage drinkers into a fraternity party

4. Student Residence
    a. Katie and Andre decide if they should keep playing a drinking game after feeling drunk (video with decision point)
    b. Food effects
    c. Andre drinks to much and passes out; should they call for help? (video with decision point)
    d. Alcohol poisoning
    e. Handling a medical emergency
    f. Andre is grateful to his friends for getting help, and embarrassed (video)
    g. Sheri, Perry, Alison, and Dante (videos with decision point)
    h. Tips on when you are most susceptible to alcohol's effects
    i. Videos modeling different ways to turn down a drink
    j. Sheri and Perry must decide if they should stop inebriated friends from going off alone (video with decision point)
    k. Tips on how to intervene and why it's important
    l. Videos modeling how Sheri or Perry could each intervene successfully
    m. Alison is glad Sheri stopped her, or embarrassed/worried because Sheri didn't (video)

5. Athletes & Alcohol
    a. Alcohol can affect athletic conditioning
    b. Statistics of athletes who believe other students drinking affects their team
    c. Paul (star athlete) is offered a free drink during sports season (videos with decision point)
    d. Safety tips for drinking (PBS-style)
    e. Sobering up
    f. If Paul drinks, video of poor team performance and consequences

6. D.U.I.
    a. Video of someone driving drunk, despite warnings from friends, and the consequences of getting a DUI
    b. Statistics for alcohol-related traffic fatalities
    c. Campus rules and regulations (outside link)
    d. D.U.I. media center
        i. Public service announcements (videos)
        ii. Personal testimonials (videos)

7. Virtual Bar
    a. Drink definitions and disclaimers
    b. Sex, weight, and state of residence entered by user
    c. Interact at bar
        i. Select a drink (various mixed drinks, wine, beer, shots, water, soda, light beer, coffee)
        ii. Select to sip (40 min), drink (20 min) or slam (1 min).
        iii. Clock records time passing

      iv. BAC readout incorporates drink decisions and passage of time
      v. "Friends" button indicates how same drink decisions impact others (difference weight or sex)
      vi. Select food to eat something

8. Alcohol & the Brain
    a. Frontal lobe (how alcohol can affect ability to self-monitor)
    b. Temporal lobe (perception, hearing, black-outs)
    c. Cerebellum (coordination, balance, eye control)
    d. Vestibular system (balance, feeling of spinning)
    e. Brain stem (respiration and vital functions: sleeping, passing out, coma, death)
    f. Hanover headache (possible mechanisms)
    g. Sexual function (linking alcohol to increased drive and decreased performance)

9. Library
    a. Need help?: Organizations for alcohol treatment, links to their websites, and descriptions of what they do
      i. Alcohol Screening
      ii. Alcoholics Anonymous
      iii. Adult Children of Alcoholics
      iv. Al-Anon/Alateen Family Groups
    b. Resources on campus: Organizations that promote alcohol education, treatment, and prevention, links to their websites, and descriptions of what they do
      i. The BACCHUS and GAMMA Peer Education Network
      ii. National Institute on Alcohol Abuse and Alcoholism
      iii. Harvard School of Public Health College Alcohol Study
      iv. Core Institute: Center for Alcohol and Other Drug Studies
      v. National Social Norms Resource Center
      vi. Promising Practices
      vii. Students Against Destructive Decisions
      viii. The Century Council
    c. From the government: Federal agencies that promote alcohol education, treatment, and prevention, links to their websites, and descriptions of what they do
      i. Department of Education
      ii. Safe & Drug-Free Schools Program Manager
      iii. Bureau of Alcohol, Tobacco, and Firearms
      iv. U.S. Department of Labor's Working Partners for an Alcohol and Drug Free Workplace
      v. National Highway Traffic Safety Administration
      vi. Department of Health and Human Services Centers for Disease Control and Preventions
      vii. Center for Substance Abuse Prevention
      viii. Center for Substance Abuse Treatment

        ix. Health Finder
        x. National Institute on Alcohol Abuse and Alcoholism
        xi. Substance Abuse Treatment Facility Locator
        xii. National Clearinghouse for Alcohol and Drug Information
        xiii. The BACCHUS and GAMMA Peer Education Network
        xiv. National Institute on Alcohol Abuse and Alcoholism

   d. Handouts (link to handouts of information available elsewhere in the program)
      i. Designated driver, Disclaimer, Does everyone do it?, Drunk driving, Food effects, Hangover, Hosting tips, How much is too much?, How does intoxication occur?, Library welcome, Medical emergency, Mixing your own drinks, Mood, Nausea, Physical effects, Prescription drugs, Real buzz, Rules and regulations, Safety tips, Saying no with a twist, Sex tips, Sobering up, Time, Top 10 ways to refuse a drink, Women are special.

