TESTING THE EFFECTS OF AN AFFECTIONATE COMMUNICATION INTERVENTION TO BOLSTER MENTAL HEALTH DURING THE COVID-19 PANDEMIC

by

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DEDICATION

To my mom and dad.

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ABSTRACT

This study tested the efficacy of an affectionate communication intervention to help adults living in the United States bolster their mental health during the height of the COVID-19 pandemic. Ninety-eight married and cohabitating adults were randomly assigned to one of three groups: increased affectionate communication (treatment), increased thankfulness (comparison), or no change in behavior (control). The final sample contained 73 adults ($n_{treatment}$ = 26, $n_{comparison}$ = 24, $n_{control}$ = 23) who completed the four-week intervention that started in September and concluded in October 2020. Although post-hoc analyses revealed that participants in the treatment group were, on average, less affection deprived, less depressed, less lonely, and less stressed than those in the comparison and the control groups halfway through the intervention and at the end of the intervention, these findings should be interpreted with caution due to a successful comparison manipulation, but a statistically nonsignificant treatment manipulation. Speculation as to why the intervention failed to reject the null hypotheses is presented in the discussion before providing methodological recommendations for future interventions in this area of research.

Keywords: affectionate communication, affection exchange theory, mental health, health communication

CHAPTER 1

INTRODUCTION & LITERATURE REVIEW

Alongside securing basic physiological needs, such as food, water, and shelter, forming and maintaining social bonds is essential for the survival of our species, *Homo sapiens* (Maslow, 1943). A fundamental *need to belong* that is engrained in our DNA (Baumeister & Leary, 1995) differentiates us from solitary animals (e.g., bobcats, *Lynx rufus*; Allen et al., 2015), and interpersonal communication plays a pivotal role in meeting our belongingness needs across the lifespan (Harwood, 2007). Above and beyond communicating out of necessity to stay alive, prosocial behaviors and positive communication that enable humans to establish close relationships also contribute to a sense of fulfillment and flourishing (Pitts & Socha, 2013).

Communicating affection directly contributes to satisfying our belongingness and survival needs (Floyd, 2006). Conceptualized as "an individual's intentional and overt enactment or expression of feeling of closeness, care, and fondness for another," affection is *sine qua non* for the development and maintenance of close relationships (Floyd & Morman, 1998, p. 145). Be it a romantic partner saying "I love you", a mother holding her newborn, or friends praising each other for their accomplishments, meeting our affection needs is consequential for our physical, mental, and relational health (Floyd, 2019; Grewen et al., 2003; Johnson, 2013; Mansson, 2013).

This is not to suggest that all humans desire the *same amount* of communication to meet social belongingness and affection needs; one's optimal range of affection, for example, can drastically differ from person to person (Floyd, 2006). Similarly, people do not always wish to be in constant communication with others, but the *voluntary decision* to momentarily withdraw from our social groups for personal solitude and *unwillingly* being forced to restrict our interpersonal interactions (e.g., solitary confinement) are fundamentally different (Shalev, 2014).

On January 20, 2020, the first laboratory-confirmed case of the SARS-CoV-2 virus responsible for the 2019 coronavirus disease (COVID-19) was detected in the United States, and two months later the World Health Organization (WHO) declared COVID-19 a global pandemic (Centers for Disease Control and Prevention [CDC], 2021; Centers for Disease Control and Prevention, 2022a). The social interactions and interpersonal communication many took for granted their entire lives were taken from them by the summer of 2020. As international flights were grounded and hospital intensive care units reached maximum capacity, empty public spaces spoke volumes of the global health crisis that had taken over the world. The health risks of face-to-face (FtF) communication were reported and discussed *ad infinitum* with seemingly hourly breaking news updates and stunning developments from the CDC on the COVID-19 death toll and positive case count of the disease. COVID-19 hit countries around the word like a maelstrom, and at the time of this study's write-up, COVID-19 is officially the deadliest pandemic in the United States' history, responsible for over 976,000 domestic deaths and 6,170,000 global deaths and counting (World Health Organization, 2022).

A concerted effort to limit FtF communication and increase physical distance between people to stop the spread of COVID-19 was made throughout most of the United States via non-pharmaceutical public health interventions (NPIs; Liu et al., 2021), such as strict rules that prohibited social gatherings in public spaces (e.g., playgrounds, parks), temporary non-essential business closures, and educational institutions transitioning to online learning indefinitely. What was initially believed to be a variant as dangerous as the common flu quickly escalated, leading to widespread panic buying (Arafat et al., 2020), hoarding (Baddeley, 2020), and global supply chain shortages (Guan et al., 2020).

Most important, perhaps, COVID-19 demurred our belongingness and affection needs like never before. News outlets began reporting on the scientific reasons and behind our need for physical touch (Blue, 2020; Campbell, 2020; Krempa, 2020; Spechler, 2020) and the extent to which people were experiencing "skin hunger" and affection deprivation (Coffey, 2020a; Miller, 2020; *The Washington Post*, 2020). Such reports were often supplemented with photos and videos highlighting the creativity of people from around the world setting up plastic "hugging walls" and "cuddle curtains" desperately trying to meet their direct nonverbal affection needs (Coffey, 2020b; Ianzito, 2020; Koeller, 2020). Transparent plastic shower curtains were not nearly as efficacious as medical-grade personal protective equipment (PPE) against such a deadly respiratory virus, but that meant very little to those who were willing to put their health and the health of their affectionate communication partners on the line to satisfy an innate desire for affection that they had reluctantly relinquished (Mercedes, 2020). Collective social distancing efforts to stop the spread were necessary but nevertheless deleterious for mental health, friendships, and romantic relationships in 2020 (Czeisler et al., 2020; Fallik, 2020; Kekatos, 2020; Parch, 2020; World Health Organization, 2020).

Behavioral interventions manipulating specific forms of affectionate communication have been implemented to improve health and well-being outcomes for decades (e.g., Clipman, 1999; Holt-Lunstad et al., 2011; van Raalte et al., 2021). At a point in time when people were longing for affection, conducting an affectionate communication intervention that met social distancing protocols was warranted. Thus, the primary goal of this study was to implement an affectionate communication intervention for adults living in the United States and examine how increasing the frequency of affectionate exchanges *in toto* could bolster their mental health during the height of the COVID-19 pandemic. The remainder of this chapter discusses a contemporary

theory of affection before reviewing the research on affectionate communication and health. Hypotheses and research questions are posited at the end to conclude the chapter.

Affection Exchange Theory

Affection exchange theory (AET; Floyd, 2006; Floyd, 2019) provides a comprehensive explanation as to why affectionate behavior is adaptive for humans. A neo-Darwinian theory, AET has three core axioms. The first axiom posits that the two superordinate goals for humans are reproduction and survival, and the second axiom asserts that affectionate behaviors do not need to benefit either superordinate goal in a proximal sense for affectionate behaviors to aid and favor reproduction and survival in an ultimate sense. Consider the formation of friendships, for example. Although such relationships often have a diminutive impact on directly providing fundamental needs for survival, the personal benefits we reap from our close relationships (e.g., social support), are positively associated with health-supportive behaviors, such as maintaining a healthy diet and lifestyle (Mladenovic et al., 2014; Yoshikawa et al., 2021), and inversely associated with threats to viability and fertility, such as self-harm behaviors (Aggarwal et al., 2017; van der Wal & George, 2018; You et al., 2013). The third axiom states that "individuals need not be consciously aware of the evolutionary goals being served by their behaviors" (Floyd, 2006, p. 161). People can engage in affectionate communicative exchanges without a deliberate intention to increase their odds of survival and/or reproduction. This is to say that people may verbally communicate "I love you" or give a hug to a romantic partner, a family member, or a friend simply because they want to. Conscious awareness of the adaptive utility of affection is not a necessary condition to reap the benefits of affectionate communication.

Affection exchange theory consists of five postulates and eight subpostulates (Floyd, 2006). Pertinent to the aims of this study are the latter three postulates. In conjunction with the

first axiom that stipulates survival and reproduction are two superordinate goals, the third postulate asserts that "affectionate communication is adaptive with respect to human viability and fertility" for individual senders and receivers (Floyd, 2006, p. 164). Subpostulate 3a posits that affectionate communication serves the superordinate goal of survival by encouraging the formation and preservation of meaningful human relationships, and subpostulate 3d asserts that affectionate communication covaries with stress buffering and immune system pathways. This is not to suggest that all humans need or seek the same quantity of affection, but AET does put forward the proposition that *some* amount of affection is required to survive. Recognizing individual differences in terms of baseline needs and maximum desired amounts of affection, AET's fourth postulate asserts that "humans vary in their optimal tolerances for affection and affectionate behavior" (Floyd, 2006, p. 171). Whereas subpostulate 4a contends that affectionate communication within one's range of optimal tolerance betters the body's regulatory and immune systems, the fifth postulate posits that affectionate communication outside of one's optimal range has disadvantageous health and well-being implications. Specifically, subpostulate 5a asserts that "a violation of the minimum threshold in the range of optimal tolerance constitutes a threat to viability" (Floyd, 2006, p. 180). Considered together, these postulates offer explanations as to how much and why affectionate communication is indispensable.

Differentiating Affectionate Behaviors from Affectionate Feelings

It is worth discussing AET's higher-order second postulate (Floyd, 2006) before turning to the succeeding section that reviews the implications of affectionate communication on our health. Although the emotional experience of feeling affection for someone or toward something often covaries with how people express their affection, the behavioral manifestation of *communicating* affection and the experience of *feeling* affection are discrete constructs. Phrased

differently, people may elect to mask, inhibit, intensify, de-intensify, or even simulate affectionate behavior in ways that are not consonant with the emotional feeling of affection they are experiencing. This could be a child communicating disinterest to downplay his affectionate feelings when his crush starts talking to him on the playground. It could be an adult who decides to deceptively amplify affectionate communication with someone to acquire material resources or satisfy other interpersonal needs when affectionate feelings for the other person do not exist or are not strongly felt. People choose to communicate in ways that sometimes differ from their feelings for various cultural, personal, and situational reasons (Canary et al., 2008), and communicating affection is no different. All of this is to say that *communicating* affection is behavioral, and the present study focusses on affectionate communication for a behavioral intervention. This is not to trivialize the role of affectionate feelings and the body's multidimensional response to such feelings (Floyd, 2020), nor to suggest that affectionate feelings are unimportant. The author does, however, want to make it clear that the present study's focus is on affection expressed as communicative *behaviors* instead of emotional experiences.

Affectionate Communication and Health

A corpus of empirical interdisciplinary research has identified the extent to which affectionate communication is associated with indices of mental health and subjective well-being (e.g., Clipman, 1999; Floyd, 2002; Schrodt et al., 2007). Of note is the stress buffering effect of affectionate communication and its overlap with the stress-buffering hypothesis (Cohen, 2004; Cohen & Wills, 1985). The association between affectionate communication and stress has received much attention from behavioral scientists in this area of study and have been largely supportive of AET's subpostulate 3d (Aloia & Brecht, 2017; Floyd et al., 2007; Schrodt et al.,

2007). When faced with an environmental stressor, increased affectionate communication is associated with less self-reported perceived stress (Burleson et al., 2007) and improved physiological indices of stress, such as lowered blood pressure (Grewen et al., 2003), reduced heart rate (Ditzen et al., 2007), and even the ability to recover from an upper respiratory infection (Cohen et al., 2015).

The extent to which people communicate affection to others and receive affection from others at a trait level has shown to be a viable predictor of laboratory-induced stress responses. Early cross-sectional work by Floyd (2002) found highly affectionate communicators to be less stressed, less depressed, more likely to engage in regular social activity, and happier than their low trait affection peers. Later experimental work by Floyd and colleagues (2010) supported these cross-sectional findings. Prior to being exposed to a battery of laboratory stressors, participants completed a questionnaire that asked them to report their trait affection. As expected, trait, as well as state, affection predicted greater increases in oxytocin from the start of the experiment to the end of the experiment for highly affectionate participants compared to less affectionate participants. Altogether, the affectionate communication and stress literature provides ample support for affectionate communication offering protective, health enhancing benefits against life stressors.

As beneficial as affection exchange can be, the lack of affection is correspondingly harmful. The deleterious outcomes associated from being deprived of affection are so severe that affection deprivation in infancy can result in cognitive impairments that inhibit human development (Chugani et al., 2001; Maselko et al., 2011). Defined as receiving “less affectionate communication than one desires” (Floyd, 2016. p. 381), affection deprivation is positively associated with loneliness (Floyd, 2014b; Floyd & Hesse, 2017), and conversely, both depression

and loneliness are negatively associated with affection (Downs & Javidi, 1990; Field et al., 2001; Floyd et al., 2005, 2009; Holt-Lunstad et al., 2011; Jorm et al., 2003; Mansson, 2013; Woo et al., 2018). Depression and loneliness are viable predictors of suicide ideation (Gijzen et al., 2021; Ribeiro et al., 2018) attempted suicide (Stickley & Koyanagi, 2016; Woodford et al., 2018), and are negatively associated with relationship satisfaction (Hesse & Mikkelson, 2017; Novak et al., 2016). Deemed a public health crisis in industrialized countries such as the United States (Murthy, 2017), Japan (Kodama, 2021), and the United Kingdom (Daley, 2018), loneliness, specifically, has a quantifiable economic impact with lonely individuals spending more on—and suffering more from—healthcare costs than their less lonely counterparts (Mihalopoulos et al., 2019). Loneliness, depression, and affection deprivation all share conceptual space to some extent, but the empirical differences that distinguish them as distinct phenomena (e.g., affection deprivation being more behavioral than emotive compared to loneliness; Floyd & Hesse, 2017) make it important for researchers to study both loneliness and depression in relation to communicating affection. In line with AET, a lack of affection presents unfavorable health and well-being implications.

Manipulating Affectionate Communication in Experimental Research

Studies utilizing true experimental designs provide further empirical support for affectionate communicating being a health-supportive behavior. Grewen et al.'s (2003) warm partner contact experiment had cohabitating couples hug and hold hands prior to a stress-inducing speaking task. Compared to participants in the control group who did not engage in any direct nonverbal affectionate behaviors, the warm contact treatment group had significantly smaller increases in blood pressure and heart rate in response to the stressful task. Pauley and colleagues (2014) also exposed participants to a stressful task and found similar stress-buffering

blood pressure and heart rate reactivity benefits from affectionate communication. Cortisol reactivity, however, was moderated by relationship type: participants who brought friends to partake in the experiment had a higher cortisol levels compared to participants who brought their romantic partner. Cortisol, the most commonly measured hormone to assess stress, is secreted by the adrenal glands after hypothalamic–pituitary–adrenal (HPA) axis activation in response to a stressor (Russell & Lightman, 2019). In response to an acute stressor, such as the set of tasks Pauley et al. (2014) subjected participants to, higher cortisol levels are indicative of more stress. Used as a physiological operationalization of stress, this suggests that the benefits—and potential detriments—of affectionate communication can differ depending on the relationship status of a dyad, which is in support of previous studies that have found relationship status to act as a moderating variable (e.g., Coan et al., 2006). Matsunaga et al.'s (2009) warm touch experiment that had married couples in the warm contact treatment group engage in affectionate behaviors (i.e., hugging, kissing) in private self-reported increased happiness and less irritability than the non-affectionate control group. β2-microglobulin, a protein indicative of an adaptive and healthy immune system, was also positively associated with happiness and negatively associated with irritability for the treatment group, but not for the control group. Considered together, these findings provide empirical support for AET's claim that affectionate communication provides physiological benefits.

