SOCIAL TECHNOLOGIES AS A CONTEXT FOR RELATIONSHIP PROCESSES

by

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(Under the Direction of Richard B. Slatcher)

ABSTRACT

Social psychologists have long considered how contexts influence people's thoughts, feelings, and behaviors. In line with this perspective, this dissertation considers how social technologies—broadly defined—are a context in which people initiate (Paper 1), maintain (Paper 2), and end (Paper 3) social relationships.

Paper 1 examines how people's expectations of and experiences with screen time, social interaction, and solitude may influence whether they talk to strangers in daily life. Findings from four studies (N = 1,258) revealed that people tend to forecast and experience a good mood when talking to a stranger, followed by using their smartphone, and then by sitting alone with their thoughts. However, participants ranked watching television and texting over talking to a stranger. Therefore, people may use their smartphones in the presence of strangers to escape the unpleasantness of solitude, or because they do not prioritize the benefits of social interaction.

Paper 2 examines how group conversations compare when in-person and over video chat. Findings from three studies (N = 1,066) suggest that overall video chat conversations are just as intimate and enjoyable as in-person ones, but differences emerge as more people are added to a conversation. When in-person, group conversations were less intimate and more enjoyable than dyadic ones. But when over video chat, group conversations were even less intimate and just as

enjoyable or less enjoyable. Thus, adding more people to a conversation seems to alter relationship processes differently across contexts.

Finally, Paper 3 examines the phenomenon of ghosting—the act of ending a relationship by ceasing communication without explanation—which has been proliferated by smartphones. Findings from three studies (N = 1,509) suggest that people with a high need for closure are slightly more likely to use ghosting to end a relationship, but they also experience a greater threat to their psychological needs when they are ghosted. Further, being ghosted resulted in lower psychological needs satisfaction than direct rejection. Together these findings suggest that being ghosted is a painful experience and that those most likely to engage in ghosting are also the ones who may be most harmed by being ghosted.

INDEX WORDS: social technology, relationships, conversation, intimacy, mood, ghosting.

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DEDICATION

In memory of my grandparents JoAnn Leckfor, Ronald Leckfor, Shirley McConnell, and Sylvester “Bud” McConnell. I am grateful for your presence at every milestone in my life—big and small—and I know you are here for this one in spirit.

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CHAPTER 1

INTRODUCTION AND LITERATURE REVIEW

Since Kurt Lewin proposed that behavior is a function of personal and environmental characteristics (Lewin, 1936), social psychologists have been interested in the many ways that people's thoughts, feelings, and behaviors are influenced by the presence of others (e.g., Asch, 1955; Milgram, 1963; Zajonc, 1965) and their cultural contexts (e.g., Gelfand et al., 2006; Markus & Kitayama, 1991). Over the last 50 years, advancements in social technologies have changed the way we communicate with others in our daily lives. From the invention of the cell phone in 1973, to the first text message sent in 1992, to the release of the iPhone in 2007—smartphones have now become ubiquitous in industrialized society. Nine in ten U.S. adults own a smartphone (Pew Research Center, 2024), and 31% report going online almost constantly (Perrin & Atske, 2021). Whereas early social psychologists were primarily interested in the influence of other people in one's immediate physical environment, today's social psychologists must also consider how people are influenced by—and relate to—close and unknown others who are omnipresent in one's smartphone, and how smartphones interfere with in-person interactions. In line with this perspective, this dissertation considers how social technologies—including smartphones and their affordances—serve as a context in which people initiate, maintain, and end social relationships.

Social Technologies Can Interfere with and Displace In-person Socializing

Smartphones are a unique type of social technology because they enable individuals access to an unlimited range of information, entertainment, and connections at any moment from

anywhere. A person waiting at a bus stop can now listen to music, check their email, text back their friend, scroll social media, and Google search a lingering question all within the span of a few minutes. With all this potential, it is no wonder that smartphones are one of the fastest adopted technologies in human history (DeGusta, 2012). This rapid adoption has made many curious about the widespread effects that smartphones may have on well-being. Current evidence suggests that there is a small, negative association between one's amount of technology use and well-being (Orben & Przybylski, 2019), and some technology use can be better for well-being compared to heavy use or no use (Przybylski & Weinstein, 2017; Twenge, Joiner, et al., 2018; Twenge, Martin, et al., 2018; Twenge & Campbell, 2019).

However, the amount of time spent using smartphones only provides a small glimpse at the overall picture, and researchers have turned their focus to how people use these devices, and what activities these devices are replacing. Although smartphones provide information, entertainment, and connection—which should positively impact well-being—they may also impose subtle costs on well-being by interfering with ongoing in-person conversations, or by displacing these conversations altogether (Kushlev, Dwyer, et al., 2019; Kushlev & Leitao, 2020; Sbarra et al., 2019; Waytz & Gray, 2018).

Smartphones can interfere with in-person social interactions with family and friends, sometimes referred to as "phubbing" (e.g., Chotpitayasunondh & Douglas, 2016; Roberts & David, 2016) or "technoference" (e.g., McDaniel & Coyne, 2016; Sbarra et al., 2019). Early experiments have demonstrated that the mere presence of a cell phone on the table during an in-person conversation can lead to lower closeness and conversation quality (Przybylski & Weinstein, 2013), and repeated acts of phubbing within an ongoing relationship are associated with lower relationship satisfaction (Chotpitayasunondh & Douglas, 2018; Roberts & David,

2016). Importantly, using one's phone in a conversation is not only harmful to the person being ignored, but it also has negative well-being outcomes for the person using their phone. For example, parents who were told to maximize their smartphone use during a museum visit with their children felt more distracted and less socially connected than those who were told to minimize their smartphone use (Kushlev & Dunn, 2019). Similarly, groups of family members and friends who had their smartphones on the table while dining at a restaurant felt more distracted and enjoyed the dinner experience less than those who did not have access to their phones (Dwyer et al., 2018; Study 1). Finally experience-sampling studies have also found that people feel more distracted, less socially connected, and in a worser mood when using their smartphones while socializing in person (i.e., mixed episodes) compared to when they were not using their smartphones during in-person social interactions (Dwyer et al., 2018; Kushlev & Heintzelman, 2018) (c.f., Kroencke et al., 2023). Altogether, these findings provide substantial evidence that using one's smartphone during ongoing conversations can negatively influence well-being.

Another potential cost of using one's smartphone in daily life is that it may displace other more enjoyable activities, such as talking to nearby strangers. For instance, an individual checking their smartphone while waiting at a bus stop may miss out on the opportunity to strike up a casual conversation with the other waiting bus riders. Although this missed opportunity is seemingly trivial, it is still concerning, as minimal social interactions with strangers—such as greeting a shuttle bus driver (Gunaydin et al., 2021), talking to a fellow commuter (Epley & Schroeder, 2014; Schroeder et al., 2022), or chatting with one's barista (Sandstrom & Dunn, 2014)—are linked to greater feelings of social connection and well-being, and it appears that access to one's smartphone may distract from these everyday interactions with strangers. For

instance, when participants were instructed to find a building on campus, those who had access to their smartphones asked fewer people for directions and felt less socially connected than those who could not use their phones (Kushlev et al., 2017). In another study, pairs of participants who had their smartphones while sitting in a waiting room were not only less likely to talk to one another, but they also exhibited fewer genuine Duchenne smiles than those who did not have their phones (Kushlev, Hunter, et al., 2019). Altogether, these findings demonstrate that smartphones may displace in-person conversations.

Although the subtle costs of using one's smartphone during or in place of social interactions are apparent, what is less clear is *why* people may turn to their smartphones during these moments. It could be that they find the experience of sitting alone with their thoughts to be unpleasant (Buttrick et al., 2019; Wilson et al., 2014); they expect conversations with strangers to be less pleasant than they experience them to be (Epley & Schroeder, 2014; Sandstrom & Boothby, 2021; Schroeder et al., 2022); or they have different priorities that are enabled by using their smartphone, such as making everyday tasks more efficient (Kushlev et al., 2017) and regulating one's emotions during stressful situations (Hunter et al., 2018). Thus, the aim of Paper 1 is to further understand why people tend to use their smartphones when in the presence of strangers by examining people's expectations of and experiences with using their smartphones, talking to strangers, and sitting alone with their thoughts.

People Can Connect with Others Through Social Technologies

Although smartphones may interfere with or displace in-person interactions, people can build and maintain social relationships by communicating through these social technologies—often referred to as computer-mediated communication—such as by using text-based messaging (e.g., texting, instant messaging), phone call, video chat (e.g., FaceTime, Zoom), and even virtual

reality. Various theories have been proposed over the years comparing computer-mediated and in-person communication—sometimes suggesting competing conclusions. Early theories suggest that computer-mediated communication should be less likely to promote affiliative outcomes like intimacy because they "filter out" non-verbal cues such as tone of voice, facial expressions, posture, hand gestures, touch, and eye contact (Culnan & Markus, 1987), and these non-verbal cues enable people to express their emotions and interest (Argyle & Dean, 1965; Bavelas et al., 2000, 2002). In this same vein, other theorists have argued that "richer" forms of computer-mediated communication that more closely resemble in-person interactions, such as video chat, should lead to better affiliative outcomes than "leaner" forms of communication, such as texting (Biocca et al., 2003; Daft & Lengel, 1986).

In contrast to these earlier theories, Social Information Processing Theory (Walther, 1992, 1996) posits that the limited affordances of computer-mediated communication are not inherently impersonal and people can adapt to these mediums. Computer-mediated communication may even lead to *better* affiliative outcomes like intimacy because it provides an opportunity for people to make connections they would otherwise miss (e.g., finding an online community) and they can curate which aspects of themselves that they share (i.e., selective self-presentation), allowing them to reveal their "true selves" (Bargh et al., 2002; McKenna et al., 2002). However, due to the lagged time between sending and receiving information, especially for text-based communication, it may take more time for people to reach the same levels of intimacy as they would when communicating in person (Walther, 1992, 1996).

Experience-sampling studies have consistently revealed that people feel lonelier, in a worser mood, and less satisfied when engaging in computer-mediated communication than when interacting in person, and these differences are largest for text-based mediums (Achterhof et al.,

2022; Kafetsios et al., 2017; Kroencke et al., 2023; Roshanaei et al., 2024). When comparing mediums to each other, people who had a getting-acquainted interaction over text felt less close to one another when compared to those communicating via phone call, video chat, and virtual reality (Agnew et al., 2022). Experimental studies comparing affiliative outcomes between in-person and computer-mediated communication have revealed significantly lower outcomes when texting or messaging (Leckfor, 2020; Sprecher, 2014, 2021b; Sprecher & Hampton, 2017), but not when talking over phone call or video chat (Croes et al., 2019; Sprecher, 2014, 2021a).

However, everyday video chat usage changed during the COVID-19 pandemic when 81% of Americans started using video chat programs like Zoom and FaceTime to replace in-person socializing (McClain et al., 2021). This led to widespread reports of increased fatigue after video chats (i.e., "Zoom fatigue"; Wiederhold, 2020), which may have been driven by the increased cognitive load of projecting and processing non-verbal cues through video, excessive eye-gaze at a close distance, and watching one's own face during video chat conversations (Bailenson, 2021; Fauville et al., 2023; Queiroz et al., 2023; Ratan et al., 2022). Survey research has also revealed that characteristics of video chat conversations are related to greater feelings of fatigue, such as having more frequent video chats for longer periods and with more people (Queiroz et al., 2023).

Previous research examining affiliative outcomes between in-person and video chat conversations has primarily done so with dyads, but not groups. Adding more people to a conversation reduces the amount of speaking time for each person and makes turn-taking more complex, which should ultimately result in lower feelings of intimacy (Cooney et al., 2020). Group conversations should be even more difficult over video chat, where only one person can speak at a time. Thus, the aim of Paper 2 is to examine if group conversations result in lower

feelings of intimacy than dyadic ones and if this effect is even stronger over video chat than when in person.

Social Technologies Have Led to New Relationship Dissolution Strategies

Although social relationships are beneficial for psychological well-being (Diener & Seligman, 2002; Sun et al., 2020) and physical health (Cohen et al., 1997; Holt-Lunstad et al., 2010, 2015; Valtorta et al., 2016), they also bring the risk of rejection and heartache. People tend to approach social relationships through a risk regulation system where they balance the goals of seeking closeness to another person while minimizing the possibility of harm and rejection (Murray et al., 2006). When a relationship does end, it can happen through various dissolution strategies ranging from a mutual break-up to a unilateral decision by one partner, from prioritizing one's own needs to protecting their partner's feelings, and from direct confrontation to indirectly ending the relationship through withdrawal (Baxter, 1985). As relationships become more commonly initiated and maintained through social technologies like dating apps (Coduto & Fox, 2024; LeFebvre, 2018), it has also become more common for social technologies to play a role in how relationships end through the emergence of new dissolution strategies, the most common if which is *ghosting*.

Ghosting has been defined as "when one person suddenly ignores or stops communicating with another person, without telling them why" (p. 408; Kay & Courtice, 2022). Ghosting is related to concepts like ostracism and the silent treatment that involve ignoring or excluding another individual (Williams, 2009). Ostracism can occur via technology, such as when individuals are excluded while playing a virtual tossing game (Williams et al., 2000), when someone is ignored by a person checking their phone (Hales et al., 2018), or when someone suddenly stops responding during a text-message conversation (Smith & Williams, 2004).

However, ghosting is distinct from ostracism because it is used to end a relationship, whereas ostracism and the silent treatment can occur within a relationship (Freedman et al., 2019).

Ghosting has become a common method for ending a relationship among emerging adults, with 65% reporting that they have ghosted a former partner, and 72% reporting that they have been ghosted (Koessler et al., 2019a). Further, ghosting is not limited to romantic relationships, and it may be used in friendships (e.g., Freedman et al., 2019) and the workplace (e.g., Wood et al., 2023) as well. Ghosting is often mediated through technology, making technology a key aspect of this dissolution strategy that is often included in its definition (e.g., LeFebvre et al., 2019). For instance, ghosting often involves cutting off all contact with a person by not responding to phone calls or text messages, unfollowing them on social media, and even blocking their phone number or social media account (Freedman et al., 2019; Koessler et al., 2019a; LeFebvre et al., 2019; Thomas & Dubar, 2021). Because smartphones are omnipresent in daily life, the experience of being ghosted is especially frustrating for the target, as they expect the other person to have their smartphone on them most of the time. Thus, when a person is first being ghosted, they may not initially recognize what is happening and become concerned and worried about the other person's well-being. This period of uncertainty often results in the targets of ghosting experiencing worse psychological outcomes compared to ghosting initiators (Freedman et al., 2024; Koessler et al., 2019b; Powell et al., 2021), as well as compared to those who were directly rejected (Pancani et al., 2021, 2022).

Because of these negative experiences, it is important to understand the motivations behind why people choose to use ghosting to end a relationship, and who is more susceptible to being harmed by ghosting. Previous research has considered how individual factors are related to ghosting intentions and experiences, especially attachment style (i.e., anxious and avoidant

attachment; Powell et al., 2021; Quirk et al., 2016; Thomas & Dubar, 2021) and implicit theories about relationships (i.e., destiny vs. growth beliefs; Freedman et al., 2019; Powell et al., 2021), but the need for closure—or the extent to which a person desires certainty and avoids ambiguity (Kruglanski & Webster, 1996)—has not been considered. Thus, the aim of Paper 3 is to examine if the need for closure is related to a person's intention to use ghosting to end a relationship, as well as their experience after being ghosted.

The Present Research

To summarize, this dissertation examines how social technologies serve as a context in which people initiate, maintain, and end social relationships. First, Paper 1 examines how people expect to feel, and how they actually feel, when using their smartphone compared to talking to a stranger or sitting in solitude. Next, Paper 2 examines how people foster intimacy in dyadic and group conversations over video chat compared to in-person. Finally, Paper 3 examines how the need for closure is related to people's intentions to use ghosting and their response to being ghosted compared to being directly rejected.

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CHAPTER 2

EXPECTATIONS AND EXPERIENCES OF SCREEN TIME, SOCIAL INTERACTION, AND SOLITUDE [1]

[1] Leckfor, C. M., Wood, N. R., Kwiatek, S. M., and Orehek, E. 2024. *Journal of Social Psychology*. 164(6):1008-1023.

Abstract

The current research examined how people forecast and experience screen time, social interaction, and solitude. When participants could freely use their smartphone, they forecasted (Study 1) and experienced (Study 2) better mood for face-to-face conversation, but worse mood for sitting alone. When participants were instructed to engage in specific screen time activities, they forecasted (Study 3) and experienced (Study 4) the best mood after watching television; followed by conversation, texting, and browsing social media (no difference); then sitting alone. Although participants in Studies 1 and 2 ranked conversation as their most preferred activity, participants in Studies 3 and 4 ranked it below television and texting, even though conversation improved mood compared to baseline (Study 4). These findings suggest that people may use their smartphones because they enable them to escape the unpleasant experience of being alone, or because they do not recognize or prioritize the mood benefits of social interaction.

Keywords: screen time, smartphone, social interaction, solitude, mood, well-being

Introduction

Smartphones and other electronic devices represent important opportunities and challenges for well-being. Scholars have suggested that smartphones may negatively impact well-being by displacing time spent with others with more time spent alone (Kushlev & Dunn, 2015; Kushlev, Dwyer, & Dunn, 2019; Turkle, 2011; Twenge, 2017). Yet, smartphones have an undeniable draw—providing an opportunity for distraction and social connection—and account for a large proportion of people's waking hours. U.S. adults spend nearly four hours a day using their smartphone (Dolan, 2022), less than 40 minutes socializing face-to-face, and about 30 minutes relaxing and thinking[2] (U.S. Department of Labor, 2020). One reason people may use their smartphone so frequently is because they believe it will be a more positive experience than engaging in other activities, such as interacting with a stranger or being alone with their thoughts. But is smartphone use actually more enjoyable than social interaction and solitude, or do people mistakenly believe it to be so? The present research aims to examine how people forecast and experience talking to a stranger, sitting alone with their thoughts, and engaging in different types of screen time.

The Underestimated Benefits of Talking to Strangers

People have a fundamental need for social connection (Baumeister & Leary, 1995), so it is no surprise that socializing is rated one of the most enjoyable activities (Kahneman et al., 2004) and setting a goal to spend more time socializing face-to-face may be one way to improve well-being (Rohrer et al., 2018). Happier people are more sociable (Diener & Seligman, 2002; Mehl et al., 2010; Milek et al., 2018) and having more frequent social interactions is associated

[2] The reported statistics are from before the start of the COVID-19 pandemic, as are the data from the present research. However, it is important to note that early reports suggest that people are spending a greater amount of time on technology during the COVID-19 pandemic (Koeze & Popper, 2020).

with greater happiness, well-being, and belongingness—even when interacting with weak ties (Sandstrom & Dunn, 2014a; Sun et al., 2019; Watson et al., 1992). Previous research has demonstrated that even social interactions with strangers can benefit well-being. For example, coffee shop patrons randomly assigned to engage in conversation with the barista felt happier and more connected compared to those who were told to be as efficient as possible when ordering (Sandstrom & Dunn, 2014b). Similarly, saying "Have a nice day" or "Thank you" to a shuttle driver has been shown to lead to a better mood compared to not engaging with the driver (Gunaydin et al., 2021). Even being acknowledged through eye-contact or smiling from a passerby has led people to feel more connected compared to those who were ignored (Wesselmann et al., 2012).

Unfortunately, the emotional benefits of interacting with strangers are often underestimated. For example, participants in one study felt just as good after interacting with a stranger as participants who interacted with their romantic partner, even though they forecasted interacting with a stranger would be much less enjoyable (Dunn et al., 2007). In a field study of commuters, those assigned to interact with a stranger felt better than those instructed to sit with their thoughts or to commute as normal, even though other commuters expected a more positive experience in solitude (Epley & Schroeder, 2014). People may underestimate the value of conversations with strangers because of worries that the other person will not enjoy the conversation. Yet, these worries decreased after a pleasant conversation with a stranger, suggesting that they were unsubstantiated (Sandstrom & Boothby, 2021; Schroeder et al., 2021).

The Aversiveness of Solitude

Another reason people may try to avoid spending time alone with their thoughts is that they anticipate its unpleasantness. In past research, participants underestimated their intrinsic

motivation (i.e., enjoyment, engagement, interest, lack of boredom) for engaging in a waiting task, forecasting significantly lower intrinsic motivation than they actually experienced after the task (Hatano et al., 2020). Although people may underestimate the benefits of just thinking, people still find it to be unpleasant. For instance, people report feeling less happy while mind-wandering, even if their thoughts are mundane or neutral (Franklin et al., 2013; Killingsworth & Gilbert, 2010), and they feel worse after just thinking compared to engaging in mundane non-social activities (Buttrick et al., 2019; Hsee et al., 2010; Wilson et al., 2014). In fact, participants in one study found just thinking to be so aversive that many were willing to self-administer unpleasant electric shocks to distract themselves (Wilson et al., 2014). If participants are willing to use mundane or unpleasant activities as a distraction from being alone with their thoughts, it can be expected that they would be willing to turn to seemingly more desirable activities as well, such as using their smartphone.

The Draw of Smartphones

Smartphones provide a simple and convenient way for people to avoid seemingly aversive experiences, such as talking to a stranger or just thinking. One reason people may prefer to use their smartphone is because they believe it will be enjoyable. For instance, when asked how they would spend their time if they had five minutes to spare, participants forecasted that they would find it more enjoyable to use their phones or watch television than to think, even though they believed thinking would be a more worthwhile activity (Alahmadi et al., 2017). Another reason people may prefer to use their smartphone is because it enables them to regulate their emotions in both social and non-social situations (Pancani et al., 2021). For instance, previous research has shown that loneliness and social anxiety are associated with greater smartphone addiction (Bian & Leung, 2015; Enez Darcin et al., 2016), and proneness to boredom

is associated with greater problematic smartphone use (Elhai et al., 2018). Further, having access to a smartphone has been shown to prevent an increase in a stress-related hormone after being ostracized (Hunter et al., 2018), and to decrease anxiety after completing an anxiety-inducing writing task (Panova & Lleras, 2016). Together, these findings suggest that people may use their smartphone to regulate their emotions, which can have both effective and problematic (i.e., addictive) consequences.

Unfortunately, smartphones can also displace in-person interactions that are beneficial for well-being. For example, pairs of strangers in a waiting room randomly assigned to have access to their phones interacted less and exhibited fewer genuine Duchenne smiles than participants who did not have access to their phones (Kushlev, Hunter, et al., 2019). Similarly, when participants were looking for a building on campus, those randomly assigned to carry their phone asked fewer people for directions and felt less socially connected than those who did not carry their phone (Kushlev et al., 2017). Thus, people may mistakenly believe that using their smartphone is more enjoyable than other worthwhile activities, such as face-to-face social interaction or solitude.

The Present Research

Previous findings suggest that people may prefer their smartphone over solitude and talking to a stranger because they—perhaps mistakenly—expect these activities to be aversive. However, previous research has not directly compared how people expect to experience these different activities, or how they actually experience these activities. Across four studies using a within-person experimental design, the present research aims to examine people's expectations and experiences of engaging in screen time, having a face-to-face conversation with a stranger, and being alone with their thoughts. Because people tend to find solitude aversive and they

underestimate the benefits of interacting with strangers, we hypothesized that participants would *forecast* screen time as the most enjoyable, followed by talking to a stranger, and then being alone with their thoughts. However, because people often report enjoying social interactions with strangers more than expected, we hypothesized that participants would *experience* talking to a stranger as the most enjoyable, followed by screen time, and then being alone with their thoughts.

We recognize that screen time is a broad construct that can encompass work, social, and/or leisure activities ranging from passive to active engagement. To address this concern, we first used a more ecologically valid operationalization in which participants were told to use their smartphone as they desired (Studies 1 and 2), thereby allowing us to examine how people forecast and experience using their smartphone as they naturally would. We then used a narrower operationalization by instructing participants to engage in three specific screen time activities—texting, browsing social media, and watching television (Studies 3 and 4)—which allowed us to better understand how people forecast and experience specific types of non-work, smartphone-related activities. Each study was approved by the researcher's university institutional review board before data collection. Below, we report each study's sample determination, measures, manipulations, and exclusions.

Study 1: Forecasted Smartphone Use

Participants

Three-hundred and twenty-four volunteer participants were recruited from public areas on a university campus in the Northeastern United States. The sample size was confined to collection in a single semester. People were eligible to participate in the study if they were over the age of 18 years and on campus at the time the study was conducted. Six participants were

excluded for incomplete data and four were excluded because they reported being 17 years old, despite indicating that they were at least 18 years old on the consent form. The final sample included 314 (58.9% female) participants. The average age of the sample was 20.42 years (*SD* = 3.12) and ranged from 18 to 47. The sample was predominately White (69.7%), followed by 15.3% Asian, 6.7% Black, 3.2% Hispanic, 1.0% Pacific Islander, 0.3% Native American, 3.5% were of another racial/ethnic background, and 0.3% did not report their race/ethnicity. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures analysis of variance (ANOVA) with 95% power (α = .05) for each dependent variable, given our final sample size (*N* = 314). We were able to detect omnibus effects of η^2 = .01 for positive affect (*r* = .31; ε = .99) and negative affect (*r* = .34; ε = .98), and a simple effect of *d* = 0.20.

Procedure and Materials

Participants provided electronic informed consent prior to starting the study. Participants imagined that they were participating in a laboratory experiment and the experimenter asked them to engage in three different counterbalanced activities for seven minutes each: (1) "sit alone by yourself" and "just think" (instructions adapted from Wilson et al., 2014), (2) "talk with another participant of this study who you do not know" (instructions adapted from Epley & Schroeder, 2014), and (3) "use your phone however you would like." Participants were instructed that no technology could be used when sitting alone or having a conversation.[3]

After imagining each activity, participants forecasted their positive and negative affect using items from the Positive and Negative Affect Schedule (PANAS; Watson et al., 1988). The measure consisted of four items that capture positive affect (engaged, interested, happy, and excited) and four items that capture negative affect (bored, irritable, sad, and anxious).

[3] The exact instructions for the activities in Studies 1–4 are available online (https://osf.io/2kybt/) and in Appendix A.

Participants indicated the extent to which they expected to feel each emotion on a sliding scale ranging from 0 (*Not at all*) to 100 (*Extremely*). Because these factors are orthogonal dimensions of mood, rather than direct opposites of each other (Watson et al., 1988), the positive affect (α = .81–.84) and negative affect (α = .58–.67) items were averaged separately to create scale totals with higher scores indicating greater affect.[4]

After imagining all three activities, participants were asked to rank the scenarios in order of their anticipated preference from their most (first) to their least (third) preferred by dragging them into their desired order. Lower scores indicate better ranking. Finally, participants reported demographic information and were thanked for their participation.

Results

Analyses were conducted using IBM SPSS, version 25. See Table 2.1 for summarized results of reported affect for Studies 1–4. Detailed statistical results (including post hoc results) are provided in Appendix A.[5] In Study 1, a repeated measures ANOVA revealed a main effect of activity on forecasted positive affect, $F(2, 314) = 141.80$, $p < .001$, $\eta_p^2 = .31$, and negative affect, $F(1.96, 314) = 50.95$, $p < .001$, $\eta_p^2 = .14$. Pairwise comparisons with a Bonferroni correction for multiple tests revealed that participants forecasted that having a face-to-face conversation with a stranger would elicit the most positive affect, followed by using their smartphone, then sitting alone (Figure 2.1A). They also forecasted that sitting alone would elicit the most negative affect, followed by having a face-to-face conversation with a stranger and using their smartphone (no difference). Furthermore, a test of Friedman's two-way analysis of variance by ranks revealed

[4] Although not a focus of this paper, participants also reported their forecasted and experienced loneliness in Studies 1 and 2. These data were analyzed and are available online (https://osf.io/2kybt/) and in Appendix A.

[5] If Mauchly's Test of Sphericity was significant, then the assumption of sphericity was violated and a Greenhouse-Geisser correction was used. However, because Greenhouse-Geisser is a conservative correction, when its epsilon was greater than 0.75 the more liberal Huynd-Feldt correction was used (see Table A.1 in Appendix A).

that when participants ranked the activities, they forecasted preferring to have a face-to-face conversation with a stranger, followed by using their smartphone, then sitting alone, $\chi^2(2, N = 314) = 74.98$, $p < .001$. See Table 2.2 for summarized results of rank-ordered preference for Studies 1–4.

Discussion

Overall, participants forecasted that they would enjoy and prefer a face-to-face conversation with a stranger the most, followed by using their smartphone, then sitting alone. These findings do not support our hypothesis that participants would expect using their smartphone to be more enjoyable than having a face-to-face conversation with a stranger. Further, these findings contradict previous research claiming that smartphones are anticipated to be preferred to social interactions. One explanation for this finding is that participants were asked to indicate their forecasted enjoyment of these activities in succession, and to actively compare these activities so they could rank their preference. By actively weighing the costs and benefits of these different activities, participants may have given more consideration to how they have previously felt when engaging in these activities, and as such, may have remembered the benefits of talking to strangers. This could be contrasted with one's day-to-day life, where a person's decision to use their smartphone is made automatically, and other activities (like talking to strangers) are not considered.

However, participants did forecast and rank sitting alone as the least desirable activity. This finding supports our hypothesis and is in line with previous research suggesting that being alone with one's thoughts is an aversive experience. In the present context, this finding is important because it may suggest that one reason why people engage in screen time is that it allows them to avoid solitude. To test our second hypothesis, Study 2 examines how people

experience smartphone use, having a face-to-face conversation with a stranger, and sitting alone with their thoughts.

Study 2: Experienced Smartphone Use

Participants

Two-hundred and thirteen participants were recruited from a student subject pool at a Northeastern United States university. The sample size was confined to collection in a single semester. Participants were compensated with partial course credit. Three participants were excluded due to incomplete data, resulting in a final sample of 210 (70.0% female) participants. The average age of the sample was 18.67 years (SD = 1.04) and ranged from 18 to 26. The sample was predominately White (62.4%), followed by 20.7% Asian, 6.1% Hispanic, 5.6% Black, 3.8% reported another race/ethnicity, and 1.4% did not report their race/ethnicity. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures ANOVA with 95% power (α = .05) for each dependent variable, given our final sample size (N = 210). We were able to detect omnibus effects of η^2 = .01 for positive affect (r = .64; ε = .96) and negative affect (r = .61; ε = .97), and a simple effect of d = 0.25.

Materials and Procedure

Participants provided written informed consent prior to starting the laboratory study.[6] Using the instructions from Study 1, participants completed three counterbalanced activities for seven minutes each: (1) "sit alone by yourself" and "just think," (2) "talk with someone who you do not know," and (3) "use your phone however you would like." The conversation partner was

[6] Before engaging in the activities, participants completed additional measures of personality, loneliness, narcissism, and social phobia that have not been analyzed. These survey measures are available online (https://osf.io/2kybt/).

drawn from a pool of 10 research assistants who were instructed to converse with the participant as they naturally would about any topic.