   e. Campus rules and regulations (outside link)

10. The Kiosk: the same "flyers" found at the Wellness Center

11. Student Union
   a. Media center (same as D.U.I. media center), Commuter Board (same as Wellness Center flyers)
   b. B4U Drink tutorial: quiz to assess knowledge about alcohol and how it affects the body and behavior

# APPENDIX B

## PROTECTIVE BEHAVIORAL STRATEGIES

How often did you use the listed drinking control strategies in the past 2 weeks?

| | None | 1 time | 2 times | 3 times | 4 times | 5 times | 6 times | 7 times | 8 times | 9 times | 10 times | More than 10 times |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1. Choose to avoid situations where heavy drinking is likely | | | | | | | | | | | | |
| 2. Choose to participate in enjoyable activities that do not include alcohol consumption | | | | | | | | | | | | |
| 3. Finding other ways besides drinking to reduce stress | | | | | | | | | | | | |
| 4. Practicing ways to be more comfortable in social settings without using alcohol | | | | | | | | | | | | |
| 5. Being prepared with effective coping strategies in situations where you think heavy drinking is likely | | | | | | | | | | | | |
| 6. Limiting cash before going out to drink | | | | | | | | | | | | |
| 7. Avoiding carrying credit cards or ATM cards when going out to drink | | | | | | | | | | | | |
| 8. Keeping track of how many drinks you have | | | | | | | | | | | | |
| 9. Drinking slowly | | | | | | | | | | | | |
| 10. Spacing drinks over time | | | | | | | | | | | | |
| 11. Eating before and while you are drinking | | | | | | | | | | | | |
| 12. Alternating alcoholic and nonalcoholic beverages when you are drinking | | | | | | | | | | | | |
| 13. Choose not to participate in drinking games when given the opportunity | | | | | | | | | | | | |
| 14. Refusing drinks | | | | | | | | | | | | |
| 15. Being aware of internal body sensations that indicate you are getting intoxicated | | | | | | | | | | | | |
| 16. Drinking beer with a lower | | | | | | | | | | | | |

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| alcohol content (light beer) instead of stronger alcoholic beverages | | | | | | | | | | | | |
| 17. Choose not to do shots when available | | | | | | | | | | | | |
| 18. Choose not to funnel, shotgun beers, or do keg stands when those activities are available | | | | | | | | | | | | |
| 19. Choose not to "pre-game" or "pre-bar" (i.e., drinking before going out) | | | | | | | | | | | | |
| 20. Engage in activities while drinking to space out drinks (e.g., dancing, playing pool, darts) | | | | | | | | | | | | |
| 21. Limit drinking to certain days of the week | | | | | | | | | | | | |

# APPENDIX C

## ALCOHOL USE QUESTIONS

On how many of the last 2 weeks did you consume alcohol? _____

On how many of the last 2 weeks did you drink to the point of getting drunk? _____

Please keep in mind that a *drink* is a 12-oz bottle or can of beer, a 5-oz glass of wine or wine cooler, a 1.5-oz shot of hard liquor such as rum, gin, vodka, or whiskey straight or in a mixed drink, or similar portion of alcohol. Use your best estimate of drinks based on this definition.



1.5 oz = 1 serving      12 oz bottle = 1 serving      5 oz = 1 serving

In the past 2 weeks, how many times have you consumed five or more drinks (if you are male) or four or more drinks (if you are female) on a single occasion? _____

Think of the one day you consumed the most alcohol in the last month; How many standard drinks did you consume on that day? _____

On this heaviest drinking day, approximately how many hours passed from the beginning of the first drink to the finishing of the last? _____

We ask that you fill in the following grid with the number of standard drinks you consumed each day in the previous two weeks. Please also indicate how many hours passed while you were drinking. Enter a "0" to indicate days on which you did not drink.