Holt-Lunstad and colleagues' (2008, 2011) interventions for married couples taught participants how to utilize multiple affectionate behaviors to implement affectionate touch therapies. Other interventions have targeted specific behaviors, such as hugging (Clipman, 1999), cuddling (van Raalte et al., 2021), kissing (Floyd et al., 2009), and writing (Floyd et al., 2007). Consistent with AET, participants in these studies had greater decreases in total

cholesterol, reported greater subjective wellness, greater relational satisfaction, and were less stressed relative to comparison and control groups. The consistent results from both self-report data and physiological indices of health present a strong argument for affectionate communication being a health-benefiting behavior.

The Present Study

COVID-19 challenged innate belongingness needs and exacerbated the deleterious effects of social isolation for people around the world (Hwang et al., 2020; Murayama et al., 2021). For many, the abrupt lack of and yearning for affectionate communication became more apparent when media outlets began reporting on the health and well-being consequences of being deprived of affection (Campbell, 2020; Miller, 2020). AET posits that communicating affection confers mental, physical, and relational health benefits when people meet their desired amount of affection. The utility and adaptiveness of affection exchange supports our superordinate goals because it promotes relational development and maintenance, which directly furthers our fundamental need to belong. These claims are supported by a robust body of research manipulating affectionate communication to enhance these outcomes and an equally strong body of research measuring the associations of the aforementioned outcomes with affectionate behavior. On the basis of AET and the empirical literature, implementing an affectionate communication intervention that affords participants an opportunity to increase their affectionate communication with the people they want through the communication channels that they prefer (treatment group) should benefit the mental health of participants more than asking them to engage in a non-affectionate behavior (comparison group) or not changing their behavior at all (control group). Given the negative associations affectionate communication has with affection

deprivation, depression, loneliness, and stress, one would predict an overall decrease across these four outcomes. As such, the first hypothesis is posed:

> H1: Participants in the treatment group self-report a greater decrease in (a) affection deprived, (b) depression, (c) loneliness, and (d) stress than the comparison and control groups.

Although people might express willingness to participate in an affectionate communication intervention, many lack the ability to fully do so during the pandemic, such as adults living by themselves who may not have a safe and reliable source of FtF affection (Koeller, 2020; Miller, 2020). To account for the possibility of obtaining a sample that would be restricted to specific channels of affection exchange, particularly mediated communication, the target population in the present study are married and cohabitating adults. Notwithstanding the fact that marriage and cohabitation do not always yield affectionate communication exchanges, let alone relational satisfaction (e.g., highly dissatisfied marriages rife with contempt and stonewalling), the author believed that, on average, *more* opportunities for communicating affection would be available for married and cohabitating adults to complete the intervention instructions than those who may be living alone during pernicious times when FtF communication with others posed a significant health risk.

A robust body of empirical research suggests affectionate communication is positively associated with relational satisfaction (Floyd, 2002; Horan & Booth-Butterfield, 2010; Hyun & Shin, 2010; Jakubiak & Feeney, 2017; Punyanunt-Carter, 2004), and past studies using various married and cohabitating couples have shown affectionate behavior to confer multiple health benefits (Floyd et al., 2009; Holt-Lunstad et al., 2008, 2011; van Raalte et al., 2021). Furthermore, AET's subpostulate 3a asserts that affectionate communication encourages the

formation and maintenance of meaningful human relationships. More important, AET asserts that affectionate communication fosters marital bonds *because* affection signals relational commitment and investment in prospective partners, which supports our superordinate goals that have selected for such prosocial relational qualities by way of natural selection.

On the basis of AET and the supporting empirical literature, one would predict, then, that asking participants who are married to increase the amount of affection over the course of a longitudinal intervention would contribute to an overall increase in their marital satisfaction in relation to a comparison group and control group that were not instructed to make a conscious effort of being more affectionate. As such, a second research hypothesis is posed:

> H2: Participants in the treatment group self-report a greater increase in marital satisfaction than participants in the comparison and control groups.

The experimental affectionate communication literature has yielded plenty of impressive results for both mental and relational health outcomes, but researchers have failed to assess the longevity of these effects. Hesse et al.'s (2021) meta-analysis of affectionate communication and health support the benefits of affection-based interventions in both clinical (e.g., L'abate, 2008) and non-clinical settings (e.g., van Raalte et al., 2021), but to what extent, if at all, do these benefits persist after such interventions conclude? This is an empirical question that remains to be thoroughly addressed, which is perhaps due in part to a lack of theoretical guidance. As it stands, AET and other theories frequently tested in the affection literature (e.g., tend and befriend theory; Taylor et al., 2000) do not unequivocally posit potential post-intervention benefits from increased affectionate behavior to reasonably derive a theory-based directional hypothesis. Thus, the present study will conduct two post-intervention follow-ups to address this query and further assess the practical utility of affection-based interventions. Posed as research questions:

RQ1: To what extent will the mental health benefits from an affectionate communication intervention persist after (a) two weeks and/or (b) six weeks?

RQ2: To what extent will the marital satisfaction benefits from an affectionate communication intervention persist after (a) two weeks and/or (b) six weeks?

CHAPTER 2

METHOD

Study 1: Initial Feasibility Evidence

Participants. In Fall 2019, undergraduate students enrolled in select communication courses at a large southwestern university (N = 112) participated in Study 1 for extra course credit. Prior to participant recruitment, a repeated measures within-between factors analysis of variance (ANOVA) power analysis was conducted with G*Power 3.1 (Faul et al., 2007). Using the mean effect size estimate in the field of communication of r = .21 (Rains et al., 2018) for three groups over five measurement timepoints with α = .05 and power set to the default 95% parameter, the target final sample size was N = 54 participants (n = 18 per group). IRB approval was obtained before participant recruitment began (see Appendix A).

One hundred and eighty-five students initially consented to participate, which resulted in 62 participants randomly assigned to the treatment group, 63 randomly assigned to the comparison group, and the remaining 60 participants randomly assigned to the control group. There was a disproportionate number of female (n = 115) compared to male (n = 70) participants. Stratified random assignment was used to ensure an equivalent number of females and males per group at T_1. When the intervention concluded, 112 participants ($n_{treatment}$ = 37, $n_{comparison}$ = 40, $n_{control}$ = 35) had completed both the mid- and post-intervention questionnaire at T_2 and T_3, respectfully, and were included in the final data set.

Procedures. Study instructions were emailed to participants en masse on September 16, 2019. Participants in the treatment group were asked to hug more often than they currently did, participants in the comparison condition were asked to verbally greet others more often than they currently did, and participants in the control group were asked to maintain their normal routine

(see Appendix B). All participants were asked to complete an online survey containing measures of the dependent variables at the following five time points: prior to the start of the intervention (T_1; September 16, 2019), halfway (two weeks) through the intervention (T_2; September 30, 2019), at the end (four weeks) of the intervention (T_3; October 21, 2019), six weeks after the intervention (T_4; December 9, 2019), and 12 weeks after the intervention (T_5; January 20, 2020).

Dependent variable measures. *Affection deprivation* was assessed with Floyd's (2016) 8-item affection deprivation scale. Participants were asked to rate the extent to which they agreed with how affection deprived they were (e.g., "I don't get enough affection from others") on a 9-point Likert-type scale (1 = *strongly disagree* to 9 = *strongly agree*). The measure obtained fair reliability throughout the intervention (M_α = .86).

Depression was measured by using the Center for Epidemiological Studies Depression scale (CES-D) 10-item Short Form (Björgvinsson et al., 2013). Participants were asked to rate how much they agreed with statements of perceived depression (e.g., "I could not get 'going'") on a 9-point Likert-type scale (1 = *strongly disagree* to 9 = *strongly agree*). The measure achieved satisfactory reliability throughout the intervention (M_α = .89).

Loneliness was evaluated via the UCLA Loneliness 6-item Short Form (ULS-6; Neto, 2014). Participants were asked to indicate the extent to which statements regarding their loneliness (e.g., "I feel left out") were true or untrue of them on a 9-point Likert-type scale (1 = *not of all true of me* to 9 = *very true of me*). The measure yielded adequate reliability throughout the intervention (M_α = .88).

Stress was assessed with the 10-item Perceived Stress Scale (PSS; Cohen et al., 1983). Participants were asked on a 9-point Likert-type scale (1 = *strongly disagree* to 9 = *strongly agree*) how much they agreed with statements of perceived stress (e.g., "I have been upset

because of something that happened unexpectedly"). The measure yielded acceptable reliability throughout the intervention (M_{α} = .88).

Results. It was hypothesized that the treatment group would be less affection deprived, less depressed, less lonely, and less stressed than participants in the comparison and control groups over the course of the intervention (T_1 to T_3). A 3 (group: treatment, comparison, control) × 3 (time: T_1, T_2, T_3) mixed model MANOVA was conducted in SPSS version 26 to assess how measures of the four mental health dependent variables changed by the end of the intervention between the three groups. The average intercorrelation between the dependent variables was r = .61, indicating a moderate, positive association and offering empirical support that affection deprivation, depression, loneliness, and stress were related (Keyton, 2018). Conceptually, these variables are related, as they are all detrimental outcomes negatively associated with affectionate communication and are mental health states and feelings that covary in the literature. Considered together with Bartlett's test of sphericity being significant, χ^2 (77) = 1699.042, p < .001, the use of a single multivariate analysis instead of four univariate analyses was justified.

Differences between the three groups over time were nonsignificant at the multivariate level, F(4, 216) = 1.501, p = .203; Wilk's Λ = 0.947, partial η^2 = .027, observed power = .461. Univariate results were examined given the directional nature of Study 1's hypothesis. Huynh-Feldt-corrected degrees of freedom were used because Mauchly's test of sphericity was violated. A significant univariate time-by-condition interaction was not found, F (2.734, 149.008) = 0.593, p = .605; partial η^2 = .011, observed power = .147. Study 1's hypothesis was rejected.

Lessons Learned from the Study 1

The nonsignificant results are disappointing, but a potential qualifier, specifically a *history* threat to internal validity, warrants consideration. Participants were asked to complete the

T_3 questionnaire in mid-October. The university that the participants attend is on the semester system with midterm exams around the time T_3 was administered. Not surprisingly, the administration of exams and the test anxiety that often occurs with examinations are associated with numerous detrimental mental health problems, physical health outcomes, and relational difficulties (Campbell et al., 2018; Hurst et al., 2012; Wyatt et al., 2017; Zhang et al., 2011). The unhealthy behaviors and overall acute decline in mental health during this time is, perhaps, simply too much for increased hugging by itself to rectify for this specific population.

Nevertheless, three positive methodological outcomes and realizations are worth mentioning. First, being able to recruit participants from a large research pool of undergraduate students allowed for oversampling to prospectively account for the attrition that occurred. Second, the dependent variables' measures maintained acceptable reliability across the intervention. Finally, the null results—although disheartening—underscored the importance and necessity of taking methodological threats to internal validity, such as history, into account when planning a longitudinal intervention. The threat to Study 1's internal validity was unexpected but is logical *ex post facto.* This realization proved useful for adjusting the present study's instructions and scrutinizing the present study's results in light of COVID-19, which are discussed in the proceeding sections.

Adjusting for the Present Study

The aforementioned shortcomings and realizations from Study 1 warrant modifications to make the present study an ambitious, yet feasible, endeavor that will provide a unique contribution to the literature. The proposed changes revolve around the target sample and ecological validity.

Funding and feasibility constraints during Study 1 prohibited acquiring a sample of the intended target population for the present study: the general adult population of the United States. The present study was funded in part by a research award that helped lessen the initial financial burden of participant recruitment. To obtain a sample of adults living in the United States, Amazon.com Mechanical Turk (MTurk) was used as the primary sampling frame. Although MTurk is not free from criticism (Stritch et al., 2017; see Chapter 4), the samples obtained from MTurk workers are consistently *more* representative of the U.S. adult population than not and are especially more so than college student convenience samples (McCredile & Morey, 2019; Mullinix et al., 2015). Furthermore, evidence suggests MTurk workers also noticeably present more attention and effort for completing Human Intelligence Tasks (HITs) compared to student samples and Qualtrics participants who complete comparable online surveys (Anson, 2018). A Census-matched probability sample is not feasible for the present study, given the budget limitations and feasibility constraints, but paying and recruiting MTurk workers is conceivable. As such, the present study used MTurk as the primary sampling frame and use MTurk "master workers" who have a history of passing attention checks and, on average, tend to pay more attention to surveys than their less frequent, non-master workers peers (Amson, 2018).

When revisiting the affectionate communication intervention literature, the author recognized a gap in the literature and an opportunity to provide a unique contribution. Without question, targeting a single affectionate behavior such as cuddling (van Raalte et al., 2021) or affectionate writing (Floyd et al., 2007) is advantageous from a methodological standpoint to isolate a causal behavioral mechanism, but the totality of what it means to communicate affection makes it more than a research variable: it is a behavior that takes time to enact, establish, and appreciate across different relationships and in different settings. The affectionate

communication intervention literature lacks studies that have prioritized ecological validity. This is not a criticism of their methodologies or a suggestion to question their findings; rather, it is an observation and opportunity to push this area of research forward and address empirical questions that lack a corpus of research.

At the opposite end of the spectrum of targeting a single behavior is asking people to be more affectionate *in toto* without offering any type of structure or guidance. Such an approach has its own potential problems. Although it might be clear to behavioral scientists who study affectionate communication that numerous ways of expressing affection exist, the average person may not be fully aware of the various ways people express their affection to others, let alone have the self-reflexivity or knowledge to thoroughly identify different channels of communication that exist (Floyd, 2020).

The complete absence of specified instruction or behavioral recommendation from a researcher to a participant in an intervention that prioritizes ecological validity is a potential concern, and because there is a clear absence of affectionate interventions that have manipulated affectionate communication in all respects, the literature empirically lacks for researchers to confidently point to the most effective course of action in this situation. One potential instrument that researchers can use is Floyd and Morman's (1998) Affectionate Communication Index (ACI). The ACI identifies three broad types of affectionate behaviors: verbal, direct nonverbal, and indirect nonverbal (socially supportive) behaviors. Besides varying in their optimal levels of affection, people also vary in how they communicate affection. For example, some adults prefer to express their affection to others via verbal statements, such as "I love you," or nonverbally, such as through hugging, but others communicate affection in more indirect, socially supportive manners (Floyd & Morman, 1998). The present study is the first to use the ACI as an informative

baseline assessment for participants in the treatment group to receive a personalized assessment of their affectionate communication behavior at the start of the intervention when they are instructed to increase their affectionate communication *in toto*.

