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Influencing Motivation for Alcohol through Social Bonding

by

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A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
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ABSTRACT

Human survival depends upon the ability to cooperate by forming affiliative social bonds. Social bonding should therefore be a powerful motivating force in practically all human decision making. Past research demonstrates that social bonding and motivation for alcohol consumption share similar psychological and neurobiological pathways. In this study, we attempted to reduce alcohol motivation by enhancing perceptions of social bonding prior to and during the hours and days when alcohol consumption was most likely. In a predominantly female college student sample, we found mixed support for our hypotheses that a novel social bonding manipulation delivered through mobile technology would satiate alcohol reward anticipation and reduce alcohol consumption during the weekend. However, the dramatic changes in social life caused by the COVID-19 pandemic made it challenging to draw robust inferences regarding our hypotheses due to uncertainty about how the phenomena under investigation may have changed. We also were unable to recruit enough participants to obtain a sample size that reached sufficient statistical power based on a priori power analysis. Nevertheless, these tentative findings provided preliminary support and guidance for future studies that may explore the role of social bonding as a motivational mechanism in alcohol consumption.

CHAPTER ONE:

INTRODUCTION

Alcohol consumption is intertwined with the social aspects of human life. Alcohol is often perceived to be a social lubricant used in everyday life and/or for social celebrations, although the potentially antisocial effects of alcohol consumption are also commonly acknowledged (Brown, Goldman, Inn, & Anderson, 1980; Fairbairn & Sayette, 2014; Robin Room, 2001). Variations in cultural and social contexts are predictive of both individual alcohol consumption levels and subjective responses to alcohol consumption (de Wit & Sayette, 2018; Fairbairn, 2017; Pliner & Cappell, 1974; Stitzer, Griffiths, Bigelow, & Liebson, 1981; Sudhinaraset, Wigglesworth, & Takeuchi, 2015). Nevertheless, researchers from a variety of disciplines including neuroscientists working with animal models of addiction, psychologists implementing alcohol-placebo administration studies, and sociologists summarizing ethnographic descriptions of alcohol's role in different societies have lamented that social processes are often ignored when explaining alcohol use, misuse, and its consequences (Fairbairn & Sayette, 2014; Heilig, Epstein, Nader, & Shaham, 2016; Robin Room, 2001; Venniro et al., 2018). Some have even suggested that a lack of incorporating social processes into neuroscientific studies of addiction has directly inhibited progress in treatments for substance use disorders, as exemplified by the fact that the most effective treatments for various substance use disorders have been around for decades despite great advances in understanding the neurobiological underpinnings of addiction (Heilig et al., 2016). This is notable, given that the former head of the National Institute on Drug Abuse famously declared addiction to be a "brain disease," while many of the neurobiological theories of addiction upon which his statement stands rarely even mention the word "social" in their treatises on addiction (Everitt & Robbins, 2016; Meyer, King, & Ferrario, 2015; Robinson & Berridge, 2008; Wise & Koob, 2014). Although Leshner (1997) did acknowledge that social context greatly influences the development and manifestation of addiction, social context was merely described as any other conditioned environmental cue. In contrast, we will take the position that social processes are more than conditioned cues for alcohol consumption, and rather that social processes play a powerful role in the motivational pathways that determine substance use.

Similar Neural Pathways Underlie Both Drug and Social Behaviors

At the neurobiological level of analysis, abundant research over the past few decades strongly suggests that the neural systems associated with substance use are also associated with social processes. General reward learning is most clearly associated with dopamine pathways among the ventral tegmental area, ventral striatum, and prefrontal cortex (Berke, 2018; Berridge, 2001; Depue & Morrone-Strupinsky, 2005; Knutson & Wimmer, 2007; Meyer et al., 2015; Navratilova & Porreca, 2014). These systems are theorized to underlie reward learning caused by both substance use and "natural reinforcers" (e.g. social affiliation), and they are responsible for supplying the motivational energy and direction for guiding behavior and generating movement (Berke, 2018; Simpson & Balsam, 2015). Critically, dopamine in these systems is regulated by oxytocin and endogenous opioids, both of which are known to be heavily influenced by affiliative social interactions (Depue & Morrone-Strupinsky, 2005; Sarnyai & Kovács, 2014). Contextual features and contingencies related to the specific rewards are also encoded in these systems, as well as in the amygdala and hippocampus, and likely reflect informational nodes of associative memory networks that anticipate future rewards (e.g. placebo effects; Benedetti, Carlino, & Pollo, 2011; Navratilova & Porreca, 2014).

The neurobiological systems underlying the negatively reinforcing properties of substance use also underlie the negatively reinforcing properties of social affiliation. "Social buffering", which can be defined as the dampening of distress when an animal has affiliative interactions with another member of its own species (e.g. soothing touch; comforting vocalizations), can decrease corticotrophin-releasing factor (CRF), which is a hormone that is involved with increased hypothalamic-pituitary-adrenal (HPA) axis activity in response to stress (Kikusui, Winslow, & Mori, 2006). The association of CRF and the HPA axis with social buffering is significant because both CRF and HPA axis activity are theorized to be part of the brain's "antireward" system that dampens the experience of reward in order to achieve homeostasis that is adaptive to environmental conditions. It has been suggested that in addiction, CRF and the HPA axis are dysregulated to produce an "allostatic" state during drug withdrawal in which CRF and the HPA axis become overactive, resulting in increased subjective experiences of stress and dysphoria (Koob & Le Moal, 2008). Oxytocin and endogenous opioids interact to inhibit CRF and HPA axis activation, and as described previously, oxytocin and endogenous opioids are critically involved in social affiliation and bonding processes (Kikusui et al., 2006; McGinty, King, O'Neill, & Becker, 2019). In contrast to social buffering that may protect against dysregulating the antireward system, social exclusion can increase activity in the insula, which is associated with both pain perception and increased drug craving (Heilig et al., 2016). Recent evidence from a rat model of addiction has even demonstrated that providing opportunities for social reward inhibits insula activity, which was associated with increased drug abstinence (Venniro et al., 2018).

Therefore, the neurobiology underlying both the positive and negative reinforcement of substance use clearly overlaps with the systems underlying social processes. It is reasonable, then, to assume that neurobiological theories of substance use behaviors might incorporate social dynamics as explanatory mechanisms. However, precise predictions on how altering the neural substrate would impact these social processes is unclear, and highlights the limitations of a biologically reductionistic perspective. This may also explain the aforementioned lack of novel pharmacological treatments for addiction. Consider the use of opioid antagonists (e.g. naltrexone) to treat opioid use disorder. By inhibiting opioid activity, naltrexone is thought to block the rewarding effects of illicit opiates (Karoly, YorkWilliams, & Hutchison, 2015). However, inhibiting opioid activity may also decrease subjective perceptions of social bonding in patients receiving naltrexone (Inagaki, 2018). Counterintuitively, decreasing subjective perceptions of social bonding may actually increase motivation for seeking out social affiliation, in the same way that subjective feelings of hunger increase motivation for seeking food. Animal models support the notion that opioid antagonists can inhibit perceptions of social bonding and increase motivation for social bonding, as Depue et al. (p. 323, 2005) bluntly stated, “Naloxone or naltrexone in small doses apparently reduces the reward derived from social interactions, because these substances increase attempts to obtain such reward…” At a pharmacological level, the increased motivation to obtain social bonding may be explained by the reduced activation of opioid receptors in the nucleus accumbens, which prevents the downregulation of dopamine (i.e. the subjective satiation produced by consummation of the reward is prevented; motivation for the reward does not subside). Therefore, beyond just inhibiting the rewarding effects of illicit opiates, the perceived lack of social bonding induced by naltrexone could be helpful for recovery from addiction because it may motivate the patient to seek out and engage potentially supportive social affiliations. However, increased motivation for social bonding comes at the heavy cost of decreased subjective perceptions of social bonding, which likely enhances sensitivity to social exclusion/rejection, and the resulting negative affect (e.g. loneliness, stress, pain, anxiety) significantly increases the risk of relapse (Koob & Le Moal, 2008).

On the other hand, consider the use of opioid agonists (e.g. methadone; buprenorphine) to also treat opioid use disorder. By stimulating opioid activity at lower levels, buprenorphine is thought to provide the desired pharmacological effects of illicit opiates (e.g. heroine) and therefore reduce craving (Volkow, Koob, & McLellan, 2016). Based on the overlapping neurobiology of drug and social reward previously discussed, it is likely that patients receiving buprenorphine will have enhanced subjective perceptions of social affiliation (Inagaki, 2018). In contrast to the effects of opioid antagonists, motivation for seeking out and maintaining social affiliations may be decreased as opioid agonists enhance subjective perceptions of social reward and induce feelings of satiation that are normally produced only by actual consummation of social rewards (Depue & Morrone-Strupinsky, 2005), in the same way that eating leads to satiation of hunger and reduced motivation to seek food. Indeed, satiation of motivation for social affiliation has been observed at the psychosocial level of analysis with some evidence showing that experimentally increasing subjective perceptions of social acceptance results in decreased efforts to obtain social acceptance (DeWall, Baumeister, & Vohs, 2008). Although buprenorphine may reduce craving, the concurrent reduction in motivation to develop affiliative social ties with potentially supportive individuals would negatively influence long-term recovery from addiction (Havassy, Hall, & Wasserman, 1991). Therefore, accounting for the bidirectional influences between the neural substrate of addiction and social processes may advance a scientific understanding of substance use behaviors that has significant clinical value.

Social Influences as Protective Factors

The complexity of the interface between social processes and substance use is apparent at the psychosocial level of analysis. Social influences are both powerful risk and protective factors in the development of substance use disorders, including alcohol use disorder (Sher, Grekin, & Williams, 2005). For instance, the so-called "maturing-out" of heavy alcohol use that occurs in American adults during their mid-to-late twenties may be attributed to shifts in social responsibility and social status that are the result of beginning careers, accruing financial resources, and starting families (Merrill & Carey, 2016). These social shifts likely produce motivational competition between desires to continue using substances (i.e. approach inclinations) based on their anticipated reinforcing effects (i.e. positive expectancies) and desires to avoid using substances (i.e. avoidance inclinations) based on their anticipated punishing effects (e.g. social disapproval from family, coworkers, employers; lost opportunity to accrue financial benefits from steady employment) (Goldman, Darkes, Reich, & Brandon, 2010; Stritzke, McEnvoy, Wheat, Dyer, & French, 2007). Assuming the veracity of the "maturing-out" hypothesis, most individuals who used substances during their adolescent and young adult years experience key motivational shifts for decreasing substance use that coincide with, and are likely partially caused by, the development of social goals that directly conflict with heavy substance use (Merrill & Carey, 2016; Sher et al., 2005).

Other evidence for the potentially protective nature of social influences comes from research suggesting that affectionate, supportive, and authoritative parental relationships (e.g. monitoring of child's social interactions; expressing disapproval of substance use) strongly predict decreased substance use in childhood and adolescence (Masten, Faden, Zucker, & Spear, 2008; Sher et al., 2005; Sudhinaraset et al., 2015; Swadi, 1999). Supportive and nurturing parental relationships can provide a host of benefits for children and adolescents that would protect against substance use, including physically limiting access to substances, preventing or mitigating stressful events, reinforcing healthy self-regulation skills, and instilling values inconsistent with heavy substance use (Biglan, Flay, Embry, & Sandler, 2009). These processes are also likely involved in the development of expectancies for substance use through social learning, so that even before a child ever uses substances, he/she has information stored in memory as associational linkages that will be activated by future contexts to guide motivational processes that facilitate or deter substance use (Bekman, Goldman, Worley, & Anderson, 2011; Donovan, Molina, & Kelly, 2009; Miller, Smith, & Goldman, 1990). If children and adolescents learn from their family and community (including peers) to anticipate rewards from goals that are inconsistent with substance use, then opposing internal motivational states may compete with and ultimately deter motivations for substance use (Stritzke et al., 2007). Besides providing a social environment that is prohibitory and disapproving of substance use, having strong parental ties may also protect against substance use by social buffering of stress and anxiety (Kikusui et al., 2006). It is apparent that social buffering of stress and anxiety is not exclusive to parental relationships, and may explain why social support is generally linked to decreased relapse rates in recovery from addiction (Havassy et al., 1991).

Social Influences as Risk Factors

On the other hand, social influences can be powerful risk factors for substance use (Sher et al., 2005). Affiliating with deviant peers during childhood and adolescence (Masten et al., 2008), frequenting social environments that encourage heavy drinking (e.g. college parties; Merrill & Carey, 2016), and experiencing social exclusion are strong predictors of substance use disorders (Heilig et al., 2016; Sher et al., 2005). These findings highlight the double-edged nature of social interactions that we all commonly experience: many of our most influential sources of both reward/comfort and stress/pain come from our relationships with others (Baumeister & Leary, 1995; Heilig et al., 2016; Kikusui et al., 2006). Relational conflicts or even the perceived absence of affiliative social interactions can result in increased stress and anxiety that may lead to maladaptive coping through negative reinforcement with substances, making social isolation and exclusion risk factors for addiction (Koob & Le Moal, 2008). In contrast, to explain why the presence (vs. absence) of affiliative social interactions can also be a risk factor for substance use, it is useful to first highlight some basic principles of learning underlying both social reward and drug reward.

At the psychological level, reward learning (and learning in general) can be explained from a memory network perspective that emphasizes associational linkages between informational nodes that are developed through personal experiences (e.g. classical and operant conditioning) or passed on through social communication and direct observation of others (Bar, 2010; Berridge, 2001; Berridge, Robinson, & Aldridge, 2009; Boyd, Richerson, & Henrich, 2011; Goldman, 2002). These associational networks and the resulting hierarchical information structures become activated by contextual cues in the environment and generate probabilistic inferences ("predictions") which are acted upon by the organism to successfully navigate through the environment (Berridge et al., 2009; Goldman, 2002; Tenenbaum, Kemp, Griffiths, & Goodman, 2011). Consequently, as an individual repeatedly uses substances in social contexts, information about the people, location, time, setting, etc. becomes paired with substance use in memory. Whenever these contextual cues (including social stimuli) activate informational nodes associated with substance use, substance use cognitions become activated and influence ongoing behavior (Groefsema, Engels, Kuntsche, Smit, & Luijten, 2016). This is why Leshner (1997) referred to social context as a conditioned environmental cue. However, beyond simply cuing substance use, social context can also influence the subjective experience of using substances in the moment, with most research suggesting that being in affiliative social environments while using substances significantly enhances the subjective reward from substance use (e.g. positive affect, "liking"; de Wit & Sayette, 2018; Pliner & Cappell, 1974; Sayette, 2017; Young, Gobrogge, & Wang, 2011). The enhanced subjective reward from using substances with other people would clearly reinforce substance use, and likely explains why alcohol expectancies of social facilitation are influenced by alcohol cues and predict future alcohol consumption (Moltisanti, Below, Brandon, & Goldman, 2013; Smith, Goldman, Greenbaum, & Christiansen, 1995).