**Week 1**

Please indicate the week you are reporting in the below grid:
( ) Monday August 30 – Sunday September 5
( ) Monday September 6 – Sunday September 12
( ) Monday September 13 – Sunday September 19
( ) Monday September 20 – Sunday September 26
( ) Monday September 37 – Sunday October 3
( ) Monday October 4 – Sunday October 10
( ) Monday October 11 – Sunday October 17
( ) Monday October 18 – Sunday October 24
( ) Monday October 25 – Sunday October 31
( ) Monday November 1 – Sunday November 7

| Personal Alcohol Use | Mon | Tues | Wed | Thurs | Fri | Sat | Sun |
|---|---|---|---|---|---|---|---|
| How many standard drinks did you consume each day during this week? | | | | | | | |
| How many hours passed during this drinking occasion? | | | | | | | |

**Week 2**

Please indicate the week you are reporting in the below grid:

( ) Monday August 30 – Sunday September 5
( ) Monday September 6 – Sunday September 12
( ) Monday September 13 – Sunday September 19
( ) Monday September 20 – Sunday September 26
( ) Monday September 37 – Sunday October 3
( ) Monday October 4 – Sunday October 10
( ) Monday October 11 – Sunday October 17
( ) Monday October 18 – Sunday October 24
( ) Monday October 25 – Sunday October 31
( ) Monday November 1 – Sunday November 7

| Personal Alcohol Use | Mon | Tues | Wed | Thurs | Fri | Sat | Sun |
|---|---|---|---|---|---|---|---|
| How many standard drinks did you consume each day during this week? | | | | | | | |
| How many hours passed during this drinking occasion? | | | | | | | |

# APPENDIX D

## ALCOHOL-RELATED PROBLEMS

The next set of questions concerns whether you have experienced any of the following

problems due to drinking **in the past 2 weeks**. Please select all that apply.

☐ 1. While drinking, I have said or done embarrassing things.

☐ 2. I have had a hangover (headache, sick stomach) the morning after I had been
drinking.

☐ 3. I have felt very sick to my stomach or thrown up after drinking.

☐ 4. I often have ended up drinking on nights when I had planned not to drink.

☐ 5. I have taken foolish risks when I have been drinking.

☐ 6. I have passed out from drinking.

☐ 7. I have found that I needed larger amounts of alcohol to feel any effect, or that I
could no longer get high or drunk on the amount that used to get me high or
drunk.

☐ 8. When drinking, I have done impulsive things I regretted later.

☐ 9. I've not been able to remember large stretches of time while drinking heavily.

☐ 10. I have driven a car when I knew I had too much to drink to drive safely.

☐ 11. I have not gone to work or missed classes at school because of drinking, a
hangover, or illness caused by drinking.

☐ 12. My drinking has gotten me into sexual situations I later regretted.

☐ 13. I have often found it difficult to limit how much I drink.

☐ 14. I have become very rude, obnoxious, or insulting after drinking.

☐ 15. I have woken up in an unexpected place after heavy drinking.

☐ 16. I have felt badly about myself because of my drinking.

☐ 17. I have had less energy or felt tired because of my drinking.

☐ 18. The quality of my work or school work has suffered because of my drinking.

☐ 19. I have spent too much time drinking.

☐ 20. I have neglected my obligations to family, work, or school because of drinking.

☐ 21. My drinking has created problems between myself and my

boyfriend/girlfriend/spouse, parents, or other near relatives.

☐ 22. I have been overweight because of drinking.

☐ 23. My physical appearance has been harmed by my drinking.

☐ 24. I have felt like I needed a drink after I'd gotten up (that is, before breakfast).

# APPENDIX E

## DEMOGRAPHICS QUESTIONS

What is your name? _____

What is your ODU email address (including "@odu.edu"): _____


What is your date of birth (MM/DD/YYYY)? _____

What is your age? _____


What is your student status?

( ) Full-time

( ) Part-time


Current residence:

( ) On-campus dormitory

( ) On-campus living-learning community

( ) Off-campus house or apartment

( ) Greek-affiliated residence (fraternity/sorority)

( ) With family

( ) Other [_____]


Are you a member or pledge of a social fraternity or sorority?