The Present Study

Participants. Participants (N = 73) were married adults living in the United States. Their average age was 40.52 years (SD = 11.80) and they ranged in age from 25 to 76 years. Most identified as white (89%, n = 65), followed by Asian (9.6%, n = 7), Native Hawaiian or other Pacific Islander (4.1%, n = 3), Hispanic or Latino (2.7%, n = 2), Black or African American (1.4%, n = 1), and American Indian or Alaskan Native (1.4%, n = 1; participants were allowed to report multiple racial identities, explaining why these percentages sum to > 100 and n > 73). More than half of the participants (69.9%, n = 51) indicated their biological sex was female and the remaining participants (30.1%, n = 22) reported male. The same percentage and number of participants who were male identified as men (30.1%, n = 22), whereas 68.5% (n = 50) identified as women, and one individual (1.4%) identified as transgender. On average, participants had known their spouse for 16.38 years (SD = 10.73, range 3–50), had lived with their spouse for 13.19 years (SD = 10.42, range 1–46), and had been married for 12.10 years (SD = 10.44, range 0–46; participants were asked to round their answers to the nearest whole number, explaining why range minimum value = 0, which indicates being married for less than a year). Most were in an opposite-sex marriage (94.5%, n = 69). The remainder of the sample (5.5%, n = 4) were in a same-sex marriage. The final sample consisted of 26 participants in the treatment group, 24 in the comparison group, and 23 in the control group. Table 1 reports changes in total and group sample sizes. IRB approval was granted before participant recruitment began (see Appendix C).

Procedure. Prior to participant recruitment, an *a priori power* analysis in G*Power 3.1 was conducted. For a repeated measures, between-within interaction MANOVA to detect the average effect size in communication of $f = .21$ (Rains et al., 2018), standard error $\alpha = .05$, power = .80 for three groups across five measurements, the target final sample size was $N = 175$. Recruitment for participants who were adults ≥ 18 years of age, married, and living with their spouse in the United States was conducted via Amazon Mechanical Turk (MTurk) and Facebook in August 2020 (see Appendix D). For MTurk, only those who were designated as "master workers" with a worker approval rating ≥ 90% were eligible to complete the initial survey. Master workers who met the eligibility criteria and completed the initial survey were paid $2.00US for their participation. The author utilized voluntary response sampling by posting a call for participants as a public social media post on Facebook. Adults recruited via Facebook completed the same initial survey as the MTurk workers, but Facebook respondents were not paid for their initial effort. Informed consent from participants was acquired before starting the survey (see Appendix E). At the end of the initial survey, participants were provided a brief description of what the longer protocol entailed (e.g., duration of the longer protocol, paid compensation) before being asked if they were interested in participating in the longer study protocol. Those who indicated interest and provided their contact information were invited to participate.

Participants who expressed interest in the intervention were randomly assigned to either an affectionate communication condition (treatment group), a thankfulness communication condition (comparison group), or a no behavior change control group. Study instructions were emailed to participants at the start of the intervention on September 17, 2020 (T_1). The intervention instructions were adapted from Floyd et al.'s (2009) kissing study and van Raalte et

al.'s (2021) hugging study (see Appendix F). Immediately following the study instructions, participants in the treatment group received a personal email that described their ACI scores in layman's terms (see Appendix G). The purpose of the ACI assessment emails was to inform participants about their affectionate communication behavior and to get participants in the treatment group thinking about how to communicate affection more often for the intervention. It was emphasized at the end of these informative emails to communicate affection to others in ways that were comfortable for them, given COVID-19.

To incentivize participation and minimize attrition, participants received a $10US Amazon or Starbucks gift card of their choice for each survey they completed if they passed two randomly shuffled attention checks. Halfway through the intervention ($T_{2;}$ October 1, 2019), participants were emailed the second survey. The four-week intervention concluded on October 15, 2019 (T_3). Participants had an opportunity to complete two post-intervention surveys: two weeks after the intervention (T_4; October 29, 2019) and six weeks after the intervention (T_5; November 27, 2020). Participants who completed each post-intervention survey and passed the attention checks were entered in a random drawing for a chance to win $100US.

Dependent Variable Measures. In addition to the measures that were used in Study 1 (see Appendix H), the Kansas Marital Satisfaction Scale (KMMS, KMS; Schumm et al., 1986) was added to assess marital satisfaction. This 3-item measure asks participants to rate how satisfied they are with (1) their marriage, (2) their partner as a spouse, and (3) their relationship with their spouse. The scale has been used in research examining the role of affection and martial satisfaction (e.g., Hyun & Shin, 2010) and is one of the most commonly used measures for assessing marital satisfaction (Grable & Britt, 2006; Omani-Samani et al., 2018). Table 2 lists the dependent measures' Cronbach's alphas (n = 25).

Post-Study Group Interviews

Participants. Per the recommendation of a senior scholar and expert in clinical health psychology, the author conducted post-study group interviews after T_5 data collection for the present study concluded. The primary purpose for conducting group interviews was to obtain participant feedback that would inform future interventions (see Appendix I). Of the 26 participants in the present study's affectionate communication treatment group who completed the intervention, seven expressed interest in participating in a group interview. Ultimately, six were able to participate. These group interviews were included as part of the present study that was approved by IRB.

The average age of the post-study group interview participants (N = 6) was 43.33 years old (SD = 13.02, range 28–61). Four identified as white, and the remaining two participants identified as Asian. Five participants indicated their biological sex was female and one participant reported being a male. The male participant identified as a man, and the five female participants identified as women. On average, participants had known their spouse for 20.50 years (SD = 11.01, range 6–40), had lived with their spouse for 16.83 years (SD = 11.78, range 3–38), and had been married for 14.67 years (SD = 12.62, range 1–38). All participants were in an opposite-sex marriage.

Procedure. Two group interviews were conducted via Zoom during the week of December 14, 2020. Each group interview lasted 60 minutes, and informed consent was obtained from all participants prior to the video and audio recording of each group interview (see Appendix J). Both group interviews were moderated by the author. After briefly explaining the purpose of the group interviews and reminding participants that their participation was voluntary, the author began the semi-structured group interviews (see Appendix K). At the end of each

group interview meeting, participants were thanked and paid $20.00US immediately after for their participation.

CHAPTER 3

RESULTS

Data Preparation and Testing Assumptions

All analyses were conducted in SPSS v.27. Mahalanobis distance scores were calculated for each participant to check for multivariate outliers. A critical value of 59.70 was calculated with $\alpha = .001$ and $df = 30$. None of the participants had a Mahalanobis distance scores greater than the critical value, and as such, none of the 73 participants was removed for data analysis.

Prior to hypothesis testing, two-tailed bivariate correlations were conducted to determine whether participant age, the number of years participants had known their spouse, the number of years participants were married to their spouse, or the number of years participants had lived with their spouse were associated with the four mental health dependent variables. Age was significantly associated with depression at T_2, r (72) = -.26, p = .03, and with stress at T_1, r (71) = -.25, p = .03, T_2, r (71) = -.24, p = .04, and T_5, r (67) = -.27, p = .03. Other correlations were nonsignificant. Given these associations, only age was included as a covariate in hypothesis tests.

Manipulation Check

A 21-item matrix of behaviors was created to assess whether the participants in the treatment group significantly increased the amount of affection they communicated during the intervention, whereas those in the comparison and control groups did not. Participants were asked, “Over the last two weeks, have you done each of the following behaviors more often than usual, about the same as usual, or less often than usual?” for each item on an 11-point Likert-type scale ranging from -5 (*much less than usual*) to 5 (*much more than usual*). Of the 21 items, one item measured increased affection (i.e., “communicating affection to others”), another item measured increased thankfulness (i.e., “communicating thanks to others”), and the remaining 19

items were distractions (e.g., “video conferencing with others”, “spending time on social media”, “listening to music”).

To assess the manipulation, two planned contrasts were conducted, the first for T_2 and the second for T_3. This pair of contrasts assessed whether the treatment group communicated more affection than either the comparison or control groups. At both time periods, the planned contrast assigned a contrast coefficient of +2 to the treatment group and coefficients of -1 to the comparison and control groups. A second pair of planned contrasts assessed whether the comparison group communicated more thankfulness than both the treatment and control groups at T_2 and T_3. This contrast assigned a coefficient of +2 to the comparison group and coefficients of -1 to the treatment and control groups (see Table 3).

The first pair of planned contrasts, assessing the success of the affection manipulation, were nonsignificant at T_2, t (70) = 1.34, p = .09, and T_3, t (70) = 1.07, p = .14. The intended manipulation failed.

Because the planned contrasts did not compare the treatment group to the comparison and control groups separately, four independent samples *t*-test were conducted to investigate potential differences in increased affection of the treatment group and the two groups at T_2 and T_3. Results showed that the treatment group did not significantly communicate more affection than the comparison group at T_2, t (48) = 1.40, p = .09, or T_3, t (48) = 0.33, p = .37. The treatment group did not significantly communicate more affection than the control group at T_2, t (47) = .642, p = .26, but did significantly communicate more affection than the control group at T_3, t (47) = 1.88, p = .03. See Table 4 for means and standard deviations of the affectionate communication and thankfulness manipulation check items used at T_2 and T_3.

The second pair of planned contrasts, assessing the success of the comparison group instructions, were significant at T_2, $t(70) = 3.26$, $p < .01$, and at T_3, $t(70) = 3.20$, $p = < .01$. These results indicate that participants in the comparison group did communicate more thankfulness to others than did participants in the treatment or control groups, as instructed.

Statistical Considerations Given a Failed Manipulation

The significant difference in affectionate communication between the treatment group and control group by the end of the intervention, but not between the treatment group and comparison group, poses a potential problem for the study's hypotheses and research questions. Ultimately, the failed manipulation casts doubt over the extent to which it is appropriate to compare all three groups to each other, which is precisely what the study's hypotheses and research questions call for. One may question whether comparing all three groups is warranted when the comparison group does not significantly differ from the treatment group. Phrased another way, the treatment and comparison groups are not distinct groups in terms of their increase in affectionate communication, although they do differ significantly in terms of their increase in communicating thanks. Thus, there is an argument to be made that the most appropriate course of action would be to drop the comparison group and test for differences in the experimental outcomes between only the treatment and control groups to truly see if the treatment (i.e., increased affectionate communication) had an effect.

On the contrary, one might argue that such a course of action constitutes "p-hacking" or "data fishing" for statistically significant results. Both positions have their justifications. Acknowledging these concerns, the author concluded the most appropriate course of action would be first to conduct the tests that the hypotheses originally called for, then test for differences in the experimental outcomes between only the treatment and control groups, and

finally conduct post-hoc tests exploring other potential differences between the treatment group and control group. The remainder of this chapter discusses these findings.

Hypotheses and Research Questions

Hypothesis 1 predicted that participants in the treatment group would self-report feeling (a) less affection deprived, (b) less depressed, (c) less lonely, and (d) less stressed than participants in the comparison and control groups over the course of the intervention (T_1 to T_3), while the first research question asked if the aforementioned mental health benefits would persist after the intervention concluded (T_4 and T_5). To address H1 and RQ1, a 3 (group: treatment, comparison, control) × 5 (time: T_1, T_2, T_3, T_4, T_5) mixed model MANCOVA was conducted to assess how measures of the four mental health dependent variables changed over time between the three groups with participant age as a covariate. The average intercorrelation between the dependent variables was $r = .66$, indicating a moderate, positive association and offering empirical support that affection deprivation, depression, loneliness, and stress were related (Keyton, 2018; see Table 5). These four variables are conceptually related, as they are negatively associated with affectionate communication and are mental health states and feelings that covary in the literature. Considered together with Bartlett's test of sphericity being significant, $\chi^2 (209) = 1743.60$, $p < .001$, the use of a single multivariate analysis instead of four univariate analyses was justified.

Differences between the three groups over time were nonsignificant at the multivariate level, $F (8, 108) = 1.753$, $p = .094$; Wilk's $\Lambda = 0.783$, partial $\eta^2 = .115$, observed power = .730. Univariate results were examined given the directional nature of H1, and Huynh-Feldt-corrected degrees of freedom were used because Mauchly's test of sphericity was violated. A significant univariate time-by-condition interaction was not found, $F (7.166, 204.229) = 1.447$, $p = .183$;

partial η^2 = .049, observed power = .614. H1 was rejected. Taking the failed manipulation and nonsignificant results into account when addressing RQ1, we cannot assert that any significant mental health benefits for participants in the treatment group existed at the end of or persisted after the intervention.

Hypothesis 2 predicted that participants in the treatment group would self-report greater marital satisfaction than participants in the comparison and control groups at the end of the intervention, and the second research question asked if increased martial satisfaction would persist after the intervention concluded. To address H2 and RQ2, a 3 (group: treatment, comparison, control) × 5 (time: T_1, T_2, T_3, T_4, T_5) repeated measures ANOVA was conducted to assess how marital satisfaction changed over time between the three groups. Experimental group did not have a significant univariate effect for marital satisfaction, F (2, 58) = 2.085, p = .133; partial η^2 = .067, observed power = .412. Taking the failed manipulation and nonsignificant results into account when addressing RQ2, we cannot assert that any significant martial satisfaction benefits for participants in the treatment group existed or persisted after the intervention concluded.

Removing the Comparison Group

The statistical analyses used to address H1 and H2 were reconducted after filtering participants from the comparison group out of the data set for the aforementioned exploratory purposes. The differences between the treatment group and control group yielded similar nonsignificant results for mental health, F (4, 33) = 1.500, p = .225; partial η^2 = .154, observed power = .412, and marital satisfaction, F (1, 37) = 1.935, p = .173; partial η^2 = .050, observed power = .273. Like the previous set of analyses for H1, univariate results were examined after using Huynh-Feldt-corrected degrees of freedom in consequence of violating Mauchly's test of

sphericity in the filtered data set. A significant univariate time-by-condition interaction was not found, F (2.899, 104.348) = 1.489, p = .223; partial η^2 = .040, observed power = .377. H1 and H2 were not supported after removing the comparison group from the analyses.

Post Hoc Analyses

For exploratory power purposes, two 3 (group: treatment, comparison, control) × 5 (time: T_1, T_2, T_3, T_4, T_5) repeated measures ANOVAs and ANCOVAs were conducted to assess how affection deprivation and loneliness changed over time between the three groups and how depression and stress changed over time between the three groups with age as a covariate, respectively. Experimental group did not have a significant univariate effect for affection deprivation, F (2, 58) = .073, p = .930; partial η^2 = .003, observed power = .061, loneliness, F (2, 58) = .044, p = .957; partial η^2 = .002, observed power = .056, depression, F (2, 57) = .130, p = .878; partial η^2 = .005, observed power = .069, or stress, F (2, 57) = .032, p = .969; partial η^2 = .001, observed power = .055.

Table 6 contains means and standard deviations of the dependent variables. Figures 1-5 graph the means of each dependent variable over time. Post hoc analyses comparing the means of the dependent variables at the end of the intervention (T_3) showed that participants in the treatment group were less depressed (M = 3.07, SD = 1.52), less lonely (M = 3.15, SD = 1.91), and less stressed (M = 3.30, SD = 1.70) than the control group (M = 3.51, SD = 1.71; M = 3.31, SD = 1.93; M = 3.61, SD = 1.64) compared to baseline at T_1. Participants in the treatment group showed a steady decline in their depression, loneliness, and stress from T_1 to T_3, whereas the control group showed an increase in depression and loneliness at T_2 before returning to slightly above baseline at T_3. These means are trending in the hypothesized direction but should be interpreted with caution given the shortcomings of the treatment manipulation.