Importance of Context and Temporal Dynamics

Integrating the literature on how social processes can both increase and decrease substance use also requires further acknowledgment of the situational complexity involved. A wide array of social processes, often multidimensional and temporally dynamic, are relevant to the issue. Indeed, the entire field of social psychology attempts to partition and intricately explore this vast array of processes. For the purposes of this study, we will focus on subjective perceptions of social bonding (“connectedness”, “affiliation”, “acceptance”, etc.), because much of the literature summarized thus far reveals clear links between the motivational systems for substance use and social affiliation at both the psychological and neurobiological levels of analysis. Furthermore, both alcohol reward anticipation and subjective perceptions of social bonding are not merely static, between-person traits, but also fluctuate over the course of hours and days in synchrony with relevant contextual cues and in anticipation of relevant behavioral outcomes (Armeli et al., 2005; Benitez & Goldman, 2019; Hall, 2016; Kleiman et al., 2017; Lee et al., 2018; Monk & Heim, 2014; Neubauer, Voss, & Ditzen, 2018). Studies assessing within-person variation in these constructs generally show that about 50% of the variability in alcohol expectancies and social bonding are due to within-person changes over hourly and daily intervals. Evidence from separate studies even suggest that both the salience of positive alcohol expectancies and desires to be in social contact with other people increase during the late afternoon and early evening hours (Benitez & Goldman, 2017; Hall, 2016). The similar temporal pattern of these processes suggests that motivational drives for social affiliation and alcohol consumption may influence each other on a moment-by-moment basis. More importantly, if motivation for social affiliation and alcohol consumption are sensitive to temporally similar contextual variations, then these motivational processes and their behavioral outputs may be most susceptible to manipulation at critical timepoints within a day (e.g. late afternoon and early evening).

These potentially critical timepoints are, however, merely proxies for endogenous and exogenous environmental cues that activate memory networks underlying the anticipation and motivation of reward. Because these memory networks are most susceptible to modification immediately after they have been activated, the stimulation of these networks prior to experimental manipulation may be essential to successfully altering the networks and any resulting behaviors (i.e. memory reconsolidation; Drexler & Wolf, 2018). In the psychotherapy literature, activating patients' memory networks by exposing patients to their feared contextual stimuli (i.e. "exposure") and then altering these memory networks with new information is thought to be a key mechanism in many therapies for anxiety-related disorders. Foa and McLean (p. 6, 2016) stated with regard to treatment for post-traumatic stress disorder, "Thus, in vivo exposures are designed to achieve the two necessary conditions for emotional processing:

activation of the trauma cognitive structure and disconfirmation of the expected disasters." Therefore, large theoretical and empirical literatures on memory reconsolidation and the cognitive mechanisms underlying in-vivo exposure therapy highlight the importance of using ecologically appropriate contexts to spontaneously activate the relevant memory networks associated with our constructs of interest (i.e. anticipation of alcohol reward; perceptions of social affiliation).

Beyond accounting for the complex temporal and contextual dynamics of motivation, it is implausible to assert that social bonding will always be linked to alcohol consumption with the same magnitude and direction (e.g. always negatively correlated). Previous ecological momentary assessment (EMA) research has found that momentary loneliness (i.e. the antithesis of perceived social bonding) was associated with *increased solitary* drinking later in the day, but was also associated with *decreased social* drinking later in the day (Arpin, Mohr, & Brannan, 2015). Using mediation analyses, these authors found that loneliness was predictive of increased solitary drinking both directly and indirectly through decreased time spent interacting socially (possibly compounding the feelings of loneliness). Importantly, they also found that the link between loneliness and decreased social drinking was fully mediated (i.e. had no direct effect) by time spent interacting with others, such that loneliness was only indirectly predictive of decreased social drinking because of its association with fewer social interactions. Mohr et al. (2005) found that although spending time with friends was associated with greater alcohol consumption overall, spending time with friends also reduced the correlation between negative affect and alcohol consumed, such that negative moods were less predictive of alcohol consumption when in the company of friends. Overall, these findings highlight the potential for faulty conclusions when oversimplifying the relationship between social processes and drinking.

Important contextual features may moderate and even reverse associations between social bonding and drinking. These observational findings also beg for an experimental manipulation of social bonding as these processes emerge in the natural environment to further support or disconfirm any causal links to alcohol motivation, which was the goal of the present study.

CHAPTER TWO:

PRESENT STUDY

The goal of the present study was to determine whether enhancing perceptions of social bonding at the critical timepoints when naturally occurring environmental cues activate relevant memory networks would influence momentary alcohol reward anticipation (i.e. positive alcohol expectancies) and reduce alcohol consumption in the next few hours. Given the aforementioned discussion on social bonding and substance use, fluctuations in momentary perceptions of social bonding may be an important pathway through which alcohol use could be manipulated. Because alcohol consumption often occurs on specific days and times (e.g. evenings on Fridays and Saturdays), the critical timepoints at which a manipulation of social bonding would have the largest, or possibly any, effect on alcohol motivation would be immediately before those timepoints when alcohol consumption was most likely.

To target those critical timepoints in the naturally occurring variation of social motivation, we used EMA methods to assess our constructs of interest and deliver an experimental manipulation of social bonding (Shiffman, 2009; Shiffman, Stone, & Hufford, 2008). EMA procedures typically require repeated assessments of a participant within a day, across multiple days. Hourly assessments are often achieved by using the participants' own electronic devices (e.g. smartphones) to receive and complete questions, stimuli, tasks, etc. The key advantage to using an EMA paradigm for our study was the delivery of experimental conditions that could influence social bonding at predetermined timepoints. EMA also allowed us to capture temporally dynamic data that reflected the non-static nature of alcohol expectancies and social bonding as they varied in synchrony with naturally occurring environmental contexts. Another advantage to using EMA was greater ecological validity, since participants engaged in the study procedures while going about their everyday lives rather than in the atypical context of a university laboratory.

The primary disadvantage of using EMA for our study was the loss of experimental control over contextual variation during the study procedure, which may have made it challenging to isolate our variables of interest for experimental manipulation. Another disadvantage was the inevitability of missing data due to missed assessments (a recent meta-analysis suggested 75% overall compliance was average for EMA studies with substance using populations; Jones et al., 2018), which can be difficult from a data analytic perspective. Nevertheless, for our study, the advantages of increased ecological validity, collection of real-world temporally dynamic data, and the ability to target constructs of interest at the critical timepoints when they were theoretically most susceptible outweighed the disadvantages of decreased experimental control and the analytic challenge of missing data. The EMA paradigm allowed for relatively non-invasive manipulation of social bonding during the critical timepoints when motivation for alcohol consumption and social bonding may be surging and most vulnerable to influence (Benitez & Goldman, 2017; Hall, 2016). Although alcohol researchers have historically utilized laboratory settings to effectively implement experimental manipulations of alcohol-related constructs (Darkes & Goldman, 1998; Marlatt, Demming, & Reid, 1973; Schlauch, Gwynn-Shapiro, Stasiewicz, Molnar, & Lang, 2013), we chose to experimentally intervene when the motivation for alcohol would be “naturally” rising in anticipation of consumption (Benitez & Goldman, 2019), which may not be feasible using a traditional laboratory setting. Furthermore, we attempted to mitigate the disadvantage of not having experimental control over other contextual influences of daily life by including a measure of the construct we were attempting to manipulate during the EMA procedure (i.e. manipulation checks on perceptions of social bonding). Such manipulation checks are often used in laboratory studies to assess the internal validity of experimental interventions; therefore, manipulation checks can be similarly used in EMA studies that incorporate experimental designs. As for missing data, various statistical methods have been developed that can mitigate the potential for bias due to missing data (Baraldi & Enders, 2010), and multilevel multiple imputation may be particularly effective at accounting for missing data in EMA studies (Grund, Lüdtke, & Robitzsch, 2018).

Manipulating Social Bonding

Beyond proper timing, our experimental manipulation required a task that could alter perceptions of social bonding. To create such a task, we relied upon extensive research covering basic social processes that facilitate social connection and bonding. In our study, participants had to interact with ostensible group members (confederates) by providing social support to these group members when they requested such support. Having participants interact with other people may enhance perceptions of social bonding, as previous research demonstrates that even minimal social interactions with strangers or acquaintances in mundane settings can enhance subjective perceptions of belongingness and increase positive affect (Sandstrom & Dunn, 2014a, 2014b). Similarly, research evaluating team processes has found that brief social interactions among group members can significantly enhance trust in other group members, even when the social interactions are only “virtual” (i.e. mediated by computers, phones; Alanah & Ilze, 2009; Zheng, Veinott, Bos, Olson, & Olson, 2002). A study of social support among college students found that peer-led social support interventions decreased loneliness and increased academic performance (Mattanah, Brooks, Brand, Quimby, & Ayers, 2012). The topics for the social support interventions included making new social connections, dealing with peer pressure, missing old relationships, and balancing work, school, and social life. These topics are, of course, common experiences with which most college students could empathize. The assignment of participants to be support givers to other students as our experimental manipulation was based on psychological and neurobiological research indicating that helping others increases positive affect, decreases negative affect, and increases perceptions of social bonding, often through the same neural pathways previously discussed as being related to both drug and social behaviors (Crocker, Canevello, & Brown, 2017; Inagaki & Orehek, 2017). Although some notable boundary conditions to this phenomenon exist (e.g. being the primary caregiver for a terminally ill family member), our experimental manipulation did not require such extensive and challenging support giving.

To enhance the potency of our social bonding manipulation, participants were required to interact with their group members prior to the actual support giving task. The purpose of requiring social interaction prior to the task was based on the well-established social psychology principle that familiarity ("mere exposure") is associated with increased liking (Montoya, Horton, Vevea, Citkowicz, & Lauber, 2017; Zojanc, 1968). Another method for enhancing the potency of our social bonding manipulation was to notify participants that they had been matched to group members based on their similarity to each other. The rationale for notifying participants that they had been matched with similar individuals was to increase the perceived similarity between the participant and their group members. Increasing participants' perceived similarity to their group members would enhance perceptions of social bonding, which was supported by another well-established social psychology principle, namely that similarity breeds interpersonal connection (McPherson, Smith-Lovin, & Cook, 2001; Youyou, Stillwell, & Schwartz, 2017). Therefore, although our experimental manipulation of social bonding may be novel in many ways (see Procedure section for details), it was ultimately grounded in extensive research literature on basic social processes that are directly relevant to social affiliation.

Hypotheses

We hypothesized that increasing perceptions of social bonding through our social support giving task would decrease alcohol consumption in the short term, particularly if the social context for bonding suggested that some amount of sobriety from the participant was required. Whether the social context for bonding presumes sobriety was important to consider because, as previously discussed, alcohol is commonly consumed in social contexts to enhance perceptions of social bonding (Brown et al., 1980; Fairbairn & Sayette, 2014; Room & Mäkelä, 2000). Giving social support to individuals who were expressing personal difficulties and requesting genuine support/advice (as in our social support task) should have been perceived by participants as requiring some amount of sobriety.

We also hypothesized that increasing perceptions of social bonding through our social support giving task would decrease the salience of positively valenced alcohol expectancies. As described previously, substantial theoretical and empirical evidence reveals that anticipatory processes are critical to motivating future behavior and ultimately necessary for survival (Bar, 2010; Berke, 2018; Berridge, 2001; Goldman, 2002; Simpson & Balsam, 2015). Consistent with this body of research, we have found that positively valenced alcohol expectancies become more salient in the hours immediately preceding increased alcohol consumption (Benitez & Goldman, 2019). Therefore, if perceptions of social bonding are part of the motivational system that drives alcohol consumption, then influencing social bonding may also influence alcohol reward anticipation. Specifically, we predicted that increasing perceptions of social bonding within a social context that presumed some amount of sobriety would dampen the salience of positively valenced alcohol expectancies.

Due to the complexity of the social processes, a couple of exploratory ideas were also evaluated. First, our two main hypotheses may be moderated by individual differences in personality characteristics that are indicative of difficulties with forming social bonds (e.g. psychopathy traits, borderline personality disorder, antisocial personality disorder; Lazarus, Cheavens, Festa, & Zachary Rosenthal, 2014; McKinley, Patrick, & Verona, 2018). For instance, individuals who are predisposed to having difficulties with social bonding may be less influenced by experimental attempts to change social bonding. Indeed, the robust link between these predispositions and substance use disorders lends further support to the potential importance of deficits in social bonding for motivating substance use (Verona, Hoffmann, & Edwards, 2018). Second, given that Arpin et al. (2015) found the relationship between loneliness and drinking was moderated by whether drinking occurred in isolation or socially, similar moderation may occur with social bonding and drinking. That is, experimental attempts to change social bonding may have a larger effect on solitary drinking than on social drinking. We, therefore, evaluated these exploratory ideas along with our main hypotheses.

COVID-19

Unfortunately, the carefully designed methods for testing our hypotheses were disrupted by the onset of the COVID-19 pandemic. At the start of data collection for our study in the Winter of 2019-2020, the COVID-19 pandemic caused mass “lockdowns”, social distancing regulations, and economic downturns that dramatically changed how participants could socialize and also accelerated declines in college student enrollment, particularly among first-year students

(Nadworny, 2020). Therefore, some of the critical assumptions about how and when humans socialize (i.e. auxiliary theories and *ceteris paribus*) that formed the foundation for our hypotheses were obviously violated (Meehl, 1990). At this time, it cannot be known exactly how these violated assumptions altered the phenomena of social bonding and alcohol motivation that were the focus of our study, although we used available empirical evidence to inform our conclusions when possible (see Limitations and Future Directions section). The broad societal changes caused by COVID-19 were also potentially reflected in our data collection process, as we recruited many fewer participants within the same timeframe as our previous study that used very similar methodology with the same participant population (Benitez & Goldman, 2019). This difficulty in recruiting participants produced a shortfall in our actual sample size that was well below our planned sample size determined by power analysis (see Statistical Analyses section). Therefore, our planned inferential statistical analyses were underpowered and may be less useful for making inferences than we originally hoped. The results and conclusions reported later in this manuscript represent a hybrid of planned inferential analyses along with various descriptive and visual assessments of the data patterns that guided our interpretations. Consequently, the unexpected impact of COVID-19 on our study both theoretically (i.e. changing the phenomena) and methodologically (i.e. reduced sample size) made any conclusions we derive tentative at best, but still potentially useful for informing future research with larger sample sizes after the pandemic "ends".

CHAPTER THREE:

METHODS

Participants

Undergraduate college students who consumed alcohol at least once in the past 30 days and who owned a smartphone were eligible to participate. Although American college students are not particularly representative of the human population in a number of important ways (Henrich, Heine, & Norenzayan, 2010), the young adult age range is associated with greater rates of alcohol use disorder and loneliness than most other segments of the population (Luhmann & Hawkley, 2016; Sher et al., 2005). College students also typically consume more alcohol in social settings (Mohr et al., 2005; Thrul & Kuntsche, 2015). Therefore, college students may be particularly susceptible to experimental manipulations of social bonding and any resulting changes in alcohol consumption or expectancies (i.e. larger effect sizes), which would increase statistical power for detecting effects that may be present. Our final sample size used in the analyses was 118 (see Attrition and Missing Data section), which was far below our planned sample size of 300 based on power analysis (see Statistical Analyses section).

Baseline Measures

Demographics. The mean age of our final sample was 22 (*SD* = 4.64), and 88% of the participants were female. Our sample was 41% White, 37% Hispanic, 7% Black, 4% Asian, and 11% multiracial or other. Relative to the university population we sampled from, our sample had proportionally more females and more Hispanics. The sex imbalance in the sample was quite substantial and may be indicative of self-selection bias that may limit the generalizability of our results to males (see Limitations and Future Directions section).