( ) Not a member

( ) Currently pledging

( ) Member


What is your GPA? _____?


What racial group BEST describes you?

( ) African-American or Black

( ) Asian or Pacific Islander

( ) Caucasian or White

( ) Latino or Latina

( ) Native American

( ) Other [_____]

What is your class standing?

( ) Freshman

( ) Sophomore

( ) Junior

( ) Senior

( ) Graduate

( ) Other [_____]

What is your gender?

( ) Male        ( ) Female

What is your marital status?

( ) Single

( ) Married

( ) Divorced

( ) In a committed relationship

( ) Other [_____]

Did you ever have Ms. Abby Braitman as a psychology course instructor?

( ) Yes        ( ) No

Have you ever received formal treatment for your alcohol use?

( ) Yes        ( ) No

If yes, please indicate the type of treatment you received:

( )     Minister, priest, rabbi, chaplain or other religious leader

( )     Psychologist, social worker, psychiatrist, substance abuse or other counselor

( )     Personal physician

( )     12 step program or other support group

( )     Outpatient alcohol treatment program

( )     Residential alcohol treatment program (such as inpatient rehab or detox)

( )     Other _____


Your weight in pounds: _____

Your height:

_____ feet

_____ inches

## Last Assessment Only

Did you receive an email from project staff with feedback from your last survey?
( ) yes        ( ) no


Please indicate your level of agreement with the following statements:

| | Strongly Disagree | Disagree | Neither Agree nor Disagree | Agree | Strongly Agree |
|---|---|---|---|---|---|
| My research coordinator cares about me. | | | | | |
| My research coordinator cares about my drinking. | | | | | |
| I feel a personal connection with my research coordinator. | | | | | |

# APPENDIX F

## BOOSTER EMAILS

Personal Positive Boosters

Dear <participant's first name>,

My name is <Abby/Edward>, and I'm your personal research coordinator for Project Health-Over-Time. I'm writing to give you feedback about your answers to the latest online survey.

First, I just wanted to let you know how you compare to other ODU students. Based on information collected during a SONA survey, I estimate that <XX>% of <female/male> ODU students drink less than you.

Second, congratulations on reducing your <drinking, alcohol-related problems, drinking and alcohol-related problems> for the last 2 weeks!! Continuing to reduce your <drinking, alcohol-related problems, drinking and alcohol-related problems> can help you avoid some of the common issues associated with alcohol use including declining grades, risky sex, relationship problems, and even legal consequences.

I also wanted to let you know that there will be one last invitation to complete the follow-up survey about 2 weeks from now.

I noticed that you reported using the following strategies to control your drinking:
<Insert strategies>

Great job! But don't forget some of the other strategies you can use to reduce your drinking and minimize harm:
- Choosing to avoid situations where heavy drinking is likely
- Choosing to participate in enjoyable activities that do not include alcohol consumption
- Finding other ways besides drinking to reduce stress
- Practicing ways to be more comfortable in social settings without using alcohol
- Being prepared with effective coping strategies in situations where you think heavy drinking is likely
- Limiting cash before going out to drink
- Avoiding carrying credit cards or ATM cards when going out to drink
- Keeping track of how many drinks you have
- Drinking slowly or spacing drinks over time
- Eating before and while you are drinking
- Alternating alcoholic and nonalcoholic beverages
- Choosing not to participate in drinking games
- Refusing drinks
- Being aware of internal body sensations that indicate you are getting intoxicated
- Drinking beer with a lower alcohol content (light beer) instead of stronger alcoholic beverages
- Choosing not to do shots when available
- Choosing not to funnel, shotgun beers, or do keg stands when those activities are available

- Choosing not to "pre-game" or "pre-bar" (i.e., drinking before going out)
- Engaging in activities while drinking to space out drinks (e.g., dancing, playing pool, darts)
- Limiting drinking to certain days of the week

Also, please confirm that you received this message by going to the link below and typing in your name:
<insert link>
I have to ask you to do this so that I can confirm you received the message and I do not have to continue to try to contact you.