Post-Study Group Interviews

More than speculating about the detrimental effects that COVID-19 had on opportunities for social interactions and affectionate communication, which may have posed a significant threat to internal validity (see Chapter 4), data from group interviews provide evidence to back this claim and conjecture that participants' normal routines were anything but normal during the intervention. All participants described various mental and relational difficulties due to COVID-19. Many expressed a desire to increase the amount of affection they communicated with people other than their spouse, primarily through hugging and communicating face-to-face with friends and other family members in places where they would socialize in pre-pandemic times (e.g., other homes, shopping centers, restaurants). Some participants mentioned that the lack of their face-to-face communication was, in part, due to adhering to local laws and federal mandates. These participants emphasized doing their job to stop the spread during the pandemic, which resulted in staying indoors and limiting outside interaction to only what was deemed essential (e.g., grocery shopping, taking pets outside). As noted by Floyd (2006) in his formal explication of AET, "that affectionate behavior should be affected by social or cultural norms may seem entirely self evident," and should nonetheless be acknowledged here (p. 188). Considered together, asking participants to be more affectionate was potentially harmful for reasons other than their physical health: it also posed a threat to their ability to follow new normative behaviors and societal expectations of limiting non-essential interactions.

CHAPTER 4

DISCUSSION

Humans have a fundamental need to belong, and with this belongingness need comes an innate desire to convey and receive affection from members of our social species (Baumeister & Leary, 1995; Floyd 2006). COVID-19 continues to illuminate the importance of affection and belongingness, as it has disrupted our social interactions and has had a deleterious effect on the collective mental health of the United States (Aknin et al., 2022; Patterson et al., 2021). This study sought to implement an affectionate communication intervention to examine how increasing the amount of affection people express could bolster mental health and relational satisfaction during the height of the COVID-19 pandemic. On the basis of AET, it was hypothesized that participants in the treatment group would be less affection deprived, depressed, lonely, and stress than those in the comparison and control groups by the end of the 4-week intervention. Unexpectedly, manipulating a significant increase in communicating affection during the pandemic failed, and none of the study's hypotheses was supported. The remainder of this chapter addresses study limitations and offers reasoned explanations for them; suggests methodological improvements to resolve the proposed limitations for future conceptual replications and interventions; and highlights strengths of this study, despite the null findings, before offering final thoughts.

Reflecting on Null Findings: Speculating and Explaining Study Limitations

One limitation of this study was the total sample size and observed power. The power to detect a significant effect is, in part, a function of sample size (Cohen, 1992). Larger sample sizes have more power to detect an effect, if such an effect exists, and are less likely to lead to a Type II error. Despite the author's efforts to recruit enough participants to account for attrition,

the author was unable to obtain a large enough sample, which resulted in an underpowered study. Falling short of the target sample size (N = 175), a sensitivity analysis conducted in G*Power 3.1 showed an effect size of f = .33 was required to detect a significant effect with standard error α = .05 and power = .80 for the 73 participants in the three groups across the five measurements. Moreover, as reported by Hesse and colleagues (2021) after data collection for the present study concluded, the mean effect estimate for mental health outcomes and affectionate communication is smaller (r = .19) than the mean effect size estimate in the field of communication that was used in the *a priori* power analysis (r = .21), and as such, a larger final sample size (N = 212 vs. N = 175) is required to detect a significant effect when keeping all other parameters the same.

The built-in repeated measures nature of a longitudinal intervention is a favorable research design component that reduces standard error and adds to statistical power, but ultimately, this was still not enough to overcome the study's small sample size. The observed power when looking at the time-by-condition interaction was .730 for the mental health outcomes and .412 for marital satisfaction. Observed power was not egregious, but it was still far from optimal. Even if the treatment manipulation had worked as intended, there is a possibility that a significant effect would not have been detected. It is unlikely that the study failed because of statistical power alone, but nevertheless, the underpowered nature of this study is a limitation and explanation that warrants addressing.

A second study limitation was a failure to adequately manipulate the treatment group's instructed behavior, which resulted in a comparison group that was not sufficiently different from the treatment group in terms of significantly increasing the amount of affectionate communication throughout the intervention. Although it is true that the comparison group did significantly communicate thankfulness more than the treatment and the control groups

throughout the intervention, the purpose and main goal of this study was to manipulate the frequency of affectionate communication. An oversight by the author is the possibility that communicating affection and communicating thankfulness have too much conceptual overlap. This is to say, when people are engaged in thanking others, affectionate behaviors manifest in conversation. The manipulation check means support this speculation (see Table 4). Even though the comparison group was not instructed to increase their affectionate communication, the comparison group did, and even more than the treatment group. This pattern was not apparent for the treatment group: the treatment group did not communicate thanks more than—or even at a comparable level—as the comparison group, but still reported a slight increase in communicating thankfulness during the intervention. For a behavioral intervention, it is essential to devise a comparison group behavior that is identical in all ways to the treatment group except for the manipulated target behavior the treatment group is asked to enact. Perhaps an affectionate intervention prioritizing ecological validity has yet to be conducted or published because devising an adequate comparison group for an overall affectionate communication intervention appears to be difficult. Developing a sufficient comparison group will require more time and is the next step for developing a worthwhile conceptual replication.

The history threat to internal validity caused by COVID-19 is a third limitation that cannot be ignored. It is important to recognize that COVID-19 vaccines were not available and the rapid COVID-19 test kits that exist today had not been developed throughout the entire data collection period of this study. In an effort to flatten the curve and stop the spread of the virus, local, state, and federal laws prohibited social events and face-to-face activities. Small businesses closed shop, public spaces fell silent, and schools transitioned online as the nation held its breath, wondering when the pandemic would end. The COVID-19 death toll and hospital admission rate

continued to increase as public health officials encouraged people to stay inside to limit nonessential interaction when our health systems were pushed to the brink (Tangcharoensathien et al., 2021). Ultimately, these efforts were largely unsuccessful at slowing the second wave, as new COVID-19 cases increased exponentially from September 2020 to January 2021 (Centers for Disease Control and Prevention, 2022b). It is easy to forget how daunting, risky, and limited it was—and still is, to a certain extent—to socialize with others during the latter half of 2020 when a cure for the deadly virus was largely out of sight.

People had to ask themselves: was affectionate communication worth the risk? For many, the answer was no. Of the 28 participants in the treatment group who completed the T_2 assessment, nine reported communicating less than or the same amount of affection since T_1 compared to only two of the 29 participants from the thankfulness comparison group who reported communicating less than or the same amount of thankfulness since T_1. Similarly, seven of the 26 participants from the treatment group reported communicating less than or the same amount of affection since T_3 versus three of the 24 participants from the comparison group who reported communicating less than or the same amount of thankfulness since T_3. Communicating affection has a number of risks, including disease transmission (Floyd, 2006), and ultimately, increasing affectionate communication for many was not worth the potential health risks when a vaccine for the virus was not available.

The intervention treatment group instructions themselves may have had a detrimental effect for some of the treatment group participants. One participant, an Asian female in the healthcare field, described in the group interview how being asked to increase her affectionate communication at the start of the intervention increased her perceived stressed. Despite her best efforts to increase her affection, her availability to communicate in general was severely limited

due to her job. Moreover, because she was an essential worker who was continuously putting herself at risk, she had gone weeks without physically seeing her friends and family members so they would not get sick. Another participant, an older male, expressed a ceiling effect for his affection: because he had known and been married to his wife for decades, there was only so much more affection he felt like he could genuinely communicate to her. It is important to note and remember that participants in the treatment group were not instructed to communicate affection to any specific person or groups of people. His self-disclosure during the group interview highlighted the fact that some participants may have relied on their spouses exclusively for communicating affection because face-to-face affection was preferred over mediated affectionate communication. These are concerns and obvious problems when it comes to manipulating the treatment variable. Ultimately, participants expressed a willingness and desire to be more affectionate, but they were unable to do so for reasons many deemed outside of their control.

Asking people to increase their affectionate communication during a global pandemic was a daunting task. Some may posit that the intervention was doomed to fail. Others may question if conducting such an intervention during this period was a worthwhile endeavor, to which the author would respond "yes." The potential benefits of an affectionate communication intervention bolstering mental health during the pandemic was too important to ignore. An *in toto* affectionate communication intervention at a point in time when people around the world were starving for affection and social connection was an opportunity to put theory into practice and an effort to make a meaningful difference. It would have forever remained an empirical question had this study been dismissed when COVID-19 officially became a pandemic shortly

after Study 1 concluded. Considered together with the previous reasoned explanations, COVID-19 is a very realistic reason for why the treatment did not produce greater beneficial results.

Looking Forward: Future Methodological Changes and Improvements

Rectifying the abovementioned shortcomings is a feasible undertaking that can, and should, happen by conducting an improved conceptual replication. The following section offers recommendations and improvements to address the limitations that were previously discussed.

Obtaining a larger sample is essential for remedying underpowered statistical analyses. Participant recruitment fell short of the target $N = 175$, and the observed power from the results reflect this shortcoming. The utility of a repeated measures design, as it relates to power, was optimized in the present study with the three groups; the addition of a second comparison or control group is unnecessary. For the added value of including a second post-intervention assessment and given the mean effect size findings from Hesse et al. (2021), an effort to recruit enough participants for a larger final $N = 212$ should be made to conceptually replicate the present study.

MTurk samples are often described as *more* representative of the general population than college student convenience samples (Paolacci et al., 2010) and not *fully* for a reason. On average, MTurk workers samples consist of more female, white-collar, overeducated, and less ethnically representative people than the general population (Castille et al., 2019). Some research suggests that differences between designated worker statuses (e.g., regular workers, master workers) also exist (Loepp & Kelly, 2020). Ultimately, samples collected from MTurk fall short to those that are obtained from probability sampling techniques (Berinsky et al., 2012). MTurk samples "should not be treated as representative of the general population," but are nevertheless

more representative than university student convenience samples (Paolacci & Chandler, 2014, p. 185).

With both the benefits and limitations of MTurk in mind, other platforms warrant consideration for online participant recruitment. Experimental evidence suggests participants recruited from Prolific and CloudResearch are more honest, more reliable, pay more attention, and understand task instructions better than participants recruited from MTurk, Qualtrics, and Dynata, with Prolific earning the highest rank based on average participant data quality composite scores (Eyal et al., 2021). More attentive, reliable, honest, and competent participants, however, come at a greater monetary price. Whereas MTurk allows researchers to determine how much they are willing to compensate workers for completing their HITs, the minimum compensation rate for Prolific is set to $6.50US per hour. Obtaining a Census-matched probability sample is possible from such online data collection platforms, but ultimately resource and funding limitations need to be considered when assessing the feasibility of a conceptual replication of this study.

In addition to volunteer sampling that the author used, another feasible avenue for participant recruitment is convenience sampling. Undergraduate students can be used to recruit adults who meet the study's inclusion criteria. In exchange for having the recruited adult complete the initial survey, undergraduate students can earn research participation credit for their courses (e.g., Floyd & Riforgiate, 2008). Although these methods are not probability sampling techniques, thus limiting the generalizability of the findings to the general population, recruiting enough participants to satisfy statistical power demands should be prioritized to address the limitation of an underpowered study.

The second study limitation revolves around the failure to sufficiently manipulate the treatment group's instructed behavior. A valid critique of the present study's instructions is the absence of frequency specificity for both the treatment and control groups' instructed behaviors. Participants in these groups were instructed to communicate affection or thankfulness "more often" in their close relationships. This, perhaps, did not offer enough structure over the 4-week intervention for participants in the two groups to follow instructions in a comparable fashion. Other affectionate communication interventions have manipulated affectionate behaviors by instructing participants to enact an affectionate communication behavior a set number of times per day (e.g., van Raalte et al., 2021) or number of minutes per day (e.g., Floyd et al., 2007), or to increase by a specific percent of what they normally do (e.g., Floyd et al., 2009). To minimize the potential variance of increased communication between both groups, instructing participants to increase their frequency of communication a set number of times per day would be a methodological improvement. This would still allow participants in the treatment group to have full autonomy over how they are communicating affection (i.e., verbal, direct nonverbal, indirect nonverbal), while adding an element of experimental control across the treatment group and comparison group.

Means from the manipulation checks indicate the comparison group increased their affectionate communication throughout the intervention, although they were not instructed to do so. Preventing affectionate behavior from manifesting via the comparison group's instructed behavior is a second necessary improvement. Positive forms of communication, such as communicating thanks, may create opportunities for affectionate feelings and communication to emerge. The purpose of the comparison group for the present study is to rule out the alternative hypothesis that increasing communication by itself is not bolstering mental health. To have more

confidence in *affectionate* communication being the causal mechanism, the comparison group needs to mimic the treatment group as closely as possible and only differ in the effective element (i.e., communicating affection). Ideally, the comparison behavior would be one that could be communicated both verbally and nonverbally, as the present study prioritized ecological validity and intentionally allowed participants to communicate affectionate through the channel(s) of their choice. Restricting the comparison behavior to a specific channel of communication (e.g., asking people to increase *talking* about a neutral topic) is not equivalent to the treatment group. Duration and effort of communication should also be comparable. It is worth reconsidering increasing greetings that Study 1 sought to employ as the comparison behavior for a conceptual replication of this intervention and also increasing the frequency per day for each behavior, rather than a percent increase. Devising a comparison group behavior for a conceptual replication of this study is an arduous task that will require more time and creativity to develop.

Fortunately, and unfortunately, there is ample time to refine the study's methodology because of the third limitation of this study: COVID-19. When it comes to addressing the history threat to internal validity that COVID-19 caused, a conceptual replication of this study needs to occur in a post-pandemic era. The destructive toll the pandemic has had on the economy (Kaye et al., 2021), educational structures (Aziz et al., 2021; Reimers, 2022), mental health (Breslau et al., 2021), health inequality (Abedi et al., 2021), social relationships (Philpot et al., 2021; Vaterlaus et al., 2021), and family functioning (Fosco et al., 2022) cannot be ignored. There is discord within the scientific community, as some researchers believe endemic equilibrium is a reasonable possibility in the coming months, whereas others posit that we are far from it (Murray, 2022; Neuman, 2022; Taylor, 2022). Exactly when we will be in a post-pandemic era is up for debate, but the adverse toll COVID-19 had on participants in 2020 is undisputable.

Participants in both group interviews mentioned COVID-19 prevented them from increasing their affectionate communication in their close relationships. Some of the NPIs that were implemented to flatten the curve (e.g., temporarily closing restaurants, limiting access to shopping centers) were discussed by group interview participants as reasons why they were unable to be more affectionate. For a behavioral intervention such as this one to work, it is pivotal that the physical environment and social conditions allow for the study behaviors and instructions to be carried out. The integrity of any conceptual replication will be severely limited if it is conducted in anything other than a post-pandemic environment.