Alcohol Consumption and Alcohol Use Disorder Symptoms. Typical alcohol consumption levels and alcohol use disorder (AUD) symptomatology were measured using the Alcohol Use Disorders Identification Test (AUDIT; Babor, Higgins-Biddle, Saunders, & Monteiro, 2001). This measure contained 10 items assessing typical drinking quantity, frequency, and AUD symptomatology (e.g. loss of control, health consequences) that were each rated on a scale from 0 to 4. The AUDIT has reasonable sensitivity and specificity when used to screen for DSM-5 alcohol use disorders in college students (Hagman, 2015, 2016). The mean AUDIT score in our sample was 5.5 (*SD* = 3.87), and the Cronbach's alpha (α) was 0.76, indicating good internal consistency.

Psychopathy Traits. Psychopathy traits were assessed using the Self-Report Psychopathy Scale (SRP-III; Neal & Sellbom, 2012; Neumann, Schmitt, Carter, Embley, & Hare, 2012). The SRP-III is theorized to measure four facets of psychopathy: interpersonal manipulation, callous affect, erratic lifestyle, and antisocial behavior. The combination of the interpersonal manipulation and callous affect facets reflects an interpersonal-affective psychopathy factor ("Factor 1"), whereas the combination of the erratic lifestyle and antisocial behavior facets reflects an impulsive-antisocial psychopathy factor ("Factor 2"). Although both psychopathy factors have been associated with substance use disorders, Factor 2 often has greater overlap with substance use, likely because of the connection between impulsivity/irresponsibility and heavy substance use (Verona et al., 2018). The strong association between Factor 2 and substance use suffers from criterion contamination, however, because a couple of the Factor 2 items explicitly ask about substance use. On the other hand, Factor 1 generally reflects a lack of empathy and a willingness to deceive and exploit others, which strongly suggests that individuals who are high on this factor would have difficulties with subjective perceptions of social affiliation. Both Factor 1 and Factor 2 have been positively associated with symptoms of borderline personality disorder and antisocial personality disorder (Few, Lynam, Maples, Mackillop, & Miller, 2015; Miller et al., 2010). Each SRP-III facet consisted of 16 items that were rated on a 5-point Likert scale from "strongly disagree" to "strongly agree". The mean SRP-III score in our sample was 2.13 (*SD* = 0.38), and $\alpha = 0.90$, indicating good internal consistency.

Trait Alcohol Expectancies. Trait levels of alcohol expectancies were assessed using the Alcohol Expectancy Multiaxial Assessment short form (A.E. Max; Goldman & Darkes, 2004). This measure was developed from an information processing perspective and concisely assesses broad expectancy dimensions that are generalizable to many different contexts. The A.E. Max contained 24 items rated on a 7-point Likert scale that measure alcohol expectancies which map onto a two-dimensional affective space representing valence (positive – negative) and arousal (low intensity – high intensity) dimensions. Three subfactor scores can be derived from the A.E. Max: positive-arousing (e.g. social, attractive), negative-arousing (e.g. dangerous, arrogant), and sedating (e.g. sleepy, sick). In college student samples, the A.E. Max has shown stability over monthly intervals and prospective prediction of alcohol consumption up to 1 year later (Goldman & Darkes, 2004; Settles, Cyders, & Smith, 2010). In our sample, we found that the negative-arousing and sedating subfactors were strongly correlated ($r = .53$), as has been observed previously (Benitez & Goldman, 2019), and so we combined the negative-arousing and sedating subfactors into one negative subfactor. The mean A.E. Max positive-arousing score in our sample was 3.08 (*SD* = 0.76), and $\alpha = 0.86$, indicating good internal consistency. The mean A.E.

Max negative score in our sample was 2.60 (*SD* = 0.84), and α = 0.90, indicating good internal consistency.

Self-construal. A self-construal task was used primarily as a basis from which to inform participants that they had been matched with group members who self-identified in similar ways (see Procedure section). Enhancing participants' perceived similarity to their group members was intended to facilitate participants' perceptions of social bonding (McPherson et al., 2001; Youyou et al., 2017). A shorter version of the Twenty Statements Test was used to obtain specific self-identification attributes from each participant (Grace & Cramer, 2002; Kuhn & McPartland, 1954). Instructions stated, "There are ten numbered blanks below. Please type ten different answers to the question 'Who am I?' in the blanks. Answer as if you were giving the answers to yourself, not to somebody else. Write the answers in the order that they occur to you and work quickly." Then participants saw the phrase "I am _______." repeated 10 times in a row. In recent decades, the large majority of responses (> 80%) on this task tend to be characteristics that reflect socially relevant information (e.g. "I am someone who enjoys sports"; Grace & Cramer, 2006), which served as a plausible basis for selecting group members based on similarity to each other.

EMA Measures

Social Bonding. To measure perceptions of social bonding during the EMA protocol, participants received four items taken from the General Belongingness Scale (Malone, Pillow, & Osman, 2012). This scale has shown strong convergent validity with other measures of belongingness and loneliness. Two items from the acceptance/inclusion subfactor ("I feel connected with others", "I feel accepted by others") and two items from the rejection/exclusion subfactor ("I feel isolated from the rest of the world", "I feel like an outsider") were used. These items were selected due to their brevity and high factor loadings. Items were rated on a 7-point Likert scale ranging from strongly disagree to strongly agree. Instructions stated “Rate how much you agree with the following statements right now.” to emphasize the momentary nature of social bonding that we were attempting to capture. For the GBS scores in our sample, the between-person reliability was 0.74 and the within-person reliability of change scores was 0.69, indicating moderate measurement reliability (Revelle & Condon, 2019; Xu & Shrout, 2013).

Group Identification. A group identification measure was used only on days when participants were engaged in the social bonding manipulation (see Procedure section). The purpose of this measure was to capture how strongly a participant identified with their group members in the social bonding task, as opposed to the more general feelings of social bonding assessed by the General Belongingness Scale. This measure served primarily as a validity check to assess whether participants were identifying with the other group members, which presumed some amount of perceived social bonding to the group. We used a single-item measure of social identification which previous research found to be a valid measure of in-group identification and perceived similarity to other group members (Reysen, Katzarska-Miller, Nesbit, & Pierce, 2013). Instructions stated “Rate how much you agree with this statement: I strongly identify with my group member right now.” to emphasize the momentary nature of social identification that we were trying to capture. The ratings were based on a 7-point Likert scale from strongly disagree to strongly agree. In our sample, the multilevel intercept was 5.11 with a random effects *SD* of 0.87, and 85% of the participants had person-mean scores that indicated they identified at least somewhat strongly with their fellow group members throughout the EMA protocol (i.e. scores greater than the scale mean of 4).

Alcohol Expectancies. Alcohol expectancies during the EMA procedure were measured using a word association task that has been theorized to tap into implicit memory networks which reflect past experiences and are highly sensitive to stimuli in the immediate environment (Nelson, McEvoy, & Dennis, 2000; Reich & Goldman, 2005; Rooke, Hine, & Thorsteinsson, 2008; Stacy, Ames, & Grenard, 2006; Stacy, Galaif, Sussman, & Dent, 1996; Stacy, Leigh, & Weingardt, 1997). Our previous EMA research found that responses to this word association task were both sensitive to alcohol-related environmental contexts (e.g. being in a bar or at a party) and predictive of alcohol consumption in the next few hours (Benitez & Goldman, 2019).

Specifically, participants were asked, "Think about the rest of your day. Fill in the blanks with the first word that you think of. Answer as fast as you can." They then responded 5 times in a row to "Drinking alcohol will make me __________." These self-generated words and phrases represent the most salient concepts at that moment which were associated with future alcohol consumption. To quantify these essentially qualitative responses, we combined two scoring methods that we have previously used (Benitez & Goldman, 2019). First, we imputed affective valence scores obtained from a large database of alcohol associate words which included ratings of the "pleasantness" for each word; these valence scores were rated on a 7-point Likert scale ranging from "extremely unpleasant" to "extremely pleasant". Second, each word received a salience score which gave more weight to words that are generated first. Weighting words by order of generation was crucial because the order in which associates were generated was reflective of how immediate the concept was in memory at that moment (Nelson et al., 2000; Smith, 1993; Sutrop, 2001). The valence and salience of each word were then multiplied, and the resulting values were summed across all the words generated by the participant at that specific time point to produce a single value which indicated the participant's overall alcohol expectancy valence at that moment ("alcohol expectancy s-valence").

Affect. Momentary affect was assessed using two unipolar valence scales that measured the intensity of negative affect and positive affect separately (Kron, Goldstein, Lee, Gardhouse, & Anderson, 2013; Mattek, Wolford, & Whalen, 2017). In contrast to using bipolar valence and arousal scales (e.g. "extremely unpleasant" to "extremely pleasant"), using separate unipolar scales allowed participants to express ambivalent affective experiences that may more accurately capture subjective realities. Specifically, participants were instructed, "Rate how you feel right now." Then they used 9-point Likert scales to rate their positive affect ("no good feelings" – "strong good feelings") and negative affect ("no bad feelings" – "strong bad feelings").

Drinks. Alcohol consumption was assessed by asking participants, "How many alcoholic drinks have you drank in the past 3 hours?" Images and descriptions of common drinks in standardized units (e.g. 12 fl. oz. of beer) taken from the National Institute on Alcohol Abuse and Alcoholism's pocket guide for alcohol screenings were included to assist participants in accurately assessing how many drinks they had consumed (National Institute on Alcohol Abuse and Alcoholism, 2011).

Social Context. Social context was measured by simply asking participants separately how many men and women they were with at the present moment. This variable was necessary to evaluate our exploratory idea that perceptions of social bonding may influence solitary drinking more than social drinking. Assessing the number of men and women separately also allowed us to potentially explore whether participants were drinking in mixed gender groups, which previous EMA research has found to be linked with more rapid alcohol consumption relative to drinking in homogenous gender groups (Thrul, Labhart, & Kuntsche, 2016).

Procedure

A 2x2 within-person and between-person study design was used to test our hypotheses (see *Figure 1* for a diagram of the experimental protocol and sample sizes). Participants completed basic demographic questionnaires during the SONA mass testing system. After signing up and giving informed consent for our study, participants provided their phone number and wireless carrier and completed the baseline measures. After completing the baseline assessment, participants were told that, if they were assigned to a group, they would be matched with other students who identified in similar ways to themselves on the self-construal task. The purpose of having participants believe that they were matched to similar group members was to increase the participant's perceived similarity to their group members, which should have enhanced the social bonding manipulation during the EMA portion of the study (McPherson et al., 2001; Youyou et al., 2017).

Participants were randomly assigned to two different EMA protocols: an experimental and a control. For both EMA protocols, participants were sent text messages to their smartphones that contained links to our Qualtrics survey every three hours from 12:00pm to 12:00am later that evening (see *Figure 2* for the EMA timeline). Participants had 2 hours to complete the assessment before the survey link expired. The purpose of collecting multiple timepoints across the same day was twofold. First, since alcohol consumption is most probable during the evening hours and positively valanced alcohol expectancies become more salient in the hours immediately before alcohol consumption (Benitez & Goldman, 2019; Kuntsche & Labhart, 2012; Kuntsche, Otten, & Labhart, 2015), any manipulation of social bonding to influence future drinking would need to occur earlier in the day (i.e. afternoons or early evening). Second, we wanted to evaluate how perceptions of social bonding dynamically interact with both alcohol expectancies and alcohol consumption throughout the day, since these processes and behaviors are not static within a day. These hourly assessments occurred at the same times on two separate days, and only on Fridays and Saturdays because these were the most likely days in which college students would drink alcohol (Del Boca, Darkes, Greenbaum, & Goldman, 2004; Kuntsche & Labhart, 2012; Wood, Sher, & Rutledge, 2007). We avoided data collection on weekends that included home football games or holidays which have atypical drinking patterns (Del Boca et al., 2004). EMA assessments for both protocols included the following EMA measures: alcohol expectancies, social bonding, positive/negative affect, social context, and alcoholic drinks consumed in the past 3 hours (see EMA Measures subsection within the Methods section for specific details). The alcohol expectancies measure was always performed first, because the implicit nature of the measure made it much more susceptible to influence by any measures that were completed before it. The order of the other measures was randomized to minimize any potential ordering effects.

Experimental EMA Protocol (Social Support). The experimental EMA protocol included the social bonding manipulation. A review of social cognition research concluded that an emotionally engaging interaction with another person (vs. merely observing them) is a fundamentally different experience that engages neurobiological pathways which overlap the same pathways as substance use (Schilbach et al., 2013). Therefore, the social bonding manipulation required participants to “interact” with other students by watching videos of group members and responding with short text messages that were ostensibly shared with their fellow group members. These short text messages were directly input into the Qualtrics survey by participants, which allowed us to determine if participants were compliant with the procedure. The group members were confederate students, and the same group member videos were used for all participants to avoid any intergroup variation that would result from having real group members interact. Although statistical techniques (e.g. multilevel models) may help account for between-group differences, we preferred to retain some amount of experimental control to minimize non-experimental influences on the outcomes. Participants were told to watch the videos privately and keep all information shared within the videos strictly confidential, so that their group members would feel comfortable sharing sensitive information with each other.

The specific sequence for the social bonding manipulation was as follows. On the Monday before the EMA weekend, participants received a Qualtrics email link to watch videos of their group members introduce themselves by sharing their names, activities they like to do for fun, and favorite types of movies or TV shows. Participants then responded by inputting a short message (4 sentences) to introduce themselves to their group members. As a manipulation check, participants were asked to input the names of their group members. Besides increasing the plausibility that a peer group had indeed been created for the participant, the purpose of these introductory videos was to increase liking (i.e. social bonding) by increasing familiarity, which is a well-established social psychology principle ("mere exposure"; Montoya, Horton, Vevea, Citkowicz, & Lauber, 2017; Zojanc, 1968). When participants saw videos of their same group members requesting social support later in the week (see more details below), having familiarity with their group members should have modestly increased liking for them, which would have enhanced the participant's sense of social bonding to their group members.

Participants were also informed that the purpose of their group was to assess how students provide social support for one another during their daily lives, and that they specifically had been assigned the role of a support giver in their group. On the day of the actual EMA assessments, participants received short self-recorded video clips of their group members (confederates) individually describing a personal difficulty they were currently experiencing and requesting social support. Participants were instructed to privately input a message (3-4 sentences) to be shared with only that group member and provide any advice or support that they believed would be genuinely helpful. If the participant already had an opportunity to support a group member in the previous time point, then they first read a positive text response ostensibly from that previous group member (e.g. "Hey everybody! thanks for the ideas for meeting new people to hangout with i might try them this week.") before moving on to watch and respond to the next group member's video. Participants responded to 4 separate videos, separately occurring at the 12pm, 3pm, 6pm, and 9pm assessments. The purpose for using these specific timepoints was to increase participants' perceptions of social bonding immediately before and during the times at which alcohol consumption was most likely to begin and escalate rapidly, which we hypothesized would decrease motivation for alcohol consumption at those critical timepoints (Kuntsche & Labhart, 2012; Kuntsche et al., 2015). The topics that the fellow group members requested social support for were inspired by a previous study with college students which found that peer-led social support interventions could decrease loneliness (Mattanah et al., 2012). These topics included common struggles that college students experience, such as meeting new people, coping with stress, choosing a major, and balancing work, school, and social life. After inputting their text response to the video of their group member requesting support/advice, participants completed the group identification measure to assess how much they identified with their fellow group member at that moment. Then they completed the other EMA measures previously mentioned that were common across all conditions.

The other day of hourly assessments served primarily as a within-person control condition. On this day, participants did not watch or record any videos, nor did they complete the group identification measure. They only completed the EMA measures which were common across all conditions at all timepoints. To control for ordering effects, participants were randomly assigned to receive either the day requiring social support first or the control day first. Participants were not told which days or times they would receive the social bonding task or only the EMA measures, so as to avoid any systematic adjustments participants might make that would confound our conclusions with reactivity to the study design (e.g. avoiding timepoints that required engagement in the social bonding task).