After engaging in each activity, participants reported their current positive affect (α = .79–.83) and negative affect (α = .43–.60)[7] with the same measures used in Study 1. Participants ranked the activities from their most preferred (first) to their least preferred (third) using the same method as in Study 1. Finally, participants reported demographic information and were thanked for their participation.

Results

A repeated measures ANOVA revealed a main effect of activity on experienced positive affect, $F(1.91, 210) = 313.05$, $p < .001$, $\eta_p^2 = .60$, and negative affect, $F(1.95, 210) = 179.88$, $p < .001$, $\eta_p^2 = .46$. Pairwise comparisons with a Bonferroni correction for multiple tests revealed that participants experienced the most favorable affect (highest positive affect, lowest negative affect) when having a face-to-face conversation with a stranger, followed by using their smartphone, and sitting alone elicited the least favorable affect (lowest positive affect, highest negative affect; Figure 2.1B). A Friedman test of activity rankings revealed that participants preferred having a face-to-face conversation with a stranger, followed by using their smartphone, then sitting alone, $\chi^2(2, N = 210) = 190.66$, $p < .001$.

Discussion

In Study 2, participants enjoyed and preferred having a face-to-face conversation with a stranger the most, followed by using their smartphone, and then sitting alone. This finding

[7] Because internal reliability of the negative affect scale was low, further examination of the scale revealed that removing the "boredom" item would increase reliability to .57–.66. Thus, we ran all reported hypothesis tests with the "boredom" item removed and found that this item did not change the pattern or statistical significance of the results. Thus, we retained "boredom" in the negative affect composite in all reported statistics. Results with the "boredom" item removed can be found in Appendix A (Tables A.2 and A.4).

supports our hypothesis and is in line with previous research suggesting that people like social interactions with strangers and dislike being alone with their thoughts.

One advantage of Studies 1 and 2 is that participants forecasted or experienced using their smartphone as they typically would, which likely included switching between activities—such as texting, using social media, or browsing the internet. However, this study design combined activities that may have little or nothing to with each other (e.g., work, social, and/or leisure activities), which may make these findings less clear and more difficult to interpret. For instance, texting a friend may be an active and desirable smartphone activity, whereas replying to an email may be an active but aversive activity. As a result, participants may have forecasted or engaged in activities that were aversive or unpleasant, thereby making talking to a stranger a better alternative. To address this concern, we conducted two additional studies (Studies 3 and 4) that separately compared three desirable smartphone-related activities—texting, social media use, and watching television—to having a face-to-face conversation with a stranger and sitting alone. However, given the nuanced ways in which people can engage in these different types of activities, we did not make specific hypotheses as to how they would compare to each other.

Study 3: Forecasted Texting, Social Media, and Television

Participants

Two samples of participants were recruited for the third study. First, 271 volunteer participants (69% female) were recruited from public areas on a university campus in the Northeastern United States (referred to as "student sample"). The sample size was confined to collection in a single semester. Individuals were eligible to participate in the study if they were over the age of 18 years and on campus during recruitment. The average age was 20.16 years (SD = 2.00) and ranged from 18 to 33. The sample was predominately White (73.4%), followed

by 15.5% Asian, 4.8% Black, 1.5% Hispanic, 0.7% Pacific Islander, 3.3% reported another racial/ethnic background, and 0.7% did not report their race/ethnicity. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures ANOVA with 95% power (α = .05) for each dependent variable, given our final sample size (N = 271). We were able to detect omnibus effects of η^2 = .01 for positive affect (r = .41; ε = .96) and negative affect (r = .42; ε = .91), and a simple effect of d = 0.22.

To help strengthen ecological validity with a non-student sample, 356 participants (55.6% male) living in the United States were recruited online through Amazon's Mechanical Turk (MTurk; referred to as "MTurk sample"). The sample size was limited by compensatory resources and participants received $0.50 for their participation. The average age was 35.62 years (SD = 11.14) and ranged from 19 to 68. The sample was predominately White (62.6%), followed by 19.9% Asian, 7.6% Black, 4.8% Hispanic, 3.4% Native American, and 1.7% were another racial/ethnic background. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures ANOVA with 95% power (α = .05) for each dependent variable, given our final sample size (N = 356). We were able to detect omnibus effects of η^2 = .004 for positive affect (r = .66; ε = .89) and η^2 = .003 for negative affect (r = .76; ε = .88), and a simple effect of d = 0.19.

Materials and Procedure

Participants provided electronic informed consent prior to starting the study. Participants in both samples imagined that they were participating in a laboratory experiment and the experimenter asked them to engage in five different counterbalanced activities for ten minutes each: (1) "sit alone by yourself" and "just think," (2) "talk with another participant of this study who you do not know," (3) "text anybody you want," (4) "watch a television show of your

choice," and (5) "browse your social media platforms" like "Twitter, Facebook, and Instagram." In the conversation and sitting alone scenarios, participants were instructed that they were not allowed to use technology. In the texting, television, and social media scenarios, participants were instructed to not use the technology for any other purposes. In the social media scenario, participants were instructed to not use the direct messaging features to engage in a private conversation.

After imagining each activity, participants completed the same measures of positive affect (α_{student} = .82–.90; α_{MTurk} = .84–.90) and negative affect (α_{student} = .63–.74; α_{MTurk} = .81–.90) as in Studies 1 and 2. Participants ranked the activities from their most preferred (first) to their least preferred (fifth) using the same method as in Studies 1 and 2. Finally, participants reported demographic information and were thanked for their participation.

Results

A repeated measures ANOVA revealed a main effect of activity on forecasted positive affect for both the student sample, $F(3.85, 271) = 69.39$, $p < .001$, $\eta_p^2 = .20$, and the MTurk sample, $F(3.56, 356) = 67.40$, $p < .001$, $\eta_p^2 = .16$. There was also a main effect of activity on forecasted negative affect for the student sample, $F(3.65, 271) = 54.27$, $p < .001$, $\eta_p^2 = .17$, and the MTurk sample, $F(3.53, 356) = 55.00$, $p < .001$, $\eta_p^2 = .13$.

Pairwise comparisons with a Bonferroni correction for multiple tests revealed that both samples forecasted that watching television would elicit the most favorable affect (highest positive affect, lowest negative affect) and that sitting alone would elicit the least favorable affect (lowest positive affect, highest negative affect; Figures 2.2A and 2.2B). Specifically, participants in the student sample forecasted the *highest* positive affect for watching television and having a face-to-face conversation with a stranger (no difference), followed by texting and

using social media (no difference), then sitting alone. Students also forecasted the *lowest* negative affect for watching television; followed by having a face-to-face conversation with a stranger, texting, and using social media (no difference); then sitting alone. Participants in the MTurk sample forecasted the *highest* positive affect for watching television; followed by using social media, having a face-to-face conversation with a stranger, and texting (no difference); then sitting alone. MTurkers also forecasted the *lowest* negative affect for watching television; followed by using social media, texting, and having a face-to-face conversation with a stranger (no difference); then sitting alone.

Finally, results from a Friedman test revealed that watching television, texting, and using social media were ranked as the most preferred activities, and having a face-to-face conversation with a stranger and sitting alone were ranked as the least preferred by both the student sample, $\chi^2(4, N = 271) = 244.95$, $p < .001$, and the MTurk sample, $\chi^2(4, N = 356) = 322.78$, $p < .001$.

Discussion

Participants in both samples forecasted that they would enjoy watching television the most; followed by having a face-to-face conversation with a stranger, using social media, and texting; and sitting alone as the least enjoyable activity. However, when asked to rank these activities, participants forecasted to prefer watching television and texting, followed by using social media, having a face-to-face conversation with a stranger, and sitting alone as the least preferred. These findings partially support our hypothesis and previous research suggesting that people expect screen time to be more enjoyable than having a face-to-face conversation with a stranger.

As in Study 1, sitting alone was forecasted and ranked as the least desirable activity. This finding further supports our hypothesis and previous research suggesting that being alone with

one's thoughts is an aversive experience, which may lead people to engage in screen time to escape solitude. To test our second hypothesis, Study 4 examines how people *experience* different screen time activities compared to having a face-to-face conversation with a stranger and sitting alone with their thoughts.

Study 4: Experienced Texting, Social Media, and Television

Participants

One-hundred and seven participants (59.8% male) were recruited from a student subject pool at a Northeastern United States university. The sample size was confined to collection in a single semester. Participants were compensated with partial course credit. The average age was 19.32 years (SD = 1.94) and ranged from 18 to 32. The sample was predominately White (70.1%), followed by 12.1% Asian, 9.3% Black, 3.7% Hispanic, and 4.7% were another racial/ethnic background. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures ANOVA with 95% power (α = .05) for each dependent variable, given our final sample size (N = 107). We were able to detect omnibus effects of η^2 = .01 for positive affect (r = .79; ε = .92) and negative affect (r = .75; ε = .80), and a simple effect of d = 0.35.

Materials and Procedure

Participants provided written informed consent prior to starting the study. Participants entered the laboratory and completed baseline measures of positive and negative affect using the same materials as in Studies 1–3. Using the instructions from Study 3, participants completed five counterbalanced activities for seven minutes each: (1) "sit alone by yourself" and "just think," (2) "talk with another participant of this study who you do not know," (3) "text anybody you want," (4) "watch a television show of your choice" (via Netflix), and (5) "browse your

social media platforms" like "Twitter, Facebook, and Instagram." The conversation partner was a participant in a different study in our lab. Thus, each participant had a different conversation partner with whom they could converse about any topic.

After engaging in each activity, participants reported their current positive affect (α = .85–.89) and negative affect (α = .52–.69) with the same measures used in Studies 1–3 and at baseline. Participants ranked the activities from their most preferred (first) to their least preferred (fifth) using the same method as in Studies 1–3. Finally, participants reported demographic information and were thanked for their participation.

Results

A repeated measures ANOVA revealed a main effect of activity on positive affect, $F(4.59, 107) = 43.22$, $p < .001$, $\eta_p^2 = .29$, and negative affect, $F(4.02, 107) = 36.98$, $p < .001$, $\eta_p^2 = .26$.). Pairwise comparisons with a Bonferroni correction for multiple tests revealed that having a face-to-face conversation with a stranger and watching television elicited the most favorable affect (highest positive affect, lowest negative affect), and sitting alone elicited the least favorable affect (lowest positive affect, highest negative affect; Figure 2.2C). Specifically, participants reported the *highest* positive affect after having a face-to-face conversation with a stranger and watching television (no difference); followed by texting and using social media (no difference); then sitting alone. Participants also reported the *lowest* negative affect after having a face-to-face conversation with a stranger and watching television (no difference); followed by texting and using social media (no difference); then sitting alone.

When compared to baseline, having a face-to-face conversation with a stranger led to the most favorable affect by *increasing* positive affect and *decreasing* negative affect. Watching

television and texting did not change positive affect but *decreased* negative affect. Finally, using social media and sitting alone *decreased* positive affect but did not change negative affect.

In contrast to the reports of experienced affect, results from a Friedman test revealed that participants preferred watching television and texting, followed by having a face-to-face conversation with a stranger and using social media, then sitting alone, $\chi^2(4, N = 107) = 123.46$, $p < .001$.

Discussion

Participants enjoyed having a face-to-face conversation with a stranger and watching television the most, followed by texting and using social media, and then sitting alone. Further, having a face-to-face conversation with a stranger improved overall mood (increased positive affect and decreased negative affect) compared to baseline. Even after experiencing the most favorable affect while having a face-to-face conversation with a stranger, participants preferred watching television and texting over having a face-to-face conversation with a stranger and using social media, and reported sitting alone as their least preferred activity. These findings partially support our hypothesis and previous research suggesting that people enjoy having a face-to-face conversation with a stranger more than screen time. However, it seems that even when people enjoy talking to a stranger, they still may not prefer it over screen time.

As in Study 2, sitting alone was experienced and ranked as the least desirable activity and overall worsened participants' mood compared to baseline. This finding further supports our hypothesis and previous research suggesting that being alone with one's thoughts is an aversive experience that people may try to avoid by turning to their screens.

General Discussion

Although smartphone use often displaces social interaction in daily life, researchers are just beginning to explore people's expectations for and experiences of using one's smartphone compared to social interaction and solitude. On the one hand, the present research suggests that solitude is less desirable than engaging in smartphone-related activities. Participants in Studies 1 and 3 forecasted that sitting alone would be less enjoyable than using their smartphone as desired, watching television, texting, using social media, and having a face-to-face conversation with a stranger, and participants in Studies 2 and 4 experienced sitting alone as the least enjoyable of these activities. These findings are in line with previous research demonstrating that being alone with one's thoughts is an aversive experience (e.g., Wilson et al., 2014), especially when not freely chosen (Nguyen et al., 2019). In the context of daily life, this finding suggests that one reason why people may use their smartphone is because it allows them to escape the unpleasant experience of being alone with their thoughts.

On the other hand, the current findings comparing smartphone-related activities to social interaction are less clear. Contrary to our prediction, participants in Studies 1 and 3 forecasted face-to-face conversation with a stranger to be more enjoyable than smartphone-related activities, even though they also ranked texting, watching television, and using social media as preferable (Study 3). In Studies 2 and 4, participants experienced having a face-to-face conversation with a stranger as the most enjoyable activity, and it improved mood compared to baseline (Study 4). Despite this, participants in Study 4 still ranked watching television and texting as preferable to having a face-to-face conversation with a stranger. These results suggest that although face-to-face social interaction is beneficial for mood, people may not recognize or prioritize this information when actively deciding which activities they prefer.

Strengths, Limitations, and Future Directions

Across all four studies, we used a repeated measures design that allowed us to compare within-person differences in mood, as well as how people consciously rank screen time, social interaction, and solitude. This design is a strength because it provides insight into the information people may be weighing when making decisions in their daily lives. For example, our findings demonstrate that even when people report being in a better mood after having a conversation, they may not prioritize this information when actively deciding whether to use their smartphone, talk to a stranger, or sit in silence. This finding is in line with other research suggesting that people may avoid talking to strangers because they are worried that the other person would not be interested (Sandstrom & Boothby, 2021; Schroeder et al., 2021). However, the repeated measures design may also be a limitation because it may have contributed to demand characteristics by exposing participants to all conditions. We counterbalanced the conditions to help mitigate this concern, but it is possible that participants guessed the hypotheses early on and, consequently, may have responded in ways that were socially desirable or that they believed confirmed the researchers' hypotheses. Future research could address this concern by using a between-subjects design in which participants are randomly assigned to engage in screen time, social interaction, or solitude.

Another strength of this research is that we used interactions with strangers—rather than known or close others—as a conservative comparison experience. Although interactions with strangers are benign and tend to enhance well-being (Van Lange & Columbus, 2021), people tend to worry about interacting with strangers (Sandstrom & Boothby, 2021; Schroeder et al., 2021) and underestimate their value (Dunn et al., 2007; Epley & Schroeder, 2014), suggesting that people would find conversations with known or close others as *even more* enjoyable than

with strangers. However, most of our samples (aside from the MTurk sample in Study 3) were comprised of college students and the (forecasted) strangers were also college students. As a result, participants may have perceived the stranger as a similar other in a setting (college campus) where social interaction is encouraged, leading them to expect or experience the interaction as more enjoyable. To ensure the generalizability of these findings, future research could examine if people still report more enjoyment when interacting with non-similar strangers than when engaging in screen time, especially in situations where social interaction is not the norm.

Despite the strengths of the current research, these findings should be interpreted with consideration for the research's limitations. One limitation is that in the solitude condition, participants were instructed to "sit alone" and "just think." We had chosen these instructions because they have been used in previous research demonstrating that people do not like to be left alone with nothing to do but think (Wilson et al., 2014). However, by explicitly instructing participants to engage in thinking when doing so is known to be unpleasant, we may have inadvertently made the solitude condition especially unpleasant. Thus, future research interested in how people experience solitude should consider allowing participants autonomy in how they spend this time.

Another limitation of this research is the low internal reliability of the negative affect measure across the four studies (α = .43–.90), especially for Study 2 (α = .43–.60). We addressed this concern by removing one of the items from the negative affect composite in Study 2, which did not change the pattern or statistical significance of the results. In contrast, the positive affect measure had high internal reliability across the four studies (α = .79–.90), and the pattern of findings for positive and negative affect were as expected (i.e., when positive affect

was higher, negative affect was lower). We believe that despite the low internal reliability of the negative affect measure, these findings are still interpretable and have merit, especially when the results of all four studies are considered. However, future research examining participants' mood after engaging in screen time, social interaction, and/or solitude could use a more reliable negative affect measure that holistically captures the construct.

Overall, our findings suggest that people may engage in screen time in daily life because it allows them to escape the unpleasant experience of being alone with their thoughts, or because they do not recognize or prioritize the mood benefits of face-to-face social interaction. Future research could test the extensiveness of these conclusions by assessing people's motivations for using their smartphones in daily life. Further, researchers could build on these findings by examining the individual differences that may make screen time especially desirable—such as higher trait loneliness, introversion, or neuroticism—as well as the mechanisms that may lead people to engage in screen time—such as worries about talking to strangers, desire for solitude, or momentarily prioritizing other goals (e.g., to check Twitter to stay up to date on current events). It seems that increasing face-to-face social interaction in lieu of solitary or device-centered activities would be beneficial for well-being, but research should continue to investigate the benefits of face-to-face social interaction and the barriers that may keep people from reaping these benefits in daily life.

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Tables

Table 2.1

Means, Standard Deviations, and Confidence Intervals of Affect Forecasts and Experiences for Studies 1–4

	Positive Affect		Negative Affect	
Activity	*M* (*SD*)	95% CI	*M* (*SD*)	95% CI
Study 1: Forecast (*n* = 314)				
Alone	28.94 (20.99)[c]	[26.61, 31.27]	31.90 (19.27)[a]	[29.77, 34.04]
Conversation	51.81 (20.45)[a]	[49.54, 54.08]	22.20 (14.09)[b]	[20.64, 23.76]
Smartphone	32.98 (20.35)[b]	[30.72, 35.24]	22.01 (15.58)[b]	[20.28, 23.74]
Study 2: Actual Experience (*n* = 210)				
Alone	30.19 (19.68)[c]	[27.51, 32.87]	26.64 (14.10)[a]	[24.72, 28.56]
Conversation	61.35 (18.14)[a]	[58.88, 63.82]	10.48 (10.50)[c]	[9.05, 11.91]
Smartphone	41.83 (19.39)[b]	[39.19, 44.47]	19.31 (13.71)[b]	[17.44, 21.17]
Study 3: Student Forecast (*n* = 271)				
Alone	29.83 (20.51)[c]	[27.38, 32.28]	34.37 (22.12)[a]	[31.72, 37.02]
Conversation	51.83 (22.13)[a]	[49.18, 54.48]	23.53 (15.36)[b]	[21.69, 25.37]
Social Media	39.90 (20.94)[b]	[37.39, 42.40]	24.70 (18.06)[b]	[22.54, 26.86]
Texting	44.21 (24.66)[b]	[41.26, 47.16]	24.11 (18.11)[b]	[21.95, 26.28]
Television	55.14 (24.02)[a]	[52.27, 58.01]	15.52 (14.26)[c]	[13.82, 17.23]
Study 3: MTurk Forecast (*n* = 356)				
Alone	42.45 (27.73)[d]	[39.56, 45.34]	33.62 (24.16)[a]	[31.10, 36.14]
Conversation	57.58 (24.56)[bc]	[55.02, 60.14]	26.27 (22.04)[b]	[23.97, 28.56]
Social Media	58.70 (25.16)[b]	[56.07, 61.32]	21.78 (23.17)[c]	[19.36, 24.19]
Texting	55.46 (26.16)[c]	[52.73, 58.18]	24.24 (23.90)[bc]	[21.75, 26.73]
Television	64.52 (22.43)[a]	[62.18, 66.86]	18.95 (22.02)[d]	[16.66, 21.25]
Study 4: Actual Experience (*n* = 107)				
Baseline	52.25 (20.12)[bc]	[48.40, 56.12]	22.03 (14.11)[ab]	[19.32, 24.73]
Alone	37.19 (22.21)[e]	[32.94, 41.45]	23.56 (16.99)[a]	[20.31, 26.82]
Conversation	57.68 (21.51)[a]	[53.56, 61.81]	10.84 (9.96)[d]	[8.93, 12.75]
Social Media	44.22 (22.26)[d]	[39.95, 48.49]	18.77 (15.43)[bc]	[15.81, 21.73]
Texting	47.56 (23.95)[cd]	[42.97, 52.15]	16.56 (14.58)[c]	[13.77, 19.36]
Television	54.62 (22.74)[ab]	[50.27, 58.98]	11.12 (11.78)[d]	[8.86, 13.38]

Note. Values for a particular measure in a particular study with different superscripts are significantly different from one another. Superscripts are listed in decreasing value with 'a' as the highest value. All mean differences between groups are significant at the $p = .042$ level or lower. *M* = Mean, *SD* = Standard deviation, CI = Confidence Interval, MTurk = Amazon's Mechanical Turk.

Table 2.2

Means and Standard Deviations of Activity Rankings for Studies 1–4

Activity	Study 1	Study 2	Study 3		Study 4
	Forecast (n = 314)	Actual Experience (n = 210)	Student Forecast (n = 271)	MTurk Forecast (n = 356)	Actual Experience (n = 107)
Alone	2.36 (0.79)[c]	2.75 (0.54)[c]	3.76 (1.35)[d]	3.46 (1.44)[c]	4.25 (1.15)[c]
Conversation	1.68 (0.79)[a]	1.45 (0.66)[a]	3.65 (1.37)[d]	4.04 (1.25)[d]	2.98 (1.36)[b]
Smartphone	1.96 (0.72)[b]	1.80 (0.60)[b]	—	—	—
Social Media	—	—	3.07 (1.17)[c]	2.82 (1.28)[b]	3.31 (1.14)[b]
Texting	—	—	1.98 (1.12)[a]	2.50 (1.12)[ab]	2.36 (1.17)[a]
Television	—	—	2.54 (1.21)[b]	2.17 (1.10)[a]	2.10 (1.17)[a]

Note. Lower score indicates higher ranking. Values in a column with different superscripts are significantly different from one another. Superscripts are listed in increasing value with 'a' as the lowest value for that column. All mean differences between groups are significant at the p = .038 level or lower. MTurk = Amazon's Mechanical Turk.

Figures

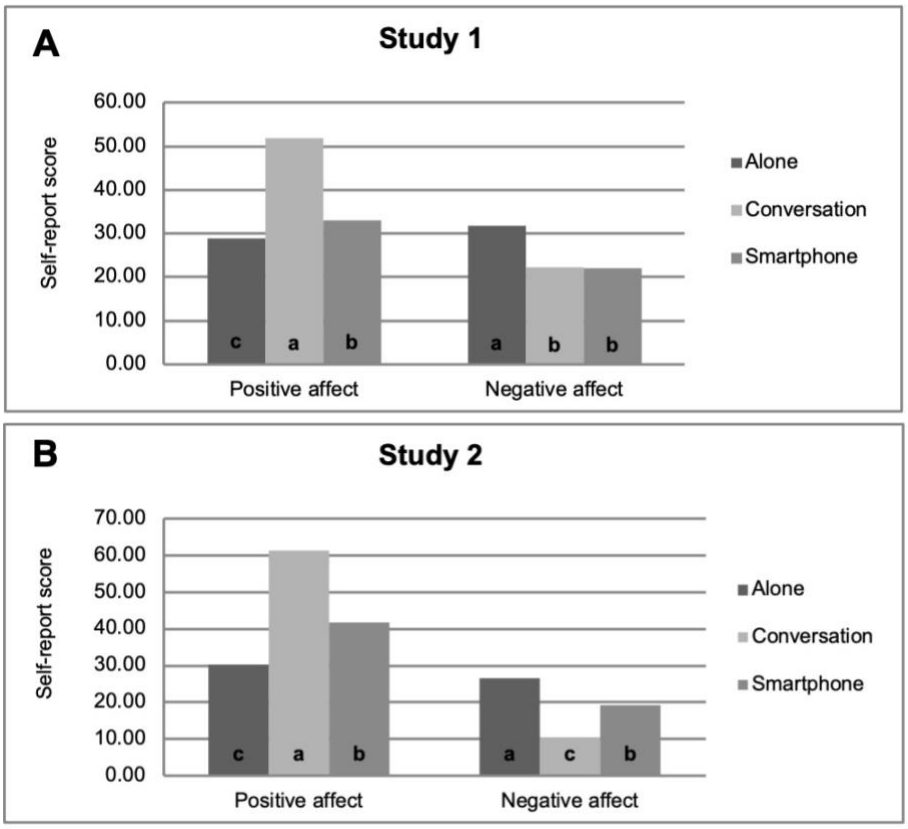

Figure 2.1. Positive and Negative Affect for Studies 1 and 2**.** Forecasted (A) and experienced (B) positive and negative affect after sitting alone, having a face-to-face conversation with a stranger, and using their smartphone. Within each measure, bars with different letters are significantly different from one another. Letters within each bar are listed in decreasing value with 'a' as the highest value. All mean differences between groups are significant at the $p = .042$ level or lower. Standard error bars depicting between-subject variability are *not* included in this graph because this data is within-subjects.

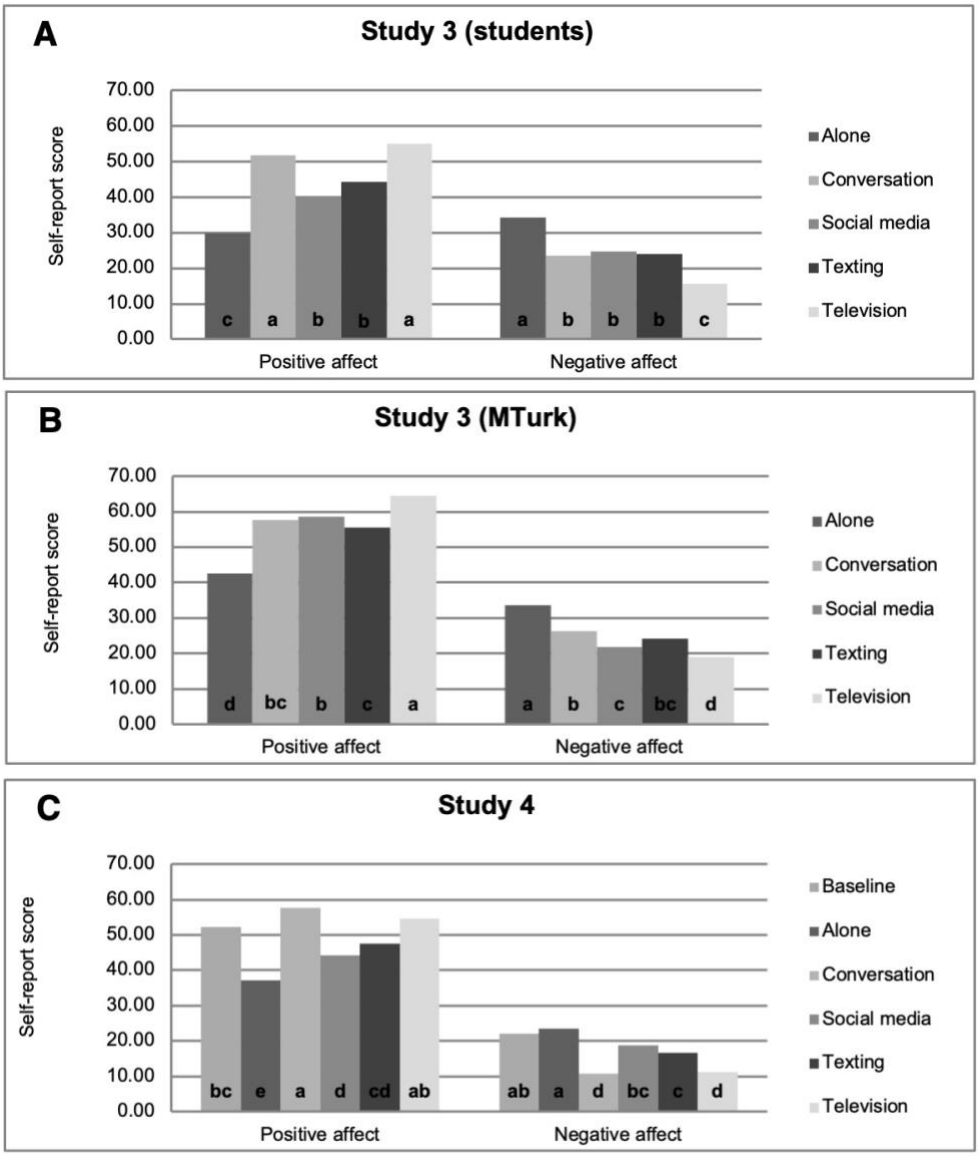

Figure 2.2. Positive and Negative Affect for Studies 3 and 4. Forecasted (A and B) and experienced (C) positive and negative affect after baseline (C only), sitting alone, having a face-to-face conversation with a stranger, using social media, texting, and watching television. Within each measure, bars with different letters are significantly different from one another. Letters within the bars are listed in decreasing value with 'a' as the highest value. All mean differences between groups are significant at the $p = .042$ level. Standard error bars depicting between-subject variability are *not* included in this graph because this data is within-subjects. MTurk = Amazon's Mechanical Turk.

CHAPTER 3

IS MORE REALLY MERRIER?

EXAMINING WHETHER AND WHY THE NUMBER OF CONVERSATION PARTNERS AFFECTS INTIMACY AND ENJOYMENT IN VIDEO CHAT AND IN-PERSON INTERACTIONS [8]

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Abstract

Two people can feel closer to each other by having intimate conversation comprised of reciprocal self-disclosure and perceived responsiveness. However, this process should become more complicated in conversations with more than two people, especially when over video chat. We examined how conversation size and medium (in-person vs. video chat) influence intimacy development and enjoyment in conversations. In Study 1, an international sample of English and Spanish-speaking adults (N = 305) reported how they generally feel in dyadic and group interaction that occurred in person or over video chat. In an online experiment, U.S. adults (N = 640) wrote about a recent dyadic or group interaction that occurred in person or over video chat (randomly assigned). In an experience-sampling study, U.S. college students (N = 150) reported on in-person and video chat interactions for one week (3,117 episodes). Across studies, participants reported lower closeness in larger (group) interactions, this effect was larger for video chat (vs. in-person) conversations, and lower closeness in group conversations was partially explained by lower self-disclosure and perceived responsiveness. Outside of the influence of COVID-19, larger (group) conversations were more enjoyable when in person, but less enjoyable when over video chat. This research has implications for understanding how people can maintain social relationships through technology in an ever-more-lonely world.

Keywords: intimacy, enjoyment, conversation, groups, computer-mediated communication video chat.

Introduction

Imagine that you just entered a restaurant. As you wait for a table, you look to one side of the restaurant and see two friends sitting across from each other engaged in quiet conversation. One friend is telling a dramatic story about a complicated situation with her boss, and the other friend listens intently as she makes eye contact, shakes her head enthusiastically, and gasps in surprise. You then glance to the other side of the restaurant and see a group of five friends sitting together in a booth. A friend on the inside of the booth is telling an animated story about something funny that happened at work. The two nearest friends are engaged in the story as they face the speaker and nod their heads, but the other two friends are distracted, with one checking their phone and the other looking around the room for their server. Still, once the story comes to an end, the entire group erupts in laughter.