At this point, we do not know which explanation or combination of explanations for the study's shortcomings are that have merit until conceptual replication attempts are made. Fortunately, the proposed changes that have been posed can be empirically tested and refined. Ruling out the efficacy of a high ecological validity affectionate communication intervention should not happen until conceptual replications with the proposed changes occur. Prioritizing ecological validity was a novel feature of the present study and one that will push this line of research forward.

Acknowledging Study Strengths

Several strengths warrant acknowledgement. First, this study benefited from its experimental design. Had both manipulations succeeded and significant differences in support of H1 and H2 emerged, an argument for causality could have been made. Causality requires two variables (*x* and *y*) to be associated, a time order effect (*x* precedes *y* in time), and nonspuriousness (the *x* and *y* relationship is not explained by a third variable, *z*). This is feasible from true experimental designs, such as the present study, whereas quasi-and non-experimental designs are limited to claims of associations (Singleton & Straights, 2017). Conducting two post-

intervention assessments also deserves recognition. If the research hypotheses were supported, it would have been possible to assess the extent to which the benefits of the intervention persisted after the study concluded. Successful interventions that do not include post-intervention follow ups in their designs leave researchers wondering if the benefits are temporary and do not offer long-term benefits. This is an empirical question that can be answered if and only if post-intervention assessments are included in the study design. Many interventions in the line of research have failed to implement such assessments, perhaps due to feasibility constraints. This line of research will benefit from future interventions that implement post-intervention assessments so researchers can answer the aforementioned question.

An intentional decision to recruit adults via MTurk resulted in a sample that was more diverse and representative of the target married U.S. adult population than a convenience sample of college students would have been. The average age (40.52 years), age range (25-76 years), marital duration (newlyweds to 46 years), how long they had known their spouse (3-50 years), and how long they had lived with their spouse (1-46 years) is respectable. Beyond participant demographics, the decision to primarily recruit master workers from MTurk should be acknowledged. A recent examination of studies that were conducted on MTurk during the pandemic suggests a decline in general worker attentiveness in 2020 (Arechar & Rand, 2021). Choosing to recruit master workers with a worker approval rating $\geq 90\%$ was a favorable decision and a strength of this study, even if it resulted in a smaller sampling frame.

The third strength of this study was how it utilized Floyd and Morman's (1998) ACI in an innovative way. With the goal of manipulating an increase in affectionate communication *in toto*, the author was interested in conducting an *a priori* assessment of how participants varied in their affectionate communication at an individual level. It is the author's hope that this study will pave

the way for future affection-related studies to utilize established measures, such as the ACI, in novel ways.

Conclusion

Qualified speculation and qualitative data give reason to conduct an improved conceptual replication of this study. One should not rule out the potential efficacy and feasibility of such an intervention until conceptual replication in a post-pandemic world occurs. In closing, much remains to be studied about affectionate communication and its effect on our health and our relationships.

Table 1

Participant Attrition Across the Intervention

Time	Treatment	Comparison	Control	Total *N*
T_1	33	33	32	98
T_2	28	29	26	83
T_3	26	24	23	73
T_4	19	22	21	62
T_5	23	24	22	69

Note. The intervention concluded after the T_3 assessment. Post-intervention responses (T_4, T_5) were included in the data analyses, so long as they passed the attention checks at T_1, T_2, and T_3; T_4 completion was not a requisite for T_5.

Table 2

Cronbach's Alpha for Dependent Variables

Variable	T_1	T_2	T_3	T_4	T_5	M_α
Affection deprivation	.94	.94	.93	.96	.95	.94
Depression	.93	.92	.92	.93	.91	.92
Loneliness	.89	.88	.90	.91	.91	.90
Stress	.93	.92	.92	.91	.94	.92
Marital satisfaction	.98	.97	.98	.98	.98	.98

Table 3

Planned Contrasts Coefficients

Condition	Communicating affection to others		Communicating thanks to others	
	T_2	T_3	T_2	T_3
Treatment	+2	+2	-1	-1
Comparison	-1	-1	+2	+2
Control	-1	-1	-1	-1

Table 4

Means and Standard Deviations of Manipulation Checks

Condition	Communicating affection to others		Communicating thanks to others	
	T_2	T_3	T_2	T_3
	M (SD)	*M (SD)*	*M (SD)*	*M (SD)*
Treatment	1.23 (1.68)	1.81 (1.72)[a]	1.00 (1.67)[b]	1.77 (1.63)[d]
Comparison	1.83 (1.31)	1.96 (1.52)	2.42 (1.28)[b, c]	2.58 (1.41)[d, e]
Control	1.35 (1.27)	0.91 (1.56)[a]	0.57 (1.88)[c]	0.61 (1.33)[e]

Note. Participants were asked, "Over the last two weeks, have you done each of the following behaviors more often than usual, about the same as usual, or less often than usual" for 21 statements. Scores were rated on a scale from -5 (*Much less often than usual*) to +5 (*Much more often than usual*).
[a] $p < .05$. [b, c, d, e] $p < .01$.

Table 5

Intercorrelation Matrix (Pearson Correlations) of the Mental Health Dependent Variables

Variables	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1. Affection deprivation (T_1)	—																			
2. Affection deprivation (T_2)	.63*	—																		
3. Affection deprivation (T_3)	.75*	.80*	—																	
4. Affection deprivation (T_4)	.71*	.81*	.86*	—																
5. Affection deprivation (T_5)	.69*	.75*	.87*	.88*	—															
6. Depression (T_1)	.57*	.48*	.49*	.49*	.48*	—														
7. Depression (T_2)	.37*	.58*	.44*	.50*	.46*	.77*	—													
8. Depression (T_3)	.49*	.61*	.54*	.55*	.55*	.76*	.82*	—												
9. Depression (T_4)	.52*	.64*	.60*	.64*	.64*	.70*	.78*	.88*	—											
10. Depression (T_5)	.44*	.55*	.54*	.52*	.60*	.71*	.78*	.82*	.80*	—										
11. Loneliness (T_1)	.66*	.56*	.54*	.58*	.49*	.68*	.62*	.67*	.68*	.60*	—									
12. Loneliness (T_2)	.55*	.63*	.59*	.58*	.52*	.69*	.73*	.69*	.74*	.72*	.87*	—								
13. Loneliness (T_3)	.56*	.70*	.72*	.64*	.62*	.56*	.62*	.74*	.74*	.66*	.81*	.84*	—							
14. Loneliness (T_4)	.62*	.68*	.67*	.74*	.66*	.57*	.61*	.75*	.77*	.63*	.80*	.79*	.88*	—						
15. Loneliness (T_5)	.59*	.68*	.69*	.68*	.70*	.59*	.67*	.79*	.78*	.76*	.78*	.83*	.89*	.90*	—					
16. Stress (T_1)	.53*	.44*	.46*	.46*	.46*	.94*	.71*	.70*	.69*	.70*	.63*	.67*	.55*	.54*	.56*	—				
17. Stress (T_2)	.34*	.55*	.45*	.50*	.47*	.76*	.93*	.82*	.76*	.78*	.58*	.68*	.62*	.58*	.65*	.72*	—			
18. Stress (T_3)	.46*	.51*	.50*	.52*	.52*	.75*	.75*	.86*	.81*	.77*	.62*	.68*	.65*	.67*	.72*	.73*	.81*	—		
19. Stress (T_4)	.51*	.58*	.52*	.60*	.56*	.69*	.73*	.83*	.89*	.70*	.58*	.63*	.61*	.66*	.67*	.71*	.79*	.88*	—	
20. Stress (T_5)	.42*	.50*	.49*	.52*	.56*	.74*	.76*	.77*	.78*	.93*	.62*	.73*	.64*	.62*	.71*	.76*	.79*	.78*	.74*	—

Note. *$p < .001$ (two-tailed).

Table 6

Means and Standard Deviations for Dependent Variables

Variable	Treatment					Comparison					Control				
	T_1 n = 26	T_2 n = 26	T_3 n = 26	T_4 n = 19	T_5 n = 23	T_1 n = 24	T_2 n = 24	T_3 n = 24	T_4 n = 22	T_5 n = 24	T_1 n = 23	T_2 n = 23	T_3 n = 23	T_4 n = 21	T_5 n = 22
	M (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)
Affection deprivation	3.33 (2.04)	3.53 (1.92)	3.37 (1.84)	3.95 (2.37)	3.46 (2.04)	3.99 (2.04)	4.20 (1.88)	3.79 (1.84)	3.56 (1.88)	4.07 (1.81)	3.86 (1.79)	4.20 (1.69)	3.84 (1.88)	4.07 (1.89)	4.25 (2.03)
Depression	3.38 (1.91)	3.30 (1.76)	3.07 (1.52)	3.21 (2.00)	2.93 (1.69)	3.90 (2.02)	3.61 (2.06)	3.46 (1.97)	3.30 (1.65)	3.95 (1.96)	3.50 (1.57)	3.71 (1.56)	3.51 (1.71)	4.17 (1.85)	3.72 (1.68)
Loneliness	3.25 (1.89)	3.16 (1.77)	3.15 (1.91)	3.22 (2.11)	3.20 (1.84)	3.24 (2.05)	3.46 (2.02)	3.27 (2.10)	3.20 (1.90)	3.61 (2.09)	3.28 (1.80)	3.35 (1.75)	3.31 (1.93)	3.35 (1.88)	3.46 (1.93)
Stress	3.90 (1.95)	3.82 (1.79)	3.30 (1.70)	3.90 (2.03)	3.55 (1.85)	4.32 (2.25)	4.03 (1.97)	3.92 (1.95)	3.89 (1.78)	4.01 (2.18)	4.01 (1.58)	3.85 (1.73)	3.61 (1.64)	4.13 (1.77)	3.90 (1.68)
Marital satisfaction	7.36 (1.85)	7.42 (1.86)	7.33 (2.13)	7.00 (2.04)	6.90 (2.26)	7.75 (1.76	7.81 (1.20)	7.72 (1.36)	8.03 (1.25)	7.68 (1.56)	7.77 (1.32)	7.72 (1.39)	7.78 (1.24)	7.62 (1.27)	7.39 (1.60)