Control EMA Protocol (Problem Solving). In the control EMA protocol, participants were not assigned to a group and did not “interact” with anyone else throughout the study. Therefore, they did not watch or respond to any “ice-breaker” videos earlier in the week or social support videos during the days of their hourly EMA assessments. Instead, participants privately responded to generic prompts that covered the same problems described by the group members seeking social support (i.e. making new friends, coping with stress, choosing a major, balancing work/school/social life), except the prompts simply asked participants how they would deal with these challenges for themselves, without any explicit reference to providing support to another person (i.e. problem solving). These prompts occurred at the same timepoints as the social bonding manipulation in the experimental EMA protocol (12pm, 3pm, 6pm, 9pm). This control EMA protocol helped to differentiate whether any within-person effects observed in the experimental EMA protocol were due to the actual social support component of the manipulation or due to other potentially confounding influences (e.g. the ideas generated to socially support other group members may have altered the participant’s own problem solving behaviors that day). However, it was useful to know whether simply engaging in this reflective problem solving activity could influence motivations for alcohol use, and so participants in this condition also completed a within-person control day that was identical to the one in the experimental EMA protocol (i.e. a day when they only completed the EMA measures which are common across all experimental conditions). To control for ordering effects, participants in the control EMA protocol were also randomly assigned to receive either the day requiring individual problem solving first or the control day first. Participants were not told which days or times they would receive the problem solving task or only the EMA measures, so as to avoid any systematic adjustments participants might make that would confound our conclusions with reactivity to the study design (e.g. avoiding timepoints that require engagement in the problem solving task).

At the end of the study, all participants in all conditions received a debriefing survey that notified them of the deception in the study and allowed the social support participants to designate if we could still use their data for analyses. To assess if any participants were aware of the purpose of the study, all participants were asked if they knew anything about the study before participating or learned anything about the study while participating. We also asked participants whether they were simultaneously participating in any other study that required using their smartphones while they were engaged in our study. Participants who expressed knowledge of the purpose of the study or who were simultaneously participating in another smartphone study were removed from the final sample (see Attrition and Missing Data section).

Statistical Analyses

The richness of the data produced by our 2x2 within-person and between-person study design allowed for a wide variety of analytic opportunities. Initial visual inspection of the raw data patterns by experimental condition was used to guide our interpretation of formal inferential analyses, especially given the shortfall in sample size that limited our statistical power. The figures generated from the raw data were particularly useful as comparisons to the figures generated from the inferential models (see Results section). To formally test our main hypotheses using inferential analyses, we used multilevel models to account for the inherently dependent nature of EMA data (Curran & Bauer, 2011). The models included main effects and cross-level interactions between the randomly assigned protocol condition (between-person: social support group vs. problem solving condition) and the specific day within the protocol (within-person: assessment only control day vs. active task day). Significant interactions would support our main hypotheses if participants in the social support group drank less alcohol and had less salient positive alcohol expectancies on the day in which they completed the social support tasks (i.e. the active day), relative to their own control day and to the problem solving participants on their active day. As a validity check on our experimental manipulation, a similar cross-level interaction tested whether participants in the social support group on the active day had increased perceptions of social bonding relative to the other conditions. Important covariates that were included in all of these models are time of day, day of the week, randomization order (i.e. control day first or active day first), sex, AUDIT score, SRP-III score, and the A.E. Max positive and negative subfactor scores.

A Bayesian negative binomial multilevel model was used to test the effect of our experimental manipulation on alcohol consumption, with hourly observations (level-1) nested within persons (level-2). A negative binomial model was used because alcoholic drinks consumed were distributed as a count variable. Intraclass correlations (ICC) were used to determine the proportion of variability attributable to within-person changes (vs. between-person differences). To determine if random intercepts, random slopes for time, and/or covariance between the random intercepts and slopes should be included in the models, we used the widely applicable information criterion (WAIC) to compare model fits (Vehtari, Gelman, & Gabry, 2017). Between-person (level-2) main effects included experimental condition (social support vs. problem solving), randomization order, sex, AUDIT score, SRP-III score, and the A.E. Max positive and negative subfactor scores. Within-person (level-1) main effects included time of day, day of the week, and day of the EMA protocol (control day vs. active day). The cross-level interaction between the experimental condition and day of the EMA protocol was also included in the model.

Separate Bayesian linear multilevel models were used to test the effects of our experimental manipulation on alcohol expectancies and social bonding. Other than different dependent variables and the use of a linear function, these models were identical to the negative binomial model predicting alcohol consumption. To test our exploratory idea of moderation by psychopathy traits, the same models were specified, but also including a 3-way interaction between the cross-level interactions of our main hypotheses and the baseline measure of psychopathy. Finally, to test our exploratory idea of moderation by social context (social drinking vs. drinking alone), the same models were specified, but including a 3-way interaction between the cross-level interactions and the social context.

The Bayesian models were implemented using the *brms* package in *R*, and the default priors were used (Bürkner, 2017). Specifically, the regression *b* coefficients used noninformative uniform priors. The centered design matrix intercepts of the Gaussian models, the standard deviations of the random effects components, and the residual standard deviations used a half Student-*t* prior with 3 degrees of freedom, a scale parameter of 10, and a location parameter set at 0. The centered design matrix intercepts of the negative binomial models used the same half Student-*t* prior, but the location parameter was set at -2. The shape parameters of the negative binomial distribution used a gamma prior with shape and rate parameters both set at 0.01. Model diagnostics using *rhat* values and visual inspection of MCMC draws suggested that the MCMC chains for parameters in all our models had converged. The highest observed *rhat* value was 1.01, which was far below the threshold of 1.1 that was considered evidence of non-convergence (Bürkner, 2017). Graphical posterior predictive checks also suggested that our models could generate data that resembled the observed data.

Inferences from our analyses were made using Bayesian 95% credibility intervals and Bayes Factors (BF_{10}) which directly tested the evidence for our hypotheses versus the evidence against our hypotheses (van de Schoot et al., 2014; Wetzels et al., 2011). Bayes Factors were interpreted using Jeffreys' categories of substantial (BF_{10}: 3 – 10), strong (BF_{10}: 10 – 30), very strong (BF_{10}: 30 – 100), and decisive (BF_{10}: >100; Wetzels et al., 2011). Of note, Bayesian credibility intervals and Bayes Factors may provide conflicting inferences, such that the Bayes Factor may substantially support our hypothesis and yet 0 may be included in the 95% credibility interval (Lovric, 2019), as was the case in some of our analyses.

Multilevel multiple imputation was used to account for missing data in both the hourly assessments (level-1) and the baseline measures (level-2) (Grund et al., 2018). Using multiple imputation provides unbiased estimates when data is missing at random and tends to reduce the magnitude of bias in estimates when data is not missing at random (Baraldi & Enders, 2010). Multilevel multiple imputation was implemented using the *mice* package in *R* (van Buuren & Groothuis-Oudshoorn, 2011). To determine sample size, variance estimates from our previous EMA study (Benitez & Goldman, 2019) were input to the *powerlmm* package in *R* (Magnusson, 2018). Assuming a small effect size (Cohen's $d = 0.2$), the results indicated that 300 total participants would achieve statistical power above 0.80 to detect differences between two comparison groups. Therefore, we planned to recruit 150 participants for each of the two experimental conditions. However, due to the differential attrition rates observed during initial data collection (see Attrition and Missing Data section), we adjusted our assignment ratio from 1:1 to 2:1 to purposefully assign more participants to the social support condition. The assignment process itself was still random to preserve the experimental nature of the study. Our final sample size used in the analyses included 118 total participants, well below the 300 we planned to recruit, suggesting that our inferential analyses had limited interpretative value.

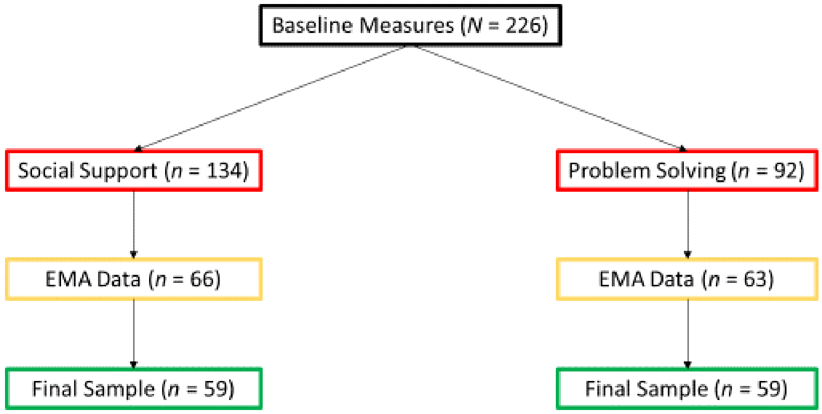

Figure 1. Experimental design and sample sizes.

Note. Diagram illustrating the process of assignment to experimental conditions and sample sizes. Red boxes indicate sample sizes that were randomly assigned. Yellow boxes indicate sample sizes that completed at least 1 EMA assessment. Green boxes indicate final sample sizes after removing a few participants for reasons explained in the Attrition and Missing Data section.

N = total sample size that was randomly assigned

n = sample size of experimental condition

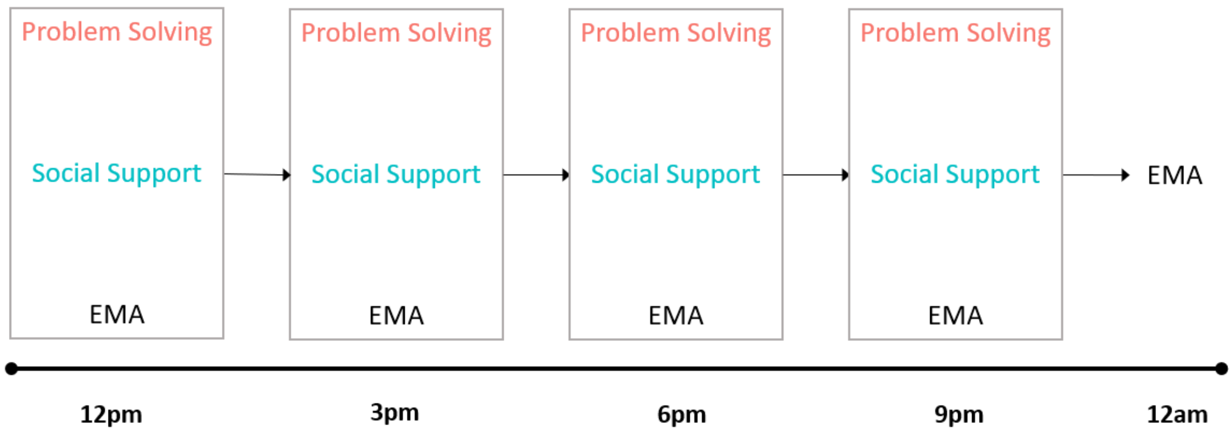

Figure 2. EMA protocol timeline.

Note. The EMA timeline showing the hourly procedure for each experimental condition. Grey boxes indicate the three possible procedures for a given day in the study. On the problem solving active day, participants randomly assigned to the control manipulation (i.e. individual problem solving) received both the problem solving task and the EMA measures at the 12pm, 3pm, 6pm, and 9pm timepoints (pink font). On the social support active day, participants randomly assigned to the experimental manipulation (i.e. giving social support) received both the social support task and the EMA measures at the 12pm, 3pm, 6pm, and 9pm timepoints (light blue font). On the within-person control day, participants in both conditions received only the EMA measures at the 12pm, 3pm, 6pm, and 9pm timepoints (black font). At the 12am timepoint, only the EMA measures were administered for all participants on both days of the study (i.e. both control day and active day).

CHAPTER FOUR:

RESULTS

Attrition and Missing Data

Of the 226 participants who completed baseline measures and were randomly assigned to conditions (problem solving *n* = 92; social support *n* = 134), 129 completed at least 1 EMA measure (43% overall attrition). However, differential attrition rates by treatment condition were observed (32% for problem solving and 51% for social support; see *Figure 1*), possibly because the social support condition required an extra step of engagement before being eligible to participate in the EMA portion of the study. Specifically, participants in the social support condition were required to respond to “ice breakers” from their ostensible group members earlier in the week before they could engage in the EMA portion of the study (see Procedure section); if they did not respond to the “ice breakers” before Friday, they did not receive any EMA prompts.

Of the 129 participants who completed at least 1 EMA prompt, 11 participants were removed from the final sample because during the debriefing they reported either simultaneously participating in another smartphone study while participating in our study (*n* = 9), identifying the purpose of the study (*n* = 1), or declining to allow us to use their data after learning about the deception (*n* = 1). Therefore, the final sample size used for analyses was 118 (problem solving *n* = 59; social support *n* = 59). We tested to see if the differential attrition rates produced baseline differences between the randomly assigned experimental conditions. We found that participants in the problem solving condition had significantly higher psychopathy (SRP-III) scores than the social support condition (Cohen’s *d* = 0.47, 95% CI [0.10 , 0.84]). The differences between conditions in alcohol use disorder symptomatology (AUDIT), age, trait alcohol expectancies (A.E. Max), and sex ratio were not statistically significant. The imbalance in psychopathy traits between our experimental conditions reinforced our decision to include this measure as a covariate in our statistical models. However, this imbalance raised concerns about selection bias confounding our results and limiting the generalizability of our findings.

In the final sample of 118 participants, 777 EMA timepoints out of a total of 1,180 possible EMA timepoints were completed (66% compliance rate). The compliance rate between experimental groups did not differ (66% for both social support and problem solving groups), and participants were slightly less compliant on the active day of the EMA protocol (64% on the active day; 68% on the control day). Thirty-five multiply imputed datasets were created to account for the overall missing data rate of 34%. Multilevel multiple imputation using the *mice* package in *R* was implemented to create the multiply imputed datasets (van Buuren & Groothuis-Oudshoorn, 2011). Variables used in the multiple imputation procedure included all the variables specified in the models used to analyze our hypotheses (see Statistical Analyses section), and a random intercept for participants was specified to impute values for level-1 variables due to the nested data structure. The results of the Bayesian multilevel models reported below were from the pooled results of the 35 multiply imputed datasets. As discussed previously, because our final sample size of 118 was well below our planned sample size of 300 informed by power analyses (see COVID-19 and Statistical Analyses sections), we also used descriptive data patterns to inform our inferences and conclusions.

Manipulation Check on Social Bonding

The descriptive means of social bonding by experimental condition in the unimputed raw data showed that participants in the social bonding active day had slightly higher levels of social bonding relative to both their within-person control day and the between-person problem solving active day (see *Figure 3*). The ICC estimate for social bonding (= 0.46, 95% credibility interval [CI: 0.37, 0.53]) indicated that 54% of the variance was due to within-person changes in social bonding over the course of hours and days. This finding was consistent with our expectation that the GBS could be used to detect within-person hourly changes in social bonding. Model comparisons using WAIC suggested that a random intercept and no random slope for time fit the data best. The main effect of the experimental condition and our hypothesis regarding the cross-level interaction on social bonding were strongly supported (BF_{10} = 11 for both main effect and interaction). Participants assigned to the social support condition had higher levels of social bonding overall (b = 0.28, 95% CI [-0.05, 0.60]), and especially on the experimentally active day (b = -0.20, 95% CI [-0.45, 0.04]; see *Figure 4*). Pairwise comparisons revealed that, on the active day, the social support participants had higher social bonding scores than the problem solving participants, and this difference was not found on the control day (see *Table 1*). However, the analyses did not suggest that the social support participants had greater social bonding on their active day than on their control day. Therefore, our inferential analyses supported the between-person difference, but not the within-person difference, observed in the descriptive data pattern shown in *Figure 3*. These mixed findings of the manipulation check suggested that our experimental manipulation may have been effective at increasing social bonding, although the absolute magnitude of the effect appears to be small.