Let me know if you have any questions, and take care!
<Abby/Edward>, Your Personal Research Coordinator

Personal Negative Boosters

Dear < participant's first name>,

My name is <Abby/Edward>, and I'm your personal research coordinator for Project Health-Over-Time. I'm writing to give you feedback about your answers to the latest online survey.

First, I just wanted to let you know how you compare to other ODU students. Based on information collected during a SONA survey, I estimate that <XX>% of <female/male> ODU students drink less than you.

Second, I noticed you haven't reduced your drinking or your alcohol-related problems over the last 2 weeks. 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. This is something you really should focus on.

I also wanted to let you know that there will be one last invitation to complete the follow-up survey about 2 weeks from now.

I noticed that you reported using the following strategies to control your drinking:
<Insert strategies>

Great job! But don't forget some of the other strategies you can use to reduce your drinking and minimize harm:
- Choosing to avoid situations where heavy drinking is likely
- Choosing to participate in enjoyable activities that do not include alcohol consumption
- Finding other ways besides drinking to reduce stress
- Practicing ways to be more comfortable in social settings without using alcohol
- Being prepared with effective coping strategies in situations where you think heavy drinking is likely
- Limiting cash before going out to drink
- Avoiding carrying credit cards or ATM cards when going out to drink
- Keeping track of how many drinks you have
- Drinking slowly or spacing drinks over time
- Eating before and while you are drinking
- Alternating alcoholic and nonalcoholic beverages
- Choosing not to participate in drinking games
- Refusing drinks
- Being aware of internal body sensations that indicate you are getting intoxicated
- Drinking beer with a lower alcohol content (light beer) instead of stronger alcoholic beverages
- Choosing not to do shots when available
- Choosing not to funnel, shotgun beers, or do keg stands when those activities are available
- Choosing not to "pre-game" or "pre-bar" (i.e., drinking before going out)
- Engaging in activities while drinking to space out drinks (e.g., dancing, playing pool, darts)
- Limiting drinking to certain days of the week

Also, please confirm that you received this message by going to the link below and typing in your name:
<insert link>
I have to ask you to do this so that I can confirm you received the message and I do not have to continue to try to contact you.

Let me know if you have any questions, and take care!
<Abby/Edward>, Your Personal Research Coordinator

Non-Booster Email for Non-Booster Participants

Dear Participant,

The following is an auto-generated message. If you are not <u>\<participant's first name\></u> you may disregard this email.

Thank you for your participation thus far for Project Health-Over-Time!

There will be one last invitation to complete the follow-up survey about 2 weeks from now. We look forward to your continued participation.

Please confirm receipt of this message by going to the following link and typing in your name: \<insert link\>
This is necessary so that we do not continue to try to contact you.

Thank you,
The Researchers for Project Health-Over-Time

# VITA

## Abby L. Braitman

| 2003 | B.A., Psychology |
| | University of Maryland |
| | College Park, MD |

2003      B.A., Psychology
         University of Maryland
         College Park, MD


2004 – 2012   Ph.D. Program, Applied Experimental Psychology
             Old Dominion University
             Norfolk, VA


2006      M.S., Experimental Psychology
         Old Dominion University
         Norfolk, VA

## HONORS and AWARDS

2005 – 2007   Dominion Scholar, Old Dominion University
2010 – 2011   University Fellow, College of Sciences, Old Dominion University

## SELECT PUBLICATIONS

Braitman, A. L., Derlega, V. J., Henson, J. M., Robinett, I., Saadeh, G. M., Janda, L. J. Hixon, M., & Miranda, J. (2008). Social constraints in talking about diabetes to significant others and diabetes self-care: A social-cognitive processing perspective. *Journal of Social and Clinical Psychology, 27*(9), 949-969.

Braitman, A. L., Kelley, M. L., Ladage, J. E., Schroeder, V., Gumienny, L., Morrow, J. A., & Klostermann, K. (2009). Alcohol and drug use among college student adult children of alcoholics, *Journal of Alcohol and Drug Education, 53*(1), 69-88.

Kelley, M. L., Braitman, A. L., Henson, J. M., Schroeder, V., Ladage, J., & Gumienny, L. (2010). Relationship among depressive mood symptoms and parent and peer relationships in collegiate children of alcoholics. *American Journal of Orthopsychiatry, 80*(2), 204-212.