Figure 1

Mean Affection Deprivation Scores by Condition Over Time

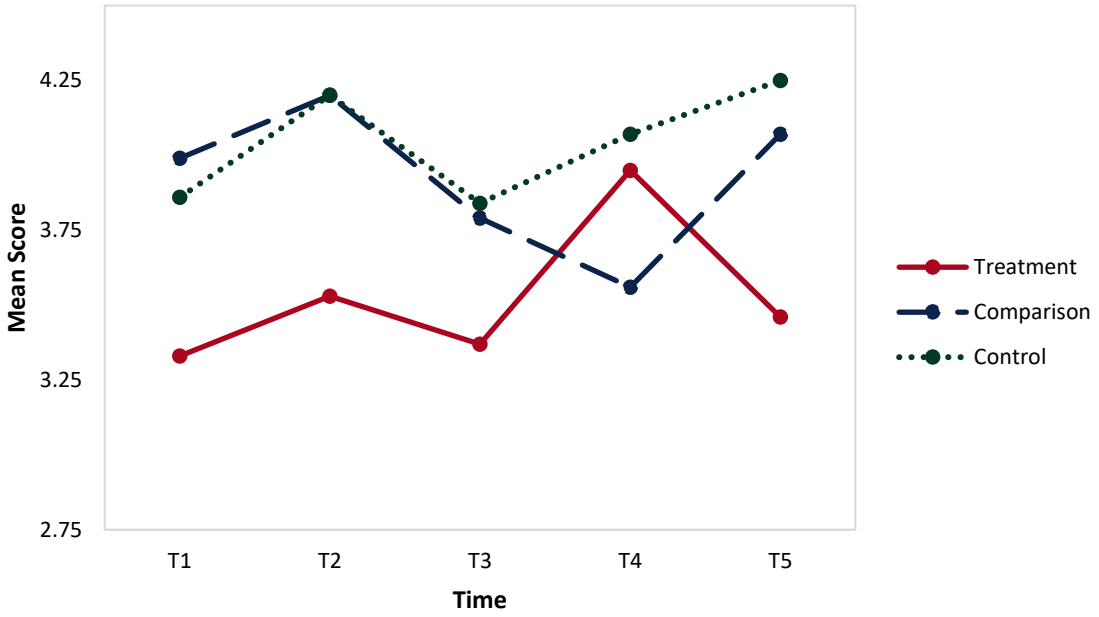


Figure 2

Mean Depression Scores by Condition Over Time

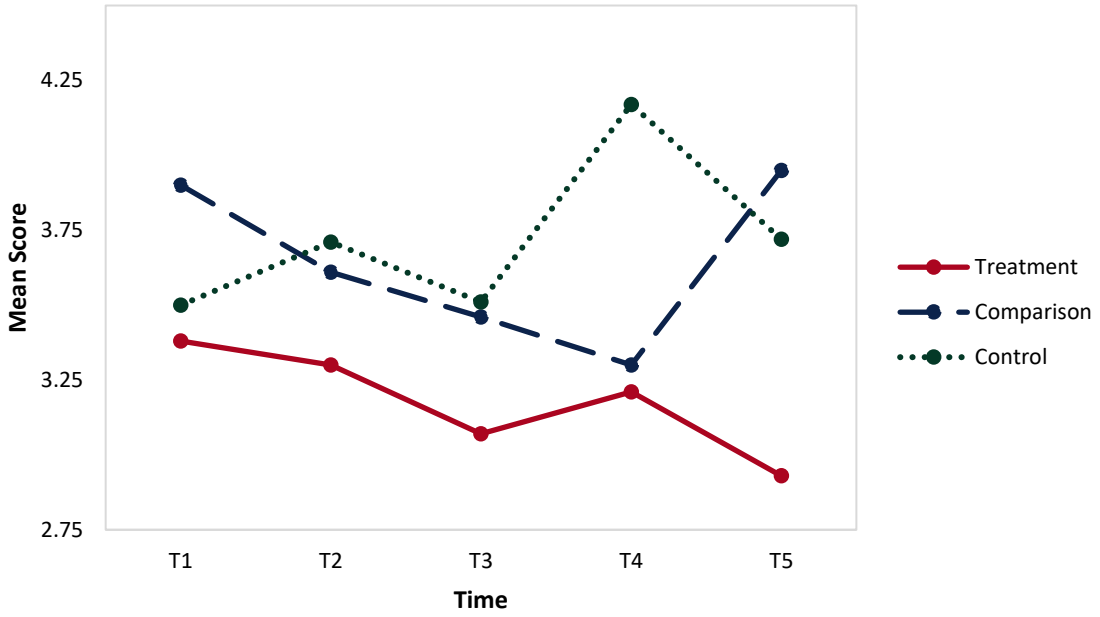


Figure 3

Mean Loneliness Scores by Condition Over Time

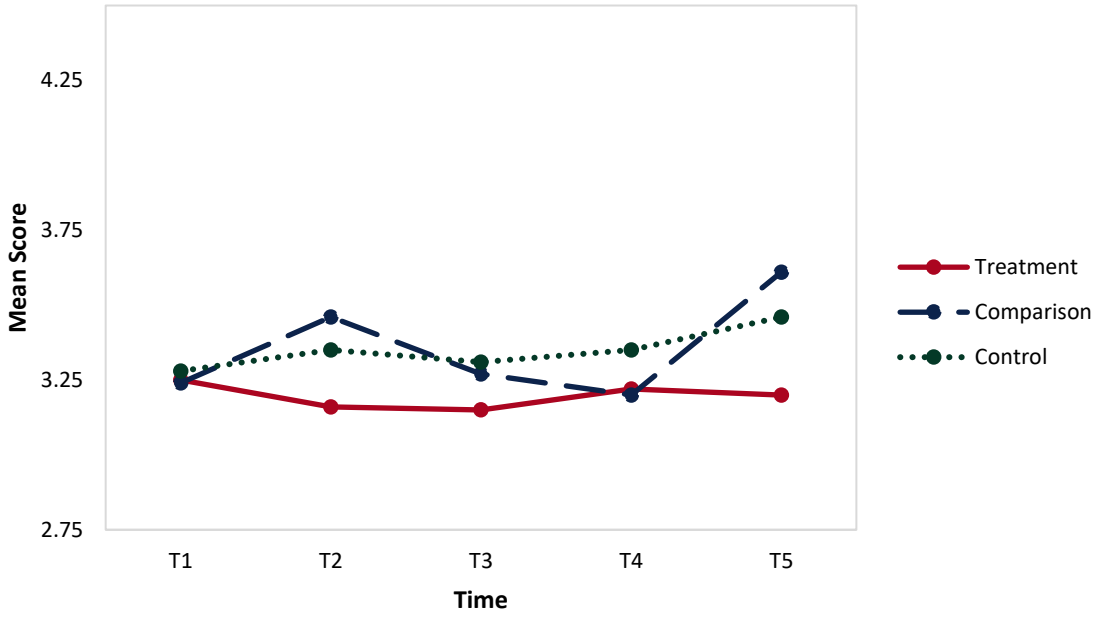


Figure 4

Mean Stress Scores by Condition Over Time

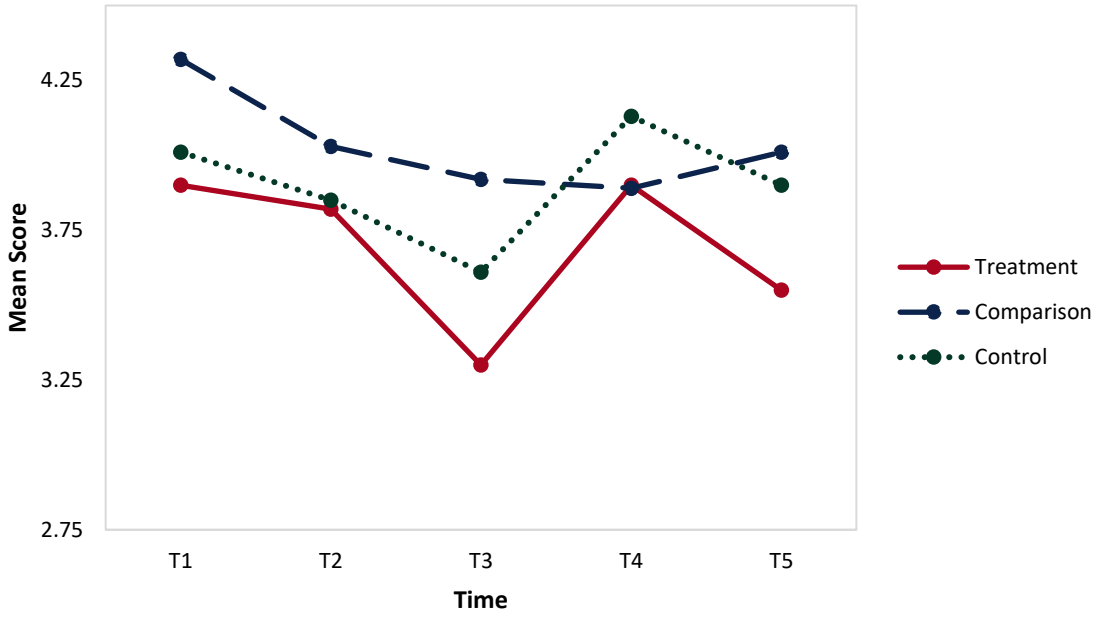
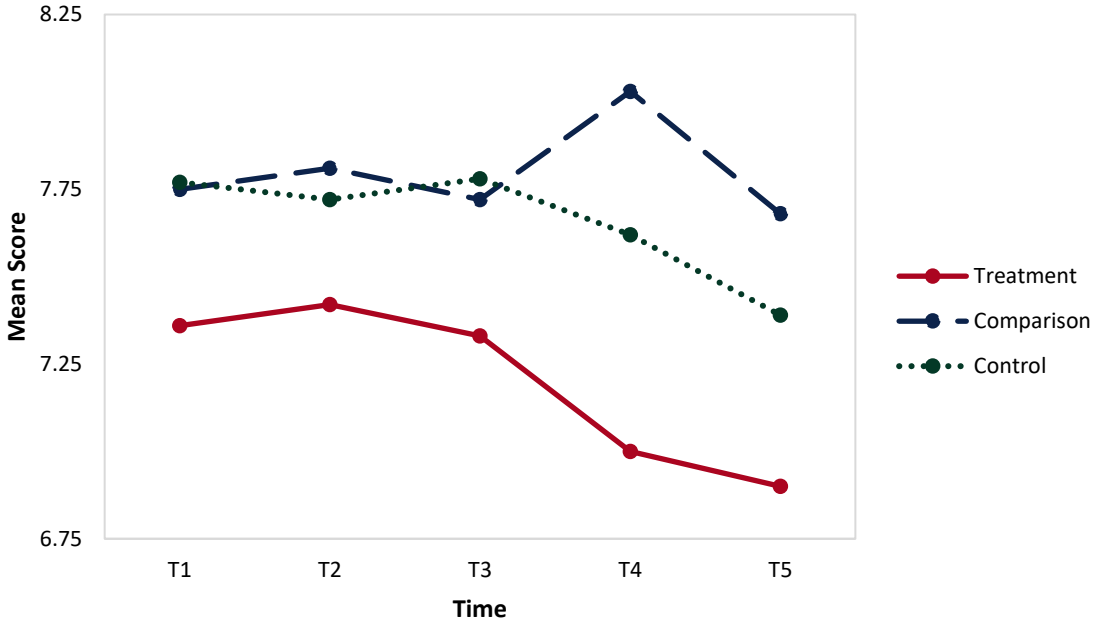


Figure 5

Mean Marital Satisfaction Scores Over Time by Condition



APPENDIX A

STUDY 1 APPROVAL

THE UNIVERSITY OF ARIZONA

Department of Communication

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P.O.Box 210025
Tucson, AZ 85721
Tel: (520) 621-1366
Fax: (520) 621-5504
www.comm.arizona.edu

Date:	August 29, 2019
Principal Investigator:	Nate Woo
Protocol Number:	19-018 COMM
Protocol Title:	Hugging study
Determination:	

Exempt 2: Use of tests, surveys, interview, or observation of public behavior.

This submission meets the criteria for exemption under 45 CFR 46.101(b).

- The University of Arizona maintains a Federalwide Assurance with the Office for Human Research Protections (FWA #00004218).
- Exempt projects do not have a continuing review requirement.
- This project should be conducted in full accordance with all applicable sections of the IRB Investigators Manual and you should notify the IRB designee immediately of any proposed changes that affect risk risk level the protocol.
- Amendments to exempt projects that change the nature of the project should be submitted to the Human Subjects Protection Program (HSPP) for a new determination. See the Investigator Manual, 'Appendix C Exemptions,' for more information on changes that affect the determination of exemption. Please contact the HSPP to consult on whether the proposed changes need further review.
- You should report any unanticipated problems involving risks to the participants or others to the IRB.
- All documents referenced in this submission have been reviewed and approved. Documents are filed with the HSPP Office. If subjects will be consented the approved consent(s) are attached to the approval notification from the HSPP Office.

UA Institutional Review Board
Member, Designated Reviewer

APPENDIX B

STUDY 1 LONGER STUDY INSTRUCTIONS

Note: Instructions were adapted from Floyd et al.'s (2009) kissing study.

Treatment Group: Increased Hugging

Good morning,

Thank you for agreeing to participate in our six-week long communication study! Below are instructions for what we would like you to do for the next six weeks.

Over the next six weeks, we would like you to **hug more often than you do now.** However often you hug others typically, we would like you to increase it by approximately 25%. It doesn't matter whom you hug or how many different people you hug in a given day, so long as you hug more often than you typically do already. It is okay if you tell people that you are participating in this study when you hug them or request a hug from them.

A "hug" is a brief embrace between two people that can occur in a variety of relationship types and in a variety of contexts. Hugs typically last between 1-5 seconds, but you might find that you hug longer than that. This is okay. The point is to hug more often than you do now. If increasing your hugging feels discomforting at any time, feel free to reach out to Nathan Woo, and remember that you can withdraw from the study at any time without penalty. Choose people to hug who are appropriate and respectful to your relationship and context.

Periodically, you will be emailed a link and asked to complete an online daily diary about who you have recently hugged. Completing this daily diary should take no more than five minutes. Remember, you will also be asked to complete an online questionnaire two more times over the next six weeks. You will be entered to win one of four $20 Amazon gift cards and receive research credit for every questionnaire you complete. If you feel as though you may not want to continue in this study after reading these instructions, you may withdraw from the study at any time without penalty.

Please ask any questions that you may have. Thanks again!

All the best
Nathan Woo

Comparison Group: Increased Verbal Greetings

Good morning,

Thank you for agreeing to participate in our six-week long communication study! Below are instructions for what we would like you to do for the next six weeks.

Over the next six weeks, we would like you to **verbally greet others more often than you do now.** However often you verbally greet others typically, we would like you to increase it by approximately 25%. It doesn't matter whom you greet or how many different people you greet in

a given day, so long as you greet more often than you typically do already. It is okay if you tell people that you are participating in this study when you greet them.

A "verbal greeting" is a brief verbal welcoming or form of acknowledgement that can occur in a variety of relationship types and in a variety of contexts. Verbal greetings typically last between 1-5 seconds, but you might find that you greet longer than that. This is okay. The point is to greet more often than you do now. If increasing your verbal greeting feels discomforting at any time, feel free to reach out to Nathan Woo, and remember that you can withdraw from the study at any time without penalty. Choose people to greet who are appropriate and respectful to your relationship and context.

Periodically, you will be emailed a link and asked to complete an online daily diary about who you have recently greeted. Completing this daily diary should take no more than five minutes. Remember, you will also be asked to complete an online questionnaire two more times over the next six weeks. You will be entered to win one of four $20 Amazon gift cards and receive research credit for every questionnaire you complete. If you feel as though you may not want to continue in this study after reading these instructions, you may withdraw from the study at any time without penalty.

Please ask any questions that you may have. Thanks again!

All the best
Nathan Woo

Control Group: No Change

Good morning,

Thank you for agreeing to participate in our six-week long communication study! Below are instructions for what we would like you to do for the next six weeks.

Over the next six weeks, we would like you to **maintain your normal routine.** You do not need to do anything different, but do let us know if you experience any major life changes during this time.

Remember, you will be asked to complete an online questionnaire two more times over the next six weeks. You will be entered to win one of four $20 Amazon gift cards and research credit for every questionnaire you complete. If you feel as though you may not want to continue in this study after reading these instructions, you may withdraw from the study at any time without penalty.

Please ask any questions that you may have. Thanks again!

All the best
Nathan Woo

APPENDIX C

IRB APPROVAL

THE UNIVERSITY OF ARIZONA

Department of Communication

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P.O. Box 210025
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www.comm.arizona.edu

Date:	August 6, 2020
Principal Investigator:	Nathan Woo
Protocol Number:	COMM 20-024
Protocol Title:	Communicating in personal relationships
Determination:	Approved

Exempt3: Research involving benign behavioral interventions

This submission meets the criteria for exemption under 45 CFR 46.104.

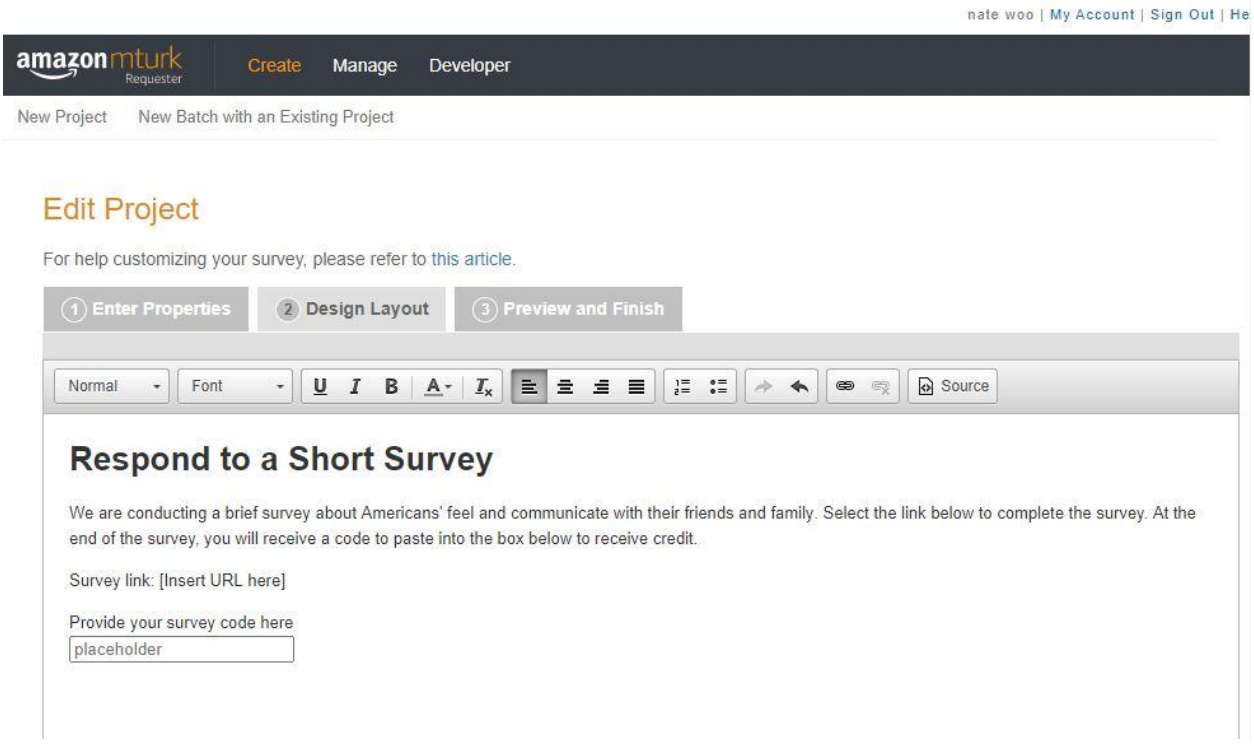
- The University of Arizona maintains a Federalwide Assurance with the Office for Human Research Protections (FWA #00004218).
- Exempt projects do not have a continuing review requirement.
- This project should be conducted in full accordance with all applicable sections of the IRB Investigators Manual and you should notify the IRB designee immediately of any proposed changes that affect risk risk level the protocol.
- Amendments to exempt projects that change the nature of the project should be submitted to the Human Subjects Protection Program (HSPP) for a new determination. See the 'Exempt Research' guidance for more information on what changes are required to be reviewed and approved by HSPP. Please contact the HSPP to consult on whether the proposed changes need further review.
- You should report any unanticipated problems involving risks to the participants or others to the IRB.
- All documents referenced in this submission have been reviewed and approved.