Although both tables appear to have a good time, these experiences *feel* different. Common sayings like "two is company, three is a crowd" and "the more the merrier" suggest that people find conversations with one person more intimate, whereas group conversations are more fun. In line with this conventional wisdom, group conversations are difficult to navigate and introduce additional risks to sharing personal information, making people less able or willing to be vulnerable in ways that foster intimacy (Cooney et al., 2020). And yet, people tend to smile and laugh more in conversations with larger groups (Mehu & Dunbar, 2008). Despite strong and repeated evidence that social interactions are related to better psychological well-being (Diener & Seligman, 2002; Sun et al., 2020), physical health (Cohen, 2021; Cohen et al., 1997; Valtorta et al., 2016), and longevity (Holt-Lunstad et al., 2010, 2015), very little is known about whether the number of people engaging in the conversation (i.e., conversation size) matters for these benefits to occur.

Another factor that may influence the experience of social interactions is the context in which they take place—specifically, whether they occur in person or virtually. Imagine that the earlier pair of friends are having their regular dinner nights online by video chatting while enjoying their respective meals at home. While it is nice to catch up, they are not sharing the same experience—each is eating a different meal and is in a different physical environment—and they must adapt to not making eye contact or using hand gestures, all while seeing their own faces on screen. Video chat interactions like these have become more common as people maintain long-distance relationships with romantic partners (Hammonds et al., 2020; Holmes, 2010), friends (Ruppel et al., 2018), and family (Bacigalupe & Bräuninger, 2017; Neustaedter et al., 2015).

Understanding how the characteristics of a conversation can influence both the emotional experience and the interpersonal relationship has downstream consequences for maintaining social relationships and mitigating feelings of loneliness. This is important now more than ever, as rates of loneliness are on the rise across the world (e.g., Buecker et al., 2021), leading the 21st Surgeon General of the United States to declare that Americans are in the midst of a loneliness and isolation epidemic (Office of the Surgeon General, 2023), and the United Kingdom and Japan to each appoint a Minster of Loneliness (Kawaguchi, 2021; Walker, 2018). Thus, the present work aims to examine whether two key aspects responsible for the benefits of social interactions (i.e., feelings of intimacy and enjoyment) differ across in-person and video chat conversations of different sizes.

Why Might Group Conversations Be Less Intimate?

According to the *Interpersonal Model of Intimacy* (Reis & Shaver, 1988), the pair of friends dining at the restaurant will feel closer to each other if they each get a chance to share

what is on their mind and they feel the other cares about what they have to say. Indeed, people feel closer to others when they can self-disclose their personal feelings, values, and beliefs (Altman & Taylor, 1973; Aron et al., 1997; Collins & Miller, 1994; Greene et al., 2006; Morton, 1978), and when they perceive the other as responsive to their disclosure—conveying validation, understanding, and care (Berg & Archer, 1980; Laurenceau et al., 1998, 2005; Manne et al., 2004; Shelton et al., 2010; Welker et al., 2014). As the conversation continues, both friends should feel even closer if they take turns sharing and responding to one another in a reciprocal fashion (Sprecher et al., 2013; Sprecher & Treger, 2015).

Although taking turns self-disclosing and responding is a rather straightforward process for the pair of friends at the restaurant, the five friends on the other side of the restaurant should have a more difficult time navigating who will speak next and for how long. Indeed, Cooney and colleagues (2020) recently proposed that self-disclosing becomes more difficult and riskier in group conversations, thereby creating a *Many Minds Problem*. Adding more people to a conversation simultaneously increases the number of possible interactions (Bostrom, 1970) while reducing the amount of airtime available to each person (Zimet & Schneider, 1969). Speakers also receive less back-channel feedback (e.g., fewer nods and “yeah”s) in group conversations, making it difficult to tell if others are paying attention and are interested in what they are sharing (Báles et al., 1951). So not only does the group of five friends each have fewer opportunities to self-disclose, but they also receive fewer cues of responsiveness from their interaction partners. This disturbance in the intimacy-developing process may explain why group conversations are shorter and less satisfying. For example, it was found that as the number of observed pub patrons involved in a conversation increased, the length of the conversation became shorter, fewer

patrons were paying attention to the speaker, and more patrons ultimately left the conversation (Dunbar et al., 2017).

Self-disclosure has the benefit of promoting intimacy, but it also carries the risk of embarrassment and rejection (Caughlin et al., 2005; Kelly & McKillop, 1996; Murray et al., 2006). As more people join a conversation, this risk should increase (Cooney et al., 2020). For instance, the pair of friends at the restaurant can quickly calculate how much they trust each other, which dictates how much each is willing to self-disclose during the conversation. In contrast, the group of five friends may trust some friends at the table more than others, and they are likely to match their self-disclosure to the friend they trust the *least*. Indeed, people tend to self-disclose less in group conversations than in dyadic ones (Solano & Dunnam, 1985). For example, school principals who were observed throughout the day tended to share less personal information about themselves (e.g., traits, abilities, feelings, and opinions) when talking to multiple people than when talking to one other person (Gardner & Martinko, 1988). If people self-disclose less in group conversations, then theory suggests these conversations should feel less intimate (Cooney et al., 2020; Reis & Shaver, 1988). Specifically, we expect that people in group conversations will self-disclose less, perceive their interaction partners as less responsive, and feel less close to their interaction partners than people in dyadic conversations.

Why Might Group Conversations Be More Enjoyable?

Despite the potential for being less intimate, group interactions make up the fabric of our daily lives. One possible explanation for people's tendency toward group gatherings is that experiences are amplified when they are shared with others. Classic research on social facilitation and emotional contagion demonstrates that dominant responses are enhanced by the mere presence of others through increased arousal (Zajonc, 1965) and that people tend to mimic

the emotional feelings and expressions of others (Hatfield et al., 1993). Indeed, people tend to enjoy live events more when attending them with others (Brand et al., 2023). Similarly, people were more likely to enjoy good-tasting chocolate and dislike bad-tasting chocolate when consuming alongside another person compared to when alone (Boothby et al., 2014). In some cases, the shared experience of an event can be so strong that it reaches the level of collective effervescence, which is the sense of harmony and shared purpose that often occurs in group gatherings like religious services, sporting events, and concerts (Rimé & Páez, 2023). Thus, the group of five friends at the hypothetical restaurant may enjoy their night out more than the pair of friends, merely because the presence of more people amplifies the experience.

Another reason why group gatherings may be more enjoyable is because social norms dictate that they *should* be more enjoyable. People often approach interactions with strangers and acquaintances with social mindfulness, or a sense of cooperation and consideration for the needs of others (Van Doesum et al., 2013, 2020; Van Lange & Van Doesum, 2015), which makes these interactions benign (Columbus et al., 2021; Van Lange & Columbus, 2021). In group conversations, people often talk to others who they are not as close to, so they should be less likely to share personal (and perhaps more serious) information and more likely to act in positive and considerate ways. If engaging in minimal social interactions with strangers like merely saying “Hello” or “Thank you” can momentarily boost mood and feelings of social connection (Epley & Schroeder, 2014; Gunaydin et al., 2021; Leckfor et al., 2023; Sandstrom & Dunn, 2014; Schroeder et al., 2022), then group interactions comprised of pleasant small talk are likely to be enjoyable, even if they are less intimate (Aron et al., 1997). This is further illustrated by work showing that people tend to smile and laugh more in conversations with larger groups, in part to demonstrate sociability and friendliness (Mehu & Dunbar, 2008). Thus, the group of five

friends may have entered their evening with the expectation that they would have fun rather than vent their current frustrations, and so they smiled more and stuck to light-hearted stories, making the overall evening more enjoyable.

Contextual Nature of In-person Gatherings: The Case of COVID-19

The literature outlined above suggests that group conversations should be more enjoyable, but this may not extend to some contexts, such as during the COVID-19 pandemic. Not only were people worried about catching the COVID-19 virus, but in-person interactions were further complicated by public health policies aimed at reducing the viral spread, such as maintaining a six-foot distance from others (i.e., social distancing) and wearing face masks (Honein et al., 2020). Group interactions were *even more* difficult during this time because they increased the risk of viral transmission, required a larger space to maintain social distancing, and it was more difficult to indicate friendliness through smiling while wearing masks. Because some of our data were collected during the height of the COVID-19 pandemic, we initially predicted that group interactions would be *less* enjoyable, regardless of whether they took place in person or over video chat.

The Complexities of Video Chat

Another consequence of the COVID-19 pandemic was that many public health policies prohibited or restricted in-person gatherings (Abouk & Heydari, 2021), leading 81% of Americans to start using video chat programs like Zoom and FaceTime to stay connected to distanced others (McClain et al., 2021). Although nearly one-third of users believed video chats were a big help for staying connected, 83% believed that digital interactions were not as good as in-person contact, and at least a quarter of Americans felt less close to their friends and family members (McClain et al., 2021). Indeed, feelings of loneliness increased during the COVID-19

pandemic (Ernst et al., 2022). Beyond the COVID-19 pandemic, video chat remains an important way for people to maintain long-distance relationships with romantic partners (Hammonds et al., 2020; Holmes, 2010), friends (Ruppel et al., 2018), and family members (Bacigalupe & Bräuninger, 2017; Neustaedter et al., 2015). Unfortunately, video chat conversations may be less satisfying because people feel less socially present with others (Croes et al., 2016) and they have a more difficult time projecting and processing non-verbal cues (e.g., Bailenson, 2021).

Although video chatting has more affordances than text-based and voice-only communication, it is different from in-person interactions in a few important ways. During a typical video chat call with another person, one's field of vision is restricted to the other person's head and shoulders, the other person's face on the screen is often physically closer than is appropriate for in-person conversations, it is impossible to make direct eye-contact even when gazing at each other, and one can view their own face throughout the conversation. These features are believed to make video chat conversations fatiguing because it takes more cognitive effort to project and process non-verbal cues, the excessive eye gaze at a close distance feels threatening, and being able to see one's face can promote concerns about one's appearance (Bailenson, 2021; Fauville et al., 2023; Queiroz et al., 2023; Ratan et al., 2022). This should have negative downstream consequences for intimacy and enjoyment, as non-verbal cues like eye contact, head nodding, facial expressions, hand movements, and pitch are used to demonstrate a person's attention and interest during a conversation (Argyle & Dean, 1965; Bavelas et al., 2000, 2002), making them appear more responsive (Itzchakov et al., 2022). The ability to convey non-verbal cues is also susceptible to technology deficits, where a poor-quality microphone, bad camera lighting, or a lagging internet connection can make it even more difficult to get one's point across and feel heard.

Video chatting with another person is also limited by lacking the physical presence of interaction partners, with those interacting being in separate places (e.g., physically and geographically separated) and feeling less socially present (Biocca et al., 2003), which is associated with lower interpersonal attraction (Croes et al., 2016). People engaging in video chats with others may also be distracted by stimuli in their immediate environment (e.g., other people or responsibilities in their physical space), and could feel even less socially present due to increased external distractions during interactions. Indeed, this was a common concern during the COVID-19 pandemic when many people tried to integrate their work and home lives, attempting video calls while their family members, roommates, or pets were nearby. Overall, people tend to feel less socially skilled when interacting over video chat compared to in person (Queiroz et al., 2023), which could influence both their overall experience and relationship with their interaction partner(s), and this may be exacerbated when people are distracted.

Despite the limited affordances of video chatting, research comparing personal and interpersonal outcomes between video chat and in-person interactions has provided mixed evidence for the benefits or detriments of these interactions. On the one hand, people report feeling more fatigued and less socially connected when they have video chats more frequently and for longer periods (Queiroz et al., 2023). In an experiment where participants viewed each other but did not speak, dyads in the video chat condition rated each other as less attractive and less intelligent than those in the in-person condition (Fullwood, 2007). On the other hand, unacquainted dyads interacting over video chat in a laboratory setting were able to convey just as many non-verbal cues (Croes et al., 2019) and reported similar self-disclosure, closeness, and liking as those interacting in person (Sprecher, 2014, 2021). So, although video chat interactions may disrupt the natural progression of non-verbal communication and feelings of social

presence, people may adapt to these medium deficits over time (Walther, 1992), especially when interacting with one other person in a distraction-free environment.

Group Interactions Over Video Chat

Much of the experimental work comparing in-person and video chat conversations has done so with only two people. If groups have a more difficult time conversing than dyads when in-person, then they should have an *even more* difficult time over video chat (Bailenson, 2021; Cooney et al., 2020). For instance, groups conversing over video chat face the additional challenge of only one person being able to speak at a time, which limits audio back-channel feedback, makes it difficult to interject with questions or comments, and prevents individuals from being able to break-off into side conversations, thereby increasing the likelihood that two people will dominate the conversation while the rest of the group observes as an audience (Parker, 1988; Stasser & Taylor, 1991). Turn-taking also becomes more complex, as people cannot naturally signal who should speak next using their eye gaze or body language (Bohus & Horvitz, 2010; Vertegaal et al., 2001). Further, the unnatural, continuous eye-gaze at a close distance that occurs in video chats is multiplied as more people are added to a conversation, which can feel daunting to the speaker (Bailenson, 2021). Together, these added complications of group video chats should make individuals feel less comfortable self-disclosing personal information, and when they do self-disclose, it should be more difficult to rely on listeners' non-verbal cues and back-channel feedback for indications of responsiveness. As such, people tend to feel more fatigued and less socially connected during video chats with more people (Queiroz et al., 2023). Altogether, the existing theoretical and empirical evidence suggests that group conversations should be less intimate and less enjoyable when over video chat compared to in-person.

Overview of Studies

The current research sought to examine three primary aims. The first aim of this research was to examine how the number of people involved in a conversation influences feelings of intimacy and enjoyment (Aim 1). In line with the *Many Minds Problem* (Cooney et al., 2020), we expected that people would self-disclose less, perceive others as less responsive, and feel less close to others when conversing with multiple people (i.e., group) compared to one other person (i.e., dyad). Due to the contextual influences of the COVID-19 pandemic (during which some of our data were collected), we initially expected that people would report *lower* enjoyment in group conversations compared to dyadic ones. The second aim of this research was to examine how feelings of intimacy and enjoyment differ between in-person and video chat conversations (Aim 2). Because people can have a more difficult time processing non-verbal information over video chat (Bailenson, 2021) and feel less socially present (Croes et al., 2016), we expected people to report lower intimacy (i.e., self-disclosure, perceived responsiveness, and closeness) and lower enjoyment for video chat conversations compared to in-person ones. However, we acknowledge that other theories and research might suggest that we would find no differences between mediums (Croes et al., 2019; Sprecher, 2014, 2021; Walther, 1992). Next, we aimed to examine if group interactions become especially difficult over video chat (Aim 3). Due to the additional complications of having group conversations over video chat when only one person can speak at a time (Bailenson, 2021), we expected that the effect of conversation size on intimacy and enjoyment would be stronger for video chats compared to in-person, such that group (vs dyadic) video chats would be associated with lower intimacy and enjoyment.

We examined these aims in three studies with complementary research designs (total N = 1,066). In Study 1, an international sample of English- and Spanish-speaking participants

reported how intimate and enjoyable they usually felt during in-person and video chat conversations that varied in size. Importantly, this research examined people's reports during the COVID-19 pandemic, a time in which many people relied on computer-mediated communication, such as video chat, to connect with friends, family members, and romantic partners, allowing us to naturally assess the impact of engaging in dyadic interactions compared to those with multiple people. In Study 2, U.S. adults recalled a recent dyadic or group conversation that occurred either in-person or over video chat, wrote about it for three minutes, and then reported how intimate and enjoyable they found it to be. Finally, Study 3 used an experience-sampling design to examine how U.S. college students felt immediately after in-person and video chat conversations of various sizes for a one-week period. For each study, we provide the preregistration link, report our sample size determinations and exclusion criteria, and identify exclusions and analyses that deviate from or go beyond the preregistered plan.

Study 1

In Study 1, we examined how people *generally* felt during in-person and video chat conversations with friends of different sizes. Data for Study 1 were collected in May and June of 2020 (the first few months of the COVID-19 pandemic) as part of a larger, longitudinal study examining the effects of the pandemic on well-being. Variables relevant to the present investigation were only available at one time point.

We hypothesized that participants would report lower self-disclosure, perceived responsiveness, and closeness in larger video chat conversations compared to smaller ones, but we did not advance predictions for in-person conversations (Aim 1). Because these data were collected at the height of the COVID-19 pandemic, we expected that in-person group conversations would be less enjoyable than dyadic ones due to COVID-19 concerns and

regulations (e.g., social-distancing, mask-wearing), and we expected group video chats to be less enjoyable than dyadic ones due to increased complications with projecting and processing non-verbal cues and lack of social presence. Thus, we hypothesized participants would report lower enjoyment in larger conversations compared to smaller ones across in-person and online domains (Aim 1). Next, we hypothesized that participants would report lower enjoyment for video chats compared to in-person conversations (Aim 2). Finally, we hypothesized that communication medium would moderate the effect of conversation size on enjoyment, such that differences in enjoyment between conversation sizes would be larger for video chat compared to in-person (Aim 3). Our hypotheses, sample size determinations, exclusion criteria, and analyses were preregistered prior to data analysis to the OSF (https://osf.io/6w7vc/?view_only=81882d38660b4d92874b00f292d59481).

Method

Participants

Volunteer participants were recruited through online social media sites (e.g., Facebook, Instagram, Reddit), by word of mouth, and through the project website. Our sample size was determined by time constraints for each wave of the larger project. The items used in the present research were distributed in English and Spanish to 681 participants. Following our preregistered criteria, we excluded 12 participants for failing at least two out of three manipulation checks. We also excluded 292 participants who indicated that they had "never" had video chat interactions with friends since the COVID-19 pandemic began, as well as 72 participants who were missing data for our outcome variables. These exclusion criteria were not preregistered, but we believe they were necessary to ensure the quality of our data. The final sample consisted of 305 participants (M_{age} = 33.59 years, *SD* = 12.46, *Range* = 18–74; 85.25% Female, 12.13% Male,

2.62% Other) from 22 countries (65.90% were from the United States; 82.62% completed the English version of the survey).[9]

Procedure

Using a 3 (size: dyad vs. small group vs. large group) × 2 (medium: in-person vs. video chat) within-subjects factorial design, participants reported how they usually feel after "in-person social interactions" and "video-chats" with one other friend (i.e., dyad), two to three friends (i.e., small group), and four or more friends (i.e., large group).

Measures

Here, we describe measures that were used to examine the proposed hypotheses.

Outcomes. For each type of conversation, participants responded to a single item using a 7-point Likert scale (1 = *Not at all*, 7 = *Extremely*) to indicate their self-disclosure ("How much do you feel like you get a chance to say what's on your mind (i.e. share your thoughts, feelings, and opinions)...?"), perceived responsiveness ("How much do you feel like your friends really listen to you and get where you are coming from...? In other words, how responsive to you are your friends...?"), closeness ("How close do you typically feel...?"), and enjoyment ("How enjoyable have you found these...?"). Higher scores indicated greater self-disclosure, perceived responsiveness, closeness, and enjoyment, and constructs were examined separately. Single-item measures are often used in large panel surveys to limit participant fatigue (Allen et al., 2022), and they can be just as valid and reliable as multi-item measures (Ahmad et al., 2014).

[9] Due to variations across countries in legality around asking participants about race (e.g., it is illegal to ask about race in certain countries in the European Union), some participants were not asked about their racial identity. Participants who did report their racial identity (N = 233) identified as White (82.40%); Asian (6.44%); Mixed Race/Ethnicity (4.72%); Hispanic or Latino (3.43%); Black or African American (0.86%); Native American, First Nation, or Alaska Native (0.86%); and another race (1.29%).

Covariates. We identified plausible third variables to include as covariates. We preregistered secondary analyses controlling for age, sex,[10] survey date, and social distancing behavior ["Over the past 2 weeks, to what extent have you been self-isolated (staying at home, avoiding public spaces)?"; 1 = *Not at all*, 5 = *Completely*]. To ensure the robustness of our findings, we added two covariates that were not preregistered. We controlled for the extent to which participants were "worried about getting or having COVID-19" (single item; 1 = *Not at all*, 5 = *Completely*), as this could influence the experience of in-person conversations. Finally, because extraversion was related to more social participation and more positive well-being after social interactions during the COVID-19 pandemic (Kroencke, Humberg, et al., 2023), we controlled for extraversion as measured by three items (α = .67) from the extra short Big Five Inventory-2 (Soto & John, 2017a) [e.g., "I am someone who tends to be quiet" (reversed scored); 1 = *Strongly disagree*, 5 = *Strongly agree*].

Results

A sensitivity power analysis (G*Power 3; Faul et al., 2007) with 90% power (α = .05) indicated that with our final sample size (N = 305), we should be able to detect a small effect of conversation size (η_p^2 = .004 – .005), conversation medium (η_p^2 = .004 – .005), and their interaction (η_p^2 = .003) for each dependent variable. Data were cleaned and analyzed in R version 4.0.3 (R Core Team, 2020). See Table B.1 in Appendix B for descriptive statistics and correlations. Participants reported having video chat conversations with one other friend the most frequently (30% most days or every day), followed by two to three other friends (11%), and then by four or more friends (7%).

[10] We recognize that gender is a more preferred measure than biological sex, but we report it here exactly as it was asked to participants.

Following our pre-registered analytic plan, we conducted a repeated measures analysis of variance (ANOVA)[11] to test the main effects and interaction effects of conversation size and medium on self-disclosure, perceived responsiveness, closeness, and enjoyment (examined separately). Means, standard deviations, and test statistics are reported in Table 3.1.[12]

Does Intimacy and Enjoyment Differ by Conversation Size (Aim 1) and Medium (Aim 2)?

As shown in Table 3.1, the omnibus tests revealed an overall effect of conversation size for each dependent variable. Planned contrasts revealed that participants reported lower self-disclosure, perceived responsiveness, closeness, and enjoyment in large group conversations compared to small group ones (all *ps* < .001), and in small group conversations compared to dyadic ones (all *ps* < .009). These results support our hypothesis for lower enjoyment in larger conversations during the COVID-19 pandemic while providing additional evidence of lower intimacy (i.e., self-disclosure, perceived responsiveness, and closeness).

As shown in Table 3.1, the omnibus tests revealed an overall effect of medium for each dependent variable, such that participants reported lower self-disclosure, perceived responsiveness, closeness, and enjoyment in video chats compared to in-person conversations (all *ps* < .001). These results support our hypothesis for lower enjoyment in video chats while providing additional evidence for lower intimacy.

Is the Effect of Conversation Size Stronger for Video Chats? (Aim 3)

As shown in Table 3.1, the omnibus tests revealed an interaction effect for self-disclosure and perceived responsiveness, but not for closeness or enjoyment. Post hoc comparisons revealed that the effect of conversation size on self-disclosure was larger for video chats, $F(1.52, 461.08)$

[11] If Mauchly's Test of Sphericity was significant, then the assumption of sphericity was violated and the Huynh-Feldt correction was used.

[12] Means and standard deviations by conversation size and medium are depicted in Figure B.1 in Appendix B.

= 350.45, $p < .001$, $\eta_p^2 = .535$., $MD = 2.04$, than for in-person conversations, $F(1.37, 416.75) = 343.53.60$, $p < .001$, $\eta_p^2 = .531$, $MD = 1.79$. Similarly, the effect of conversation size on perceived responsiveness was larger for video chats, $F(1.44, 437.90) = 304.70$, $p < .001$, $\eta_p^2 = .501$, 1.77, than for in-person conversations, $F(1.49, 453.63) = 289.79$, $p < .001$, $\eta_p^2 = .488$, $MD = 1.57$.

As hypothesized, planned contrasts revealed significant differences in intimacy and enjoyment over video chats of various sizes. Specifically, participants reported lower self-disclosure, perceived responsiveness, closeness, and enjoyment in large group video chats compared to small group ones (all *ps* < .001), and in small group video chats compared to dyadic ones (all *ps* < .014). We found similar differences for in-person interactions, where participants also reported lower self-disclosure, perceived responsiveness, closeness, and enjoyment in large group conversations compared to small group ones (all *ps* < .001), and in small group conversations compared to dyadic ones (all *ps* < .001) except for enjoyment ($p = .588$). Thus, even though the interaction effects were significant for self-disclosure and perceived responsiveness, the fact that all post-hoc comparisons for video chat and in-person conversations were significant suggests that these interaction effects are likely too small to be meaningful. Instead, it appears that larger conversations were less intimate and enjoyable when in-person and over video chat.

Secondary Analyses with Covariates

Secondary analyses were conducted controlling for age, sex, survey date, social distancing, COVID-19 worry, and extraversion (see Table B.2 in Appendix B). Of these covariates, only COVID-19 worry and extraversion had a significant (positive) effect for each dependent variable. The main effect of conversation size remained significant for all dependent

variables, whereas the main effect of medium only remained significant for closeness. Further, the interaction effect between conversation size and medium for self-disclosure and perceived responsiveness became non-significant when including covariates.

Discussion

In Study 1, participants reported how they typically feel during in-person and video chat conversations involving one other friend, a small group of friends, and a large group of friends. In support of our hypotheses, participants reported lower self-disclosure, perceived responsiveness, closeness, and enjoyment in larger groups when over video chat, as well as in person, and this effect held when controlling for plausible third variables (e.g., COVID-19 worry, extraversion). Participants also reported lower intimacy and enjoyment when interacting over video chat compared to in person, but when controlling for plausible third variables, this effect only held for closeness.

When examining if the influence of conversation size differed by medium, we found a significant interaction effect for self-disclosure and perceived responsiveness, but not closeness or enjoyment. Still, post hoc comparisons between conversation sizes were significant when over video chat and in person, and these interaction effects did not hold when controlling for plausible third variables, suggesting that they were too small to be meaningful.

We did not find strong evidence that video chat conversations were less intimate and enjoyable than in-person conversations, nor did we find evidence that the complexities of group conversations were exacerbated over video chat. However, one possible explanation for why video chat and in-person conversations were so similar in Study 1 is that these data were collected during the height of the COVID-19 pandemic when in-person interactions were unusually complicated by worries about catching COVID-19 and public health policies around

social distancing and mask-wearing. Thus, it is possible that larger differences will emerge when the COVID-19 pandemic has less of an influence on in-person conversations. These findings are also limited by using single-item measures, cross-sectional reporting, and a within-subjects design that may have resulted in participants becoming privy to our hypotheses and directly comparing conditions. To address these concerns, Study 2 was conducted over a year after the onset of the COVID-19 pandemic and utilized a between-subjects experimental design with a recall paradigm.

Study 2

For Study 2, we hypothesized that participants would report lower self-disclosure, perceived responsiveness, closeness, and enjoyment for group conversations compared to dyadic ones (Aim 1), and in conversations over video chat compared to in-person (Aim 2). Because we found insufficient evidence of an interaction between conversation size and medium in Study 1, we did not make a specific hypothesis about this effect, but we planned to examine if the effect of conversation size differs by medium as an open research question (Aim 3).

In Study 2, we also added a new aim to examine if lower closeness and enjoyment in group conversations could be explained by the *Interpersonal Model of Intimacy* (Reis & Shaver, 1988), namely self-disclosure and perceived responsiveness (Aim 4). Specifically, we hypothesized a serial indirect effect, such that group (vs. dyadic) conversations will result in lower self-disclosure, which in turn will be associated with lower perceived responsiveness, which in turn will be associated with lower closeness and enjoyment. Our hypotheses, sample size determinations, exclusion criteria, and analyses were preregistered prior to data collection to the OSF (https://osf.io/bg9wn/?view_only=7cf9e198c42e45bdaba02173fde1f9fc).

Method

Participants

We recruited 693 U.S. adults from Prolific in November 2021 who were compensated for their participation.[13] Our sample size was determined by monetary constraints. Following our preregistered criteria, we excluded one participant for not writing something for the recall prompt, one for failing an attention check, three for failing the data quality check, and 49 for failing the manipulation check.[14] Although all participants reported having each of the four types of conversations in the previous month, 29 participants wrote about a conversation that occurred over a month prior. To ensure that participants were able to recall details about the conversation and their experience, we excluded these participants from analyses (not preregistered).[15] The final sample consisted of 611 participants (M_{age} = 31.04 years, *SD* = 11.87, *Range* = 18–68; 71.19% Female, 26.84% Male, 1.96% Other) who identified as White (71.85%), Asian (9.82%), Hispanic or Latino/a/x (6.87%), Black or African American (6.38%), Mixed Race/Ethnicity (3.27%), Native American or Alaska Native (0.49%), Native Hawaiian or other Pacific Islander (0.33%), and of another race (0.98%).

[13] Eligible participants were identified through a prescreening survey in which participants indicated whether they had each of the four types of conversations (dyadic/in-person, dyadic/video chat, group/in-person, group/video chat) in the last month. Eligible participants were then invited to complete the main survey.

[14] For the manipulation check, participants were asked to report if the conversation they described was "in-person or over video chat," and if it was "with only one other person (one-on-one) or with a group (2 or more other people)." If participants indicated the wrong response for conversation size or medium, they were excluded. A Chi-squared Test revealed that exclusions differed by condition, $\chi^2(3) = 13.36$, $p = .004$, Cramer's $V = 0.14$, such that dyadic/in-person conversations had the most exclusions ($n = 20$), followed by dyadic/video chat ($n = 15$), then group/in-person ($n = 11$), then group/video chat ($n = 3$).

[15] A Chi-squared Test revealed that exclusions based on time since the conversation differed by condition, $\chi^2(3) = 26.62$, $p < .001$, Cramer's $V = 0.21$, such that group/video chat conversations had the most exclusions ($n = 19$), followed by dyadic/video chat ($n = 7$), then group/in-person ($n = 2$), then dyadic/in-person ($n = 1$).

Procedure

Participants completed an autobiographical reliving paradigm in which they reflected on a recent social interaction and wrote about it for three minutes. Using a 2 (size: dyad vs. group) × 2 (medium: in-person vs. video chat) between-subjects factorial design, participants were randomly assigned to write about one of four types of conversations: dyadic/in-person (n = 153), dyadic/video chat (n = 151), group/in-person (n = 156), group/video chat (n = 151). Immediately after writing, participants reported on their self-disclosure, perceived responsiveness, closeness, and enjoyment during the conversation.

Measures

Here, we describe measures relevant to the current hypotheses.