Hypothesis: Social Bonding to Reduce Drinking

In the unimputed raw dataset, 43% of the sample reported drinking alcohol at least once during the EMA portion of the study. The descriptive drinking means by experimental condition in the unimputed raw data showed that participants in the social bonding active day drank less alcohol than both their within-person control day and the between-person problem solving active day (see *Figure 5*). The ICC estimate for alcohol consumption (= 0.85, 95% CI [0.47, 0.97]) indicated that 15% of the variance was due to within-person changes in alcohol consumption over the course of hours and days. However, the wide 95% CI for the ICC estimate suggested substantial uncertainty for the amount of variance attributable to within-person changes, anywhere from 3% to 53%. Given the relatively large proportion of participants who did not report drinking any alcohol during the EMA protocol, we assessed whether a zero-inflated negative binomial model might fit the data better; however, model comparison using WAIC revealed that adding the zero-inflated parameter did not improve the model fit. Further model comparisons using WAIC suggested that a random intercept and no random slope for time fit the data best.

The main effect of the experimental condition on alcohol consumption was not supported by the analyses (BF_{10} = 0.89) and neither was our hypothesized interaction (BF_{10} = 0.32; see *Figure* 6). However, the main effect of the EMA protocol day was strongly supported (BF_{10} = 21), such that participants drank more alcohol on the control day than on the active day (b = 0.57, 95% CI [0.02, 1.13]). The incidence rate ratio for this main effect indicated that participants drank 77% more alcoholic drinks on the control day than on the active day. Pairwise comparisons did not suggest that the social support participants drank less alcohol than the problem solving participants on the active day (see *Table 2*). The evidence also did not suggest that the social support participants drank less alcohol on their active day than on their control day, but the evidence strongly suggested that the problem solving participants drank less alcohol on their active day than on their control day. Therefore, our inferential analyses did not support the between-person difference nor the within-person difference observed in the descriptive data pattern shown in *Figure 5*. These findings were not consistent with our main hypothesis that a social bonding manipulation would decrease alcohol consumption, but this outcome may have reflected inadequate statistical power.

A scatterplot and regression lines of drinks by experimental condition and psychopathy in the unimputed raw data showed a positive slope between psychopathy and drinking on the active day for participants in the problem solving group, but not for participants in the social bonding group (see right side of *Figure 7*). However, the difference in the slopes between the active day and the control day for participants in the social bonding group appeared minimal (compare blue lines of *Figure 7*). Overall, comparing the slope patterns between the 2 graphs in *Figure 7* did not suggest much of a difference in the 2-way interactions, and therefore did not suggest a 3-way interaction. Unsurprisingly, then, the exploratory 3-way interaction between our main hypothesis for drinking and psychopathy was not supported (BF_{10} = 2). Both the descriptive data patterns and the inferential analyses did not provide much evidence for moderation of our hypothesis by psychopathy traits.

Inspection of drinking by experimental condition and social context in the unimputed raw data showed that, on the active day, the reduction in drinking between being with others and being alone was not greater for the social support group than the control group (see right side of *Figure 8*). Similarly, when alone, the reduction in drinking between the control day and the active day was not greater in the social support group than the problem solving group (compare the bar clusters labeled “alone” in *Figure 8*). The exploratory 3-way interaction between our main hypothesis for drinking and social context was substantially supported (BF_{10} = 6; see *Figure 9*). However, pairwise comparisons did not suggest that the social support participants drank less than the problem solving participants while alone on the active day (BF_{10} = 0.08). The results also did not suggest that social support participants drank less alcohol while alone on their active day than on their control day (BF_{10} = 0.54). Therefore, both the descriptive data patterns and the inferential analyses did not provide evidence for moderation of our hypothesis by social context.

We also reasoned that our hypothesis would be most apparent only at times when drinking was most likely (i.e. the evening), and the descriptive patterns of drinking over time in the unimputed raw data shown in *Figures 10* and *11* reinforced our reasoning. A post-hoc 3-way interaction between our main hypothesis for drinking and time of day was substantially supported (BF_{10} = 7; see *Figure 12*). Simple slopes comparisons revealed that the rate of dinking over time was reduced by 27% for the social support participants on their active day compared to their control day (see *Table 3*). The rate of dinking over time was also reduced by 41% for the social support participants on their active day compared to the problem solving participants on their active day. Therefore, the post-hoc 3-way interaction with time of day suggested that our social bonding manipulation effectively reduced the rate of alcohol consumption over time both between-person (i.e. compared to the problem solving condition) and within-person (i.e. compared to the control day of the EMA protocol).

Hypothesis: Social Bonding to Reduce Positive Alcohol Expectancies

The descriptive alcohol expectancy means by experimental condition in the unimputed raw data showed that participants in the social bonding active day had less positively valenced alcohol expectancies than both their within-person control day and the between-person problem solving active day (see *Figure 13*). The ICC estimate for alcohol expectancies (= 0.32, 95% CI [0.21 , 0.41]) indicated that 68% of the variance was due to within-person changes in alcohol expectancies over the course of hours and days. Model comparisons using WAIC suggested that a random intercept and no random slope for time fit the data best. The main effect of the experimental condition was decisively supported (BF_{10} = 132) and our hypothesized interaction was substantially supported (BF_{10} = 6), such that participants assigned to the social support condition had less positively valenced alcohol expectancies than the problem solving condition overall (b = -1.02, 95% CI [-1.70, -0.33]), and this difference was greater on the active day than on the control day (b = 0.44, 95% CI [-0.26, 1.11]; see *Figure 14*). Therefore, our social bonding manipulation effectively reduced the positive valence of alcohol expectancies on the experimentally active day. Pairwise comparisons revealed decisive evidence for the social support participants having less positively valenced alcohol expectancies than the problem solving participants on the active day (see *Table 4*). Substantial evidence also suggested that the social support participants had less positively valenced alcohol expectancies on their active day than on their control day. Therefore, both the descriptive data patterns and inferential analyses were largely consistent with our main hypothesis that the social bonding manipulation would decrease positively valenced alcohol expectancies.

The scatterplot and regression lines of alcohol expectancies by experimental condition and psychopathy in the unimputed raw data showed similar positive slopes between psychopathy and alcohol expectancy valence on the active day for participants in both the problem solving group and the social bonding group (see right side of *Figure 15*). For the social bonding group, we observed a negative slope between psychopathy and alcohol expectancy valence on the control day, but a positive slope on the active day (compare blue lines of *Figure 15*). The exploratory 3-way interaction between our main hypothesis for alcohol expectancies and psychopathy was not supported (BF_{10} = 1). Therefore, both the descriptive data patterns and the inferential analyses did not provide evidence for moderation of our hypothesis by psychopathy traits as we anticipated.

Inspection of alcohol expectancies by experimental condition and social context in the unimputed raw data showed that, on the active day, the reduction in positively valenced alcohol expectancies between being with others and being alone was slightly greater for the social support group than the control group (see right side of *Figure 16*). When alone, the reduction in positively valenced alcohol expectancies between the control day and the active day was greater in the social support group than the reduction for the problem solving group (compare the bar clusters labeled “Alone” in *Figure 16*). However, the exploratory 3-way interaction between our main hypothesis for alcohol expectancies and social context was not supported (BF_{10} = 1). The descriptive data patterns suggested moderation of our hypothesis by social context as we anticipated, but the inferential analyses did not support this conclusion, indicating that statistical power was potentially inadequate.

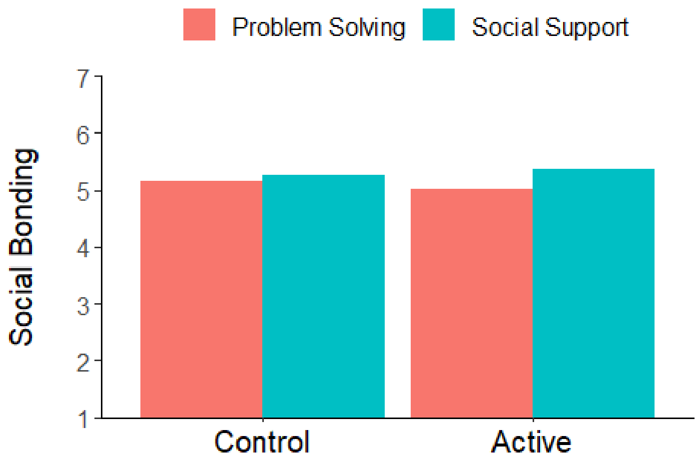

Figure 3. Social bonding (descriptive means).

Note. Descriptive mean social bonding scores by experimental condition. Only the unimputed raw data was used to generate this figure. Participants in the social support active day had slightly higher levels of social bonding relative to both their within-person control day and the between-person problem solving active day.

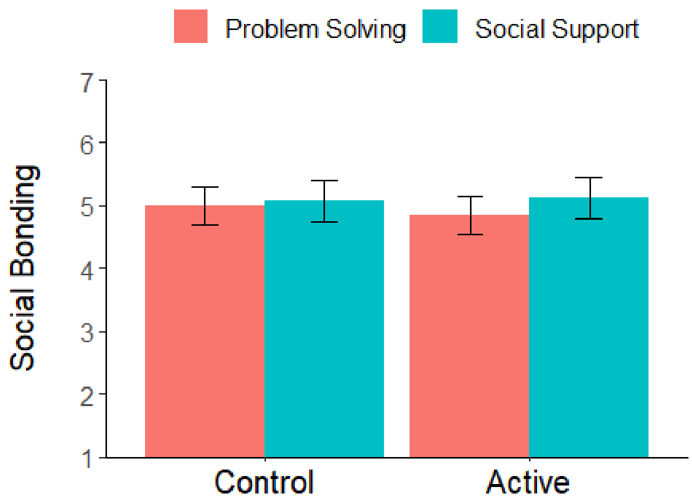

Figure 4. Social bonding multilevel model results.

Note. Results of the multilevel model comparing experimental conditions on perceived social bonding (GBS score). The expected means and 95% credibility intervals are displayed. On the active day, the social support participants had higher social bonding scores than the problem solving participants, and this difference was not found on the control day (see *Table 1*). The social support participants did not have greater social bonding on their active day than on their own control day.

Table 1. Pairwise comparisons of social bonding.

Comparison Groups			BF_{10}	*b* [95% CI]
SS active	>	SS control	3	0.06 [-0.12, 0.23]
SS active	>	P active	11**	0.28 [-0.05, 0.60]
SS active	>	P control	3	0.13 [-0.19, 0.45]
SS control	>	P control	2	0.07 [-0.24, 0.38]
SS control	>	P active	7*	0.22 [-0.10, 0.53]
P active	>	P control	0.08	-0.14 [-0.30, 0.02]

Note. Results of the multilevel model comparing experimental conditions on social bonding as measured by the General Belongingness Scale. Bayes factors (BF_{10}) and beta coefficients (*b*) with 95% credibility intervals (95% CI) are displayed.

* substantial evidence for the hypothesis

** strong evidence for the hypothesis

SS: social support condition

P: problem solving condition

Active: active day of the EMA protocol

Control: control day of the EMA protocol

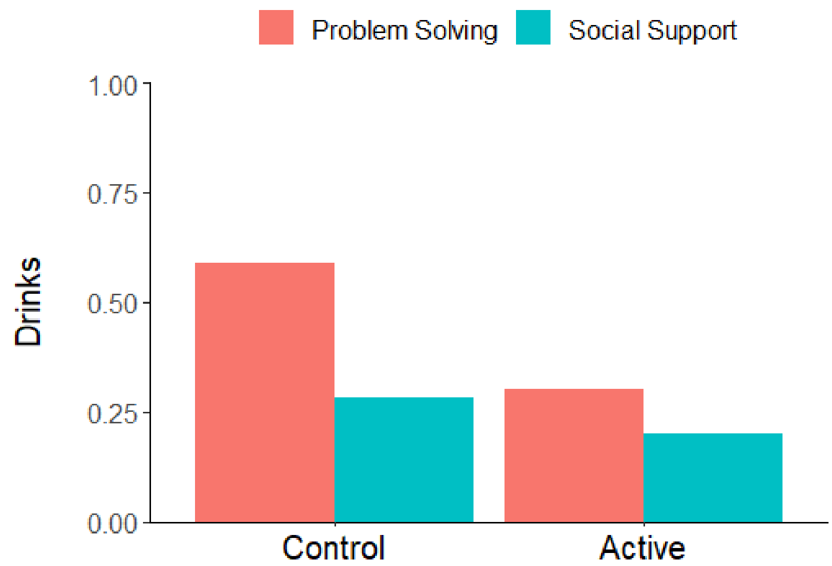

Figure 5. Alcoholic drinks (descriptive means).

Note. Descriptive mean alcoholic drinks consumed by experimental condition. Only the unimputed raw data was used to generate this figure. Participants in the social bonding active day drank less alcohol than both their within-person control day and the between-person problem solving active day.

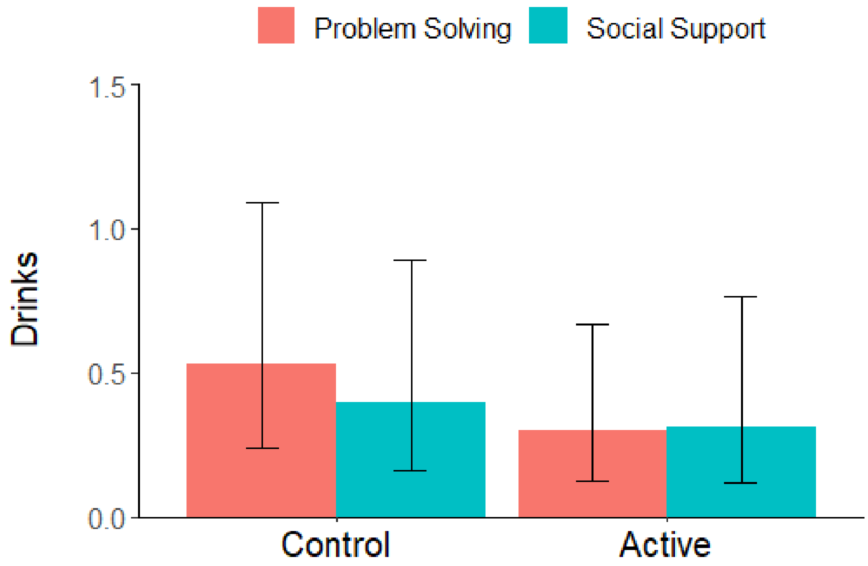

Figure 6. Alcoholic drinks multilevel model results.

Note. Results of the multilevel model comparing experimental conditions on alcoholic drinks consumed. The expected means and 95% credibility intervals are displayed. Social support participants did not drink less alcohol than the problem solving participants on the active day (see *Table 2*). The social support participants did not drink less alcohol on their active day than on their control day, but the problem solving participants drank less alcohol on their active day than on their control day.

Table 2. Pairwise comparisons of alcoholic drinks.