Patricia Sias, Ph.D.
UA Institutional Review Board
Member, Designated Reviewer

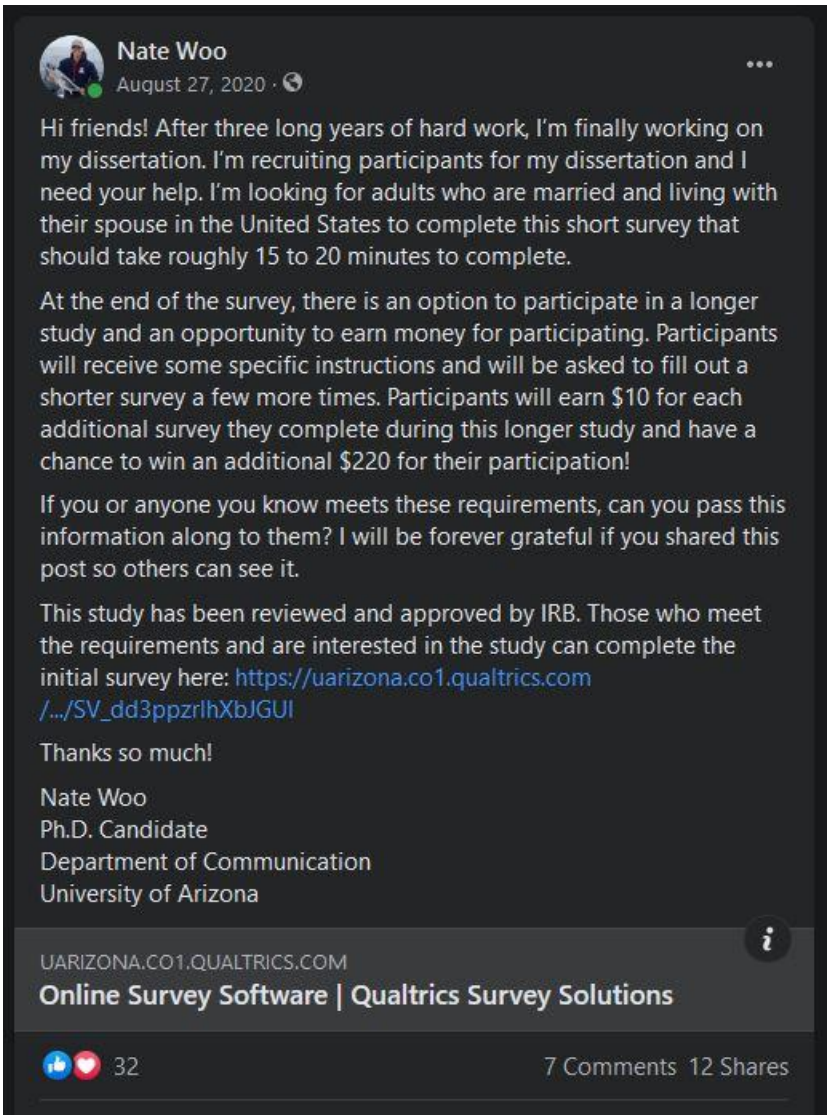
APPENDIX D

PARTICIPANT RECRUITMENT

Amazon Mechanical Turk (MTurk)



Facebook Recruitment Post



Facebook Recruitment Post Text

Hi friends! After three long years of hard work, I'm finally working on my dissertation. I'm recruiting participants for my dissertation and I need your help. I'm looking for adults who are married and living with their spouse in the United States to complete this short survey that should take roughly 15 to 20 minutes to complete.

At the end of the survey, there is an option to participate in a longer study and opportunity to earn money for participating. Participants will receive some specific instructions and will be asked to fill out a shorter survey a few more times. Participants will earn $10 for each additional survey they complete during the longer study and have a chance to win an additional $220 for their participation! If you or anyone you know meets these requirements, can you pass this information along to them? I will be forever grateful if you shared this post so others can see it.

This study has been reviewed and approved by IRB. Those who meet the requirements and are interested in the study can complete the initial survey here:
https://uarizona.co1.qualtrics.com/jfe/form/SV_dd3ppzrlhXbJGUl

Thanks so much!

Nate Woo
Ph.D. Candidate
Department of Communication
University of Arizona

APPENDIX E

INFORMED CONSENT

University of Arizona
Consent to Participate in Research

Study Title: Communicating in Personal Relationships
Principal Investigator: Nathan T Woo

You are being asked to participate in a research study. Your participation in this research study is voluntary and you do not have to participate. This document contains important information about this study and what to expect if you decide to participate. Please consider the information carefully. Feel free to ask questions before making your decision whether or not to participate.

Why is this study being done?
The purpose of this study is to examine how people communicate in their personal relationships.

What will happen if I take part in this study?
You will be asked to complete an online survey.

How long will the study last?
The survey will take approximately twenty minutes.

How many people will take part in this study?
Approximately 200 individuals will participate in the study.

Can I stop being in the study?
Your participation is voluntary. You may refuse to participate in this study. If you decide to take part in the study, you may leave the study at any time. No matter what decision you make, there will be no penalty to you and you will not lose any of your usual benefits. Your decision will not affect your future relationship with The University of Arizona. If you are a student or employee at the University of Arizona, your decision will not affect your grades or employment status.

What risks or benefits can I expect from being in the study?
Due to the personal nature of some of the items in the survey, you might reflect on your own life and friendships.

Will I be paid for taking part in this study?
In exchange for completing this survey, you will be paid $2.00. In accordance with MTurk policies, your payment may be rejected if the HIT was not completed correctly or you did not

follow instructions. MTurk Worker IDs will only be collected for the purposes of distributing compensation and will not be associated with your survey response. However, it may be possible to determine your identity if you have a publicly available Amazon profile.

This study contains a number of checks to make sure you are finishing the tasks honestly and completely. As long as you read the instructions and complete the tasks, your HIT will be approved. If you fail these checks, your HIT will be rejected.

Will my study-related information be kept confidential?
The information that you provide in the study will be handled confidentially. Your name will not be used in any report. However, there may be circumstances where this information must be released or shared as required by law. The University of Arizona Institutional Review Board may review the research records for monitoring purposes.

Any work performed on MTurk can be linked to your public profile page. Data collected from this study may be collected and used by Amazon per its privacy agreement. Your MTurk Worker ID will not be shared with anyone.

Who can answer my questions about the study?
For questions, concerns, or complaints about the study you may contact the principal investigator, Nate Woo, at nathantwoo@email.arizona.edu.

For questions about your rights as a participant in this study or to discuss other study-related concerns or complaints with someone who is not part of the research team, you may contact the Human Subjects Protection Program at 520-626-6721 or online at http://rgw.arizona.edu/compliance/human-subjects-protection-program.

Signing the consent form
I have read (or someone has read to me) this form, and I am aware that I am being asked to participate in a research study. I have had the opportunity to ask questions and have had them answered to my satisfaction. I voluntarily agree to participate in this study. I am not giving up any legal rights by signing this form.

APPENDIX F

EMAIL TO BEGIN FULL STUDY

Treatment Condition: Increased Affection

Hi [insert name],

Thank you so much for expressing interest in participating in the longer study. Over the next four weeks, we would like you to **communicate affection more often in your close relationships**. You should continue to express affection to your family and friends as you normally would, but perhaps more often and/or in different ways. It does not matter where or when you are affectionate, as long as you are being more affectionate to others than you typically are right now. It is okay if you tell people that you are participating in this study if they ask about your affectionate behavior. You will receive an email that provides useful information about affectionate communication and different ways you can express affection very soon.

Again, the purpose is to be more affectionate than you typically are with others for the next four weeks. We hope you will enjoy this part of the study and hope you will make increased affection a priority over the next four weeks.

As mentioned in the first survey you completed, you will be paid $10 for each survey you complete from here on out. Please let me know if you have any questions.

Cheers,

Nate Woo

Comparison Condition: Increased Thankfulness

Hi [insert name],

Thank you so much for expressing interest in participating in the longer study. Over the next four weeks, we would like you to **communicate thankfulness more often in your close relationships**. You should continue to express thanks to your family and friends as you normally would, but perhaps more often and/or in different ways. It does not matter where or when you are thankful, as long as you are expressing thanks to others more than you typically are right now. It is okay if you tell people that you are participating in this study if they ask about your thankful behavior.

Again, the purpose is to be more thankful than you typically are with others for the next four weeks. We hope you will enjoy this part of the study and hope you will make increased thankfulness a priority over the next four weeks.

As mentioned in the first survey you completed, you will be paid $10 for each survey you complete from here on out. Please let me know if you have any questions.

Cheers,

Nate Woo

Control Condition: No Change in Behavior

Hi [insert name],

Thank you so much for expressing interest in participating in the longer study. Over the next four weeks, we would like you to **maintain your normal routine.** You do not need to do anything different, but do let us know if you experience any major life changes during this time.

As mentioned in the first survey you completed, you will be paid $10 for each survey you complete from here on out. Please let me know if you have any questions.

Cheers,

Nate Woo

APPENDIX G

EXAMPLE TREATMENT GROUP AFFECTIONATE COMMUNICATION INDEX EMAILS

Example Email #1

Hi [name removed for privacy],

Thank you once again for participating in the longer study. As I mentioned in my previous email, I wanted to send you a quick follow-up email about affectionate communication. The first survey you completed contained a few measures on how people communicate affection in their various relationships. Based on your response, I would like to provide some information that may assist you in thinking about how to communicate affection more often in your close relationships over the next four weeks.

Overall, you tend to communicate affection to your partner more than your friends and family members. You tend to communicate affection quite evenly in terms of expressing your affection verbally (e.g., saying "I love you"), through direct nonverbal ways (e.g., hugging), and also via indirect nonverbal ways (e.g., tasks that are socially supportive in nature). You seem to favor an indirect nonverbal way of communicating affection to your close, nonromantic relationships slightly more than verbally and through direct nonverbal means.

Depending on how you feel about these various forms of affection, over the next four weeks you may want to prioritize communicating affection in ways you already like to do, try new ways of communicating affection that you typically do not do, or perhaps a combination of sorts. These are just suggestions based on your answers from the first survey. At the end of the day, please communicate affection to others **in a way that is comfortable for you!**

I hope this email was helpful. Again, please let me know if you have any questions or concerns.

Hi [name removed for privacy],

Thank you once again for participating in the longer study. As I mentioned in my previous email, I wanted to send you a quick follow-up email about affectionate communication. The first survey you completed contained a few measures on how people communicate affection in their various relationships. Based on your response, I would like to provide some information that may assist you in thinking about how to communicate affection more often in your close relationships over the next four weeks.

Overall, you tend to communicate affection to your partner more than your friends and family members. Compared to your close, nonromantic relationships, you seem to use direct nonverbal ways of communicating affection more with your partner. You tend to communicate affection verbally (e.g., saying "I love you") and via indirect nonverbal/socially supportive actions more than direct nonverbal ways (e.g., hugs). The same style appears to be consistent across both your romantic and nonromantic relationships.

Depending on how you feel about these various forms of affection, over the next four weeks you may want to prioritize communicating affection in ways you already like to do, try new ways of

communicating affection that you typically do not do, or perhaps a combination of sorts. These are just suggestions based on your answers from the first survey. At the end of the day, please communicate affection to others **in a way that is comfortable for you!**

I hope this email was helpful. Again, please let me know if you have any questions or concerns.

APPENDIX H

SURVEY MEASURES

A. Björgvinsson et al.'s (2013) Center for Epidemiological Studies Depression scale (CES-D)

INSTRUCTIONS: Thinking specifically about last week, how much do you agree or disagree with each of these statements?

1. I was bothered by things that usually don't bother me. [D]
2. I had trouble keeping my mind on what I was doing. [D]
3. I felt depressed. [D]
4. I felt like everything I did was an effort. [D]
5. I felt hopeful about the future. [D] [R]
6. I felt fearful. [D]
7. My sleep was restless. [D]
8. I was happy. [D] [R]
9. I felt lonely. [D]
10. I could not get "going." [D]

Factors: D = Depression
Scoring: [R] = Reverse scored
Scale: 9-Point Likert-Type Scale (1 = *Strongly disagree;* 9 = *Strongly agree*)

B. Neto's (2014) UCLA Loneliness 6-item Short Form (ULS-6)

INSTRUCTIONS: How much do you agree or disagree with each of these statements?

1. I lack companionship. [L]
2. I feel part of a group of friends. [L] [R]
3. I feel left out. [L]
4. I feel isolated from others. [L]
5. I am unhappy being so withdrawn. [L]
6. People are around me but not with me. [L]

Factors: L = Loneliness
Scoring: [R] = Reverse scored
Scale: 9-Point Likert-Type Scale (1 = *Strongly disagree;* 9 = *Strongly agree*)

C. Cohen et al.'s (1983) Perceived Stress Scale (PSS-10)

INSTRUCTIONS: Thinking specifically about last week, how much do you agree or disagree with each of these statements?

1. I have been upset because of something that happened unexpectedly. [PS]
2. I felt I was unable to control the important things in my life. [PS]
3. I felt nervous and "stressed." [PS]
4. I felt confident about my ability to handle personal problems. [PS] [R]
5. I often felt that things were going my way. [PS] [R]
6. I felt I could not cope with the things I had to do. [PS]

7. I have been able to control irritations in my life. [PS] [R]
8. I have felt I was "on top of things." [PS] [R]
9. I have been angered because of things that were outside of my control. [PS]
10. I have felt that difficulties were piling up so high that I could not overcome them. [PS]

Factors: PS = Perceived stress
Scoring: [R] = Reverse scored
Scale: 9-Point Likert-Type Scale (1 = *Strongly disagree;* 9 = *Strongly agree*)

D. Floyd's (2017) Affection Deprivation Scale

INSTRUCTIONS: How much do you agree or disagree with each of these statements?

1. I don't get enough affection from others. [AD]
2. I often wish I got more affection from others. [AD]
3. I wish the people in my life would hug me more often. [AD]
4. One thing I would change about my close relationships is to receive more affection. [AD]
5. I get enough affection in my life. [AD] [R]
6. I don't wish for more affection than I already get. [AD] [R]
7. Affection is something I could use more of in my life. [AD]
8. In general, I feel deprived of affection. [AD]

Factors: AD = Affection deprivation
Scoring: [R] = Reverse scored
Scale: 9-Point Likert-Type Scale (1 = *Strongly disagree;* 9 = *Strongly agree*)

E. Lyubomirsky & Lepper's (1999) Subjective Happiness Scale (SHS-4)

INSTRUCTIONS: For each of the following statements and/or questions, please mark the point on the scale that you feel is most appropriate in describing you.