Outcomes. As a measure of self-disclosure, participants indicated across three items (α = .92) the extent to which they shared their "thoughts," "feelings," and "opinions" during the conversation (1 = *Not at all*, 7 = *Completely*). Perceived responsiveness was measured with four items (α = .95) from the Perceived Responsiveness-Insensitivity Scale (Crasta et al., 2021), whereby participants indicated how much their conversation partner(s) "really listened," "seemed interested," "were understanding," and "tried to see where [they] were coming from" (1 = *Not at all*, 5 = *Completely*). Closeness was measured with a face-valid item ("How close did you feel toward the other(s) during this interaction?"; 1 = *Not at all*, 7 = *Extremely*) and the Inclusion of Other in the Self scale (Aron et al., 1992), whereby participants indicated how close they felt to their conversation partner(s) by selecting one of seven pairs of increasingly overlapping circles labeled "Self" and "Partner." Scores from these two items were averaged to create a composite for closeness (r = .88). Finally, as a measure of enjoyment, participants indicated across three items (α = .92) how much they found the conversation to be "enjoyable"

and "fun," and how much "they and the other(s) laughed" during the conversation (adapted from Sprecher et al., 2016; 1 = *Not at all*, 7 = *Extremely*).

Covariates. To ensure the robustness of our findings, we identified plausible third variables to include as covariates. We preregistered secondary analyses controlling for age, sex, and the impact of COVID-19 (if the item is correlated with a key outcome variable at $p < .10$). Specifically, participants responded to four items (examined separately) indicating the extent to which they have been "interacting with people who do not live with [them] or [their] 'quarantine pod'" (reverse scored), "wearing a mask when interacting with people outside of [their] residence/pod," "practicing social distancing (i.e., 6 feet) when interacting with people outside [their] residence/pod," and "worried about catching COVID-19" (0% = *Never*, 100% = *Always*). Further, we added two covariates that were not preregistered, but that we expected to be strongly correlated with our key outcome variables. As in Study 1, we controlled for extraversion, which was measured using 12 items ($\alpha = .67$) from the short Big Five Inventory-2 (Soto & John, 2017b) (e.g., "I am someone who is talkative"; 1 = *Disagree strongly*, 5 = *Agree strongly*). We also controlled for whether a close other (i.e., romantic partner, friend, or family member) was involved in the conversation described.

Results

A sensitivity power analysis (G*Power 3; Faul et al., 2007) with 90% power ($\alpha = .05$) indicated that with our final sample size ($N = 611$), we should be able to detect a small-to-medium effect of conversation size ($d = .26$), conversation medium ($d = .26$), and their interaction ($\eta_p^2 = .02$) for each dependent variable. Data were cleaned and analyzed in R version 4.0.3 (R Core Team, 2020). See Table B.3 in Appendix B for descriptive statistics and correlations. When examining the characteristics of participants' conversations, 59% involved a

close other (i.e., romantic partner, friend, or family member), and 45% of group conversations involved two to three other people.

We conducted an ANOVA to test the main effects and interaction effect of conversation size and medium on self-disclosure, perceived responsiveness, closeness, and enjoyment (examined separately). This is a deviation from the pre-registration, for which we indicated we would analyze our data using multiple regression. Although both methods are acceptable, we opted to report ANOVA analyses here to remain consistent with reporting in Study 1 and for easier interpretation of the main effects and interaction effects. Means, standard deviations, and test statistics are reported in Table 3.2.[16]

Does Intimacy and Enjoyment Differ by Conversation Size (Aim 1) and Medium (Aim 2)?

Results for the main effects of conversation size and medium are reported in Table 3.2. In support of our hypothesis that group interactions are less intimate, participants who wrote about a group (vs. dyadic) conversation reported significantly lower self-disclosure, perceived responsiveness, and closeness. However, contrary to our hypothesis and findings from Study 1, participants reported similar enjoyment in dyadic and group interactions. When examining the effect of conversation medium, we found partial support for our hypotheses, such that participants who wrote about a video chat (vs. in-person) conversation reported significantly lower perceived responsiveness, closeness, and enjoyment, but similar self-disclosure.

Is the Effect of Conversation Size Stronger for Video Chats? (Aim 3)

Next, we examined if the effect of conversation size differed when over video chat and in person. As shown in Table 3.2, the interaction effect was significant for each dependent variable. Post hoc comparisons (see Table 3.3) revealed that participants reported similar levels of self-

[16] Means and standard deviations by conversation size and medium are depicted in Figure B.2 in Appendix B.

disclosure and closeness between dyadic and group conversations when in person, but when over video chat, group conversations resulted in significantly lower self-disclosure and closeness than dyadic ones. For perceived responsiveness, participants reported significantly lower responsiveness in group (vs. dyadic) conversations for both mediums, but this effect was larger for video chats. For enjoyment, participants reported *greater* enjoyment in group (vs. dyadic) conversations when in person, but *lower* enjoyment when over video chat. In summary, group conversations were much less intimate than dyadic ones when over video chat than in-person, but group conversations were *more* enjoyable than dyadic ones when in-person, but *less* enjoyable when over video chat.

Secondary Analyses with Covariates

Secondary analyses were conducted controlling for age, sex, mask-wearing, social distancing, COVID-19 worry, extraversion, and the presence of a close other during the conversation (see Table B.4 in Appendix B). Isolating behavior was not correlated with the key outcome variables at $p < .10$ (see Table B.3 in Appendix B), so it was not included as a covariate.

Of the covariates, only extraversion and the presence of a close other had a significant (positive) effect for each dependent variable, and social distancing had a significant (positive) effect for enjoyment. The main effect of conversation size remained significant for self-disclosure, responsiveness, and closeness. The main effect of medium remained significant for responsiveness and enjoyment, but not closeness. Similarly, the interaction effect remained significant for self-disclosure, responsiveness, and enjoyment, but not closeness. It is likely that the effects for closeness dropped out when controlling for whether a close other was present during the conversation because the two variables were strongly correlated ($r = .50$). Indeed, an

additional analysis including all covariates except for close other revealed that the main effect of conversation size and medium on closeness, as well as their interaction, remained significant (see Table B.5 in Appendix B). Together, these findings demonstrate the results were robust, as they held when accounting for demographic variables, COVID-19-related behavior and concerns, and extraversion.

Is There a Serial Indirect Effect of Conversation Size Through Self-Disclosure and Perceived Responsiveness? (Aim 4)

Next, we examined if the *Interpersonal Model of Intimacy* (Reis & Shaver, 1988) could be used to explain *why* participants experienced lower closeness and enjoyment in group conversations. Specifically, we examined if there was a serial indirect effect of conversation size on closeness and enjoyment (examined separately) through self-disclosure and perceived responsiveness. Because there was a significant interaction effect between conversation size and medium on self-disclosure, we included medium as a moderator, allowing us to examine if a serial indirect effect exists for both in-person and video chat conversations. This is a deviation from the preregistration, as we did not initially predict a statistical interaction. However, we believe that accounting for the moderation is a more informative test.

We ran the moderated mediation analyses using PROCESS for R (version 3.5.3; Model 85; Hayes, 2017) with conversation medium moderating the paths from conversation size to self-disclosure, conversation size to perceived responsiveness, and size to closeness/enjoyment. As recommended by Yzerbyt and colleagues (2018), we report the direct effects and an index for each indirect effect drawn from 5,000 percentile bootstrapped confidence intervals. We used effects coding for the conversation size (-0.5 = *dyad*, 0.5 = *group*) and medium (-0.5 = *in-person*, 0.5 = *video chat*) variables so that direct effects could be interpreted in relation to the average

score for other variables. The direct effects of the moderated mediation models are reported in Table B.6 in Appendix B.

First, we examined the index of moderated mediation for each possible indirect effect. For closeness, the index of moderated mediation was significant for the indirect effect of self-disclosure alone, b = -0.23, SE = .11, 95% CI [-0.44, -0.03], and the serial indirect effect through self-disclosure and perceived responsiveness, b = -0.16, SE = .07, 95% CI [-0.30, -0.02], but not the indirect effect through perceived responsiveness alone, b = -0.18, SE = .09, 95% CI [-0.38, 0.002]. For enjoyment, the index of moderated mediation was significant for the indirect effect of self-disclosure alone, b = -0.19, SE = .09, 95% CI [-0.37, -0.02], and the serial indirect effect through self-disclosure and perceived responsiveness, b = -0.15, SE = .07, 95% CI [-0.29, -0.02], but not for the indirect effect through perceived responsiveness alone, b = -0.17, SE = .09, 95% CI [-0.35, 0.002]. Overall, these findings provide evidence of a serial moderated mediation effect. The direct effects of conversation size are depicted separately for in-person and video chat conversations in Figure 3.1 for closeness and in Figure 3.2 for enjoyment. The indirect effects of conversation size on closeness and enjoyment are reported in Table 3.4.

As shown in Table 3.4, the indirect effects of conversation size on closeness and enjoyment for in-person conversations were not significant through self-disclosure alone, responsiveness alone, or serially through self-disclosure and responsiveness, but they were significant for video chat conversations. In other words, participants having in-person group and dyadic conversations did not differ in their self-disclosure and responsiveness, which therefore could not explain their reports of closeness (Figure 3.1, Panel A) or enjoyment (Figure 3.2, Panel A). In contrast, participants having group video chats self-disclosed less than those in dyadic video chats, which in turn was associated with perceiving their interaction partners as less

responsive, which in turn was associated with feeling less close to them (Figure 3.1, Panel B) and finding the conversation less enjoyable (Figure 3.2, Panel B).

Discussion

In Study 2, participants recalled a recent dyadic or group conversation occurring in person or over video chat. Providing partial support for our hypotheses, participants reported lower self-disclosure, perceived responsiveness, and closeness in group conversations compared to dyadic ones, and this effect held when controlling for plausible third variables (e.g., COVID-19 worry, extraversion). Participants also reported lower responsiveness, closeness, and enjoyment in video chat conversations compared to in-person, and these effects held when controlling for plausible third variables. However, we did not find a main effect of conversation size on enjoyment, nor did we find a main effect of conversation medium on self-disclosure.

These two unexpectedly null findings are likely the result of having found strong interaction effects between conversation size and medium for each outcome variable. Indeed, group conversations were much less intimate and enjoyable than dyadic ones when over video chat, and group conversations were even *more* enjoyable than dyadic ones when in person. This finding is contrary to the results from Study 1 and our initial hypothesis that group conversations would be less enjoyable due to COVID-19 concerns and public health policies (e.g., social distancing, mask-wearing). However, these findings are in line with previous theory and research suggesting that the presence of others amplifies experiences (Boothby et al., 2014; Zajonc, 1965) and that group conversations are more likely to be pleasant due to expectations of social mindfulness (Van Doesum et al., 2013, 2020; Van Lange & Van Doesum, 2015).

Finally, results from Study 2 revealed that lower closeness and enjoyment in video chat interactions were partially explained by lower self-disclosure and perceived responsiveness.

Specifically, group video chats resulted in lower self-disclosure, which in turn was associated with lower perceived responsiveness, which in turn was associated with lower closeness and enjoyment. These findings help to provide theoretical evidence for *how* group conversations over video chat disrupt the interpersonal process of developing intimacy. However, because all outcome variables were measured concurrently, we acknowledge that these findings *cannot* provide causal evidence for these associations.

A strength of the between-subjects design of Study 2 is that it ensured participants were not directly comparing the four different types of conversations, which was a limitation of Study 1. Still, the recall design has its limitations, as participants may have had incorrect or biased recollections of what happened during the conversation and how they felt during it. To address these concerns, Study 3 used an experience-sampling design in which participants provided immediate reports of how they felt after their conversations for a week-long period.

Study 3

The aim of Study 3 was to examine how conversation size and medium influence intimacy and enjoyment in everyday interactions. Specifically, Study 3 used experience-sampling methodology (ESM) that enabled us to test between- and within-person effects. In line with the findings from Study 1 and 2, we hypothesized that participants would report lower self-disclosure, perceived responsiveness, and closeness when they have more group conversations on average (compared to others; between-person effect), as well as after having group (vs. dyadic) conversations (within-person effect; Aim 1). We also hypothesized that participants would report lower self-disclosure, perceived responsiveness, and closeness when they have more video chat conversations on average (compared to others; between-person effect), as well as after having video chat (vs. in-person) conversations (within-person effect; Aim 2). Because

we found inconsistent evidence of an interaction between conversation size and medium in Studies 1 and 2, we did not make a specific hypothesis about this effect, rather we planned to examine if the effect of conversation size differs by medium as an open research question (Aim 3). Similarly, we did not make hypotheses about enjoyment, as the findings from Study 1 suggested that group conversations resulted in *lower* enjoyment overall, but findings from Study 2 suggested that this depends on the conversation medium: *lower* for video chat, but *greater* for in-person.

In line with the *Interpersonal Model of Intimacy* (Reis & Shaver, 1988) and findings from Study 2, we expected that lower closeness after group interactions would be explained by lower self-disclosure and perceived responsiveness (Aim 4). In other words, we hypothesized a serial indirect effect of conversation size on closeness, such that group conversations will result in lower self-disclosure, which in turn will be associated with lower perceived responsiveness, and which in turn will be associated with lower closeness. Although we did not preregister a hypothesis for the serial indirect effect on enjoyment, we examined this as well. Our hypotheses, sample size determinations, exclusion criteria, and analyses were preregistered after data collection but prior to data analysis to the OSF (https://osf.io/be75u/?view_only=b317457b7b78426c983062a1ce31bc79).

Method

Participants

We recruited 181 adult undergraduate students from a large university in the Southeastern United States for introductory psychology course credit. Data were collected during March and

April of 2021.[17] Our sample size was determined by time constraints (how many participants we could recruit by the end of the semester). Following our preregistered eligibility and exclusion criteria, we excluded one participant for being under 18 years old, 15 participants for not participating in the ESM portion of the study, and 15 participants for having fewer than 15% (< 7) of eligible ESM responses (see the "Processing ESM Responses" section below). The final sample consisted of 150 participants (M_{age} = 19.46 years, *SD* = 1.17, *Range* = 18–24; 72.00% Female, 26.67% Male, 1.33% Other) who identified as White (63.33%), Asian (16.00%), Black or African American (10.00%), Mixed Race/Ethnicity (6.67%), Hispanic or Latino/a/x (2.67%), and of another race (1.33%).

Procedure

First, participants completed an intake survey that included questions about their demographic information, social life, and trait characteristics (e.g., personality). For the ESM portion of the study, participants were emailed six surveys per day for seven days (42 total surveys). ESM surveys were distributed every two hours starting at 11 a.m. and ending at 9 p.m. (participant's local time). Participants were asked to complete the survey as soon as possible, but links were available for up to two hours after being sent. Participants needed to complete at least five out of six surveys to receive course credit for a given day.

Processing ESM Responses. The exclusion criteria for the ESM responses are in line with previous research (Kroencke, Harari, et al., 2023; McCabe et al., 2012) and were preregistered prior to data analysis. We initially started with 5,427 ESM responses.[18] Following our preregistered criteria, we excluded 1,832 ESM responses for not involving an interaction,

[17] These data were collected as the COVID-19 vaccine was rolled out in the U.S. and university classes were being taught in a hybrid model with a mask-requirement. Although these data were collected before Study 2, they were analyzed after, and the results from Study 2 informed the preregistered hypotheses and analyses for Study 3.
[18] This number excludes the 25 responses from the participant who was under 18 years old.

242 responses for involving an interaction that did not occur in-person or over video chat, 44 responses that were incomplete, 71 responses that failed the attention check, 64 responses that occurred within 15 minutes of a previous response, and 3 responses that took longer than 60 minutes to complete. We then excluded 15 participants (54 responses) for having fewer than 15% (< 7) of eligible ESM responses, resulting in a final sample of 3,117 ESM responses (N = 150; M = 20.78 responses per person).

Measures

Conversation Size and Medium. Upon receiving the ESM survey, participants were asked whether they "had a spoken conversation with another person" in the last two hours. If participants selected "Yes," they were then asked over which medium the interaction took place: in-person, video chat, phone call, virtual reality, or other. In line with our research question, we created a dummy-coded variable for conversation medium with two groups (0 = *in-person*, 1 = *video chat*) and all other mediums were excluded from analyses.

Participants reported how many other people were involved in the conversation on an 11-point scale (1 = *1*, 11 = *More than 10*). We treated the conversation size variable as continuous. This is a deviation from the preregistration, as we initially planned to examine conversation size as a dichotomous variable (dyad vs. group). However, using a continuous variable allows for a more straightforward interpretation of the within-person effects. Specifically, examining the dichotomous within-person effect tells us how having a dyadic or group interaction that is *more or less common* for a given person is related to intimacy, whereas the within-person effect of the continuous variable tells us if interacting with *more people* than one usually does is related to intimacy. Thus, we report the continuous variable results here.

Outcomes. For each outcome measure, participants responded to a single-item using a 7-point scale (1 = *Not at all*, 7 = *Extremely*). After each interaction, participants indicated their self-disclosure, ("How much did you feel like you were able to say what was on your mind [i.e., share your thoughts, feelings, and opinions] to the people you were interacting with?"), perceived responsiveness, ("How much did you feel like the people you were interacting with really listened to you and got where you were coming from? In other words, how responsive to you were they?"), closeness, ("How close did you feel toward the people you were interacting with?"), and enjoyment ("How enjoyable was this interaction?"). Higher scores indicate greater self-disclosure, perceived responsiveness, closeness, and enjoyment.

Covariates. To ensure the robustness of our findings, we identified plausible third variables to include as covariates. We preregistered that we would control for weekend in our primary analyses, as the context of interactions may differ on weekdays compared to weekends. Although not preregistered, we also controlled for the presence of a close other (i.e., romantic partner, friend and/or family), as this was strongly related to our outcome variables in Study 2.

Next, we conducted secondary analyses (not preregistered) controlling for age, sex, COVID-19 worry, and extraversion. COVID-19 worry and extraversion (α = .86) were measured using the same items from Study 2.

Results

Data were cleaned and analyzed in R version 4.0.3 (R Core Team, 2020). See Table B.7 in Appendix B for descriptive statistics and correlations. A large portion of conversations (90.73%) took place in person, and they involved 2.60 other people on average (*SD* = 2.29, *Median* = 2; 43.12% were dyadic). Nearly three-fourths of conversations (72.09%) were on a

weekday, 88.45% involved a close other (i.e., romantic partner, friend, and/or family member), and 63.34% were an hour long or less.

Following our preregistration, we used the *lme4* package (Bates et al., 2015) to fit linear multilevel models (MLMs) using restricted maximum likelihood (REML) estimation to account for measurement occasions (Level 1) that were nested within participants (Level 2). We controlled for weekend (0 = *weekday*, 1 = *weekend*) and the presence of a close other (0 = *not present*, 1 = *present*) at Level 1. We included a random intercept for each person because the between-person variance of the outcomes ranged from 24% to 35% (intraclass correlation coefficients are reported in Table B.7 in Appendix B). Adding a random slope for the conversation size variable improved model fit and was retained. All Level 1 predictors were person-mean centered, and all Level 2 variables were sample-mean centered, which allowed us to disaggregate within- and between-person effects (Yaremych et al., 2023). Each outcome variable (self-disclosure, perceived responsiveness, closeness, and enjoyment) was examined separately. The equation for the interaction model is as follows:

Within-person (Level 1):

$$\text{Outcome}_{ti} = \beta_{0i} + \beta_{1i}\text{Size}_{ti} + \beta_{2i}\text{Medium}_{ti} + \beta_{3i}\text{Size*Medium}_{ti} + \beta_{4i}\text{Weekend}_{ti} + \beta_{5i}\text{CloseOther}_{ti} + e_{ti}$$

Between-person (Level 2):

$$\beta_{0i} = \gamma_{00} + \gamma_{01}\text{Size}_{i} + \gamma_{02}\text{Medium}_{i} + \gamma_{03}\text{Size*Medium}_{i} + \gamma_{04}\text{Weekend}_{i} + \gamma_{05}\text{CloseOther}_{i} + u_{0i}$$

$$\beta_{1i} = \gamma_{10} + u_{1i}$$

$$\beta_{2i} = \gamma_{20} + u_{2i}$$

$$\beta_{3i} = \gamma_{30} + u_{3i}$$

$$\beta_{4i} = \gamma_{40} + u_{4i}$$

$$\beta_{5i} = \gamma_{50} + u_{5i}$$

At Level 1, outcomes (i.e., closeness, self-disclosure, perceived responsiveness, and enjoyment) at time *t* for individual *i* were equal to a person-specific intercept (β_{0i}), plus the person-specific within-person effect of size (β_{1i}), medium (β_{2i}), size*medium (β_{3i}), weekend (β_{4i}), and close other (β_{5i}), plus the residual for person *i* at time *t* (e_{ti}). At Level 2, the person-specific intercept (β_{0i}) was equal to a fixed effect (γ_{00}), plus the between-person effect of size (γ_{01}), medium (γ_{02}), size*medium (γ_{03}), weekend (γ_{04}), and close other (γ_{05}), plus a person-specific random effect (u_{0i}). The person-specific within-person effect of size (β_{1i}), medium (β_{2i}), size*medium (β_{3i}), weekend (β_{4i}), and close other (β_{5i}) were equal to a fixed effect ($\gamma_{10} - \gamma_{50}$) plus a person-specific random effect ($u_{1i} - u_{5i}$).

Does Intimacy and Enjoyment Differ by Conversation Size (Aim 1) and Medium (Aim 2)?

First, we entered our main predictors (size and medium) and covariates into the models to examine the main effects for each outcome variable at the between- and within-person levels (Table 3.5).

Between-person Effects (Level 2). Contrary to our hypothesis, the between-person effect of conversation size was not significant for self-disclosure, perceived responsiveness, or closeness, suggesting that having conversations with more people on average compared to others was not related to intimacy. However, the between-person effect of conversation size was significant for enjoyment, such that participants who had conversations with more people on average compared to others reported greater enjoyment during conversations overall. Contrary to our hypothesis, the between-person effect of conversation medium was not significant for self-disclosure, perceived responsiveness, closeness, or enjoyment, suggesting that having more

video chat conversations on average compared to others was not related to intimacy or enjoyment.

Within-person Effects (Level 1). The within-person effect of conversation size was significant for each outcome variable. In support of our hypothesis, participants self-disclosed less, perceived their conversation partners as less responsive, and felt less close to their conversation partners when interacting with more people than one usually did (i.e., larger-than-average conversations for oneself). But despite feeling lower intimacy, participants enjoyed these larger-than-average conversations *more*. The within-person effect of medium was not significant for self-disclosure, perceived responsiveness, closeness, or enjoyment, suggesting that participants felt similarly during in-person and video chat conversations.

Is the Effect of Conversation Size Stronger for Video Chats? (Aim 3)

Next, we entered the conversation size × medium interaction term into the models to examine the interaction effects for each outcome variable at the between-person and within-person levels (Table 3.6).

Between-person Effects (Level 2). The between-person effect of conversation size on enjoyment remained significant, but the between-person interaction effect was not significant for self-disclosure, perceived responsiveness, closeness, or enjoyment. This suggests that the association between a person's average conversation size and intimacy and enjoyment did not depend on how many video chat conversations that participant had.

Within-person Effects (Level 1). The within-person effect of conversation size remained significant for self-disclosure, closeness, and enjoyment, but not perceived responsiveness. The within-person interaction effect was significant for perceived responsiveness, closeness, and enjoyment, but not self-disclosure.

We probed the within-person interaction effect by conducting follow-up simple slopes analyses at one standard deviation below and above the mean for the within-person, person-mean-centered medium variable (see Table 3.7 and Figure 3.3). In other words, we examined the within-person effect of conversation size for in-person conversations among participants who had 26% video chats (-1SD) and for video chat conversations among participants who had 74% video chats (+1SD). Because the interaction effect was not significant for self-disclosure, interacting in larger groups was associated with lower self-disclosure for both in-person (-1SD) and video chat (+1SD) conversations. However, differences emerged for perceived responsiveness, closeness, and enjoyment. For in-person conversations (-1SD), interacting with more people was not associated with perceived responsiveness or closeness, but it was associated with *greater* enjoyment. For video chat conversations (+1SD), interacting with more people was associated with lower perceived responsiveness and closeness, but not related to enjoyment. Thus, interacting with more people is associated with *greater* enjoyment when in-person, but *lower* perceived responsiveness and closeness when over video chat.

Secondary Analyses with Person-Level Covariates

Secondary analyses were conducted controlling for age, sex, COVID-19 worry, and extraversion at Level 2 (see Tables B.7–B.9 in Appendix B). Of these covariates, only extraversion had a significant (positive) effect on closeness and enjoyment. Sex had a significant effect on perceived responsiveness (females reported higher perceived responsiveness than males) in the main effects model, but this effect dropped out when the conversation size × medium interaction term was added to the model. The between-person effect of conversation size on enjoyment remained significant. The within-person main effect of conversation size remained significant for each dependent variable, and the interaction effect remained significant for

perceived responsiveness, closeness, and enjoyment. Thus, these findings are robust, as they hold when accounting for demographic variables, COVID-19 concerns, and extraversion.

Is There a Serial Indirect Effect of Conversation Size Through Self-Disclosure and Perceived Responsiveness? (Aim 4)

Next, we examined if the negative within-person association between conversation size and closeness could be explained by lower self-disclosure and perceived responsiveness.[19] Following our preregistration, we used multilevel structural equation modeling (MSEM) via the *lavaan* package (Rosseel, 2012) to calculate indirect pathways with 5,000 bootstrapped confidence intervals. We controlled for conversation medium, size*medium, weekend, and close other for all pathways at the within- and between-person levels. We also controlled for conversation size at the between-person level. Although we did not make a hypothesis about a serial indirect effect for enjoyment, we investigated this as well. The standardized parameters and bootstrapped confidence intervals for the direct and indirect effects on closeness and enjoyment are reported in Table 3.8. All other direct effects are reported in Table B.11 in Appendix B.

When controlling for the conversation medium, the size*medium interaction, day of the week, and if a close other was present, there was a significant within-person serial indirect effect of conversation size through self-disclosure and perceived responsiveness on closeness. In support of our hypothesis, having a conversation with more people than one usually had resulted in lower self-disclosure ($p < .001$), which in turn was associated with lower perceived responsiveness ($p < .001$), which in turn was associated with lower closeness ($p < .001$) (see Figure 3.4, Panel A). When examining the within-person indirect effect through self-disclosure

[19] We did not explore a moderated-mediation effect because the conversation size × medium interaction on self-disclosure (the first mediator) was not significant.

and perceived responsiveness individually, we found a significant indirect effect through self-disclosure, but not perceived responsiveness.

Although not hypothesized, we found a similar pattern of results for enjoyment. Specifically, there was a significant within-person serial indirect effect of conversation size through self-disclosure and perceived responsiveness on enjoyment, such that having a conversation with more people than one usually had resulted in lower self-disclosure ($p < .001$), which in turn was associated with lower perceived responsiveness ($p < .001$), which in turn was associated with lower enjoyment ($p < .001$) (see Figure 3.4, Panel B). When examining the within-person indirect effect through self-disclosure and perceived responsiveness individually, we found a significant indirect effect through self-disclosure, but not perceived responsiveness. Still, even though more self-disclosure and perceived responsiveness were associated with *lower* enjoyment, the positive effect of conversation size on enjoyment remained significant.

Discussion

In Study 3, participants reported how they felt after conversations of various sizes occurring in-person and over video chat for a week-long period. In support of our hypotheses, participants reported lower self-disclosure, perceived responsiveness, and closeness (i.e., intimacy) in conversations that were larger than what they usually experienced (within-person effects), and these effects held when controlling for plausible third variables (e.g., COVID-19 worry, extraversion, presence of a close other). Still, participants who had conversations with more people on average compared to others did not experience lower intimacy overall (between-person effects).

Contrary to our hypotheses for the conversation medium, participants found video chat and in-person conversations similarly intimate (within-person effects), and people who had more

video chat conversations on average did not report lower intimacy (between person effects). Although we did not find evidence that video chat conversations were less intimate overall, we did find evidence that larger conversations may be more strongly linked to intimacy when over video chat compared to in-person. Specifically, for people who tend to have video chat conversations, larger-than-average video chat conversations resulted in *lower* perceived responsiveness and closeness, but this did not occur for larger-than-average in-person conversations among people who tend to have in-person conversations. These findings provide further evidence that conversations with more people are less intimate, and this may be especially true when over video chat.

We also found evidence in Study 3 supporting our hypothesis that lower closeness in conversations with more people is explained by lower self-disclosure and perceived responsiveness. Specifically, larger-than-average conversations resulted in lower self-disclosure, which in turn was associated with lower perceived responsiveness, which in turn was associated with lower closeness. This complements findings from Study 2, suggesting that conversations with more people that occur over video chat *and* in-person tend to result in lower closeness, and this is partially explained by disruptions to the interpersonal process of developing intimacy.

Although we did not make hypotheses for enjoyment in Study 3, findings were in line with those from Study 2. Participants reported greater enjoyment for conversations with more people than one usually had (within-person effect), and people who had larger-than-average conversations tended to report greater enjoyment overall (between-person effect). As with intimacy, participants reported similar levels of enjoyment across video chat and in-person conversations (within-person effects), and people who had more video chat conversations on average did not report lower enjoyment (between person effects). When examining if the

influence of conversation size differed for video chat and in-person conversations, we found that for participants who mostly had in-person conversations in-person conversations with more people resulted in *greater* enjoyment, but conversation size and enjoyment were not related for video chats among people who mostly had video chat conversations. In other words, we found evidence that the presence of more people during in-person conversation may amplify these experiences in a way that does not occur over video chat.

Interestingly, we also found indirect effects of conversation size on enjoyment through lower self-disclosure and lower perceived responsiveness, but the positive direct effect of conversation size on enjoyment remained significant. This suggests that people still reap the enjoyment rewards of conversations with more people, even if self-disclosing less and perceiving others as less responsive should make these conversations less enjoyable.

A strength of Study 3 is that it used an experience-sampling design where participants reported on multiple conversations throughout the day for seven days, allowing us to capture participants' feelings almost immediately after their naturally occurring conversations. Still, this study is limited as the data collected came from a college student sample in the Southeastern U.S., which limits the generalizability of these findings. Another limitation of this study is that there were significantly fewer video chat conversations than in-person ones, which may have altered our ability to make strong comparisons across mediums. Finally, because all outcome variables were measured at the same time, we acknowledge that our evidence for indirect effects *cannot* provide causal evidence for these associations.

General Discussion

Conventional wisdom implies that conversations *feel* different when they involve more people. To our knowledge, this body of work is the first to empirically examine if group

conversations are characterized by a *many minds problem* (Cooney et al., 2020) that makes self-disclosure more difficult and riskier, and if this has downstream consequences for feelings of intimacy and enjoyment. Although previous research has compared affiliative outcomes between video chat and in-person conversations (e.g., Sprecher, 2014, 2021), we are the first to our knowledge to examine how interacting with more than just one other person over video chat can influence affiliative outcomes in social interactions. An overall summary of the findings across Studies 1–3 is presented in Table 3.9. First, we will consider the findings for self-disclosure, perceived responsiveness, and closeness (i.e., intimacy) across the three studies, and then we will turn to the findings for enjoyment.

Implications for Intimacy

Across all three studies, we found consistent evidence that conversations with more people are less intimate. Participants self-disclosed less, perceived conversation partners as less responsive, and felt less close to others when conversing in groups compared to dyads (Studies 1 and 2) and in larger groups compared to smaller ones (Studies 1 and 3). Thus, we found empirical evidence of a *many minds problem* in group conversations (Cooney et al., 2020), and these effects held when controlling for plausible third variables like COVID-19 concerns and extraversion.