Comparison Groups			BF_{10}	*b* [95% CI]
SS active	<	SS control	3	-0.24 [-0.82, 0.35]
SS active	<	P active	0.89	0.05 [-0.72, 0.89]
SS active	<	P control	7*	-0.52 [-1.27, 0.25]
SS control	<	P control	3	-0.28 [-0.98, 0.41]
SS control	<	P active	0.37	0.29 [-0.47, 1.07]
P active	<	P control	21**	-0.57 [-1.13, -0.02]

Note. Results of the multilevel model comparing experimental conditions on alcoholic drinks consumed. Bayes factors (BF_{10}) and beta coefficients (*b*) with 95% credibility intervals (95% CI) are displayed.

* substantial evidence for the hypothesis

** strong evidence for the hypothesis

SS: social support condition

P: problem solving condition

Active: active day of the EMA protocol

Control: control day of the EMA protocol

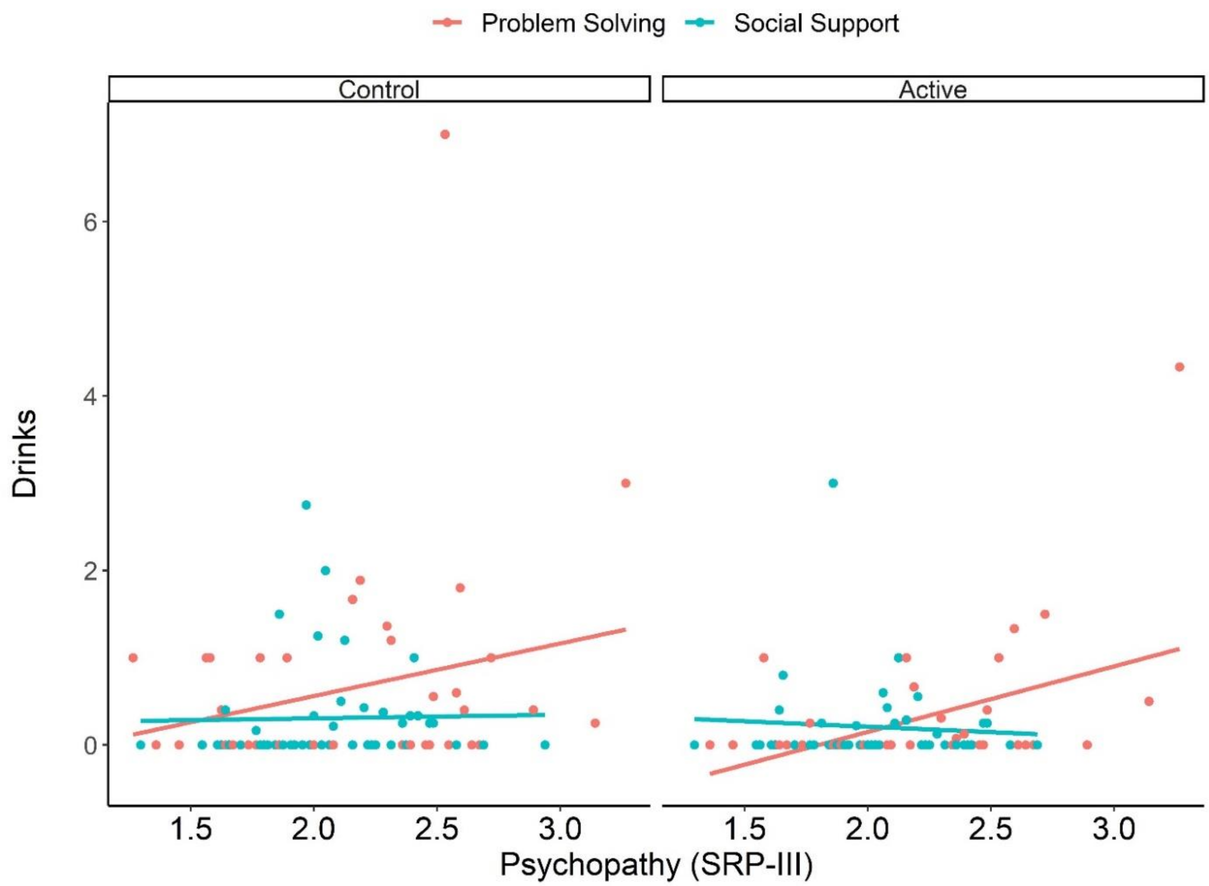

Figure 7. Alcoholic drinks by psychopathy traits (scatterplot).

Note. Scatterplot with linear regression lines of drinks by experimental condition and psychopathy traits. Only the unimputed raw data was used to generate this figure. A positive slope between psychopathy and drinking on the active day was observed for participants in the problem solving group, but not for participants in the social bonding group (compare red and blue lines on the right side graph). The difference in the slopes between the active day and the control day for participants in the social bonding group appeared minimal (compare blue lines across both graphs). However, the 3-way interaction was not supported in the analyses.

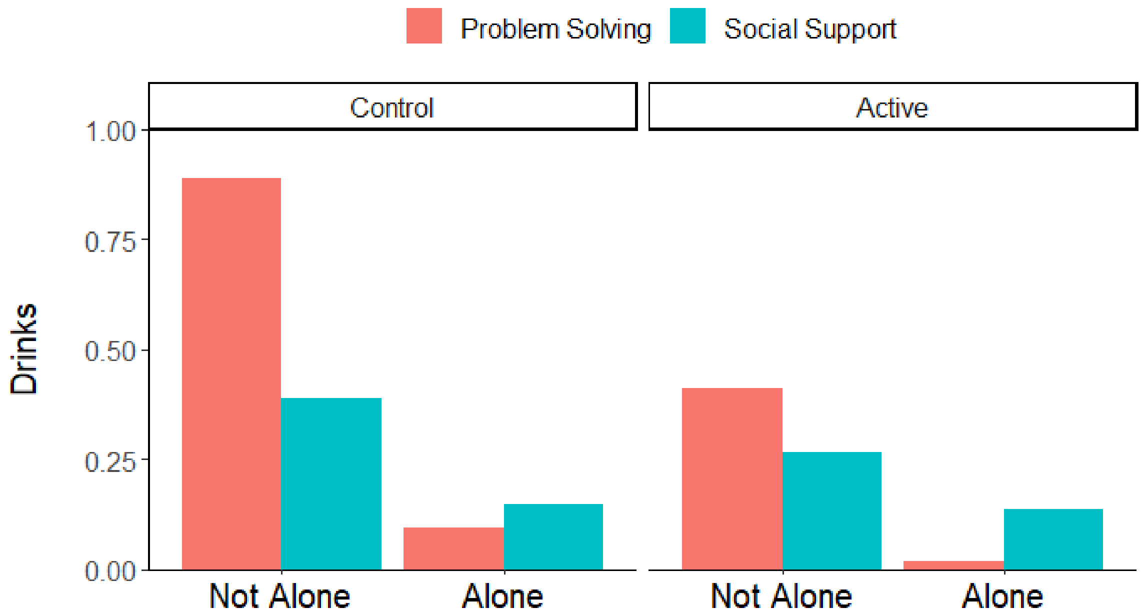

Figure 8. Alcoholic drinks by social context (descriptive means).

Note. Descriptive mean alcoholic drinks consumed by experimental condition and social context. Only the unimputed raw data was used to generate this figure. On the active day, the reduction in drinking between being with others and being alone was not greater for the social support group than the problem solving group (compare “Not Alone” and “Alone” bar clusters in the right side graph). Similarly, when alone, the reduction in drinking between the control day and the active day was not greater in the social support group than the problem solving group (compare the bar clusters labeled “Alone” across both graphs).

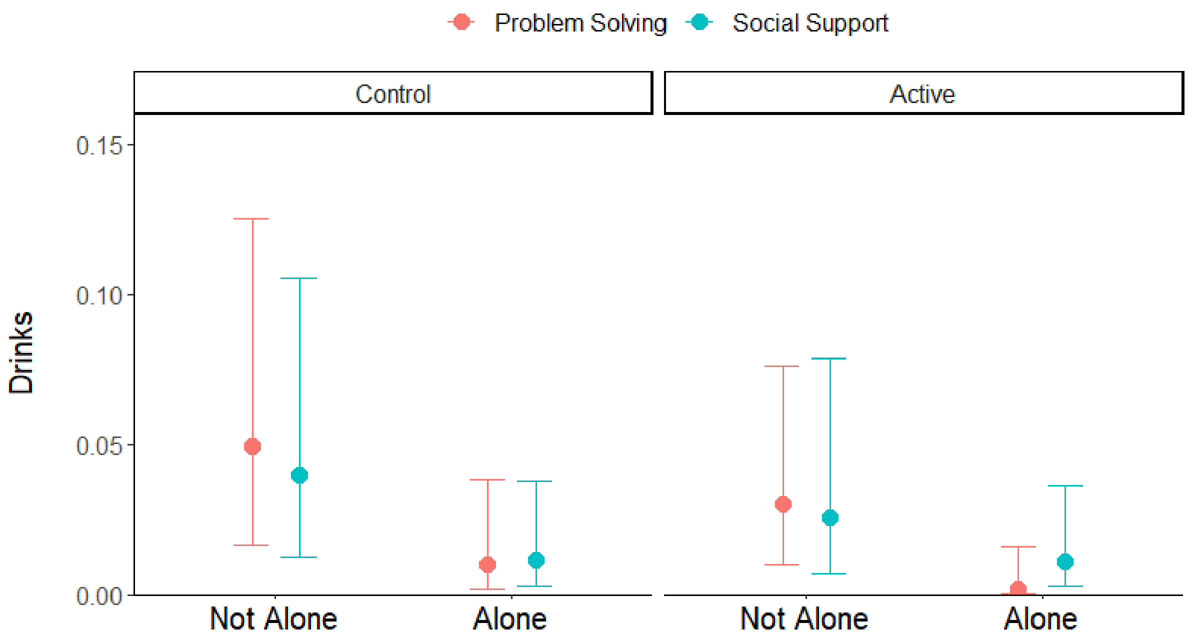

Figure 9. Alcoholic drinks by social context multilevel model results.

Note. Results of the multilevel model comparing experimental conditions by social context (alone vs. not alone) on alcoholic drinks consumed. The expected means and 95% credibility intervals are displayed. The social support participants did not drink less than the problem solving participants while alone on the active day ($BF_{10} = 0.08$). The social support participants did not drink less alcohol while alone on their active day than on their control day ($BF_{10} = 0.54$).

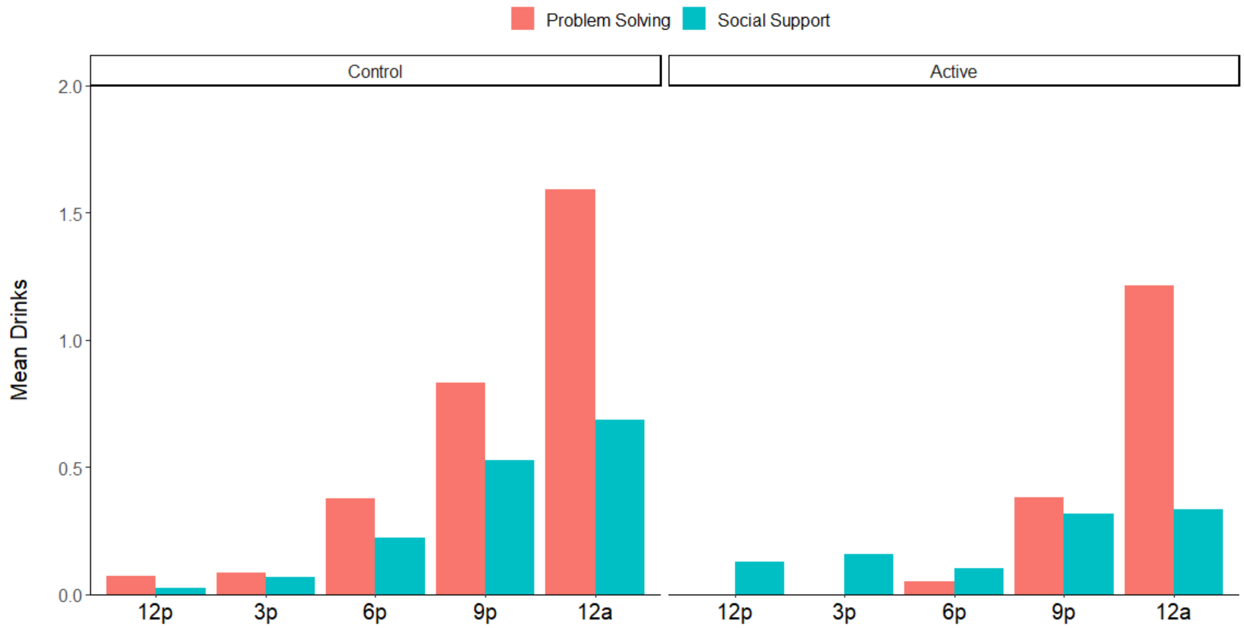

Figure 10. Alcoholic drinks over time (descriptive means).

Note. Descriptive mean alcoholic drinks consumed by experimental condition and time of day. Only the unimputed raw data was used to generate this figure. Participants in the social support active day drank less alcohol over time than both their own control day and the problem solving active day.

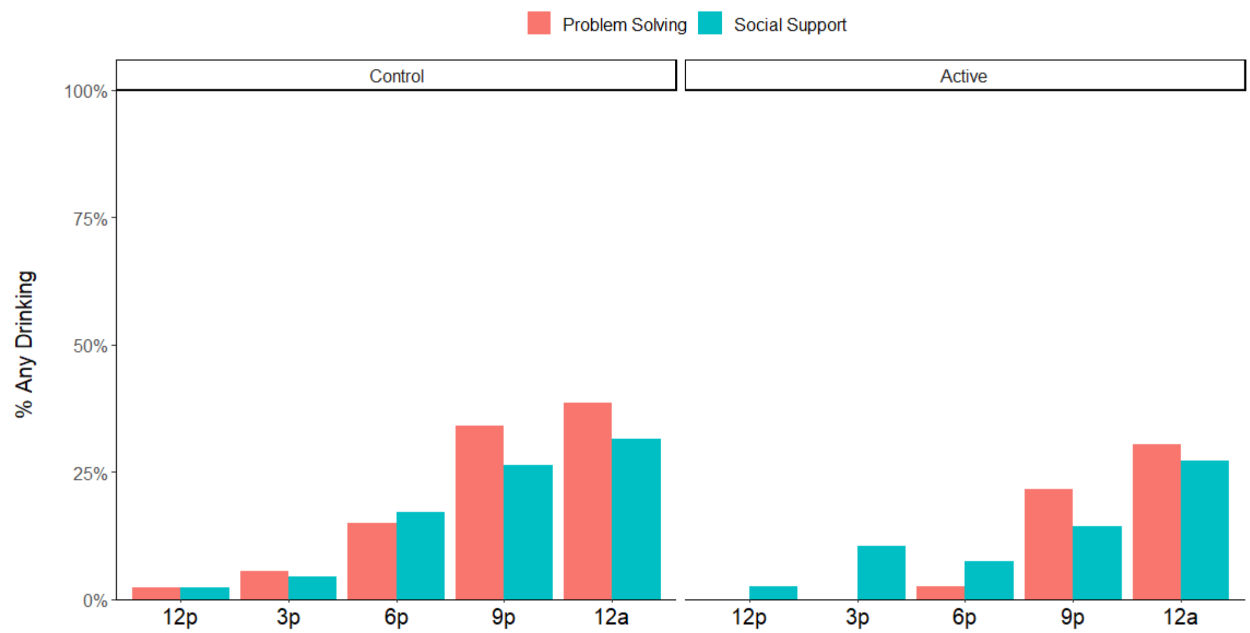

Figure 11. Alcoholic drinks over time (proportions).