1. In general, I consider myself:[SH] *Not a very happy person* (1), *A very happy person* (9)
2. Compared to most of my peers, I consider myself: [SH] *Less happy* (1), *More happy* (9)
3. Some people are generally very happy. They enjoy life regardless of what is going on, getting the most out of everything. To what extent does this characterization describe you? [SH] *Not at all* (1), *A great deal* (9)
4. Some people are generally not very happy. Although they are not depressed, they never seem as happy as they might be. To what extent does this characterization describe you? [SH] [R] *Not at all* (1), *A great deal* (9)

Factors: SH = Subjective happiness
Scoring: [R] = Reverse scored
Scale: 9-Point Likert-Type Scale

F. Schumm et al.'s (1986) Kansas Marital Satisfaction Scale (KMSS)

INSTRUCTIONS: Please rate how satisfied or dissatisfied you are with the following:

1. How satisfied are you with your marriage? [MS]
2. How satisfied are you with your partner as a spouse? [MS]
3. How satisfied are you with your relationship with your spouse? [MS]

Factors: MS = Marital satisfaction
Scale: 9-Point Likert-Type Scale (1 = *Extremely dissatisfied;* 9 = *Extremely satisfied*)

G. Floyd's (2002) Trait Affection Scales (TAS-G, TAS-R)

INSTRUCTIONS: We would like you to think about yourself. Indicate your response by selecting the appropriate number on the line preceding each item, according to the scale below.

1. I consider myself to be a very affectionate person.[G]
2. I am always telling my loved ones how much I care about them.[G]
3. When I feel affection for someone, I usually express it.[G]
4. I have a hard time telling people that I love them or care about them.[G] [R]
5. I'm not very good at expressing affection.[G] [R]
6. I'm not a very affectionate person.[G] [R]
7. I love giving people hugs or putting my arms around them.[G]
8. I don't tend to express affection to other people very much.[G] [R]
9. Anyone who knows me well would say that I'm pretty affectionate.[G]
10. Expressing affection to other people makes me uncomfortable.[G] [R]
11. People hug me quite a bit.[R]
12. People are always telling me that they like me, love me, or care about me.[R]
13. I don't get very much affection from other people.[R] [R]
14. I get quite a bit of affection from others.[R]
15. Many people I know are quite affectionate with me.[R]
16. Most of the people I know don't express affection to me very often.[R] [R]

Factors: G = Affection given, R = Affection received
Scoring: [R] = Reverse scored
Scale: 9-Point Likert-Type Scale (1 = *Strongly disagree;* 9 = *Strongly agree*)

H. Floyd & Morman's (1998) Affectionate Communication Index (ACI). Adapted specifically for affectionate communication with one's spouse.

INSTRUCTIONS: We would like you to think about how you communicate affection to your spouse. That is, how do you let your spouse know that you love him or her? To what extent would you say that you do each of the following things *as a way to express affection to your close friends and other family members*? Indicate your response by selecting the appropriate number on the line preceding each item, according to the scale below.

1. Help my spouse with problems.[IN]
2. Say "I love you".[V]
3. Kiss on lips.[DN]
4. Acknowledge my spouse's birthday.[IN]
5. Say how important my spouse is to me.[V]
6. Hug my spouse.[DN]
7. Praise my spouse's accomplishments.[IN]
8. Wink at my spouse.[DN]
9. Say my spouse is one of my best friends.[V]
10. Hold my spouse's hand.[DN]
11. Share private information.[IN]
12. Say "I care about you".[V]
13. Kiss on cheek.[DN]
14. Give my spouse compliments.[IN]
15. Say my spouse is a good friend.[V]
16. Put my arm around my spouse.[DN]
17. Sit close to my spouse.[DN]
18. Give my spouse a massage or backrub.[DN]

Factors: V = Verbal, DN = Direct nonverbal, IN = Indirect nonverbal (socially supportive)
Scale: 9-Point Likert-Type Scale (1 = *Never or almost never do this;* 9 = *Always or almost always do this*)

I. Floyd & Morman's (1998) Affectionate Communication Index (ACI). Adapted specifically for affectionate communication with one's close friends and other family members.

INSTRUCTIONS: Now we would like you to think about how you communicate affection to your close friends and other family members. That is, how do you let them know that you love them? To what extent would you say that you do each of the following things *as a way to express affection to your close friends and other family members*? Indicate your response by selecting the appropriate number on the line preceding each item, according to the scale below.

1. Help them with problems.[IN]
2. Say "I love you".[V]
3. Kiss on lips.[DN]
4. Acknowledge their birthday.[IN]
5. Say how important they are to me.[V]
6. Hug them.[DN]
7. Praise their accomplishments.[IN]
8. Wink at them.[DN]
9. Say they are one of my best friends.[V]
10. Hold their hand.[DN]
11. Share private information.[IN]
12. Say "I care about you".[V]

13. Kiss on cheek.[DN]
14. Give them compliments.[IN]
15. Say they are a good friend or family member.[V]
16. Put my arm around them.[DN]
17. Sit close to them.[DN]
18. Give them a massage or backrub.[DN]

Factors: V = Verbal, DN = Direct nonverbal, IN = Indirect nonverbal (socially supportive)
Scale: 9-Point Likert-Type Scale (1 = *Never or almost never do this;* 9 = *Always or almost always do this*)

J. Andersen and Leibowitz's (1978) Touch Avoidance Instrument (TA-18)

INSTRUCTIONS: We are also interested in your perceptions of interpersonal touch. The items on this page are about sharing touch with your friends. Please indicate the degree to which you agree with each statement by selecting the appropriate number. Lower numbers mean you agree less, and higher numbers mean you agree more. Although some of these statements may seem repetitious, take your time and try to be as honest as possible.

1. A hug from a same-sex friend is a true sign of friendship.[S]
2. Other-sex friends enjoy it when I touch them.[O]
3. I often put my arm around friends of the same sex.[S]
4. When I see two people of the same sex hugging, it revolts me.[S] [R]
5. I like it when members of another sex touch me.[O]
6. People shouldn't be so uptight about touching persons of the same sex.[S]
7. I think it is vulgar when members of another sex touch me.[O] [R]
8. When a member of another sex touches me, I find it unpleasant.[O] [R]
9. I wish I were free to show my emotions by touching members of the same sex.[S]
10. I'd enjoy giving a massage to an other-sex friend.[O]
11. I enjoy kissing persons of the same sex.[S]
12. I like to touch friends that are the same sex as I am.[S]
13. Touching a friend of the same sex does not make me uncomfortable.[S]
14. I find it enjoyable when my date and I embrace.[O]
15. I enjoy getting a back rub from a member of another sex.[O]
16. I dislike kissing relatives of the same sex.[S] [R]
17. Intimate touching with members of another sex is pleasurable.[O]
18. I find it difficult to be touched by a member of my own sex.[O] [R]

Factors: S = Touch avoidance for same sex, O = Touch avoidance for opposite sex
Scoring: [R] = Reverse scored
Scale: 9-Point Likert-Type Scale (1 = *Strongly disagree;* 9 = *Strongly agree*)

K. Manipulation Check

INSTRUCTIONS: Thank you for participating in this study. To start, we would like to know a little bit about some of your behaviors.

Over the last two weeks, have you done each of the following behaviors more often than usual, about the same as usual, or less often than usual?

1. Cardiovascular exercise (e.g., walking, biking, swimming)
2. Spending time on social media
3. Ordering takeout
4. Consuming alcohol
5. Meditation
6. Communicating affection to others
7. Weight training
8. Socializing with others
9. Communicating thanks to others
10. Listening to music
11. Spending time in nature
12. Video conferencing with others
13. Communicating to others face-to-face
14. Communicating to others remotely (e.g., texting, talking on the phone)
15. Cooking
16. Watching TV
17. Playing video games
18. Grocery shopping
19. Reading
20. News consumption (e.g., watching, listening, reading the news)
21. Spending time with my spouse

All items were randomized and presented in a matrix.
Scale: 11-Point Likert-Type Scale (-5 = *Much less often than usual;* 5 = *Much more often than usual*)

L. Health Prescreening Questions

1. Are you currently diagnosed with depression?
    Yes
    No
2. Are you currently diagnosed with social anxiety disorder?
    Yes
    No
3. In the past six months, have you used any type of antidepressants (e.g., Prozac)?
    Yes
    No
4. In the past six months, have you used any type of anti-anxiety medications (e.g., Valium)?
    Yes
    No

M. Spouse Questions

1. Do you and your spouse live together?
   Yes
   No
2. How many years have you known your spouse? Please round your answer to the nearest whole number and select it from the dropdown list below.
   Range: 0 to 75
3. How many years have you been married to your spouse? Please round your answer to the nearest whole number and select it from the dropdown list below.
   Range: 0 to 75
4. How many years have you and your spouse lived together? Please round your answer to the nearest whole number and select it from the dropdown list below.
   Range: 0 to 75
5. What type of marital relationships are you in?
   Same-sex marriage
   Opposite-sex marriage

N. Demographic Questions

1. What is your age in years? Please select from the dropdown list below.
   Range: 18 to 100
2. Please select the ethnicity you identify with.
   Hispanic or Latino
   Not Hispanic or Latino
3. Please select the race you identify with. You may select multiple categories.
   American Indian or Alaskan Native
   Asian
   Black or African American
   Hispanic or Latino
   Native Hawaiian or Other Pacific Islander
   White
4. Please indicate the biological sex you were assigned at birth.
   Male
   Female
5. Please select the gender with which you most strongly identify.
   Man
   Woman
   Transgender
   Other

6. You have the opportunity to earn additional money beyond what you are receiving for this survey. If you are interested, your participation would take place over the next four weeks. You would receive some specific instructions and would be asked to fill out a shorter survey on a few more occasions. If you take part in the longer study, you would earn $10 for each additional survey and would also have a chance to win an additional $220 for

your participation. Your participation in the longer study is completely voluntary. Please indicate below whether you are interested.

Yes, I would like to participate in the longer study.

No, I do not wish to participate in the longer study.

O. Matching Questions

1. Please provide your first name.
2. Please provide your last name.
3. Please provide an email address you check regularly.

APPENDIX I

GROUP INTERVIEW RECRUITMENT SCRIPT

Group Interview Recruitment Script

Hi [insert name],

We are contacting you because you participated in the 4-week long study title *Communicating in Personal Relationships*. We would like to learn more about what you thought of the study and how you communicated affection in your personal relationships during this time.

If you decide to participate, you would be invited to join a focus group interview with four other people. These individuals participated in the 4-week long study just like you. We have only a few questions that guide our conversation, and we expect our conversation to last approximately one hour. The interview will be conducted via Zoom at a time that is convenient for everyone in the focus group.

You will be compensated $20.00 for your participation. Many people find these interviews personally beneficial as it gives them an opportunity to reflect upon and talk about their close relationships. Your participation is voluntary and we will maintain your privacy and confidentiality throughout the research process. We will never disclose your identity in any reports or presentations of the data.

Would you be willing to participate?

APPENDIX J

GROUP INTERVIEW INFORMED CONSENT

University of Arizona
Consent to Participate in Research

Study Title: Communicating in Personal Relationships
Principal Investigator: Nathan T. Woo

You are being asked to participate in a research study. Your participation in this research study is voluntary and you do not have to participate. This document contains important information about this study and what to expect if you decide to participate. Please consider the information carefully. Feel free to ask questions before making your decision whether or not to participate.

Why is this study being done?
The purpose of this study is to understand what participants thought of and experienced during the 4-week long study that took place during the fall of 2020. We are interested in learning about how participants communicated affection in their various relationships, both romantic and platonic, during the study. We are also interested in understanding what aspects of the longer study participants found interesting, difficult, and beneficial in their lives.

What will happen if I take part in this study?
If you choose to participate, you will be asked to partake in a focus group with four other participants who also participated in the 4-week long study. The focus group interview will be held virtually via Zoom.

How long will the study last?
The focus group interview will take approximately one hour of your time.

How many people will take part in this study?
Ten individuals will participate in the study.

Can I stop being in the study?
Your participation is voluntary. You may refuse to participate in this study. If you decide to take part in the study, you may leave the study at any time. No matter what decision you make, there will be no penalty to you and you will not lose any of your usual benefits. Your decision will not affect your future relationship with The University of Arizona. If you are a student or employee at the University of Arizona, your decision will not affect your grades or employment status.

What risks or benefits can I expect from being in the study?

There are no expected risks from participating in this study. Many people find talking about affectionate communication within meaningful relationships to be fulfilling.

Will I be paid for taking part in this study?
In exchange for your participation, you will be paid $20.00. Your payment may be rejected if you consent to participate but fail to virtually attend the one-hour Zoom focus group.

Will my study-related information be kept confidential?
The information that you provide in the study will be handled confidentially. With your permission, we would like to record the Zoom meeting interview so that we can make an accurate transcript of the entire focus group conversation. Once the transcript is complete, the recording will be permanently erased. During the transcription process, you will be assigned a pseudonym. Your name will not be in the transcript. Your name will not be used in any report. Identifiable research data will be encrypted and password protected.

There may be circumstances where this information must be released or shared as required by law. The University of Arizona Institutional Review Board may review the research records for monitoring purposes.

Who can answer my questions about the study?
For questions, concerns, or complaints about the study you may contact the principal investigator, Nate Woo, at nathantwoo@email.arizona.edu.

For questions about your rights as a participant in this study or to discuss other study-related concerns or complaints with someone who is not part of the research team, you may contact the Human Subjects Protection Program at 520-626-6721 or online at http://rgw.arizona.edu/compliance/human-subjects-protection-program.

Signing the consent form
I have read (or someone has read to me) this form, and I am aware that I am being asked to participate in a research study. I have had the opportunity to ask questions and have had them answered to my satisfaction. I voluntarily agree to participate in this study. I am not giving up any legal rights by signing this form.

APPENDIX K

GROUP INTERVIEW QUESTION POOL

Group Interview Question Pool

Note: A semi-structured interview method will be employed. Additional related questions are expected to emerge in the conversational-style interviews due to this method.

1. How did you feel about being told to increase the amount of affection you communicated to others without being told specifically what to do (e.g., to only focus on hugging more often, only kissing, etc.?)
    a. Did the lack of specificity make it easier or harder to incorporate affection in your daily routine? How so?

2. Can you tell me about how you changed the amount of affection you communicated to others at the start of the study?
    a. Follow-up: Did you start expressing affection to more people or did you express affection to fewer people more often?
    b. Follow-up: Was it through different channels (e.g., verbally, nonverbally, or socially supportive behaviors)? Did your relationship with the person you expressed affection to affect the channel you communicated affection to them?
    c. Follow-up: How did these habits and behaviors change over the course of the study?

3. Can you tell me about some of the difficulties you experienced making affectionate communication a priority during the study?

4. How did physical distancing and social interaction laws and restrictions due to the pandemic affect your participation in the study?
    a. Follow up: How would you have communicated affection more if the pandemic didn't exist?
    b. Follow-up: Would you have been able to make increased affection more of a priority during the study if the pandemic didn't exist?

5. What benefits, if any, came from being more affectionate to others during this study?

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