Using the *Interpersonal Model of Intimacy* (Reis & Shaver, 1988) as a theoretical framework, we aimed to provide further evidence that self-disclosure and perceived responsiveness serve as mechanisms for lower closeness experienced in group conversations. Specifically, we expected that people would feel less close to others in conversations with more people because they would have fewer opportunities to self-disclose, and in turn they would have fewer opportunities to perceive others as being responsive to them. In support of this prediction,

we found a significant serial indirect effect of conversation size on closeness through self-disclosure and perceived responsiveness when participants recalled a video chat conversation (Study 2) and during everyday conversations that occurred in-person and over video chat (Study 3). Interestingly, we also found indirect effects through self-disclosure alone, but not perceived responsiveness alone, providing further evidence that being able to share one's thoughts, feelings, and beliefs is especially important for feeling close to others (Collins & Miller, 1994) and that doing so may provide opportunities to perceive others as responsive (Reis & Shaver, 1988). Overall, these findings advance theory on the process of developing intimacy beyond dyadic conversations.

Contrary to our expectation that people would feel less intimate in video chat conversations compared to in-person ones, we found little evidence for overall differences between mediums, especially after controlling for third variables. Participants reported similar self-disclosure between video chat and in-person conversations across all three studies, and we found similar results for perceived responsiveness in Studies 1 and 3, and closeness in Study 3. These findings are in line with experimental studies that did not find differences in closeness and other affiliative outcomes between pairs of participants having a getting-acquainted conversation (Sprecher, 2014, 2021). Thus, the differences in perceived responsiveness and closeness found in Studies 1 and 2 may have been driven by biased expectations or recollections of video chat conversations, rather than *actual* experiences. In other words, it could be that participants *expect* to feel less close to others in video chat conversations, so they recalled them as being less close. Further, these findings are in line with previous work suggesting that people can communicate non-verbal information just as well over video chat (Croes et al., 2019), but if there are deficits,

they are able to adapt to them over time (Walther, 1992). Unfortunately, our study designs did not allow us to test for these possible explanations, which could be examined in future research.

Although we did not find overall differences in intimacy between video chat and in-person conversations, we did find that conversations with more people may be *even less* intimate over video chat. At the height of the COVID-19 pandemic, participants found both video chat *and* in-person conversations with more people to be less intimate (Study 1), perhaps because in-person gatherings were further complicated by COVID-19 concerns and policies (e.g., social distancing and mask-wearing). But the following year, participants recalled group conversations as much less intimate than dyadic ones only when over video chat (Study 2), and conversations with more people were lower in perceived responsiveness and closeness when over video chat, but not in-person (Study 3).

Our findings suggest that although dyadic conversations over video chat tend to be just as intimate as when in-person, this may not be the case for group conversations. Although video chat may be able to successfully mimic in-person conversations with only two people, this becomes increasingly more difficult to do in group conversations when only one person can speak at a time. Indeed, others have theorized that group conversations over video chat also led to greater cognitive load as multiple faces stare in one's direction from a close distance—something that does not naturally occur in person (Bailenson, 2021), thereby interfering with one's ability to feel intimate in that context. Thus, our work adds to the existing literature comparing affiliative outcomes between mediums by considering how the number of people involved in a conversation may exacerbate differences.

Implications for Enjoyment

The current work captures a nuanced picture of how adding more people to a conversation influences enjoyment. In support of our initial prediction, we found that conversations with more people were less enjoyable during the height of the COVID-19 pandemic (Study 1), but our findings changed as the influence of COVID-19 waned the following year and participants recalled group conversations to be just as enjoyable as dyadic ones (Study 2), and everyday conversations with more people than one usually experienced were *more* enjoyable (Study 3). Thus, it appears that outside of the influence of the COVID-19 pandemic, people tend to enjoy conversations more when they involve more people.

Interestingly, we also found that people who had conversations with more people on average *compared to others* reported *more* enjoyment overall, even when controlling for extraversion (Study 3). This may be because reward sensitivity is a more defining characteristic of extraversion than sociability (Lucas et al., 2000), and extraverts tend to rate situations more positively when they are more pleasant, but not necessarily when they are more social (Lucas & Diener, 2001). Therefore, one's willingness to attend more group interactions may be distinct from extraversion. However, because these data are cross-sectional, it is still unclear if attending larger gatherings leads people to experience greater enjoyment overall, if experiencing more enjoyment leads people to attend larger gatherings, or if there is another third variable—such as happiness, well-being, or sociability—that may explain this association. Future research should further examine these possible explanations.

When comparing conversation mediums, we had expected that video chat conversations would be less enjoyable than in-person ones, but we found little evidence of an overall difference. Participants generally felt video chat conversations were just as enjoyable as in-

person ones during the height of the COVID-19 pandemic (Study 1) and in their day-to-day lives the following year (Study 3). It was only when participants recalled a recent video chat conversation that they found it less enjoyable (Study 2). Thus, it appears that differences in enjoyment between video chat and in-person conversations depend on how many people are involved.

During the height of the COVID-19 pandemic, participants found conversations with more people to be equally less enjoyable when over video chat and in-person (Study 1). However, differences emerged the following year. Participants recalled in-person group conversations as *more* enjoyable than dyadic ones, but group video chats were *less* enjoyable than dyadic video chats (Study 2). Similarly, participants found in-person conversations with more people than usual to be *more* enjoyable, but when over video chat, enjoyment did not vary by conversation size (Study 3). These findings are in line with previous research demonstrating that experiences are amplified when shared with others (Boothby et al., 2014; Brand et al., 2023; Zajonc, 1965), but our findings suggest that this amplification effect may be limited to *in-person* experiences that are characterized by greater social presence. Further, we found some evidence that larger conversations over video chat may *lower* enjoyment, but additional evidence is needed.

Finally, we examined if self-disclosure and perceived responsiveness play a similar role in promoting or thwarting enjoyment as they do for closeness. As with closeness, we found that when over video chat, recalled group conversations resulted in lower self-disclosure, which in turn was associated with lower perceived responsiveness and then lower enjoyment (Study 2). In other words, video chats in groups were less enjoyable than when with one other person, and this was explained by lower self-disclosure and perceived responsiveness. We also found an indirect

effect of conversation size on enjoyment for all conversations (video chat *and* in-person) in Study 3. However, in Study 3, conversations with more people than usual were associated with *greater* enjoyment. This suggests that even though larger conversations resulted in lower self-disclosure and perceived responsiveness, this was not enough to overcome the increased feelings of enjoyment.

Strengths, Limitations, and Future Directions

The present work has several strengths that are worth highlighting. First, we used three complementary study designs that each addressed limitations of the others. Study 1 used a within-subjects experiment that allowed us to control for individual characteristics across conditions, but it also enabled participants to become privy to our hypotheses and directly compare conversation types. Study 2 addressed this limitation with a between-subjects experiment that provided greater control over the manipulation, but it was susceptible to recall errors and bias. Study 3 addressed this limitation using an experience-sampling design where participants reported on conversations as they happened in daily life, but it was limited to a sample of U.S. college students. Studies 2 and 1 partially addressed this concern with adult samples from the United States and 21 other countries, respectively. Additionally, each study contained a large, well-powered sample, enabling us to detect small effects.

Second, we measured conversation size in various ways—comparing dyads to groups (Studies 1 and 2), breaking groups into different sizes (Study 1), and measuring the number of conversation partners (Study 3)—all of which revealed a consistent pattern of results. We also statistically controlled for plausible third variables that may influence the experience of conversations, including demographic characteristics (i.e., age, sex), COVID-19 worry and behaviors, extraversion, and the pre-existing relationship of interaction partners (i.e., if they were

a family member, friend, or romantic partner). Thus, our findings are robust, as they withstand design variations and plausible third variables.

Third, the present work goes beyond just identifying differences in intimacy and enjoyment based on the number of interaction partners, but it aimed to identify mechanisms for these differences. Using the *Interpersonal Model of Intimacy* (Reis & Shaver, 1988) as a theoretical framework, we demonstrated that self-disclosure and perceived responsiveness may explain *why* people do not feel as close to one another in group conversations with more people, especially over video chat. However, even though our findings were consistent with theory, our mediators (self-disclosure and perceived responsiveness) were measured cross-sectionally with our outcomes (closeness and enjoyment), rather than manipulated or measured longitudinally. Thus, we cannot draw causal conclusions about the relationships between these variables, and our findings of indirect effects should be interpreted with caution until they are replicated in future work.

Despite its strengths, the present work has limitations that should be considered. All three studies were limited by a selection bias, such that participants were required to have had each type of conversation to be eligible to participate (Study 2) or for their data to be compared across conditions (Study 1 and 3). As such, these studies did not include participants who may have been limiting in-person gatherings—especially in large groups—due to COVID-19 concerns, or those who rarely or never have video chat conversations, and the present findings may not accurately represent these groups. This limitation could be addressed in future research using a laboratory experiment in which participants are randomly assigned to have a dyadic or group conversation that occurs in-person or over video chat.

A laboratory experiment would also help to address other limitations of this work, such as how we primarily collected self-report data that came from only one participant of a conversation. As such, we were not able to examine how dynamics of the conversation—such as turn-taking, distribution of speaking time, and non-verbal communication—influence the experience of dyadic and group conversations occurring in-person and over video chat. Although we and others have speculated about how these conversational processes should be more difficult in group conversations (Cooney et al., 2020), especially over video chat (Bailenson, 2021), we were not able to test these mechanisms in the present work. Further, future research could assess self-disclosure and responsiveness using behavioral indicators that are captured in recorded conversations (e.g., Maisel et al., 2008). Doing so would help to address any concerns that self-reports were indicative of generalized perceptions about the conversation, as well as to help to demonstrate the temporal nature of these relationships (i.e., examining if behavioral indicators of self-disclosure during the conversation predicts feelings of closeness to conversation partners afterward). Alternatively, researchers could manipulate self-disclosure and/or responsiveness to further demonstrate the causal effect of these potential mechanisms.

Another limitation of only collecting data from one perspective is that we were not able to examine if *all* participants of a group conversation felt similar levels of intimacy and enjoyment, or if this depended on how one participates in the conversation. For instance, it could be that all people in a group conversation feel that they self-disclose less, or it could be that self-disclosure corresponds to the amount of time each person speaks (i.e., those who speak more report self-disclosing more). If the latter is true, then the intimacy and enjoyment experienced in group conversations should vary depending on how—and how much—one participates in the conversation, and this possibility should be investigated in future work.

Although the participants from Study 1 were recruited from 22 countries, most participants across our three samples were from the United States and spoke English. This limits our ability to generalize our findings to other populations with different social norms and cultural expectations about conversations. For example, previous research has found that people from a Western cultural context are more likely to amplify positive feelings and diminish negative feelings than people from an Eastern Asian cultural context (Miyamoto et al., 2014; Miyamoto & Ma, 2011). As such, individuals from East Asia and other cultural contexts less influenced by the principle of hedonism may be less likely to express positive emotions in group conversations, which could make the conversation *less* enjoyable overall. Similarly, Japanese students have been found to self-disclosure less than American students in conversations (Kito, 2005) and perceiving one's romantic partner as responsive was found to be more strongly related to greater hedonic and eudemonic well-being in the United States than in Japan (Tasfiliz et al., 2018). Thus, self-disclosure and perceived responsiveness may not play as important of a role in the development of intimacy during group conversations in other cultures. Finally, the data collected in this study were limited to participants who use video chat, which limits the generalizability of our findings to populations where video chat technology is commonly used or accessible. Future research should continue to examine feelings of intimacy and enjoyment in group conversations in other cultural contexts, and to not limit this exploration to those who have access to video chat technologies. However, given the frequency with which video chat technology is used in many parts of the world, how intimacy operates in these contexts is still important to understand, even if not generalizable to every cultural context.

Conclusion

The present work examined whether and why conversations with more people are less intimate and enjoyable. Our findings highlight that the number of people involved in a conversation influences its overall experience. Specifically, we found strong evidence that people feel less close to one another when in conversations with more people—especially over video chat—because they self-disclose less and perceive their conversation partners as less responsive. Although less intimate, in-person conversations with more people are also *more* enjoyable. Taken together, dyadic and group conversations not only appear to have different qualities, but they may even meet different psychological needs. As we return to our restaurant scene, we can now expect that the pair of friends will feel closer to each other by the end of the meal, but the group of five friends will have a more enjoyable evening.

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Tables

Table 3.1

Means, Standard Deviations, and Repeated Measures ANOVA Results (Study 1)

	Main Effect: Conversation Size					
	Dyad	Small	Large	Test Statistic		
Outcome	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*F*	*p*	η_p^2
Self-disclosure	$6.16\ (1.16)_a$	$5.33\ (1.32)_b$	$4.24\ (1.65)_c$	**482.89**	**< .001**	**.61**
Responsiveness	$6.13\ (1.05)_a$	$5.43\ (1.17)_b$	$4.47\ (1.52)_c$	**418.39**	**< .001**	**.58**
Closeness	$5.65\ (1.27)_a$	$5.18\ (1.26)_b$	$4.39\ (1.52)_c$	**286.72**	**< .001**	**.49**
Enjoyment	$5.83\ (1.18)_a$	$5.65\ (1.23)_b$	$4.98\ (1.58)_c$	**117.15**	**< .001**	**.28**
	Main Effect: Conversation Medium					
	In-person	Video Chat		Test Statistic		
Outcome	*M* (*SD*)	*M* (*SD*)		*F*	*p*	η_p^2
Self-disclosure	5.49 (1.49)	5.00 (1.66)		**63.47**	**< .001**	**.17**
Responsiveness	5.55 (1.34)	5.14 (1.50)		**42.46**	**< .001**	**.12**
Closeness	5.38 (1.36)	4.77 (1.47)		**70.24**	**< .001**	**.19**
Enjoyment	5.80 (1.27)	5.17 (1.43)		**81.32**	**< .001**	**.21**
	Interaction Effect: Size × Medium					
	In-person			Video Chat					
	Dyad	Small	Large	Dyad	Small	Large	Test Statistic		
Outcome	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*F*	*p*	η_p^2
Self-disclosure	$6.32\ (1.09)_a$	$5.63\ (1.18)_c$	$4.52\ (1.57)_e$	$6.00\ (1.22)_b$	$5.03\ (1.39)_d$	$3.96\ (1.68)_f$	**7.60**	**.001**	**.02**
Responsiveness	$6.29\ (0.94)_a$	$5.64\ (1.09)_c$	$4.72\ (1.45)_e$	$5.98\ (1.13)_b$	$5.23\ (1.21)_d$	$4.21\ (1.55)_f$	**3.76**	**.031**	**.01**
Closeness	$5.97\ (1.11)_a$	$5.49\ (1.14)_b$	$4.67\ (1.48)_c$	$5.33\ (1.33)_b$	$4.87\ (1.30)_c$	$4.10\ (1.51)_d$	0.55	.534	.002
Enjoyment	$6.10\ (1.05)_a$	$5.98\ (1.10)_a$	$5.31\ (1.48)_c$	$5.56\ (1.24)_b$	$5.32\ (1.26)_c$	$4.64\ (1.60)_d$	1.77	.179	.01

Note. Different subscript letters within rows indicate conditions that significantly differ ($p < .05$) from each other. Bold values indicate significant effects. Type 3 sum of squares was used.

Table 3.2

Means, Standard Deviations, and ANOVA Results (Study 2)

	Main Effect: Conversation Size				
	Dyad	Group	Test Statistic		
Outcome	*M* (*SD*)	*M* (*SD*)	*F*	*p*	η_p^2
Self-disclosure	5.13 (1.56)	4.61 (1.69)	**16.07**	**< .001**	**.026**
Responsiveness	3.96 (0.91)	3.50 (1.07)	**33.81**	**< .001**	**.053**
Closeness	4.55 (1.74)	4.20 (1.74)	**6.25**	**.013**	**.010**
Enjoyment	4.55 (1.61)	4.45 (1.74)	0.67	.415	.001
	Main Effect: Conversation Medium				
	In-person	Video Chat	Test Statistic		
Outcome	*M* (*SD*)	*M* (*SD*)	*F*	*p*	η_p^2
Self-disclosure	4.89 (1.59)	4.85 (1.69)	0.12	.735	< .001
Responsiveness	3.82 (0.94)	3.64 (1.09)	**4.82**	**.028**	**.008**
Closeness	4.56 (1.63)	4.18 (1.84)	**7.67**	**.006**	**.012**
Enjoyment	4.81 (1.58)	4.18 (1.72)	**22.11**	**< .001**	**.035**

	Interaction Effect: Size × Medium						
	In-person		Video Chat				
	Dyad	Group	Dyad	Group	Test Statistic		
Outcome	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*F*	*p*	η_p^2
Self-disclosure	5.01 (1.65)	4.78 (1.53)	5.26 (1.45)	4.44 (1.82)	**5.34**	**.021**	**.009**
Responsiveness	3.93 (0.94)	3.71 (0.92)	4.00 (0.88)	3.29 (1.17)	**9.09**	**.003**	**.015**
Closeness	4.57 (1.74)	4.56 (1.51)	4.52 (1.74)	3.83 (1.88)	**5.97**	**.015**	**.010**
Enjoyment	4.63 (1.61)	4.98 (1.53)	4.47 (1.62)	3.90 (1.78)	**12.24**	**< .001**	**.020**

Note. Bold values indicate significant effects. Type 3 sum of squares was used.

Table 3.3

Post Hoc Comparisons between Dyadic and Group Conversations (Study 2)

	In-person (*df* = 307)				Video Chat (*df* = 300)			
Outcome	*t*	*SE*	*p*	*d*	*t*	*SE*	*p*	*d*
Self-disclosure	-1.23	.18	.221	.07	**-4.37**	**.19**	**< .001**	**.25**
Responsiveness	**-2.10**	**.11**	**.036**	**.12**	**-5.90**	**.12**	**< .001**	**.34**
Closeness	-0.04	.19	.967	.002	**-3.30**	**.21**	**.001**	**.19**
Enjoyment	**1.99**	**.18**	**.048**	**.11**	**-2.92**	**.20**	**.004**	**.17**

Note. Negative *t*-values indicate that the mean is smaller for group (vs. dyadic) conversations. Bold values indicate significant effects.

Table 3.4

Conditional Indirect Effects of Conversation Size on Closeness and Enjoyment Disaggregated by Medium (Study 2)

	In-person			Video Chat		
Pathway	*b*	*SE*	95% CI	*b*	*SE*	95% CI
Closeness (*C*)						
Size → *SD* → *C*	-0.08	.07	[-0.22, 0.05]	**-0.31**	**.08**	**[-0.48, -0.16]**
Size → *PR* → *C*	-0.10	.06	[-0.23, 0.02]	**-0.28**	**.08**	**[-0.45, -0.14]**
Size → *SD* → *PR* → *C*	-0.06	.05	[-0.16, 0.03]	**-0.22**	**.06**	**[-0.33, -0.11]**
Enjoyment (*E*)						
Size → *SD* → *E*	-0.07	.06	[-0.19, 0.04]	**-0.26**	**.07**	**[-0.41, -0.12]**
Size → *PR* → *E*	-0.10	.06	[-0.22, 0.02]	**-0.27**	**.07**	**[-0.42, -0.13]**
Size → *SD* → *PR* → *E*	-0.06	.05	[-0.15, 0.03]	**-0.21**	**.06**	**[0.32, -0.10]**

Note. Bold values indicate significant effects. The size variable is effects-coded (-0.5 = *dyad*, 0.5 = *group*). Closeness and enjoyment were examined in separate models but are presented in the same table for brevity. CI = confidence interval; *SD* = Self-disclosure; *PR* = Perceived Responsiveness.

Table 3.5

Between-person and Within-person Main Effects of Conversation Size and Medium (Study 3)

	Self-disclosure			Perceived Responsiveness			Closeness			Enjoyment		
Fixed Effects	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI
Between-person (Level 2)												
Size	-0.01 (.08)	.881	[-0.17, 0.14]	0.02 (.08)	.748	[-0.12, 0.17]	0.09 (.07)	.184	[-0.04, 0.23]	**0.17 (.07)**	**.011**	**[0.04, 0.30]**
Medium	0.61 (.54)	.261	[-0.44, 1.66]	0.63 (.51)	.220	[-0.36, 1.61]	-0.07 (.47)	.890	[-0.99, 0.86]	-0.41 (.45)	.370	[-1.29, 0.48]
Weekend	0.60 (.65)	.360	[-0.67, 1.87]	0.56 (.61)	.361	[-0.63, 1.76]	1.01 (.57)	.079	[-0.10, 2.12]	0.81 (.55)	.141	[-0.25, 1.88]
Close Other	**1.22 (.50)**	**.016**	**[0.24, 2.19]**	0.73 (.47)	.125	[-0.19, 1.64]	**2.54 (.44)**	**< .001**	**[1.69, 3.39]**	**0.97 (.42)**	**.021**	**[0.16, 1.79]**
Within-person (Level 1)												
Size	**-0.05 (.01)**	**.002**	**[-0.07, -0.02]**	**-0.03 (.01)**	**.026**	**[-0.06, -0.004]**	**-0.07 (.02)**	**< .001**	**[-0.10, -0.03]**	**0.05 (.02)**	**.003**	**[0.02, 0.08]**
Medium	0.16 (.09)	.065	[-0.01, 0.33]	0.03 (.08)	.721	[-0.13, 0.19]	-0.06 (.09)	.489	[-0.23, 0.11]	-0.02 (.09)	.852	[-0.20, 0.17]
Weekend	-0.04 (.05)	.449	[-0.13, 0.06]	0.03 (.05)	.460	[-0.06, 0.12]	**0.19 (.05)**	**< .001**	**[0.09, 0.29]**	0.05 (.05)	.321	[-0.05, 0.16]
Close Other	**1.32 (.08)**	**< .001**	**[1.16, 1.47]**	**0.78 (.07)**	**< .001**	**[0.63, 0.92]**	**2.87 (.08)**	**< .001**	**[2.71, 3.03]**	**1.61 (.09)**	**< .001**	**[1.44, 1.78]**

Note. Bold values indicate significant effects. Regression coefficients and their respective confidence intervals are reported. Level 1 predictors are person-mean centered. Level 2 variables are sample-mean centered. Dummy coding was used for the Medium (0 = *in person*, 1 = *video chat*), Weekend (0 = *weekday*, 1 = *weekend*), and Close Other (0 = *not present* 1 = *present*) variables. CI = *confidence interval*.

Table 3.6

Between-person and Within-person Interaction Effects Between Conversation Size and Medium (Study 3)

	Self-disclosure			Perceived Responsiveness			Closeness			Enjoyment		
Fixed Effects	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI
Between-person (Level 2)												
Size	0.00 (.08)	.956	[-0.16, 0.16]	0.06 (.08)	.472	[-0.09, 0.21]	0.12 (.07)	.106	[-0.02, 0.26]	**0.20 (.07)**	**.004**	**[0.07, 0.33]**
Medium	0.59 (.65)	.211	[-0.37, 1.75]	0.76 (.52)	.142	[-0.24, 1.76]	0.02 (.48)	.969	[-0.92, 0.95]	-0.32 (.46)	.481	[-1.22, 0.57]
Size × Medium	-0.42 (.51)	.411	[-1.42, 0.57]	-0.67 (.48)	.169	[-1.61, 0.27]	-0.44 (.45)	.324	[-1.33, 0.44]	-0.47 (.43)	.274	[-1.32, 0.37]
Weekend	0.59 (.65)	.365	[-0.68, 1.86]	0.55 (.61)	.368	[-0.64, 1.75]	1.00 (.57)	.081	[-0.10, 2.11]	0.79 (.55)	.151	[-0.27, 1.85]
Close Other	**1.20 (.50)**	**.018**	**[0.22, 2.17]**	0.64 (.47)	.179	[-0.28, 1.55]	**2.47 (.44)**	**< .001**	**[1.62, 3.33]**	**0.86 (.42)**	**.041**	**[0.05, 1.67]**
Within-person (Level 1)												
Size	**-0.04 (.01)**	**.003**	**[-0.07, -0.02]**	-0.03 (.01)	.071	[-0.05, 0.002]	**-0.06 (.02)**	**< .001**	**[-0.09, -0.03]**	**0.06 (.02)**	**< .001**	**[0.03, 0.09]**
Medium	0.16 (.09)	.063	[-0.01, 0.33]	0.02 (.08)	.686	[-0.13, 0.19]	-0.06 (.09)	.472	[-0.24, 0.11]	-0.01 (.09)	.906	[-0.19, 0.17]
Size × Medium	-0.03 (.03)	.568	[-0.09, 0.04]	**-0.14 (.03)**	**< .001**	**[-0.20, -0.07]**	**-0.12 (.04)**	**.001**	**[-0.19, -0.05]**	**-0.18 (.04)**	**< .001**	**[-0.25, -0.10]**
Weekend	-0.04 (.05)	.440	[-0.14, 0.06]	0.03 (.05)	.507	[-0.06, 0.12]	**0.19 (.05)**	**< .001**	**[0.09, 0.29]**	0.05 (.05)	.356	[-0.05, 0.15]
Close Other	**1.31 (.08)**	**< .001**	**[1.15, 1.47]**	**0.74 (.08)**	**< .001**	**[0.59, 0.89]**	**2.84 (.08)**	**< .001**	**[2.68, 3.00]**	**1.56 (.09)**	**< .001**	**[1.39, 1.73]**

Note. Bold values indicate significant effects. Regression coefficients and their respective confidence intervals are reported. Level 1 predictors are person-mean centered. Level 2 variables are sample-mean centered. Dummy coding was used for the Medium (0 = *in person*, 1 = *video chat*), Weekend (0 = *weekday*, 1 = *weekend*), and Close Other (0 = *not present* 1 = *present*) variables. CI = *confidence interval*.

Table 3.7

Within-person Simple Slopes Examining the Effect of Conversation Size at Different Levels of Medium (Study 3)

	In-Person (-1SD)				Video Chat (+1SD)			
Outcome	*b*	*SE*	*p*	95% CI	*b*	*SE*	*p*	95% CI
Self-disclosure	**-0.04**	**.02**	**.037**	**[-0.07, -0.002]**	**-0.05**	**.02**	**.002**	**[-0.08, 0.02]**
Responsiveness	0.01	.02	.588	[-0.02, 0.04]	**-0.06**	**.02**	**< .001**	**[-0.09, -0.03]**
Closeness	-0.03	.02	.124	[-0.06, 0.01]	**-0.09**	**.02**	**<.001**	**[-0.12, -0.05]**
Enjoyment	**0.10**	**.02**	**<.001**	**[0.06, 0.14]**	0.01	.02	.500	[-0.02, 0.05]

Note. Simple slopes of conversation size at two levels of medium: 1 SD above and below the mean. -1SD represents in-person conversations among participants who had 26% video chats; +1SD represents video chat conversations among participants who had 74% video chats. Analyses are controlling for weekend and close other. Bold values indicate significant effects. CI = confidence interval.

Table 3.8

Within-person Indirect Effects of Conversation Size on Closeness and Enjoyment (Study 3)

Pathway		*SE*	*p*	95% CI
Closeness (*C*)				
Size → *SD* → *C*	**-.03**	**.01**	**< .001**	**[-.04, -.01]**
Size → *PR* → *C*	-.001	.002	.566	[-.01, .003]
Size → *SD* → *PR* → *C*	**-.01**	**.002**	**< .001**	**[-.01, -.004]**
Enjoyment (*E*)				
Size → *SD* → *E*	**-.02**	**.01**	**< .001**	**[-.03, -.01]**
Size → *PR* → *E*	-.002	.004	.565	[-.01, .01]
Size → *SD* → *PR* → *E*	**-.02**	**.004**	**< .001**	**[-.02, -.01]**

Note. Standardized regression coefficients, standard errors, and 5,000 bootstrapped confidence intervals are reported. Size is person-mean centered, such that larger values indicate a larger-than-average conversation for a given participant. We controlled for conversation medium, size*medium, weekend, and close other for all pathways at the within- and between-person levels. We also controlled for size at the between-person level. Closeness and enjoyment were examined in separate models but are presented in the same table for brevity. CI = confidence interval; *C* = Closeness; *SD* = Self-disclosure; *PR* = Perceived Responsiveness; *E* = Enjoyment.

Table 3.9

Summary of Effects When Controlling for Covariates (Studies 1–3)

	Self-disclosure				Perceived Responsiveness				Closeness				Enjoyment			
Effect	S1	S2	S3*b*	S3*w*	S1	S2	S3*b*	S3*w*	S1	S2	S3*b*	S3*w*	S1	S2	S3*b*	S3*w*
Main Effect of Groups (Aim 1)	(–)	(–)	(*ns*)	(–)	(–)	(–)	(*ns*)	(–)	(–)	(–)	(*ns*)	(–)	(–)	(*ns*)	(+)	(+)
Main Effect of Video Chats (Aim 2)	(*ns*)	(*ns*)	(*ns*)	(*ns*)	(*ns*)	(–)	(*ns*)	(*ns*)	(–)	(–)^	(*ns*)	(*ns*)	(*ns*)	(–)	(*ns*)	(*ns*)
Size × Medium Interaction (Aim 3)	(*ns*)	(∗)	(*ns*)	(*ns*)	(*ns*)	(∗)	(*ns*)	(∗)	(*ns*)	(∗)^	(*ns*)	(∗)	(*ns*)	(∗)	(*ns*)	(∗)
In-person Groups	(–)	(*ns*)	—	(–)	(–)	(–)	—	(*ns*)	(–)	(*ns*)	—	(*ns*)	(–)	(+)	—	(+)
Video Chat Groups	(–)	(–)	—	(–)	(–)	(–)	—	(–)	(–)	(–)	—	(–)	(–)	(–)	—	(*ns*)
Serial Indirect Effect (Aim 4)	—	—	—	—	—	—	—	—	—	—	—	(–)	—	—	—	(–)
Moderated Mediation Effect (Aim 4)	—	—	—	—	—	—	—	—	—	(∗)	—	—	—	(∗)	—	—
In-person Serial Indirect Effect	—	—	—	—	—	—	—	—	—	(*ns*)	—	—	—	(*ns*)	—	—
Video Chat Serial Indirect Effect	—	—	—	—	—	—	—	—	—	(–)	—	—	—	(–)	—	—

Note. Em dashes (—) indicate untested paths. The serial indirect effect is through self-disclosure and perceived responsiveness. S1 = Study 1; S2 = Study 2; S3*b* = Study 3 between-person effects; S3*w* = Study 3 within-person effects; *ns* = not significant. (+) Indicates a positive effect. (–) Indicates a negative effect. (∗) Indicates significant interaction or moderated mediation effect. ^ Indicates an effect that held for all other covariates except for the presence of a close other.