Note. Descriptive proportions (%) of participants who reported any drinking by experimental condition and time of day. Only the unimputed raw data was used to generate this figure. Fewer participants in the social support active day drank alcohol over time than both their own control day and the problem solving active day.

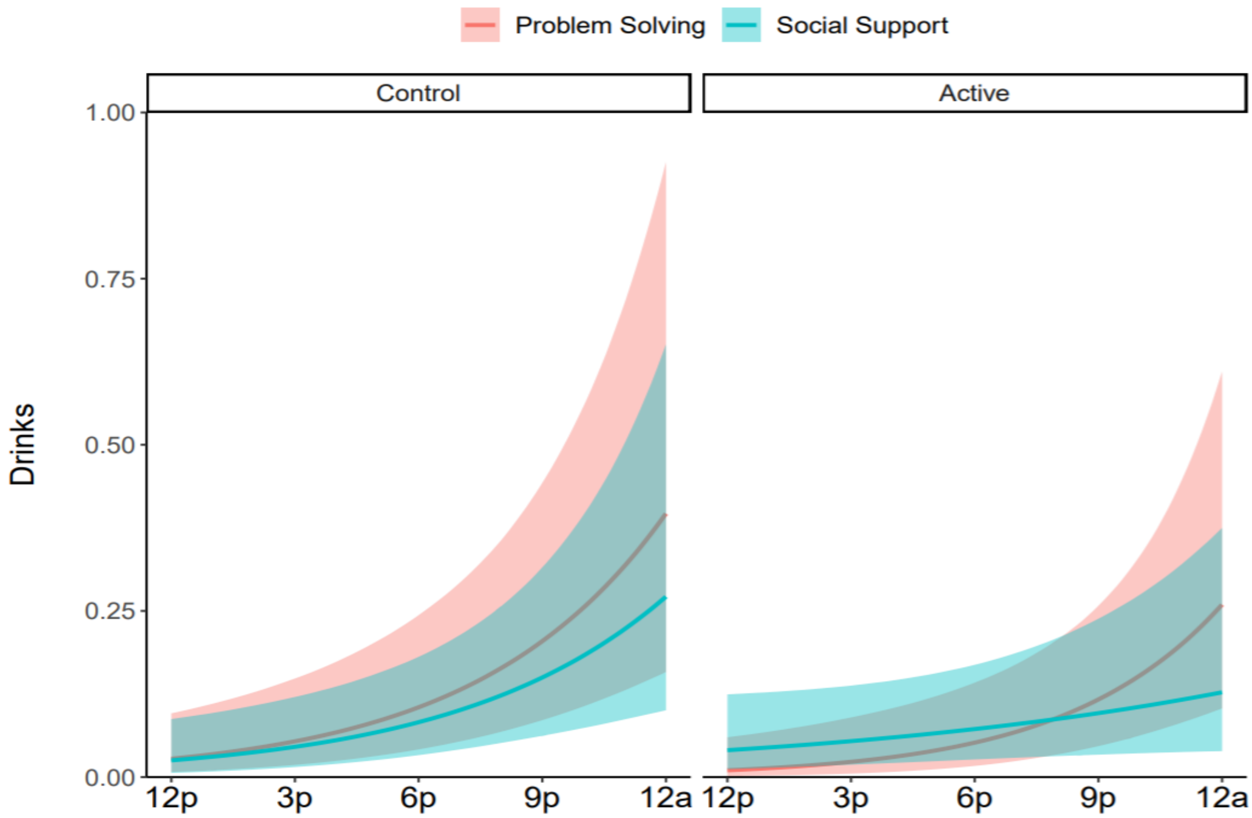

Figure 12. Alcoholic drinks over time multilevel model results.

Note. Results of the multilevel model comparing experimental conditions by time of day on alcoholic drinks consumed. The regression lines and 95% credibility intervals are displayed. The rate of dinking over time was reduced for the social support participants on their active day compared to their control day (compare blue lines across both graphs; see *Table 3*). The rate of dinking over time was also reduced for the social support participants on their active day compared to the problem solving participants on their active day (compare red and blue lines in the right side graph).

Table 3. Simple slopes comparisons of alcoholic drinks over time.

Comparison Groups			BF_{10}	*b* [95% CI]
SS active	<	SS control	8*	-0.31 [-0.72, 0.12]
SS active	<	P active	21**	-0.52 [-1.02, -0.01]
SS active	<	P control	14**	-0.38 [-0.78, 0.04]
SS control	<	P control	2	-0.07 [-0.47, 0.36]
SS control	<	P active	3	-0.21 [-0.72, 0.29]
P active	<	P control	0.47	0.14 [-0.34, 0.63]

Note. Results of the multilevel model comparing experimental conditions on alcoholic drinks consumed by time of day. Slopes of alcohol consumption over time are compared by experimental condition. Bayes factors (BF_{10}) and beta coefficients (*b*) with 95% credibility intervals (95% CI) are displayed.

* substantial evidence for the hypothesis

** strong evidence for the hypothesis

SS: social support condition

P: problem solving condition

Active: active day of the EMA protocol

Control: control day of the EMA protocol

Figure 13. Alcohol expectancy (descriptive means).

Note. Descriptive mean alcohol expectancy s-valence by experimental condition. Only the unimputed raw data was used to generate this figure. Participants in the social bonding active day had less positively valenced alcohol expectancies than both their within-person control day and the between-person problem solving active day

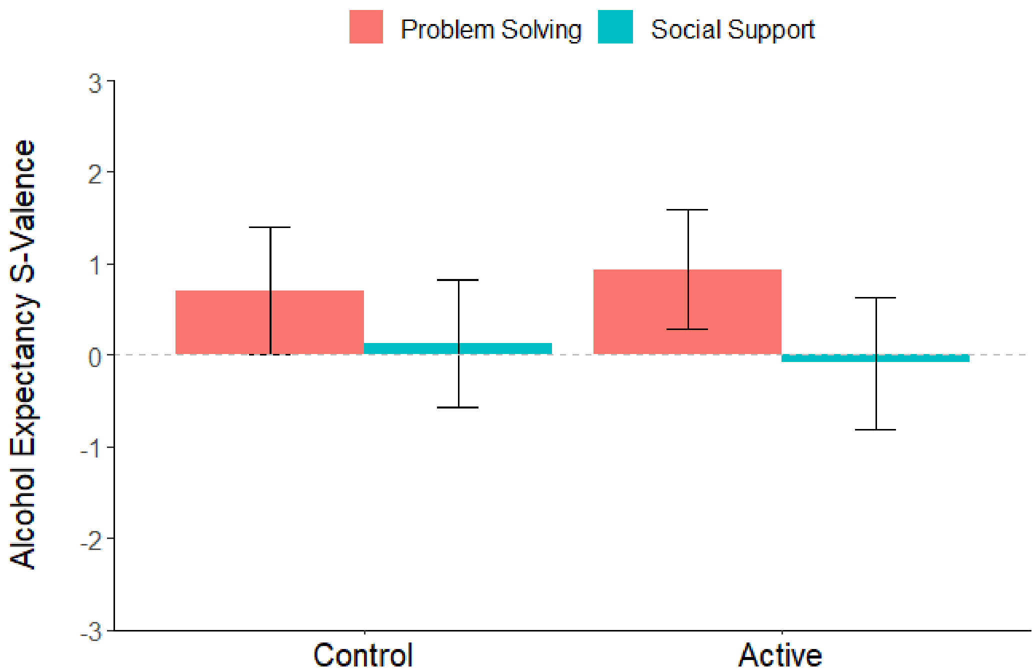

Figure 14. Alcohol expectancy multilevel model results.

Note. Results of the multilevel model comparing experimental conditions on alcohol expectancy valence s-valence. The expected means and 95% credibility intervals are displayed. The social support participants had less positively valenced alcohol expectancies than the problem solving participants on the active day (compare red and blue bars on the right side graph; see *Table 4*). The social support participants had less positively valenced alcohol expectancies on their active day than on their control day (compare the blue bars across both graphs).

Table 4. Pairwise comparisons of alcohol expectancy.

Comparison Groups			*BF*$_{10}$	*b* [95% CI]
SS active	<	SS control	4*	-0.22 [-0.64, 0.21]
SS active	<	P active	132†	-1.02 [-1.70, -0.33]
SS active	<	P control	36***	-0.79 [-1.47, -0.12]
SS control	<	P control	11**	-0.58 [-1.26, 0.11]
SS control	<	P active	46***	-0.80 [-1.45, -0.15]
P active	<	P control	0.27	0.22 [-0.24, 0.71]

Note. Results of the multilevel model comparing experimental conditions on alcohol expectancy s-valence. Bayes factors (*BF*$_{10}$) and beta coefficients (*b*) with 95% credibility intervals (95% CI) are displayed.

* substantial evidence for the hypothesis

** strong evidence for the hypothesis

*** very strong evidence for the hypothesis

† decisive evidence for the hypothesis

SS: social support condition

P: problem solving condition

Active: active day of the EMA protocol

Control: control day of the EMA protocol

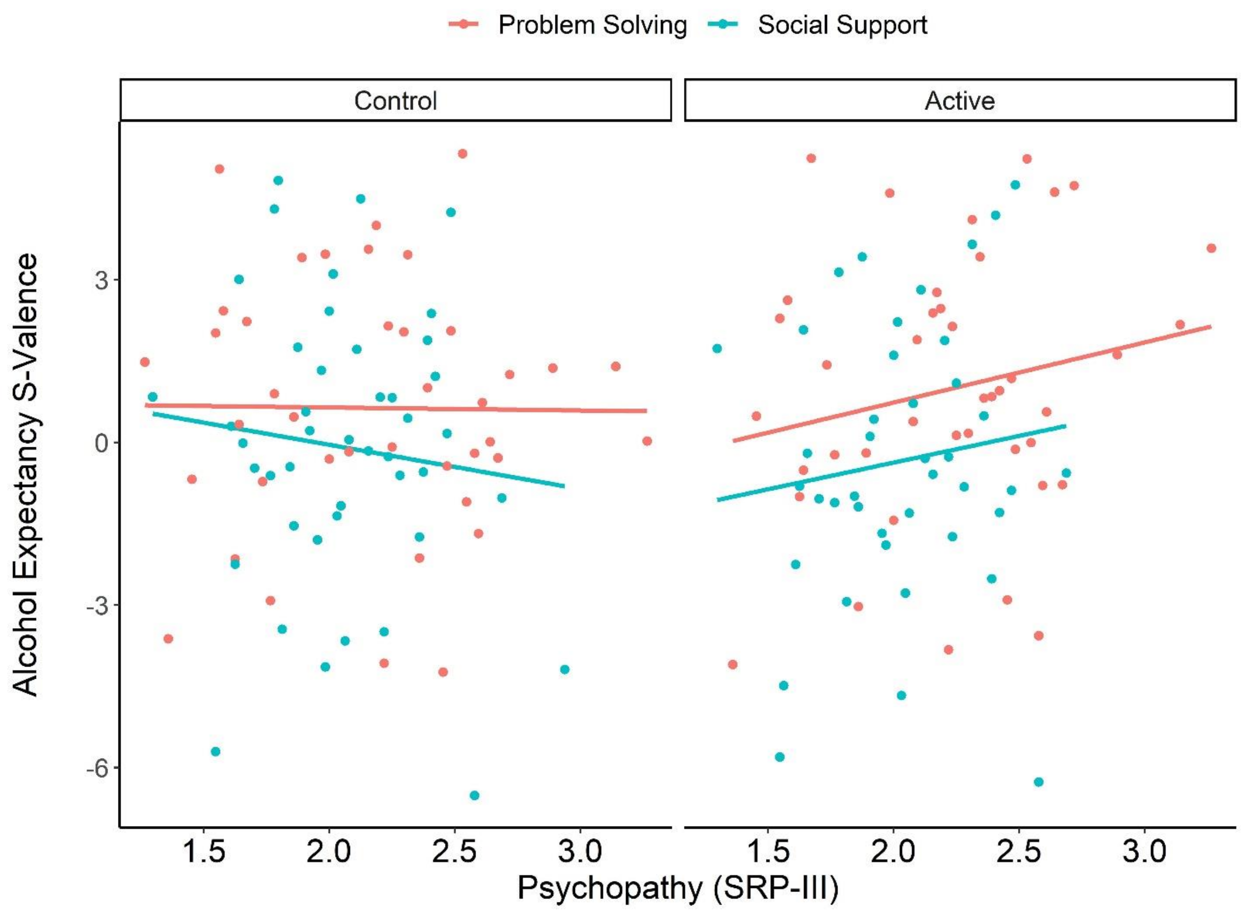

Figure 15. Alcohol expectancy by psychopathy traits (scatterplot).

Note. Scatterplot with linear regression lines of alcohol expectancy s-valence by experimental condition and psychopathy traits. Only the unimputed raw data was used to generate this figure. Similar positive slopes between psychopathy and alcohol expectancy valence on the active day for participants in both the problem solving group and the social bonding group (compare red and blue lines on the right side graph). For the social bonding group, we observed a negative slope between psychopathy and alcohol expectancy valence on the control day, but a positive slope on the active day, which is unlike the pattern we anticipated (compare blue lines across both graphs). However, the 3-way interaction was not supported in the analyses.

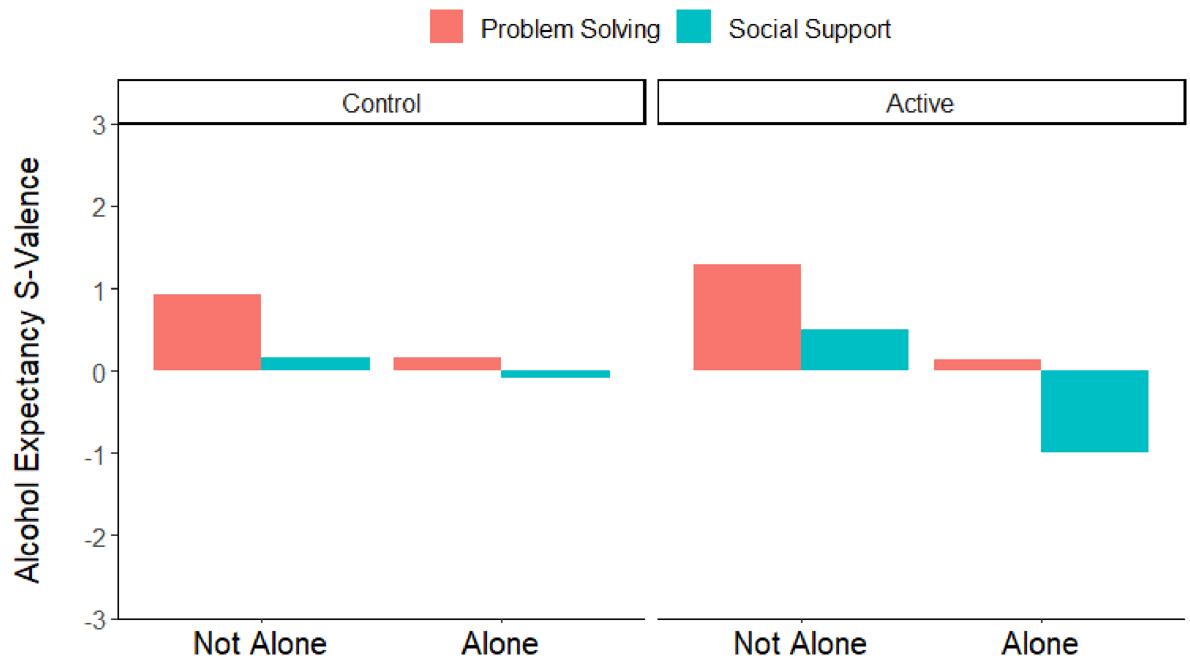

Figure 16. Alcohol expectancy by social context (descriptive means).