Figures

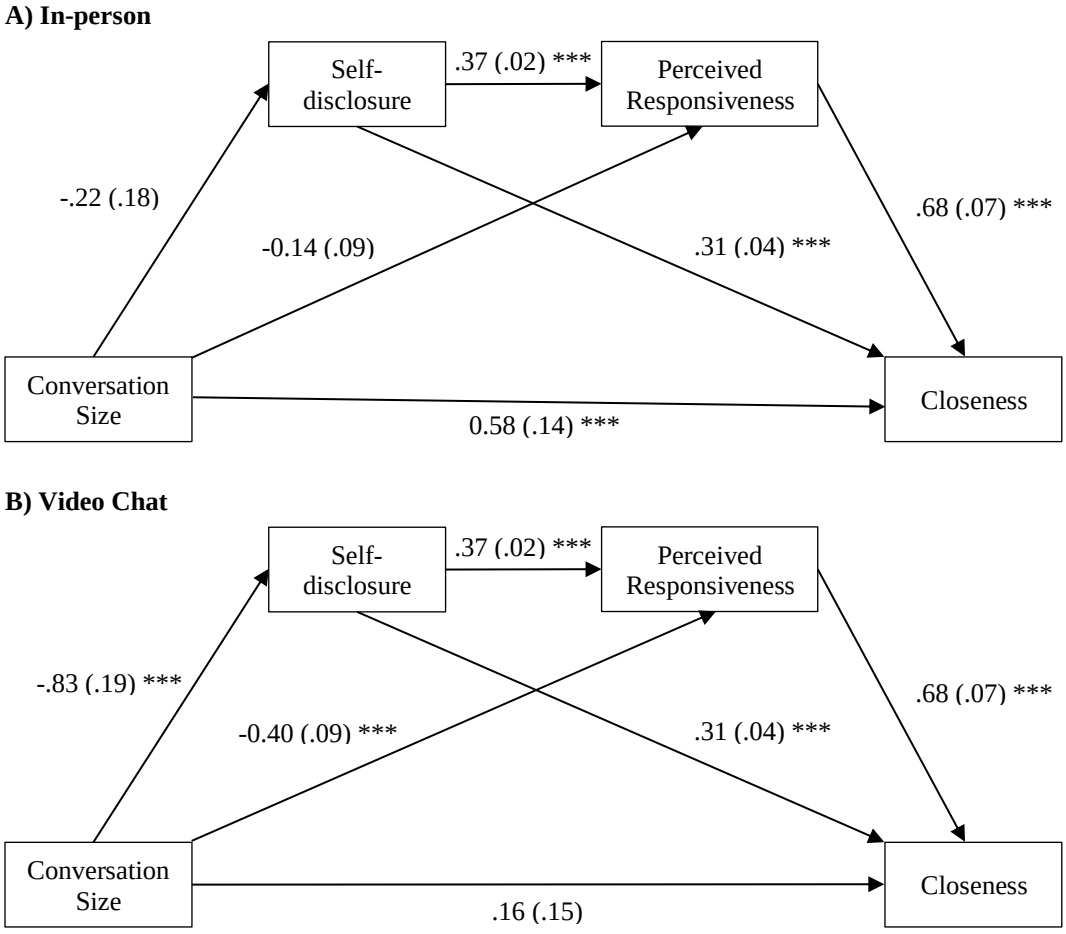


Figure 3.1. Conditional Direct Effects of Conversation Size Disaggregated by Medium for Closeness (Study 2). Unstandardized coefficients and standard errors are reported. Conversation medium moderated the paths from size to self-disclosure, size to perceived responsiveness, and size to closeness. *** $p < .001$, ** $p < .01$, * $p < .05$.

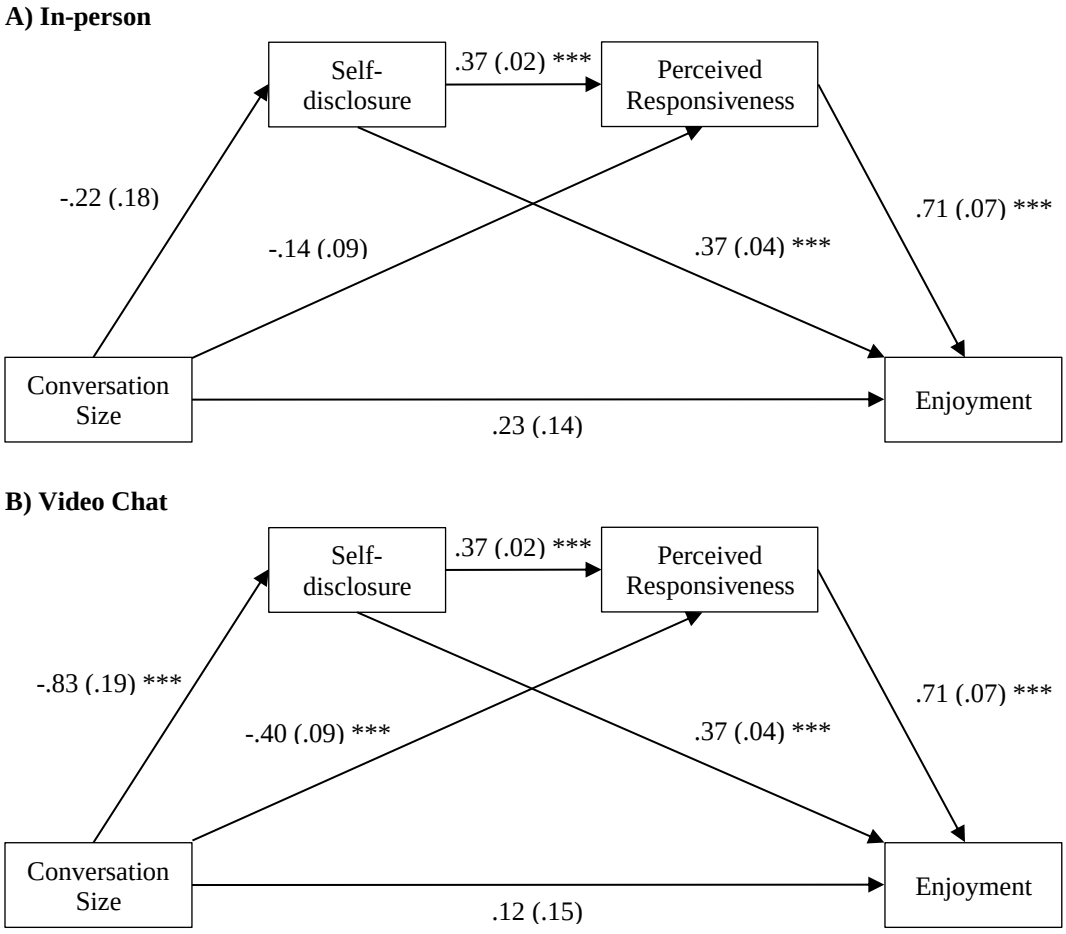


Figure 3.2. Conditional Direct Effects of Conversation Size Disaggregated by Medium for Enjoyment (Study 2). Unstandardized coefficients and standard errors are reported. Conversation medium moderated the paths from size to self-disclosure, size to perceived responsiveness, and size to enjoyment. *** $p < .001$, ** $p < .01$, * $p < .05$.

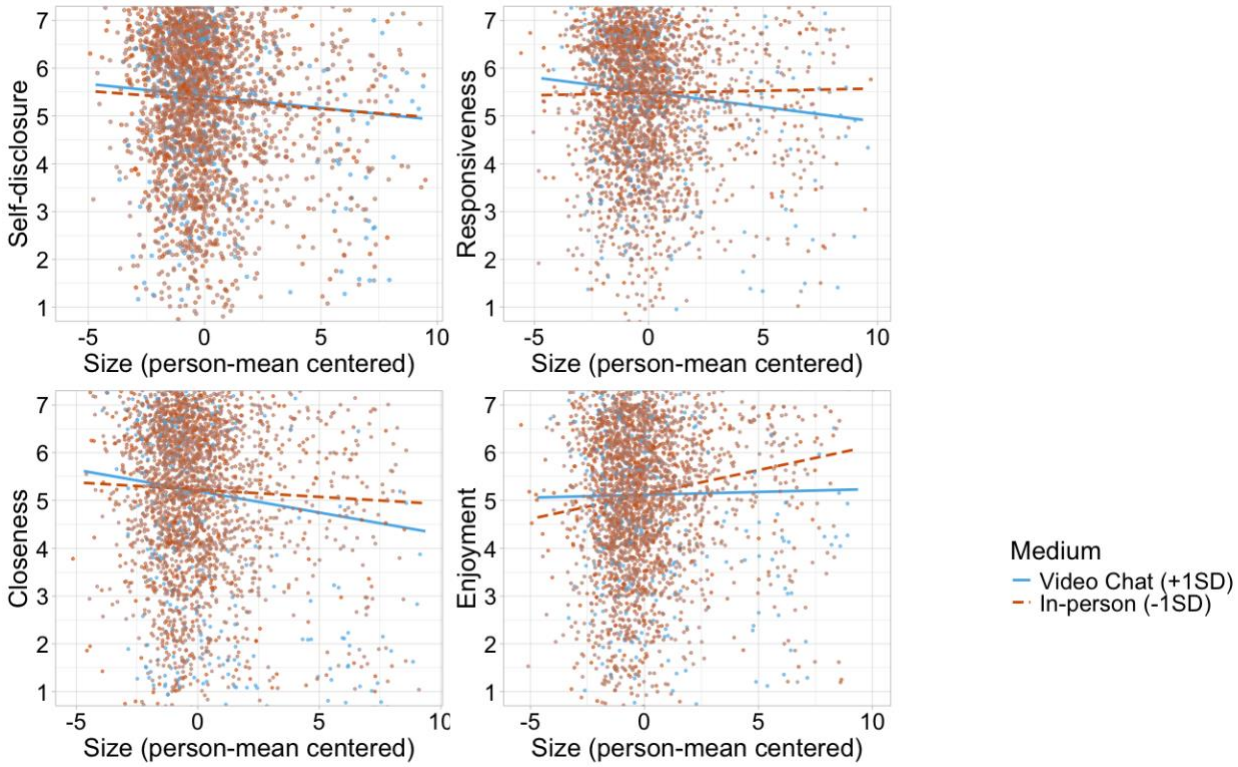
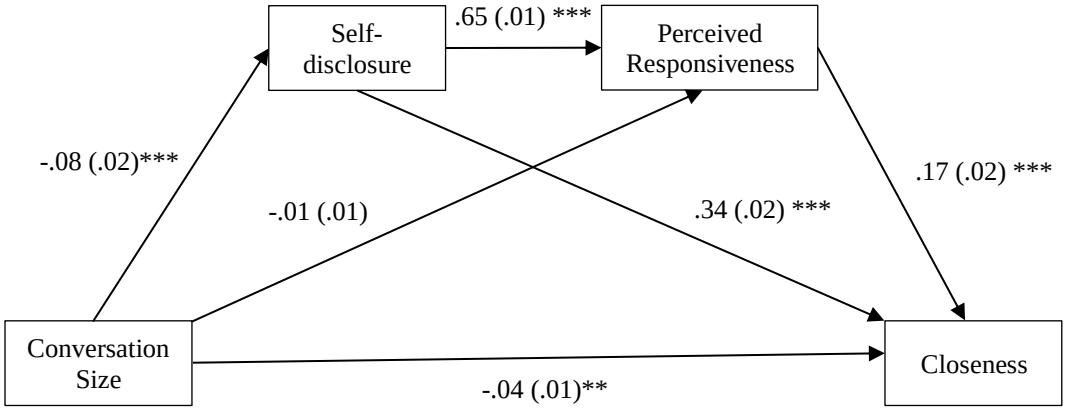


Figure 3.3. Within-person Interaction Effect of Conversation Size and Medium (Study 3). Simple slopes of conversation size at two levels of medium: 1 SD above and below the mean. +1SD represents video chat conversations among participants who had 74% video chats; -1SD represents in-person conversations among participants who had 26% video chats. Analyses are controlling for weekend and close other. Each data point represents a conversation. Data points are jittered to ease interpretation.

A)



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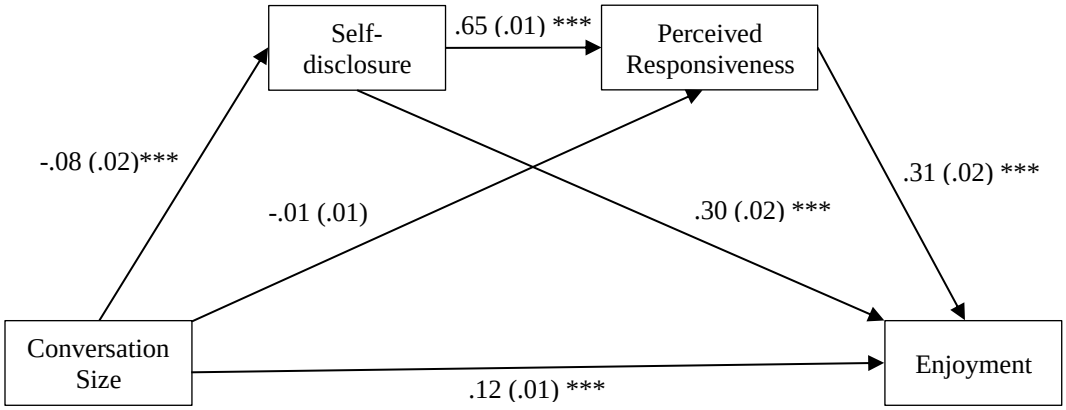

Figure 3.4. Direct Effects for the Within-person Serial Indirect Effect Model for Closeness and Enjoyment (Study 3). Standardized coefficients and standard errors are reported. Bold arrows indicate significant indirect pathways. Size is person-mean centered, such that larger values indicate a larger-than-average conversation for a given participant. We controlled for conversation medium, size*medium, weekend, and close other for all pathways at the within- and between-person levels. We also controlled for size at the between-person level. *** $p < .001$, ** $p < .01$, * $p < .05$.

CHAPTER 4

FROM CLOSE TO GHOST:

EXAMINING THE RELATIONSHIP BETWEEN THE NEED FOR CLOSURE, INTENTIONS TO GHOST, AND REACTIONS TO BEING GHOSTED [20]

[20] Leckfor, C. M., Wood, N. R., Slatcher, R. B., and Hales, A. H. 2023. *Journal of Social and Personal Relationships.* 40(8):2422–2444.

Abstract

Ghosting—the act of ending a relationship by ceasing communication without explanation—is a type of ostracism that threatens a person's basic psychological needs for belonging, self-esteem, meaningful existence, and control. The experience of ghosting creates uncertainty within the relationship and may vary based on individual differences in the need for closure, which is the desire to avoid ambiguity. Across three preregistered studies with emerging adults, we predicted that a greater need for closure would be associated with lower intentions to use ghosting (Studies 1 and 2) and lower needs satisfaction after being ghosted (Study 3). Results from Study 1 (N = 553) and Study 2 (N = 411) were inconsistent, but together indicate that a higher need for closure is *not* negatively associated—and may be positively associated—with ghosting intentions. In Study 3 (N = 545), participants who recalled a time when they were ghosted reported lower needs satisfaction than included and directly rejected participants. Further, a higher need for closure was associated with lower needs satisfaction after being ghosted and after being directly rejected, but with *greater* needs satisfaction after being included. Overall, these findings suggest that the need for closure is less influential when deciding how to end a relationship, but it appears to play an important role in amplifying both positive and negative experiences within a relationship.

Keywords: ghosting, need for closure, close relationships, relationship dissolution, ostracism, rejection, emerging adulthood, psychological needs satisfaction

"I had texted her, but didn't hear anything back. At first I thought maybe she didn't have her phone on her. As time went on, I grew more anxious waiting for her text back. Another unanswered text, and I started to realize I was being ghosted."

"I even reached out to him again years later and asked for an explanation for some closure and he still wouldn't tell me why he did it. It made it so much harder to move on because I didn't know the reason. Even today it still bugs me that I don't know why he ghosted me."

- Participants describing a time when they were ghosted

Introduction

Social technologies have changed the way people initiate, maintain, and even end their relationships (Weisskirch & Delevi, 2013). One such change is the proliferation of *ghosting*, which is "when one person suddenly ignores or stops communicating with another person, without telling them why" (p. 408; Kay & Courtice, 2022). As indicated by the participant quotations above, when a person is ghosted (i.e., the target), they may experience feelings of uncertainty and confusion as they try to decipher the *why* behind the silence. Similarly, when a person uses ghosting (i.e., the initiator), they may feel that the end of the relationship is unclear as the target still attempts to communicate with them. The extent to which this period of uncertainty has adverse effects for both partners may depend on their individual differences in the tolerance of ambiguity—in other words, their need for closure. The present research aims to examine the role of the need for closure in ghosting experiences by examining if it is related to a person's intentions to use ghosting to end a relationship (Studies 1 and 2), and if it influences a person's reaction to being ghosted (Study 3).

The Need for Closure

In general, people find uncertainty aversive (Hogg, 2007), though there are individual differences in people's tolerance for ambiguity (Webster & Kruglanski, 1994). The need for closure is a person's desire to avoid ambiguity and to have a firm answer—regardless of accuracy (Kruglanski & Webster, 1996). This desire varies along a continuum, ranging from a strong need to achieve closure to a strong need to avoid closure. Due to the motivational aspects of this need, people behave in ways that fulfill their goals, and experience discomfort when the need is not met. In response to uncertainty, people with a strong need to *achieve* closure are more likely to engage in activities to attain closure; they tend to quickly make a decision and stick with it, even in light of disconfirming information; and experience negative affect when closure is not achieved (Kruglanski & Webster, 1996; Webster & Kruglanski, 1994). Epistemically, someone with a high need for closure desires immediate closure (the *urgency tendency* to seize) and then desires to remain in a state of closure (the *permanence tendency* to freeze; Kruglanski & Freund, 1983; Kruglanski & Webster, 1996), and they feel distressed when not in a state of closure (Roets & Van Hiel, 2008). A strong need to *avoid* closure is characterized by desiring uncertainty and an unwillingness to commit to a specific belief. Situationally, the need for closure is influenced by the perceived benefits and costs of closure versus openness (Kruglanski & Webster, 1991).

The need for closure has been linked to many interpersonal phenomena, including persuasion (Kruglanski et al., 1993), creative output in groups (Chirumbolo et al., 2004), and prejudice (Van Hiel et al., 2004). A high need for closure (versus a low need for closure) is associated with lower trust in strangers (Acar-Burkay et al., 2014) and lower empathy with dissimilar others (Nelson et al., 2003).

However, less is known about the role of the need for closure on outcomes in close relationships. Past research suggests that high decisiveness (a subscale of the need for closure) is associated with high relationship satisfaction (Rempala, 2009). Many factors within a relationship, including uncertainty, can lead to relationship termination (Quirk et al., 2016). The current project examines if differences in the need for closure are associated with the type of strategy a person uses to end a relationship, and how a person responds to different breakup strategies, especially ghosting. Because breakups can have distressing implications for the initiator and target (Sprecher et al., 1998), examining how individual motivations can influence this process will shed light on a frequent experience (Rhoades et al., 2011).

Ghosting

Ghosting is a strategy for ending a romantic or platonic relationship, either suddenly or gradually, by cutting off all online and in-person communication (Koessler et al., 2019a; LeFebvre et al., 2019; Thomas & Dubar, 2021). This may involve not initiating or responding to phone calls and text messages; blocking a phone number; un-tagging photos, changing a relationship status, or blocking on social media; and cutting ties with mutual acquaintances (Collins & Gillath, 2012; Freedman et al., 2019; Koessler et al., 2019a; Sas & Whittaker, 2013; Smith & Duggan, 2013). Early data on the frequency of ghosting suggests that up to 65% of emerging adults have ghosted a former partner, and up to 72% have been ghosted (Koessler et al., 2019a).

Ghosting is distinct from other relationship dissolution strategies because it does not clearly indicate to the target that the relationship has ended. Rather, the absence of communication leaves the target feeling “puzzled” (LeFebvre et al., 2020), speculating why the initiator has ceased contact (LeFebvre & Fan, 2020; Manning et al., 2019). Thus, ghosting can be

considered a type of ostracism—a form of social exclusion by which a person is ignored or left out—that is used to end a relationship (Freedman et al., 2019).

Ostracism, which can occur within an existing relationship, is a painful experience that threatens a person's basic psychological needs for belonging, self-esteem, meaningful existence, and control (Williams, 2009). Although most research has examined how ostracism threatens the psychological needs of the target, the Responsive Theory of Social Exclusion postulates that the needs of the initiator should also be threatened (Freedman et al., 2016; see also Zadro & Gonsalkorale, 2014). Because ghosting is a distinct relationship dissolution strategy, the motivations for using ghosting and the response to being ghosted may be unique compared to that of direct rejection. Thus, it is beneficial to examine the occurrence of ghosting from the perspective of both the initiator and the target.

The Motivations of the Initiator

Many factors influence the decision to end a close relationship (Joel et al., 2018) and which method to use once this decision is made. For instance, recent research suggests that the decision to use ghosting may be influenced by qualities of the relationship (e.g., the casualness of the relationship; Manning et al., 2019), characteristics of the target (e.g., personality, undesirability; Timmermans et al., 2021), threats to personal safety (Freedman, Hales et al., 2022), or self-blame (e.g., previously the target of ghosting; Navarro et al., 2020). Being the initiator of ghosting (compared to the target) can elicit mixed emotions, such as guilt and relief (Freedman, Powell et al., 2022). Further, the extent to which ghosting is seen as an acceptable way of ending a relationship also varies, with some people indicating that ghosting (versus direct rejection) is the more acceptable breakup strategy (Halversen et al., 2022), whereas others see it is as inappropriate (LeFebvre et al., 2019).

People who engage in the silent treatment—a dyadic form of ostracism related to ghosting—feel their belonging, self-esteem, and meaningful existence needs are threatened (Williams et al., 1998), suggesting that the experience of ghosting is distressing for the initiators. However, ghosting is a less distressing breakup strategy for the initiator than direct rejection (Koessler et al., 2019a), and people may use it for convenience—especially for online relationships (Timmermans et al., 2021)—or to help their interaction partner retain respect and avoid the humiliation that may follow a direct rejection (Brown & Levinson, 1978). Previous research suggests that there are individual differences that predict the preference for using ghosting. For example, people with strong, versus weak, destiny beliefs—a fixed belief that people are either meant to be together or not—are more likely to have used ghosting in the past and express stronger intentions to ghost in the future (Freedman et al., 2019). Furthermore, anxious attachment—having negative self-views in relationships—is lower in people who have used ghosting than those who have been ghosted (Powell et al., 2021). However, other research has failed to find significant correlations between individual differences and ghosting intentions (e.g., self-esteem, assertiveness, sense of power, and empathetic concern; Navarro et al., 2021).

Although the decision by the initiator to use ghosting to end a relationship might be clear and apparent to them (Baxter, 1984), commentators have speculated that ghosting can still create an ambiguous situation for both the initiator and target (LeFebvre et al., 2019). For instance, previous research found that even when a break-up initiator made the unilateral decision to end the relationship, those who used an indirect strategy had a significantly lower likelihood of initial acceptance by the target compared to those who used a direct strategy (Baxter, 1984). In the case of ghosting, up to 40% of targets try to maintain contact with their partner (Koessler et al., 2019b), perhaps because they are unaware that the relationship is ending. This situation prolongs

the breakup and makes the relationship between the partners unclear, even for the initiator. If an initiator anticipates that using ghosting will create an ambiguous situation, then individual differences in the tolerance for ambiguity may influence their desire to use ghosting over other relationship dissolution strategies. The motivational aspect of the need for closure suggests that people should select a breakup strategy that satisfies their comfort level for ambiguity. Thus, we predict that people with a high need for closure would be *less* open to ghosting others—a situation that would create ambiguity in the relationship and may lead to distress.

The Experience for the Target

Although the experience of ghosting can have negative consequences for the initiator of ghosting, these consequences are even more evident for the target. In a sample of adults who used a mobile dating app, 37% of those who had been ghosted blamed themselves for the situation, and 44% reported that it had long-term effects on their mental health (Timmermans et al., 2021). Compared to *ghosting initiators*, targets of ghosting reported higher attachment anxiety (Powell et al., 2021), greater distress (Koessler et al., 2019b), and more negative emotional experiences (i.e., lonelier, sadder, less happy, and less proud; Freedman, Powell et al., 2022).

Recent research also suggests that being ghosted leads to worse well-being outcomes *than being directly rejected*. For instance, when participants were asked to recall a regrettable dating experience, more people recalled a missed opportunity than a rejection, and regret was more anticipated in response to a missed opportunity (Joel et al., 2019). Further, qualitative evidence suggests that targets of ghosting (versus direct rejection) experienced greater surprise, confusion, guilt, and sadness (Pancani et al., 2021). Quantitative evidence suggests that ghosting

targets felt more excluded, and they perceived the breakup as less expected and fair (Pancani et al., 2022).

As mentioned previously, ghosting is a type of ostracism that leaves the target with a sense of uncertainty and lack of closure (LeFebvre et al., 2020; LeFebvre & Fan, 2020; Manning et al., 2019). Because ghosting is closely related to ostracism, it stands to reason that being ghosted also threatens a person's basic psychological needs. For instance, being ghosted signals that the relationship is ending, which should thwart the target's sense of belonging (Williams et al., 2000a). Being ghosted should also threaten a person's needs for self-esteem and meaningful existence by signaling that the target is not valued (Leary, 1990) or worthy of acknowledgment (Williams et al., 2000b). And because targets of ghosting are unable to communicate with the recipient, or at least their attempts to communicate go unanswered, their need for control should also be thwarted (Wesselmann et al., 2010).

Previous research provides initial evidence that being ghosted does threaten psychological needs satisfaction. In one study, participants who were ostracized by a new acquaintance over text message—a similar experience to being ghosted—reported worse mood and lower needs satisfaction than participants who were included (Smith & Williams, 2004). Compared to *ghosting initiators*, targets of ghosting reported lower belongingness, self-esteem, meaningful existence, and control (Freedman, Powell, et al., 2022). Additionally, when compared to *targets of direct rejection*, targets of ghosting reported lower satisfaction of their belonging and control needs, although there was no difference in self-esteem and meaningful existence (Pancani et al., 2022). Thus, we predict that targets of ghosting would experience lower psychological needs satisfaction than targets of direct rejection.

In addition to the four basic psychological needs, ostracism has been shown to threaten a person's self-certainty (e.g., Hales & Williams, 2018). This suggests that differences in the tolerance for uncertainty should be associated with varying responses to ostracism, as well as ghosting. As stated earlier, people with a high need for closure are more likely to respond to uncertainty by engaging in activities that attain closure, jumping to conclusions, and experiencing negative affect when attempts to achieve closure are frustrated (Kruglanski & Webster, 1996; Webster & Kruglanski, 1994). When ghosted, this may translate to attempting to communicate with the initiator, coming to quick (and possibly inaccurate) assumptions for why the initiator ceased communication, and experiencing intense negative affect during this period of uncertainty. Although being ghosted should be a distressing experience for everyone, those who have a high need for closure should be especially susceptible to negative feelings and cognitions because they may fixate on understanding why the initiator has gone quiet. Thus, we predict that for targets of ghosting, those with a higher need for closure would experience lower psychological needs satisfaction.

The Present Research

Ghosting continues to be a commonly used break-up strategy, even though it often results in negative well-being outcomes for the target (LeFebvre et al., 2019; Manning et al., 2019). Thus, it is valuable to understand the individual differences that influence both the motivations of the ghosting initiator, as well as the experience for the ghosting target. Although the decision by the initiator to use ghosting may be distinct, this breakup strategy still creates an ambiguous situation for both partners, implying that one's tolerance for uncertainty would be related to both ghosting motivations and experiences. Thus, Studies 1 and 2 explore how the need for closure is

associated with a person's intentions to use ghosting, and Study 3 examines how the need for closure moderates a person's response to being ghosted.

Ghosting thrives due to the proliferation of online dating, which is most commonly used by emerging adults (Smith & Duggan, 2013), who have the largest portion (41%) of single people and the highest proportion (21%) of relationships to have met online (Brown, 2020). Emerging adulthood is the transitional life period between adolescence and adulthood (18-29 years; Arnett, 2000). This unique developmental period usually involves romantic and sexual exploration, with relationships that are often unstable and fleeting compared to those of older adults (van Dulmen et al., 2014). Online dating facilitates these relational goals and suggests that emerging adults are more likely to use technology-mediated communication to initiate, maintain, and end their relationships. In turn, emerging adults are uniquely positioned to have many ghosting experiences and research supports this notion (LeFebvre et al., 2019). Thus, the present studies specifically focus on the experience of ghosting within samples of emerging adults.[21]

Study 1

The first study explored the association between the dispositional need for closure and ghosting intentions in a sample of U.S. emerging adults. We hypothesized that a greater dispositional need for closure would be associated with lower ghosting intentions. Our hypothesis, stopping rule, and analyses were preregistered prior to data collection (https://aspredicted.org/35z7h.pdf).

[21] The data and materials used in this research are publicly posted. The registrations, data, and materials can be obtained at https:// researchbox.org/777

Method

Participants

Five hundred and seventy-three U.S. emerging adults were recruited from Prolific Academic. Following our preregistered exclusion criteria, 19 participants were excluded for failing a single-item attention check and one participant was excluded for reporting they were over 29-years-old. The final sample included 553 participants (49.00% women; 47.60% men; 3.44% other/non-binary; M_{age} = 23.95, SD_{age} = 3.17). The sample was predominantly White (60.2%), followed by 15.7% Asian, 9.0% Black, 8.1% Latinx, 5.8% multiracial, 0.4% Native American, and 0.7% of another race. Most of the sample was heterosexual (67.80%), followed by 20.40% bisexual, 8.32% lesbian or gay, and 3.44% were another sexual orientation. About half of the sample (47.22%) had a bachelor's degree or higher. A sensitivity power analysis indicates that with an $\alpha = .05$ and 95% power, our final sample size can detect an effect of $r = .15$ (G*Power; Faul et al., 2007).

In line with previous research examining the prevalence of ghosting in emerging adults (Koessler et al., 2019a), our findings suggest that most have ghosted a former partner (62.90%) and have been ghosted by a former partner (66.00%). We also asked participants to report the percentage of people their age that they believe use ghosting. Interestingly, the average response was 57.97%, which is below the proportion of people in this sample who have ghosted a former partner.

Procedure and Materials

The need for closure and ghosting intentions measures were presented in random order.

Dispositional Need for Closure. Participants completed the brief version of the Need for Closure Scale (Roets & Van Hiel, 2011) as a measure of the dispositional need for closure.

Participants rated 15 items on a seven-point Likert scale (1 = *Completely disagree*, 7 = *Completely agree*). Example items include, "I don't like situations that are uncertain" and "I enjoy having a clear and structured mode of life." Greater scores averaged across the 15 items indicate a greater need for closure ($\alpha = .87$).

Ghosting Intentions. To measure ghosting intentions, we first provided participants with a definition of ghosting: "Ghosting is when someone ends a friendship, romantic relationship, or casual dating situation by cutting off all communication without explanation (e.g., not responding to calls or text messages, blocking on social media or dating apps)." Participants then rated how likely they were to use ghosting in 19 situations on a seven-point Likert scale (1 = *Very unlikely*, 7 = *Very likely*). These situations were modified from previous research (Freedman et al., 2019) and include short- and long-term relationships with friends and romantic partners. Example items include, "How likely are you to use ghosting to end a relationship after 1 date or less" and "How likely are you to use ghosting to end a friendship with someone who you have many mutual friends/acquaintances?" Greater scores averaged across the 19 items indicate greater intention to use ghosting ($\alpha = .95$).

Results and Discussion

Data for all three studies were analyzed in R (R Core Team, 2020). We conducted a bivariate correlation to examine the association between dispositional need for closure (M = 4.55, SD = 0.90) and ghosting intentions (M = 2.81, SD = 1.25). There was a significant *positive* association between the need for closure and ghosting intentions, $t(551) = 3.43$, $p < .001$, $r = .14$, 95% CI [.06, .23], such that participants with a greater need for closure were more likely to use ghosting as a relationship dissolution strategy.

Using a Williams' test from the psych package (version 2.2.5; Revelle, 2022) to assess the difference between two dependent correlations, we investigated whether the correlation between the need for closure and ghosting intentions is different for romantic relationships, $t(551) = 2.86$, $p = .004$, $r = .12$, 95% CI [.04, .20], and friendships, $t(551) = 3.81$, $p < .001$, $r = .16$, 95% CI [.08, .24]. Although participants reported slightly more willingness to use ghosting to end a friendship ($M = 2.89$, $SD = 1.42$) compared to a romantic relationship ($M = 2.77$, $SD = 1.31$), the correlation with need for closure did not differ between the relationship types, $t(550) = -1.19$, $p = .240$.

As exploratory analyses, we examined if previously ghosting a former partner or having been ghosted by a former partner moderated the association between the need for closure and ghosting intentions. To do so, we ran separate linear regressions on ghosting intentions with previous ghosting behavior or experience included in the models, along with the need for closure and the appropriate interaction term (see Appendix C for a detailed description of the exploratory results of all studies). Neither past ghosting behavior, $b = -0.19$, $SE = 0.11$, $p = .084$, nor experience, $b = -0.17$, $SE = 0.12$, $p = .151$, moderated the association between the need for closure and ghosting intentions. These results suggest that the influence of the need for closure on intentions to use ghosting as a relationship dissolution strategy does not depend on if someone has ghosted a partner or been ghosted by a partner in the past.

Results from this first study suggest that a greater need for closure is associated with *greater* intentions to use ghosting to end both friendships and romantic relationships. These results contradict our original hypothesis of a negative association. One possible explanation for these findings is that people with a higher need for closure are more likely to end a relationship

than people with a lower need for closure, regardless of the breakup method. Thus, we conducted a follow-up study to investigate this possibility.

Study 2

The second study examined the association between the dispositional need for closure and breakup intentions (i.e., ghosting and direct rejection) in a sample of U.S. emerging adults. We hypothesized that, in accordance with the results of Study 1, a greater dispositional need for closure would be associated with *greater* ghosting intentions. We also investigated if the need for closure association differed by breakup type, though we did not have a hypothesis for this research question. Our hypothesis, stopping rule, and analyses were preregistered prior to data collection (https://aspredicted.org/se3dm.pdf). Given the clear (albeit unexpected) positive association between the need for closure and ghosting intentions observed in Study 1, we preregistered a one-tailed test to maximize power.

Method

Participants

Four hundred and thirty-four U.S. emerging adults were recruited from Prolific Academic. Following our preregistered exclusion criteria, 21 participants were excluded for failing a single-item attention check and two participants were excluded for reporting they were over 29-years-old. The final sample included 411 participants (48.90% women; 47.90% men; 3.16% other/non-binary; M_{age} = 24.23, SD_{age} = 3.11). The sample was predominantly White (54.5%), followed by 19.2% Asian, 13.9% Latinx, 5.8% Black, 5.8% multiracial, 0.5% Native American, and 0.2% of another race. Most of the sample was heterosexual (71.50%), followed by 18.70% bisexual, 5.84% lesbian or gay, and 3.89% were another sexual orientation. About half of the sample (46.26%) had a bachelor's degree or higher. A sensitivity power analysis

indicates that with an α = .05 and 95% power, our final sample size can detect an effect of *r* = .16 for a one-tailed test and *r* = .18 for a two-tailed test.

The reported prevalence of ghosting in this sample is consistent with the first study, such that most participants have ghosted a former partner (66.20%) and have been ghosted by a former partner (70.60%). Additionally, participants believe 60.21% of people their age use ghosting on average, which is again below the proportion of people in this sample who have ghosted a former partner.

Procedure and Materials

The need for closure and breakup intentions measures were presented in random order. Within the breakup intentions block, ghosting intentions and direct rejection intentions were presented in random order.

Dispositional Need for Closure. Participants completed the same measure of dispositional need for closure as in Study 1 (α = .86).

Ghosting Intentions. Ghosting intentions were measured using the same measure as in Study 1 (α = .95).

Direct Rejection Intentions. To measure direct rejection intentions, we first provided participants with a definition of direct rejection: "Direct rejection is when someone ends a friendship, romantic relationship, or casual dating situation by directly communicating that they want to end the relationship." Participants then used a seven-point Likert scale (1 = *Very unlikely*, 7 = *Very likely*) to indicate how likely they were to use direct rejection in the same 19 situations from the ghosting intentions measure. Greater scores averaged across the 19 items indicate greater intention to use direct rejection (α = .93).

Results and Discussion

To examine the association between dispositional need for closure (M = 4.67, SD = 0.87) and ghosting intentions (M = 2.86, SD = 1.22), we conducted a preregistered one-tailed bivariate correlation. There was not a significant association between need for closure and ghosting intentions, $t(409) = 1.22$, $p = .113$, $r = .06$, 95% CI [-.02, 1.00]. We conducted a two-tailed bivariate correlation to examine the association between the dispositional need for closure and direct rejection intentions (M = 4.57, SD = 1.25). There was not a significant association between need for closure and direct rejection intentions, $t(409) = -0.33$, $p = .743$, $r = -.02$, 95% CI [-.11, .08].

Using a Williams' test, we investigated whether the correlation between the need for closure and breakup intentions is different for ghosting and direct rejection. The correlation with the need for closure did not differ between the breakup methods, $t(408) = -0.55$, $p = .580$. However, an exploratory, non-preregistered, paired-samples t-test suggested that participants reported more willingness to use direct rejection than ghosting to end a relationship, $t(410) = -17.30$, $p < .001$, $d = -1.53$.

Similar to Study 1, we conducted exploratory analyses to examine if previously ghosting a former partner or having been ghosted by a former partner moderated the association between the need for closure and ghosting intentions. Neither past ghosting behavior, $b = -0.07$, $SE = 0.13$, $p = .619$, nor experience, $b = -0.04$, $SE = 0.15$, $p = .768$, moderated the association between the need for closure and ghosting intentions.

Even though the prevalence of ghosting is high, results from this study suggest that emerging adults are more likely to end a relationship by direct rejection, and the role of the need for closure in making this decision is still unclear. Results from this study did not replicate the

significant positive association between the need for closure and ghosting intentions found in Study 1. However, a combined analysis pooling both datasets estimates a composite *r* of .11, 95% CI [.05, .17], suggesting that if there is an association, it is small and in the opposite direction than first predicted. Overall, results from Studies 1 and 2 did not provide evidence that people with a higher need for closure are more likely to end relationships, by either ghosting or direct rejection. This may be because the initiator of ghosting may not experience uncertainty and ambiguity when they ghost someone. Instead, the ambiguity may lie solely with the target of ghosting, which is the focus of the final study.

Study 3

The third study examined the effect of being ghosted on psychological needs satisfaction in a sample of U.S. emerging adults. We hypothesized that being ghosted (vs. included or directly rejected) would cause lower psychological needs satisfaction. We also hypothesized that the need for closure would moderate the effect of being ghosted on needs satisfaction, such that for ghosted participants, having a higher need for closure would be associated with lower needs satisfaction. Our hypotheses, stopping rule, and analyses were preregistered prior to data collection (https://aspredicted.org/jk5nw.pdf).

Method

Participants

Six hundred and seventy U.S. emerging adults were recruited from Prolific Academic. Following the preregistered exclusion criteria, 59 participants were excluded for opting out of the writing prompt because they did not have a relevant situation to recall, five participants were excluded for failing two attention checks, 56 participants were excluded for failing the manipulation check, and five participants were excluded for reporting they were over 29-years-

old. Exclusions did not differ based on condition, $\chi^2(2) = 3.40$, $p = .183$, $V = 0.07$. The final sample included 545 participants (51.38% women, 45.50% men, 3.12% other/non-binary; $M_{age} = 24.68$, $SD_{age_e} = 3.23$). The sample was predominantly White (63.49%), followed by 14.50% Asian, 12.11% Black, 4.77% Latinx, 4.22% multiracial, 0.37% Native American, and 0.55% of another race. Most of the sample was heterosexual (69.91%), followed by 19.82% bisexual, 5.69% lesbian or gay, and 4.59% were another sexual orientation. About half of the sample (55.42%) had a bachelor's degree or higher.

Given the final sample size and three-condition design, a sensitivity power analysis indicates that with an $\alpha = .05$ and 95% power, we can detect a group difference in needs satisfaction of $d = 0.37$ and a simple effect of the need for closure within each experimental condition of $r = .25$. The reported prevalence of ghosting in this sample is consistent with the previous two studies, such that most participants have ghosted a former partner (65.69%) and have been ghosted by a former partner (74.86%).

Procedure and Materials

Dispositional Need for Closure. Participants first completed the same measure of dispositional need for closure as in Studies 1 and 2 ($\alpha = .89$).

Experimental Manipulation. Next, participants completed an autobiographical reliving paradigm from the ostracism literature (e.g., Hales et al., 2018; Pickett et al., 2004) in which they reflected on a situation from their own lives and wrote about it for four minutes. Participants were randomly assigned to receive one of three prompts. Participants in the included (control) condition ($n = 190$) were asked to write about a time when someone "expressed that they wanted to continue and/or maintain a friendship, romantic relationship, or casual dating situation." Participants in the ghosted condition ($n = 183$) wrote about a time when someone ended a

relationship by "suddenly cutting off all communication without explanation." Participants in the directly rejected condition (n = 172) wrote about a time when someone "directly communicate[d] that they wanted to end the relationship." Nearly half (49%) of participants wrote about a friendship, followed by a romantic relationship (29%), and then a casual dating situation (22%).

Basic Psychological Needs Satisfaction. Immediately after writing, participants completed the eight-item Brief Need Threat Scale (Hales et al., 2015) as a measure of satisfaction for four basic psychological needs: belonging, self-esteem, meaningful existence, and control. Using a seven-point Likert scale (1 = *Not at all*, 7 = *Extremely*), participants responded to eight items by indicating "how [they] felt during the situation [they] just described." Example items include, "I felt liked" and "I felt powerful." Greater scores averaged across the eight items indicate greater psychological needs satisfaction (α = .94).

Results and Discussion

To test the main effect of condition on psychological needs satisfaction (M = 3.76, SD = 1.85), we conducted a multiple linear regression with the condition variable dummy-coded so that the ghosted condition was the reference group. The model was significant, accounting for 72.27% of the variance (R^2) in needs satisfaction, $F(2, 542) = 710.00$, $p < .001$. As predicted, participants who recalled a time when they were ghosted (M = 2.45, SD = 1.08) reported significantly lower needs satisfaction than participants who recalled a time when they were included (M = 5.90, SD = 0.78), b = 3.46, $t(542) = 34.35$, $p < .001$, 95% CI [3.27, 3.66], d = 1.47, or directly rejected (M = 2.81, SD = 1.04), b = 0.37, $t(542) = 3.59$, $p < .001$, 95% CI [0.17, 0.57], d = 0.15. This study provides further evidence that being ghosted by someone may lead to worse well-being outcomes than being directly rejected.

To test the moderation effect, we added the need for closure (M = 4.75, SD = 0.96) and the corresponding interaction terms to the regression model. The interaction was significant, accounting for an additional 1.32% of the variance (R^2) in needs satisfaction, $F(5, 539) = 300.37$, $p < .001$ (see Figure 4.1). The interaction between need for closure and the ghosted vs. included comparison was significant, $b = 0.42$, $t(539) = 4.05$, $p < .001$, 95% CI [0.21, 0.62], suggesting that the association between need for closure and needs satisfaction was different for ghosted and included participants. However, the interaction between need for closure and the ghosted vs. directly rejected comparison was not significant, $b = 0.01$, $t(539) = 0.12$, $p = .907$, 95% CI [-0.20, 0.22], suggesting that the association between need for closure and needs satisfaction was similar for ghosted and directly rejected participants.

To probe the interaction, we examined the simple effect of the need for closure on needs satisfaction for each condition. As predicted, for participants in the ghosted condition, having a higher need for closure was associated with lower needs satisfaction, $b = -0.23$, $t(539) = -3.29$, $p = .001$, $r = -.22$, 95% CI [-.35, -.07]. Unexpectedly, having a higher need for closure was also associated with lower needs satisfaction for directly rejected participants, $b = -0.22$, $t(539) = -2.82$, $p = .005$, $r = -.20$, 95% CI [-.34, -.05], and with *greater* needs satisfaction for included participants, $b = 0.18$, $t(539) = 2.45$, $p = .015$, $r = .22$, 95% CI [.08, .35]. These results support our hypothesis that having a higher need for closure is related to more negative outcomes after being ghosted, however, it also appears to amplify the negative outcomes of being directly rejected *and* the positive outcomes of being included.

Finally, we conducted exploratory analyses to examine if the interaction between the need for closure and condition on needs satisfaction depended on the type of relationship participants wrote about or whether they had ghosted someone in the past. An analysis of

covariance (ANCOVA) on needs satisfaction revealed that the three-way interaction between the need for closure, condition, and relationship type was not significant, $F(4, 527) = 0.79$, $p = .530$, $\eta_p^2 = .006$, suggesting that the interaction between the need for closure and condition did *not* depend on whether participants wrote about a friendship, romantic relationship, or casual dating situation. A separate ANCOVA on needs satisfaction revealed that the three-way interaction between the need for closure, condition, and previous ghosting behavior was also not significant, $F(2, 533) = 0.77$, $p = .463$, $\eta_p^2 = .003$, suggesting that the interaction between the need for closure and condition did *not* depend on whether participants had ghosted someone else in the past. Although we examined past ghosting experience as an additional moderator in Studies 1 and 2, we were not able to do this for Study 3 because participants in the ghosting condition were required to have been previously ghosted, thereby violating the assumption of equal variances across groups that is needed for ANCOVA.

General Discussion

Advancements in social technologies have proliferated the use of ghosting to end relationships, especially among emerging adults. Indeed, results from our studies suggest that there is a high prevalence (63%-75%) of ghosting among emerging adults, which is in line with previous findings (e.g., Koessler et al., 2019a). However, despite its popularity, results from Study 2 suggest that people are more willing to directly reject someone than to ghost them, all else being equal.

The aim of the current project was to explore the relationship between the need for closure and the experience of ghosting for both the initiator and target. Previous research has found that individual differences are related to a person's willingness to use ghosting to end a relationship (e.g., Freedman et al., 2019; Powell et al., 2021), but the role of the need for closure

is still unclear. We had hypothesized that having a greater need for closure would be associated with lower ghosting intentions (i.e., a negative association), but our studies had mixed results. For Study 1, there was a significant positive association between the need for closure and ghosting intentions, and there was no significant association for Study 2. The combined data from both studies showed that there is a small, positive association between the need for closure and ghosting intentions, which does not support our initial prediction.

Overall, these results suggest that using ghosting to end the relationship may not create an ambiguous experience for the initiator, perhaps because they are making the unilateral decision to end the relationship using their preferred method. The initiator may know—with certainty—that they will no longer be communicating with the target, even if the target still attempts to communicate with them. This decision may allow them to achieve closure, thereby preventing ambiguity. This would also explain why we did not find a difference in the need for closure association between ghosting intentions and direct rejection intentions. It seems that for the initiator, ghosting provides just as much closure as direct rejection. Lefebvre and colleagues (2019) suggest that ghosting is a sudden experience for the initiator, and this suddenness may indicate the achievement of closure.

Breakups are distressing, so it is important to understand how their negative outcomes can be mitigated (e.g., Sprecher et al., 1998). Ghosting can be particularly distressing because, like ostracism more broadly, it leaves the target in a period of uncertainty. As we predicted, results from Study 3 show that being ghosted threatens basic psychological needs more than being included and directly rejected. These results are in line with previous research suggesting that the experience of being ghosted is harmful to well-being, even more so than direct rejection (Pancani et al., 2021, 2022; Timmermans et al., 2020).

Additionally, results from Study 3 supported our prediction that having a higher need for closure is associated with lower needs satisfaction after being ghosted. However, we unexpectedly found a similar pattern of results for direct rejection. One possible explanation for this finding is that we measured participants' reflexive needs satisfaction (i.e., how people felt *during* the experience) rather than the satisfaction of their reflective needs (i.e., how people feel *after* the experience). If the study procedure had instead measured participants' needs satisfaction a few days after the recalled experience, it is possible that those who were directly rejected may have recovered more quickly because they were able to achieve closure, whereas those who were ghosted may have been worse off because they were still ruminating on the ambiguous situation.

Another explanation for this finding could be that the *actual* ambiguity of a ghosting experience may be less important than the *perceived* ambiguity by the target. People with a higher need for closure are prone to "seize and freeze" when closure is obtained (Kruglanski & Webster, 1996). This could mean that no matter how a relationship is ending—whether by ghosting or direct rejection—if a person with a high need for closure *perceives* that the relationship is ending, they will react accordingly. In contrast, people with a lower need for closure tend to suspend judgment in ambiguous situations. In the case of breakups, this could mean that a person with a need to *avoid* closure may deny or second guess that that relationship is ending, regardless of whether they are being ghosted or directly rejected. Indeed, restarting a relationship after a breakup (i.e., on-off relationships) are quite common among young adults (Dailey et al., 2009).

We also found that for participants who recalled a time when they were included, having a higher need for closure was associated with *greater* needs satisfaction. Together, these findings

suggest that the need for closure may play an important role in how people respond in their close relationships, by amplifying the effects of both positive (being included) and negative (being ghosted and directly rejected) experiences.

Strengths, Limitations, and Future Directions

The present research adds to the close relationships, social exclusion, and social cognition literatures by addressing a timely and prevalent experience among emerging adults. To our knowledge, the need for closure has yet to be examined within the context of social exclusion—an overarching term used to incorporate ostracism (a construct similar to ghosting) and rejection (which we refer to as direct rejection in our studies to clearly differentiate it from ghosting). The results from Studies 1 and 2, while inconsistent, suggest that individual differences in the need for closure may not influence the role of the initiator of exclusion (i.e., ostracizer and rejecter). However, results from Study 3 suggest that the target of exclusion is impacted by their need for closure. Although the current research is limited to close relationships, future research could examine if the role of the need for closure for both the initiator and target generalizes to social exclusion in other research contexts (e.g., the ostracism paradigm Cyberball), and in daily life more generally.

Another strength of the present research is that we used a general trait measure of the need for closure, which provided a conservative test for whether this trait plays a motivational role in the context of close relationships. This is consistent with theoretical conceptions of the need for closure as a domain-general stable trait (Webster & Kruglanski, 1994). Although we did find significant results using this general trait scale, the use of a relationships-specific measure of the need for closure may have produced effects of greater magnitude. A relationships-specific measure also would allow for a more nuanced understanding of our findings. Future research

could adapt the need for closure measure to focus on the desire to avoid ambiguity within the context of close relationships, which could be used to examine motivational phenomena and relationship outcomes at all stages of the relationship process, including ghosting motivations and experiences.

Ghosting involves the (lack of) communication between two people, so another strength of the present research is that it examines the influence of the need for closure on the experience of ghosting from both the initiator's and target's perspectives. Past research and theory suggest that individual differences in the need for closure should impact the experience of ghosting, such that those with a high need for closure would be less likely to use ghosting as a relationship dissolution strategy and have a more negative response to being ghosted. However, by examining the impact of the need for closure from both perspectives, we can conclude that the need for closure is more strongly related to the target's experience of ghosting, rather than the initiator's experience. Future research can experimentally manipulate a person's role in the experience of ghosting (i.e., initiator or target), which would expand upon the current findings by examining if the impact of the need for closure depends on the relationship role, directly bridging the current Studies 1 and 2 with Study 3. This future direction would also advance research on the experience of being the initiator of ghosting (on well-being or psychological needs satisfaction), which we did not investigate in the current project.

Because we measured—rather than manipulated—the dispositional need for closure across our studies, we could not make causal claims about the role of the need for closure in ghosting experiences. Future research could experimentally manipulate the situational need for closure by using time pressure (Kruglanski & Webster, 1991) or ambient noise (Kruglanski et al., 1993) paradigms. In addition to the benefits of being able to examine causal effects,

conceptually replicating these results with both dispositional and situational needs for closure would provide useful information about their robustness. Indeed, past studies that have measured the dispositional need for closure and experimentally manipulated the situational need for closure have found similar results (Webster & Kruglanski, 1994). Thus, it is reasonable to expect that our findings would replicate with experimental operationalizations.

To examine the experience of being the target of ghosting, Study 3 implemented an open-ended recall paradigm. There are limitations to this method, such as participants' difficulty in remembering their emotions exactly as they were experienced (Robinson & Clore, 2002). However, we believe this is currently the strongest and most ecologically valid method at our disposal for manipulating the experience of being ghosted. Autobiographical reliving paradigms are often used in ostracism research (e.g., Hales et al., 2018; Pickett et al., 2004) because they have the ability to place participants in the same state of mind as when an event occurred, thereby influencing their current mood (Baker & Guttfreund, 1993). Additionally, because ghosting is an understudied phenomenon, it is important that researchers examine and understand peoples' lived experiences of being ghosted before attempting to simulate these experiences. Therefore, these results can be used to inform a traditional laboratory experiment designed to manipulate the experience of being ghosted and to measure its immediate effects.

A limitation of this research is that our samples are comprised of people residing in the United States. Thus, we are unable to comment on how the need for closure plays a role in the experience of ghosting for people living in other countries. However, a strength of this research is that the gender and racial breakdown of our samples are similar to the general United States population (U.S. Census Bureau, 2021). In terms of age, we intentionally limited our sample to emerging adults so that we could target an age group that is likely to experience ghosting (see the

Present Research section for a more thorough explanation). However, emerging adults only make up about 20% of the total United States population (U.S. Census Bureau, n.d.), and with our current research, we are unable to generalize our findings to people younger than 18 or older than 29 years. Future research could examine the generalizability of these findings with more age-diverse samples and samples of non-U.S. residents.

Conclusion

Research on ghosting is in its infancy, and the present work enhances our understanding by exploring a motivation—the need for closure—that has yet to be examined within the context of close relationships. We found that the need for closure is related to the different experiences of initiators and targets. For break-up initiators, those with a higher need for closure had greater intentions to use ghosting to end a relationship. This suggests that the decision to use ghosting is just as definite as the decision to use direct rejection, thereby leaving little ambiguity about the relationship status in the eyes of the initiator. For break-up targets, being ghosted led to lower well-being than being directly rejected, and those with a higher need for closure felt even worse after being ghosted or directly rejected. This research contributes to the psychological, sociological, and communication fields by revealing how ghosting fares as a method of (not) communicating a breakup, as well as how the trait motivation of the need for closure influences one's choice to use ghosting and response to being ghosted. These findings could be further explored and developed to help practitioners and developers generate tools for emerging adults to navigate dating in this ever-changing online context.

The current research sheds light on a modern relationship dissolution strategy that has become prevalent among emerging adults, which in turn impacts their well-being. Rejection—in

any form—is a painful experience, and research that explores the ending of close relationships further adds to our understanding of the causes and consequences of social (dis)connection.

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Figures

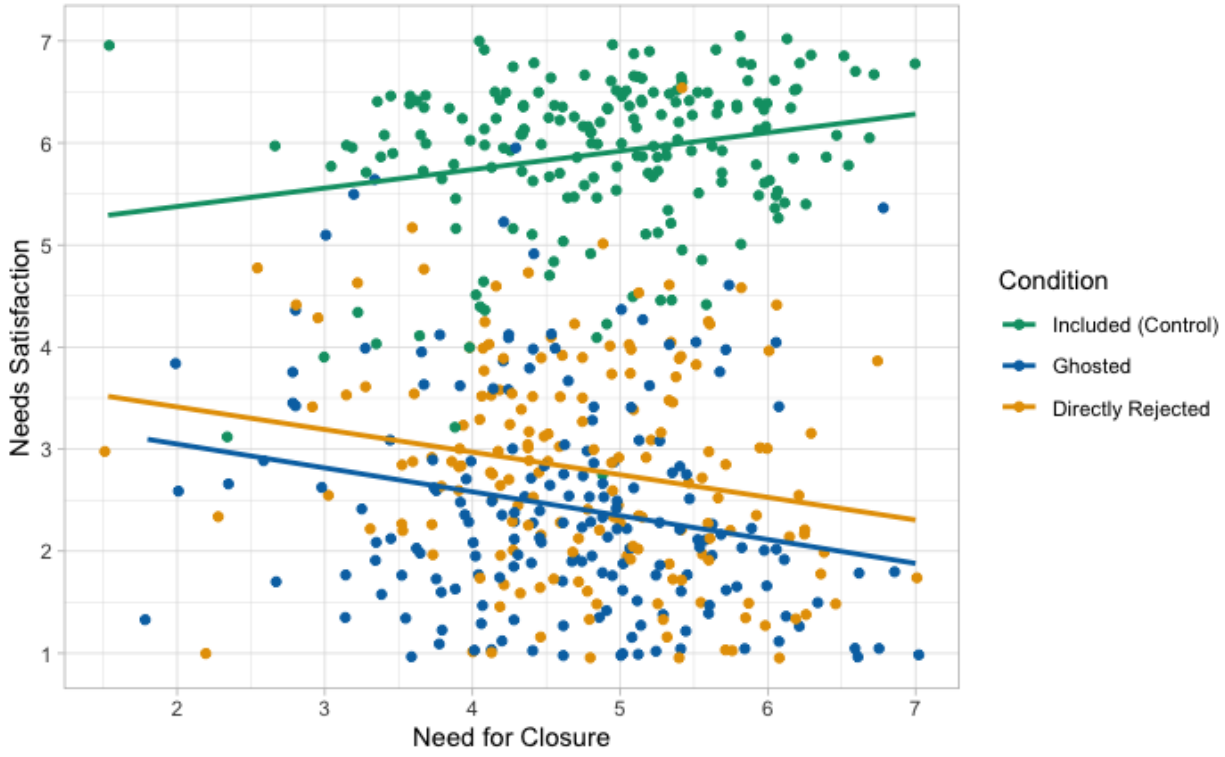

Figure 4.1. Association Between the Need for Closure and Needs Satisfaction by Condition for Study 3. Each dot represents an individual participant. Data were jittered to ease interpretation.

CHAPTER 5

GENERAL DISCUSSION

Social relationships contribute to better psychological well-being (Diener & Seligman, 2002; Sun et al., 2020), physical health (Cohen, 2021; Cohen et al., 1997; Valtorta et al., 2016), and longevity (Holt-Lunstad et al., 2010, 2015). As the nation faces a loneliness epidemic (Office of the Surgeon General, 2023), it is imperative that researchers investigate how people can foster meaningful social relationships and satisfy their need for belonging (Baumeister & Leary, 1995). Social technologies, such as smartphones, have become ubiquitous in everyday life, forever changing how people communicate with each other. Thus, research investigating relationship processes must consider the influential role of social technologies. The focus of this dissertation was to consider how social technologies serve as a context that influences how people initiate, maintain, and end their social relationships. Specifically, this research examined why smartphones might displace social interactions with nearby strangers (Paper 1), how people feel after dyadic and group conversations over video chat (Paper 2), and how ghosting intentions and experiences vary by one's need for closure (Paper 3).

Summary of Findings

The aim of Paper 1 was to further understand why people choose to use their smartphones in daily life. We considered that people might choose to use their smartphone because they expect solitude to be an aversive experience (Wilson et al., 2014), they expect using their smartphone to be more enjoyable than talking to a stranger (Boothby et al., 2018; Epley & Schroeder, 2014), or they prioritize the affordances of their smartphone (e.g., information,

entertainment) over talking to a stranger (Hunter et al., 2018; Kushlev et al., 2017). In line with our predictions, participants expected sitting alone with their thoughts to be aversive, and they experienced it as such. These findings replicates past work demonstrating that solitude is unpleasant (Wilson et al., 2014), especially when it is not freely chosen (Nguyen et al., 2022). Further, this suggests that one reason people may use their smartphones in daily life is to avoid the aversiveness of solitude.

When comparing people's expectations for and experiences with using their smartphone compared to talking to a stranger, we found that participants expected talking to a stranger to be more enjoyable, and they experienced it as such. Talking to a stranger even boosted mood compared to baseline. These findings align with previous work demonstrating that talking to strangers is an enjoyable experience (Epley & Schroeder, 2014; Gunaydin et al., 2021; Sandstrom & Dunn, 2014). However, it seems that people may still expect to enjoy social interactions more than using their smartphone, even though previous research has found that people underestimate how much they will enjoy a conversations and be liked by others (i.e., the liking gap; Boothby et al., 2018). Still, participants ranked specific screen time activities—watching television and texting—as preferable to talking to a stranger, providing evidence that the mood-boosting benefits of talking to a stranger may not always be a priority in daily life.

Next, the aim of Paper 2 was to examine how adding more people to a conversation changes the process of developing intimacy and enjoyment when in-person and over video chat. Contrary to our predictions, video chat conversations were overall just as intimate and enjoyable as in-person conversations. However, when considering the number of people involved, we discovered differences. Conversations with more people were *less* intimate when in person, and even more so when over video chat. Further, conversations with more people were *more*

enjoyable when in person, but either just as enjoyable or *less* enjoyable when over video chat. In other words, participating in group conversations over video chat instead of in person seemed to exacerbate the negative consequences for intimacy, while also mitigating the boosting effects for enjoyment.

Our findings are in line with previous research demonstrating similar affiliative outcomes in dyadic conversations over video chat and in person (Croes et al., 2019; Sprecher, 2014, 2021). However, our findings extend this area of research to consider group conversations. Specifically, this work demonstrates that conversations with more people can present a *many minds problem* (Cooney et al., 2020), especially over video chat. Further, although theory suggests that people can adapt to these mediums (Walther, 1992, 1996), it seems that this may be harder to do in group conversations over video chat.