Note. Descriptive mean alcohol expectancy s-valence by experimental condition and social context. Only the unimputed raw data was used to generate this figure. On the active day, the reduction in positively valenced alcohol expectancies between being with others and being alone was slightly greater for the social support group than the control group (compare bar cluster in the right side graph). When alone, the reduction in positively valenced alcohol expectancies between the control day and the active day was greater in the social support group than the reduction for the problem solving group (compare the bar clusters labeled "Alone" across both graphs). However, the 3-way interaction was not supported in the analyses.

CHAPTER FIVE:

DISCUSSION

In spite of the sample size limitations and dramatic changes to social life caused by the COVID-19 pandemic, our tentative findings provided mixed support for the idea that increasing social bonding can reduce motivation for alcohol consumption. Although our social bonding manipulation did not reduce total drinking quantity when aggregated across an entire day as we hypothesized, post-hoc analyses found that the social bonding manipulation did reduce the acceleration of drinking throughout the evening. This effect was found when compared to both the between-person and within-person control conditions. Reducing the rate of alcohol consumption has significant clinical relevance, because greater acceleration of alcohol consumption in the evening has been associated with high-risk ("binge") drinking (Groefsema & Kuntsche, 2019). The social bonding manipulation also reduced the positive valence of alcohol expectancies across an entire day, and this effect was found when compared to both the between-person and within-person control conditions. The reduction in the rate of drinking and the reduction of positively valenced alcohol expectancies was largely consistent with the hypothesis that increasing social bonding can reduce motivation for alcohol consumption.

Evidence to support the validity of our social bonding manipulation was mixed, however. Our social bonding manipulation increased perceived social bonding relative to the between-person control group, but not relative to the within-person control condition. One explanation for the lack of within-person change was that we intentionally avoided notifying participants which days or hours they would engage in the social support task, so as to minimize any potential reactivity caused by the study design. Consequently, participants may have felt more socially connected on both study days by simply expecting to engage in the social support task at any time during both days, regardless of whether they actually engaged in the task or not. Consistent with this reasoning, we found a main effect difference in social bonding between the social bonding group and the problem solving group. Another explanation for this main effect difference, however, was that the social bonding differences between groups were an artifact of pre-existing trait differences. Baseline comparisons showed that participants in the social support group had less psychopathy traits than participants in the problem solving group. These results made it difficult to determine if the increased social bonding in the social support group was simply an artifact of pre-existing trait differences between the samples, although our analysis did covary for important demographic and trait differences, including psychopathy. Therefore, the degree to which the observed reductions in drinking and positive alcohol expectancy valence can be attributed to experimentally induced changes in perceived social bonding remained unclear.

Other important challenges to our interpretation include the notable within-person reduction in aggregated alcohol consumption for the control group (i.e. the problem solving condition), even though the control group did not experience a within-person increase in social bonding. Simply providing a required alternative activity may provide enough incentive to deter drinking behavior, regardless of perceived social bonding at that moment. Enhancing social bonding may not be a necessary condition to reduce alcohol motivation, and other important processes were likely influencing alcohol motivation, as we would reasonably expect. However, since our control manipulation was presumed a priori to not affect alcohol motivation, the mechanism of change for the general problem solving task we used as a control manipulation was unclear. Another challenging question was why the control group’s within-person reduction in drinking was not reflected by a within-person reduction in positively valenced alcohol expectancies, unlike the social bonding group which did show within-person reductions in both drinking and positively valenced alcohol expectancies. It may be that another mechanism besides expectancies produced the change in alcohol consumption for the control group. Finally, it was unclear why the social bonding manipulation reduced only the hourly acceleration of alcohol consumption, but not aggregated daily alcohol consumption, as we originally hypothesized. These challenges to our interpretations paint a mixed picture regarding how well the results supported our thesis that changing social bonding can change alcohol motivation. A more adequately powered study during “normal” sociocultural conditions (i.e. not during a global pandemic that dramatically alters basic conditions of human socialization) may clarify our mixed findings.

Nevertheless, it was important to note the magnitude of the effects we found on the rate of drinking. Although slightly fewer than half of the participants reported drinking alcohol during our study, the 27% (within-person) to 41% (between-person) reduction in the rate of drinks consumed during the social support manipulation was not trivial. Slowing the rate of alcohol consumption would reduce peak blood alcohol concentration and help prevent negative consequences associated with acute alcohol intoxication, since increasing the time between drinks allows for more alcohol to be metabolized and eliminated from the body. Furthermore, the 77% main effect increase in overall alcohol consumption on the control day relative to the experimentally active day (i.e. within-person changes) suggested that motivation for alcohol consumption might be substantially reduced using active mobile technology interventions in real-world environments during times when the risk for heavy alcohol consumption is greatest.

Although our findings provided mixed support for the hypothesis that social bonding can change motivation for alcohol use, other important implications might still be noted. We found that perceptions of social bonding were dynamic and fluctuate during the hours and days when alcohol consumption was most likely, which hinted at the potential for social bonding processes to guide alcohol motivation. Our results also highlighted the importance of engaging these social processes when alcohol motivation is accelerating, as our social bonding manipulation was effective at reducing the rate of alcohol consumption during the evening. The satiation of social rewards prior to and during alcohol consumption may disrupt momentary surges in alcohol motivation that generate rapid alcohol consumption. At the neurobiological level of analysis, we speculate there may have been analogous activation of opioid receptors that downregulate dopamine in the nucleus accumbens, which may have decreased reward motivation by inducing perceptions of satiation. Further support for the satiation of alcohol reward by our social bonding manipulation was found in the reduction of positively valenced alcohol expectancies (both between-person and within-person). Although the within-person reduction in positively valenced alcohol expectancies appeared to be relatively small, the between-person reduction was relatively large in magnitude. Overall, these results were modestly consistent with the idea that enhancing social bonding can satiate alcohol motivation.

Our study also provided further support for the importance of considering social bonding as integral to human motivation. Although alcohol consumption is only one of a wide array of rewarding behaviors humans can engage in, the necessity of social connection for human survival presumes that potent desires to obtain and maintain rewarding relationships would be present in essentially all decision making (Baumeister & Leary, 1995; Depue & Morrone-Strupinsky, 2005; Over, 2016). Therefore, the psychological and neurobiological principles that characterize motivation may always be understood as having some functional explanation in how the behavior being studied facilitates and/or maintains social bonding. Our study, along with many others, suggested that alcohol use is one particular behavior among many that can be strongly influenced by desires for social bonding. This line of reasoning is consistent with the "common factors" psychotherapy research which indicates that the therapeutic alliance (i.e. the social bond between the therapist and the client) is strongly predictive of treatment outcomes and may be the most important mechanism in any effective psychotherapy (Wampold, 2015). Creating a sense of belonging in the client through the therapeutic alliance likely produces "social buffering" effects and their associated neurobiological correlates that downregulate distress. The value of social bonding in clinical therapeutics is also highlighted by the importance of social rituals in the potency of placebo effects that generate the anticipation of reward and relief from pain (Benedetti et al., 2011; Shaibani, Frisaldi, & Benedetti, 2017). Our study contributes to these vast literatures on the importance of social bonding in human motivation and treatment by suggesting that targeting social bonding processes when motivation for maladaptive behaviors is developing in real-time may alter decision making in the near future and generate more adaptive behaviors instead.

Limitations and Future Directions

Careful consideration of our findings in the context of some notable limitations may offer insights for future directions in this line of research. Our sample included relatively few males, raising questions about the generalizability of our findings to males. Although the undergraduate pool from which participants were drawn is often predominantly female (~ 75%; e.g. Benitez & Goldman, 2019), our final sample was nearly 90% female. This sampling imbalance for sex suggests that males may have systematically avoided participation in a study that required them to "interact with your group members by watching videos (60 seconds or less) and sharing messages with them… request or provide social support by watching videos and sharing messages throughout the day" (exact language from the informed consent document). It seems plausible that requesting and providing nondescript social support which required relational communication was perceived as gendered behavior that was less appealing to male college students (Eagly, 2009). Future social bonding studies directed at male college populations may benefit from intentionally using gendered behaviors that are perceived as more culturally appropriate for male college students, such as playing cooperative violent video games (Terlecki et al., 2011). Similarly, we found that participants who had greater psychopathy traits were less likely to participate in the social bonding experimental condition, which raises questions about how well our results would generalize to college students with greater psychopathy traits. Future studies could build on our work by exploring these phenomena in other populations, especially since American college students tend to be extreme on a number of important dimensions relative to most other human populations (Henrich et al., 2010).

Another generalizability concern for our study was that nearly all the data was collected during the COVID-19 pandemic, which may have altered how alcohol motivation and social bonding phenomena would normally occur outside of a pandemic context. Alcohol consumption during the COVID-19 pandemic appears to have increased nationally across the United States (Barbosa, Cowell, & Dowd, 2020; Koob, 2020), suggesting that atypical drinking patterns may have been observed in our sample, although *Figures 10* and *11* still showed the escalation of drinking throughout the evening that was characteristic of college student binge drinking. Similarly, social distancing mandates would have necessarily changed how students in our sample socialized (e.g. virtually vs. in-person; less temporal contingency on in-person class schedules or work schedules), which may have influenced social bonding processes in an atypical fashion. Outside of a direct replication of our study when the pandemic "ends", it was unclear how our results would generalize to a non-pandemic context.

Methodological limitations included obtaining a smaller sample size than we planned for a priori. Consequently, we observed a large amount of uncertainty around the effect size estimates, which limited our interpretation for the importance of social bonding in alcohol motivation. A larger effect size would suggest greater importance of social bonding as a mechanism for alcohol motivation. For instance, the 95% credibility interval around the difference in the rates of drinking over time between the social support group and the problem solving condition on the active day suggested that reductions in drinking could plausibly range from 1% to 64%. A 64% reduction in the rate of drinking would certainly indicate greater importance for social bonding as a motivational mechanism than a 1% reduction. Future studies with larger sample sizes could clarify the motivational importance of social bonding by increasing the precision of effect size estimates for social bonding manipulations on drinking. Another methodological limitation was that we purposefully chose to not tell participants which days or times they would be engaging in the active tasks (i.e. social support giving or problem solving). Our rationale was to alter participants' perceptions of social bonding while they lived their typical daily lives without making significant adjustments to accommodate the study on the active day or systematically avoid responding at timepoints that require active engagement, which could have confounded our conclusions. Nevertheless, our method may have unintentionally led participants to expect engaging in the social support task on both days of the study, which may have minimized within-person changes between the control day and the active day for the social bonding group. Future studies may clarify the effect of social bonding manipulations on within-person changes by clearly specifying when participants should prepare for social bonding encounters in the near future, although reactivity to the study design may raise concerns about confounding influences.

Conclusion

Since humans are designed to survive in social groups, desires to affiliate with other humans appear fundamental to human motivation. Alcohol and other addictive psychoactive substances tap into the same psychological and neurobiological pathways that underlie experiences of social bonding, suggesting a strong link between motivation for alcohol consumption and motivation for social connection. In a sample of predominantly female college students, we found mixed support for the idea that the desire for social bonding is part of the motivational pathway for alcohol consumption. Using a relatively novel manipulation designed to enhance a person's sense of social connection while they live their normal everyday lives, we found that participants drank alcohol less quickly and expected alcohol to be less rewarding during the weekend, but we did not find evidence that they drank less alcohol overall. Further research is needed to explore how to manipulate social bonding in-vivo (i.e. outside the laboratory) when the motivational dynamics of alcohol consumption and social bonding operate naturally. From a broader perspective, although the public health goal of clinical research is to find mechanisms to reduce human suffering, and social bonding seems like a palatable panacea, it is important to also explore potential negative consequences that social bonding may have to increase human suffering (e.g. exacerbating ingroup vs. outgroup conflict) and avoid simplistic interpretations that may limit scientific discovery (Sapolsky, 2018). Nevertheless, the innate propensity for humans to affiliate and the potency of social processes to motivate human behavior suggest that further research on social bonding and motivation for alcohol consumption would be fruitful.

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APPENDIX A:

IRB APPROVAL LETTER

UNIVERSITY OF SOUTH FLORIDA
USF RESEARCH & INNOVATION

RESEARCH INTEGRITY & COMPLIANCE
Institutional Review Boards, FWA No. 00001669
12901 Bruce B. Downs Blvd, MDC35, Tampa, FL 33612-4799
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9/26/2019

Bryan Benitez

Psychology

3626 Jefferson Commons Drive

102B

Tampa, FL 33613

RE: **Full Board Approval for Initial Review**

IRB#: Pro00041243

Title: Social Support and Health Behaviors

Study Approval Period: 9/20/2019 to 9/20/2020

Dear Mr. Benitez:

On 9/20/2019, the Institutional Review Board (IRB) reviewed and **APPROVED** the above application and all documents contained within, including those outlined below. **Please note that this research is approved under the 2018 version of 45 CFR 46. The IRB determined that future reviews of this study qualify under expedited category 9 (Continuing review of research, not conducted under an investigational new drug application or investigational device exemption where categories two (2) through eight (8) do not apply but the IRB has determined and documented at a convened meeting that the research involves no greater than minimal risk and no additional risks have been identified) and you will be asked to confirm ongoing research annually in place of a full Continuing Review.**

Approved Item(s):

Protocol Document(s):

Benitez IRB protocol version 3 (8-21-19).docx

Consent/Assent Document(s)*:

Dissertation Online Consent (0.03).docx **

*Please use only the official IRB stamped informed consent/assent document(s) found under the "Attachments" tab. Please note, these consent/assent documents are valid until the consent document is amended and approved. ** Please note, verbal and online consent documents will not have the official IRB stamp.

Your study qualifies for a waiver of the requirements for the documentation of informed consent as outlined in the federal regulations at 45 CFR 46.117(c), which states that an IRB may waive the requirement for the investigator to obtain a signed consent form for some or all subjects if it finds any of the following: (1) That the only record linking the subject and the research would be the consent document and the principal risk would be potential harm resulting from a breach of confidentiality. Each subject (or legally authorized representative) will be asked whether the subject wants documentation linking the subject with the research, and the subject's wishes will govern; (2) That the research presents no more than minimal risk of harm to subjects and involves no procedures for which written consent is normally required outside of the research context; or (3) If the subjects or legally authorized representatives are members of a distinct cultural group or community in which signing forms is not the norm, provided that the research presents no more than minimal risk of harm to subjects and provided there is an appropriate alternative mechanism for documenting that informed consent was obtained.

As the principal investigator of this study, it is your responsibility to conduct this study in accordance with IRB policies and procedures and as approved by the IRB. Any changes to the approved research must be submitted to the IRB via an Amendment for review and approval. Additionally, all unanticipated problems must be reported to the USF IRB within five (5) business days.

We appreciate your dedication to the ethical conduct of human subjects research at the University of South Florida and your continued commitment to human research protections. If you have any questions regarding this matter, please call 813-974-5638.

Sincerely,

Kristen Salomon, Ph.D., Chairperson

USF Institutional Review Board