Finally, the aim of Paper 3 was to examine how a motivational disposition—the need for closure—is related to a person's intention to use ghosting to end a relationship, as well as their experience after being ghosted. According to this research, ghosting is common among emerging adults, with 63%–75% of participants having experience with this dissolution strategy. Surprisingly, for participants who wrote about a ghosting experience, most wrote about a friendship (56%), followed by a casual dating situation (27%), and then by a romantic relationship (17%). This suggests that the use of technology to end a relationship is not limited to those that begin over technology. Further, when examining the experience of targets, participants who wrote about a ghosting experience reported lower psychological needs satisfaction than those who wrote about a time when they were directly rejected, which aligns with previous research (Pancani et al., 2021, 2022).

When examining the role of the need for closure in ghosting intentions, we found that people with a high need for closure were slightly *more* likely to intend to use ghosting to end a relationship. This was contrary to our prediction that the need for closure would be associated with *lower* ghosting intentions, suggesting that the decision to use ghosting provides closure even if the target continues to reach out. For the targets of ghosting, having a higher need for closure was predictably associated with lower psychological needs satisfaction. However, we also found that the need for closure and psychological needs satisfaction were negatively associated for directly rejected participants, and they were positively associated for included participants. Overall, these findings suggest that having a high need for closure may amplify both positive and negative relationship experiences, and it may not be limited to periods of uncertainty like being ghosted.

Strengths, Limitations, and Future Directions

When considering the results of these papers together, there are several strengths and implications worth noting. First, this body of work touches on three distinct relationship processes—initiation/displacement, maintenance, and dissolution—which were examined among 3,833 participants across 10 individual studies. This body of work also utilized various complementary research designs, including self-report surveys, experience sampling, recall experiments, and laboratory experiments, thereby providing an expansive and robust investigation of these processes.

Although social technologies were a throughline across these three papers, this body of work was based on theories from the social cognition, motivation, communication, and intimate relationships literatures. The research presented here not only examined these areas in a new context but extended theory in these respective literatures. For instance, Paper 2 not only

examined intimacy development in video chat interactions, but it also provided a first look at how adding more people to a conversation leads to a *many minds problem* (Cooney et al., 2020) when in-person and over video chat. Similarly, Paper 3 was not only the first to examine the need for closure in relation to ghosting, but it also extended this theory to the realm of intimate relationships, providing a glimpse of how the need for closure may contribute to relational processes beyond ghosting. Thus, this body of work further extends these areas of literature to the context of social technologies.

Another strength of the present work is that it was not limited to one type of relationship. For instance, Paper 1 considered how smartphones displace interactions with strangers, Paper 2 examined how people talk to known and close others over video chat, and Paper 3 investigated how people end existing relationships with friends and romantic/dating partners. However, considering different types of relationships across the three papers also limited our ability to connect these papers together to demonstrate how a single relationship progresses across time. Future research focusing on social technologies may explicitly examine one type of relationship (e.g., friendships or romantic relationships) to better understand the course of how people initiate, maintain, and end these relationships vis-à-vis social technologies.

Although the present research examined three distinct relationship processes that may be influenced by social technologies, this work excluded other processes that can also be influenced. For instance, although Paper 1 examined how smartphones can displace interactions with strangers, it did not examine how smartphones interfere with ongoing in-person conversations (e.g., Dwyer et al., 2018; Kushlev & Dunn, 2019), nor did it explicitly examine how people initiate relationships *through* social technologies like dating apps (e.g., LeFebvre, 2018). Similarly, Paper 2 examined how dyadic and group conversations occur over video chat,

but it did not examine other mediums like texting or phone calls (e.g., Sprecher, 2014, 2021). Finally, although Paper 3 examined the new dissolution strategy of ghosting, it did not examine other new strategies that are also proliferated through social technologies, such as breadcrumbing (i.e., the act of keeping a person interested by sending occasional messages without commitment; Navarro et al., 2020) and orbiting (i.e., when a ghosting initiator continues to engage with the target indirectly by following them on social media and occasionally engaging with their posts; Pancani et al., 2021). Although these various processes have been examined previously, this is still a gap in the present work's contribution to our overall understanding of how relationship processes are influenced by social technologies.

Another limitation of the present work is that the data collected across these papers came from only one partner's perspective. Thus, we were not able to examine dyadic or group processes in each of the three papers. For instance, the findings from Paper 2 suggest that *all* participants of a group conversation feel lower intimacy, but it could be that this does not occur for those who contribute the most to a conversation. Similarly, Paper 3 examined perspectives from ghosting initiators and targets separately, rather than examining both perspectives within a relationship. These limitations could be addressed by using experimental laboratory designs that assigned participants to talk more or less during a group video chat conversation, or by assigning participants to the initiator and target roles of a ghosting exchange.

It is also worth noting that all but one sample in the research were limited to participants from the United States, and half were limited to college students. This restrains our ability to generalize our findings to countries outside of the United States, especially those that may have limited access to social technologies. Future research should examine these processes in other countries and consider how cultural influences, such as individualism vs. collectivism

(Miyamoto et al., 2014; Miyamoto & Ma, 2011) and tightness vs. looseness (Gelfand et al., 2006, 2011), may further influence how social technologies influence relationship processes. For example, although we found that ghosting was rather common among U.S. emerging adults, ghosting may be less common in collectivistic cultures that prioritize the well-being of the group over the individual, or in tighter cultures that have stricter social norms prescribing what is an appropriate way to end a relationship. This is just one example of how culture may influence the relationship processes examined in this body of work, and future research should identify and examine other cultural influences.

Conclusion

In the same vein as early social psychologists, this dissertation examined how social technologies serve as a context for initiating, maintaining, and ending social relationships. There are three main takeaways from this research. First, smartphones may displace social interactions with strangers because people do not prioritize the mood-boosting benefits of these interactions over the affordance of their smartphones, or because they do not want to be left alone with their own thoughts. Second, conversations with more people are less intimate but more enjoyable when in person, but even less intimate and less enjoyable when over video chat. Third, ghosting is more painful than direct rejection, and people who have a high need for closure are slightly more likely to use ghosting to end a relationship while also being more likely to experience being ghosted as painful. This body of work extends theory in the social cognition, motivation, communication, and intimate relationships literatures to help address a modern epidemic of loneliness that has implications for psychological well-being and physical health.

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APPENDICES

APPENDIX A

PAPER 1 SUPPLEMENTAL MATERIALS

Supplemental Methods

Study 1

In this forecasting study, 324 participants (58.9% female) were recruited from public areas on an American university campus between June and September 2018. Participants were not compensated for their participation. People were eligible to participate in the study if they were over the age of 18 years and on campus at the time the study was conducted. Ten participants were excluded from the study: Six participants had incomplete data and four participants reported that they were 17 years old, even though they indicated that they were 18 years or older on the consent form. Thus, the final sample size for data analyses was 314. The average age was 20.42 years (SD = 3.12) and ranged from 18 to 47 years. The sample was predominately White (69.7%), followed by 15.3% Asian, 6.7% Black, 3.2% Hispanic, 1.0% Pacific Islander, 0.3% Native American, 3.5% were of another racial background, and 0.3% did not report their race. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures analysis of variance (ANOVA) with 95% power (α = .05) for each dependent variable, given our final sample size (N = 314). We were able to detect omnibus effects of η^2 = .01 for loneliness (r = .43; ε = .95), negative affect (r = .34; ε = .98), and positive affect (r = .31; ε = .99), and a simple effect of d = 0.20.

The study employed a within-subject experimental design in which participants imagined the experience of engaging in three activities for seven minutes each: sitting alone, having a face-

to-face conversation with a stranger, and using their phone. The order of these activities was random. Instructions for the forecasting directions were modified from those used by Epley and Schroeder (2014) and Wilson et al. (2014). In the sitting alone condition, participants were told, "Imagine you are participating in a study. You arrive at the lab and sign a consent form. The experimenter seats you in a room and asks you to complete a questionnaire. Imagine that you are then given these instructions: 'Please *SIT ALONE BY YOURSELF* in this room for seven minutes and *JUST THINK*. Please do not use your phone or the computer for any purpose. In other words, you should think about whatever you would like, and only think, for the next seven minutes. The experimenter will notify you when seven minutes have passed.' Please rate how you would feel during the situation." In the phone condition, participants were asked to imagine they would "use your phone however you would like." In the conversation condition, participants imagined what it would be like to "talk with another participant of this study who you do not know."

After imagining each activity, participants recorded their forecasted positive and negative affect using items from the Positive and Negative Affect Schedule (PANAS; Watson et al., 1988). The measure consisted of four items that capture positive affect (engaged, interested, happy, and excited) and four items that capture negative affect (bored, irritable, sad, and anxious). Participants recorded the extent to which they expected they would feel each emotion by responding on a sliding scale ranging from 0 (*not at all*) to 100 (*extremely*). Positive affect items were averaged, and negative affect items were averaged to create scale totals. Cronbach's alphas for positive affect ranged from .81 to .84 and alphas for negative affect ranged from .58 to .67.

Participants also reported their loneliness. Items were adapted from the UCLA Loneliness Scale (Russell et al., 1980), including "I feel left out," "I feel that people barely know me," "I

feel isolated from others," "I feel that people are around me but not with me," "I feel that I can find companionship when I want it," "I feel that there are people who really understand me," "I feel in tune with other people," and "I feel that there are people I can turn to." Two additional items asked participants the extent to which they felt "socially connected" and "socially disconnected." Responses were reported on a sliding scale ranging from 0 (*not at all*) to 100 (*extremely*). After reverse-scoring appropriate items, a composite score was created by averaging across items, with higher scores indicating more loneliness. Alphas for the loneliness composite ranged from .79 to .81.

After imagining the three activities, participants were asked to rank the scenarios in order of their most preferred to their least preferred by dragging the activities into their desired order. After completing the survey, participants then completed demographic questions, were thanked for their participation, and were debriefed.

Study 2

For this laboratory experiment, 210 participants (70.0% female) were recruited from an American university psychology student subject pool between October and December 2018. Participants were compensated for participating in the study with course credit. Three participants were excluded due to incomplete data for one or more of the scenarios. The average age was 18.67 years (SD = 1.04) and ranged from 18 to 26 years. The sample was predominately White (62.4%), followed by 20.7% Asian, 6.1% Hispanic, 5.6% Black, 3.8% reported another race/ethnicity, and 1.4% did not report their race/ethnicity. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures ANOVA with 95% power (α = .05) for each dependent variable, given our final sample size (N = 210). We were able to detect

omnibus effects of $\eta^2 = .01$ for loneliness ($r = .79$; $\varepsilon = .86$), negative affect ($r = .61$; $\varepsilon = .97$), and positive affect ($r = .64$; $\varepsilon = .96$), and a simple effect of $d = 0.25$.

Participants entered the laboratory individually and after consent was obtained, they completed personality measures that have not been analyzed and are not reported in the present paper. Next, participants completed three activities (sitting alone, using their smartphone, having a face-to-face conversation with a stranger) in random order. Instructions for the activity directions were modified from those used by Epley and Schroeder (2014) and Wilson et al. (2014). For the solitude scenario, participants were instructed to "Please *SIT ALONE BY YOURSELF* in this room for seven minutes and *JUST THINK*. Please do not use your phone or computer for any purpose. Please try your best not to fall asleep. You should think about whatever you would like, and only think, for the next seven minutes." Participants were notified by the experimenter when the seven minutes had elapsed. Instructions for the other two activities were similar to the solitude scenario, with slight variations. During the conversation scenario, participants were instructed to "talk with someone who you do not already know." The conversation partner was drawn from a pool of 10 research assistants who were instructed to converse with the participant as they naturally would about any topic. They were not trained or instructed to have any particular type of conversation. They were instructed that no technology could be used during this activity (i.e., participants and conversation partners could not check their phone). During the phone activity, participants were instructed to "use your phone however you would like."

After each activity was experienced, participants reported their current positive affect, negative affect, and loneliness with the same measures as in Study 1. Positive affect, negative affect, and loneliness were reported after engaging in each activity. Cronbach's alphas ranged

from .79 to .83 for positive affect, from .43 to .60 for negative affect, and from .85 to .86 for loneliness. Reliability of the negative affect items was low. Examination of the scale revealed that removing the "boredom" item would increase reliability to between .57 and .66. Thus, we ran all reported hypothesis tests with the boredom item removed and found that this did not change the pattern or the significance of the results (see Tables A.2 and A.4).

After completing the three activities, participants ranked them from their most preferred to their least preferred activity using the same method as in Study 1. They then completed demographic survey questions, were thanked for their participation, and were debriefed.

Study 3

Two samples of participants were recruited for this forecasting study, including 356 participants (55.6% male) living in the United States who were recruited through Amazon's Mechanical Turk (MTurk) on one day in December 2017. Participants in this first sample received $0.50 for their participation. The average age was 35.62 years (SD = 11.14) and ranged from 19 to 68 years. The sample was predominately White (62.6%), followed by 19.9% Asian, 7.6% Black, 4.8% Hispanic, 3.4% Native American, and 1.7% were another racial/ethnic background. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures ANOVA with 95% power (α = .05) for each dependent variable, given our final sample size (N = 356). We were able to detect omnibus effects of η^2 = .003 for negative affect (r = .76; ε = .88) and η^2 = .004 for positive affect (r = .66; ε = .89), and a simple effect of d = 0.19.

Additionally, 271 participants (69.0% female) were recruited from public areas on an American university campus between December 2017 and April 2018. Participants in this sample were not compensated for their participation in the study. Individuals were eligible to

participate in the study if they were over the age of 18 years and on campus during recruitment. The average age was 20.16 years (*SD* = 2.00) and ranged from 18 to 33 years. The sample was predominately White (73.4%), followed by 15.5% Asian, 4.8% Black, 1.5% Hispanic, 0.7% Pacific Islander, 3.3% reported another racial/ethnic background, and 0.7% did not report their race/ethnicity. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures ANOVA with 95% power (α = .05) for each dependent variable, given our final sample size (*N* = 271). We were able to detect omnibus effects of η^2 = .01 for negative affect (*r* = .42; ε = .91) and positive affect (*r* = .41; ε = .96), and a simple effect of *d* = 0.22.

Each of these samples employed the same within-subject experimental design in which participants imagined the experience of engaging in five activities for ten minutes each: sitting alone, having a face-to-face conversation with a stranger, texting, using social media, and watching television. The order of these activities was random. Instructions for the forecasting directions were modified from those used by Epley and Schroeder (2014) and Wilson et al. (2014). Participants were told the following: "Imagine you are participating in a study. You arrive at the lab and sign a consent form. The experimenter seats you in a room alone and asks you to complete a questionnaire. Imagine that you are then given these instructions: 'Please *SIT ALONE BY YOURSELF* in this room for ten minutes and *JUST THINK*. Please do not use your phone or the computer for any purpose. In other words, you should think about whatever you would like, and only think, for the next ten minutes. The experimenter will notify you when ten minutes have passed.' After ten minutes, you are asked to complete another questionnaire. Imagine the questionnaire asks you the following questions. Please rate how you would feel during this situation." Instructions for each activity were the same, excluding the description of the activity (the italicized text). Participants were also instructed to imagine the following: "text

anybody you want," "watch a television show of your choice," "browse your social media platforms," and "talk with another participant of this study who you do not know."

After imagining each scenario, participants completed the same measures of positive and negative affect as in Study 1. For the MTurk sample, Cronbach's alphas ranged from .84 to .90 for positive affect and from .81 to .90 for negative affect. In the student sample, Cronbach's alphas ranged from .82 to .90 for positive affect and from .63 to .74 for negative affect. After forecasting these three activities, participants ranked them from their most desired to their least desired activity. Last, participants completed demographic survey questions, were thanked for participating in the study, and were debriefed.

Study 4

In this laboratory experiment, 107 participants (59.8% male) were recruited from an American university psychology student subject pool between February and June 2018. Participants were compensated for participating in the study with course credit. The average age was 19.32 years (SD = 1.94) and ranged from 18 to 32 years. The sample was predominately White (70.1%), followed by 12.1% Asian, 9.3% Black, 3.7% Hispanic, and 4.7% were another racial/ethnic background. We conducted a sensitivity power analysis (G*Power 3; Faul et al., 2007) of a repeated measures ANOVA with 95% power (α = .05) for each dependent variable, given our final sample size (N = 107). We were able to detect omnibus effects of η^2 = .01 for negative affect (r = .75; ε = .80) and positive affect (r = .79; ε = .92), and a simple effect of d = 0.35.

Participants entered the laboratory individually and after consent was obtained they completed personality measures that are not reported in the present paper. Participants then completed a baseline measure of positive and negative affect using the same materials as in

Study 1. Next, participants experienced each of the five activities that were imagined by the participants in Study 3 for seven minutes each. Activities were completed in random order. Instructions for the activity directions were modified from those used by Epley and Schroeder (2014) and Wilson et al. (2014). For the solitude activity, participants were told, "Please *SIT ALONE BY YOURSELF* in this room for seven minutes and *JUST THINK*. Please do not use your phone or the computer for any purpose, excluding completion of survey items. In other words, you should think about whatever you would like, and only think, for the next seven minutes." In addition, participants experienced a texting activity, in which they could text anybody they wanted. During the television scenario, participants had access to the laboratory's Netflix streaming service and could watch any show of their choosing. In the social media scenario, participants were instructed to browse their social media platforms, including Twitter, Facebook, and Instagram, and to not use technology for any other purpose. Instructions stated, "Please do not use the instant or direct message features of these platforms. You may post status updates or write messages on someone's wall, but you should not engage in a private conversation." In the conversation activity, participants were asked to talk to a stranger they had just met. The conversation partner was a participant in a different study in our lab. Thus, each participant had a different conversation partner, who was not trained or instructed to have any particular type of conversation. The dyad could converse about any topic but were not allowed to use technology during the interaction.

After each seven-minute activity, participants were instructed to complete the same measure of positive and negative affect utilized in Study 1 and at baseline. Cronbach's alphas ranged from .85 to .89 for positive affect and from .52 to .69 for negative affect.

After completing the five activities participants ranked their preference for the scenarios from their most preferred to their least preferred. Last, participants completed demographic survey questions, were thanked for participating in the study, and were debriefed.

Supplemental Results

All analyses were conducted using IBM SPSS, version 25. Descriptive statistics are presented in Tables A.4–A.7 and Figure A.1. In all studies (Studies 1–4), repeated measures ANOVAs were run on positive affect and negative affect. In Studies 1 and 2, repeated measures ANOVAs were run on loneliness. The results of these analyses are reported in Table A.2. If Mauchly's Test of Sphericity was significant (see Table A.1), then the assumption of sphericity was violated and the Greenhouse-Geisser correction was used. However, because Greenhouse-Geisser is a conservative correction, when its epsilon was greater than 0.75 the more liberal Huynd-Feldt correction was used. Within each study, to determine differences between activities, pairwise comparisons were run between all activities using the Bonferroni correction for multiple tests. Results from the pairwise comparisons can be seen in the superscripts of Table A.4 for Studies 1 and 2 and Table A.6 for Studies 3 and 4.

Related-samples nonparametric Friedman's two-way ANOVA by ranks were run in Studies 1–4 (see Table A.3) to determine whether there is an overall difference between the ranking of the activities because each study contained more than two activities that were being ranked. Additionally, pairwise comparisons, using the Bonferroni correction for multiple tests, were used to determine the differences between each activity ranking. Descriptive statistics for the activity ranking are presented in Table A.5 for Studies 1 and 2 and in Table A.7 for Studies 3 and 4, with the superscripts indicating the results from the pairwise comparisons.

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Supplemental Tables

Table A.1

Mauchly's Test of Sphericity of Affect and Loneliness Forecasts and Experiences for Studies 1–4

Source	*df*	χ^2	*p*	GG(ε)	HF(ε)
Study 1 (Forecast)					
PA	2	3.50	.174	.99	.99
NA	2	7.98	.019	.98	.98
Loneliness	2	18.96	< .001	.94	.95
Study 2 (Actual Experience)					
PA	2	11.65	.003	.95	.96
NA	2	7.60	.022	.97	.97
NA ("boredom" removed)	2	9.90	.007	.96	.96
Loneliness	2	38.67	< .001	.86	.86
Study 3 (MTurk Forecast)					
PA	9	88.67	< .001	.88	.89
NA	9	96.04	< .001	.77	.88
Study 3 (Student Forecast)					
PA	9	27.94	.001	.95	.96
NA	9	62.81	< .001	.90	.91
Study 4 (Actual Experience)					
PA	14	42.80	< .001	.88	.92
NA	14	80.08	< .001	.77	.80

Note. GG = Greenhouse-Geisser; HF = Huynh-Feldt; MTurk = Amazon's Mechanical Turk; PA = Positive affect; NA = Negative affect.

Table A.2

Repeated Measures ANOVA of Affect and Loneliness Forecasts and Experiences for Studies 1–4

Source	*df*	*F*	*p*	η_p^2
Study 1 (Forecast)				
PA	2	141.80	< .001	.31
NA	1.96	50.95	< .001	.14
Loneliness	1.90	171.69	< .001	.35
Study 2 (Actual Experience)				
PA	1.91	313.05	< .001	.60
NA	1.95	179.88	< .001	.46
NA (“boredom” removed)	1.91	41.45	< .001	.17
Loneliness	1.72	144.71	< .001	.41
Study 3 (MTurk Forecast)				
PA	3.56	67.40	< .001	.16
NA	3.53	55.00	< .001	.13
Study 3 (Student Forecast)				
PA	3.85	69.39	< .001	.20
NA	3.65	54.27	< .001	.17
Study 4 (Actual Experience)				
PA	4.59	43.22	< .001	.29
NA	4.02	36.98	< .001	.26

Note. MTurk = Amazon’s Mechanical Turk; PA = Positive affect; NA = Negative affect.

Table A.3

Nonparametric Test of Activity Rankings for Studies 1–4

Source	*df*	χ^2	*p*
Study 1 (Forecast)	2	74.98	< .001
Study 2 (Actual Experience)	2	190.66	< .001
Study 3 (MTurk Forecast)	4	322.78	< .001
Study 3 (Student Forecast)	4	244.95	< .001
Study 4 (Actual Experience)	4	123.46	< .001

Note. MTurk = Amazon’s Mechanical Turk.

Table A.4

Means and Standard Deviations of Affect and Loneliness Forecasts and Experiences for Studies 1 and 2

Activity	Study 1: Forecast (n = 314)			Study 2: Actual Experience (n = 210)			
	PA	NA	Loneliness	PA	NA	NA ("boredom" removed)	Loneliness
Alone	28.94 (20.99)[c]	31.90 (19.27)[a]	59.62 (16.44)[a]	30.19 (19.68)[c]	26.64 (14.10)[a]	16.82 (15.64)[a]	36.81 (17.24)[a]
Conversation	51.81 (20.45)[a]	22.20 (14.09)[b]	38.60 (15.06)[c]	61.35 (18.14)[a]	10.48 (10.50)[c]	9.59 (11.12)[c]	21.88 (13.71)[c]
Smartphone	32.98 (20.35)[b]	22.01 (15.58)[b]	51.22 (16.58)[b]	41.83 (19.39)[b]	19.31 (13.71)[b]	13.59 (14.30)[b]	28.88 (15.69)[b]

Note. Values in a column with different superscripts are significantly different from one another. Superscripts are listed in decreasing value with 'a' as the highest value for that column. All mean differences between groups are significant at the $p = .011$ level. PA = Positive affect; NA = Negative affect.

Table A.5

Means and Standard Deviations of Activity Rankings for Studies 1 and 2

Activity	Study 1	Study 2
	Forecast (n = 314)	Actual Experience (n = 210)
Alone	2.36 (0.79)[c]	2.75 (0.54)[c]
Conversation	1.68 (0.79)[a]	1.45 (0.66)[a]
Smartphone	1.96 (0.72)[b]	1.80 (0.60)[b]

Note. Lower score indicates higher ranking. Values in a column with different superscripts are significantly different from one another. Superscripts are listed in increasing value with 'a' as the lowest value for that column. All mean differences between groups are significant at the $p = .001$ level.

Table A.6

Means and Standard Deviations of Affect Forecasts and Experiences for Studies 3 and 4

	Study 3				Study 4	
	MTurk Forecast (n = 356)		Student Forecast (n = 271)		Actual Experience (n = 107)	
Activity	PA	NA	PA	NA	PA	NA
Baseline	--	--	--	--	52.25 (20.12)^{bc}	22.03 (14.11)^{ab}
Alone	42.45 (27.73)^{d}	33.62 (24.16)^{a}	29.83 (20.51)^{c}	34.37 (22.12)^{a}	37.19 (22.21)^{e}	23.56 (16.99)^{a}
Conversation	57.58 (24.56)^{bc}	26.27 (22.04)^{b}	51.83 (22.13)^{a}	23.53 (15.36)^{b}	57.68 (21.51)^{a}	10.84 (9.96)^{d}
Social Media	58.70 (25.16)^{b}	21.78 (23.17)^{c}	39.90 (20.94)^{b}	24.70 (18.06)^{b}	44.22 (22.26)^{d}	18.77 (15.43)^{bc}
Texting	55.46 (26.16)^{c}	24.24 (23.90)^{bc}	44.21 (24.66)^{b}	24.11 (18.11)^{b}	47.56 (23.95)^{cd}	16.56 (14.58)^{c}
TV	64.52 (22.43)^{a}	18.95 (22.02)^{d}	55.14 (24.02)^{a}	15.52 (14.26)^{c}	54.62 (22.74)^{ab}	11.12 (11.78)^{d}

Note. Higher score indicates more affect. Values in a column with different superscripts are significantly different from one another. Superscripts are listed in decreasing value with 'a' as the highest value for that column. All mean differences between groups are significant at the $p = .042$ level. MTurk = Amazon's Mechanical Turk; PA = Positive affect; NA = Negative affect.

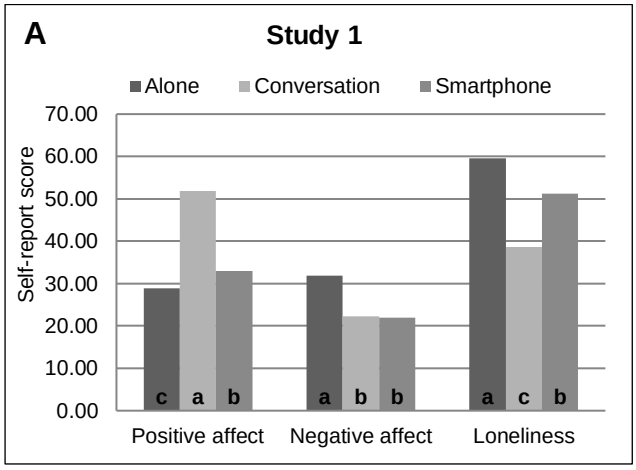
Table A.7

Means and Standard Deviations of Activity Rankings for Studies 3 and 4

	Study 3		Study 4
Activity	MTurk Forecast (n = 356)	Student Forecast (n = 271)	Actual Experience (n = 107)
Alone	3.46 (1.44)^{c}	3.76 (1.35)^{d}	4.25 (1.15)^{c}
Conversation	4.04 (1.25)^{d}	3.65 (1.37)^{d}	2.98 (1.36)^{b}
Social Media	2.82 (1.28)^{b}	3.07 (1.17)^{c}	3.31 (1.14)^{b}
Texting	2.50 (1.12)^{ab}	1.98 (1.12)^{a}	2.36 (1.17)^{a}
TV	2.17 (1.10)^{a}	2.54 (1.21)^{b}	2.10 (1.17)^{a}

Note. Lower score indicates higher ranking. Values in a column with different superscripts are significantly different from one another. Superscripts are listed in increasing value with 'a' as the lowest value for that column. All mean differences between groups are significant at the $p = .038$ level. MTurk = Amazon's Mechanical Turk.

Supplemental Figures



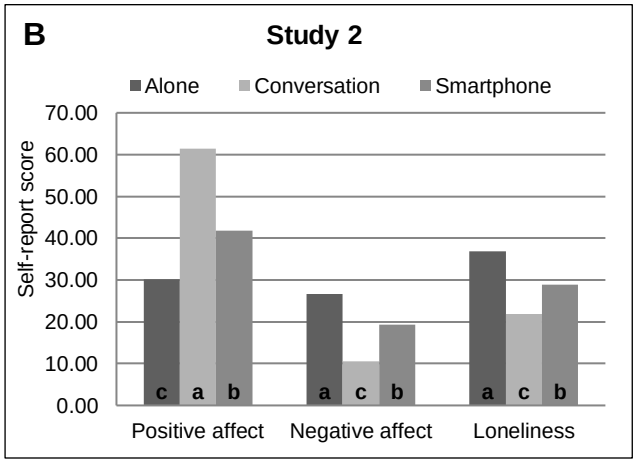


Figure A.1. Affect and Loneliness for Studies 1 and 2. Forecasted (A) and experienced (B) positive affect, negative affect, and loneliness after sitting alone, having a face-to-face conversation with a stranger, and using their smartphone. Within each measure, bars with different letters are significantly different from one another. Letters within each bar are listed in decreasing value with 'a' as the highest value. All mean differences between groups are significant at the p = .042 level or lower.

APPENDIX B

PAPER 2 SUPPLEMENTAL MATERIALS

Supplemental Tables

Table B.1

Descriptive Statistics and Correlations (Study 1)

Variable	1	2	3	4	5	6	7	8
1. Self-disclosure	—	.73**	.59***	.51***	.07	.01	.20**	.22***
2. Responsiveness	.74***	—	.54***	.56***	.01	.06	.20***	.15**
3. Closeness	.59***	.59***	—	.64***	-.06	.03	.11	.14*
4. Enjoyment	.50***	.54***	.64***	—	-.05	.11	.11	.16**
5. Age					—	.12*	.25***	.09
6. Social Distancing						—	.34***	.004
7. COVID-19 Worry							—	.07
8. Extraversion								—
M	5.24	5.34	5.07	5.49	33.59	3.72	2.95	3.39
SD within	1.60	1.44	1.45	1.39				
SD between	1.02	0.88	0.95	0.91	12.46	0.99	1.07	0.95
Minimum	1	1	1	1	18	1	1	1
Maximum	7	7	7	7	86	5	5	5

Note. Correlations above the diagonal reflect between-person correlations; correlations below the diagonal reflect within-person correlations. *** $p < .001$. ** $p < .01$. * $p < .05$.

Table B.2

Repeated Measures ANOVA Results with Covariates (Study 1)

	F	df1, df2	*p*	η_p^2
Self-disclosure				
Size^	**16.38**	**1.37, 407.80**	**< .001**	**.052**
Medium	1.27	1, 297	.261	.004
Size × Medium^	0.30	1.71, 506.53	.707	.001
Age	0.00	1, 297	.989	< .001
Gender	0.47	2, 297	.626	.003
Survey Date	0.55	1, 297	.457	.002
Social Distancing	1.68	1, 297	.196	.006
COVID-19 Worry	**11.94**	**1, 297**	**.001**	**.039**
Extraversion	**14.39**	**1, 197**	**< .001**	**.046**
Responsiveness				
Size^	**9.41**	**1.39, 413.60**	**.001**	**.031**
Medium	0.62	1, 297	.432	.002
Size × Medium^	0.25	1.67, 496.05	.738	.001
Age	0.96	1, 297	.328	.003
Gender	0.31	2, 297	.732	.002
Survey Date	1.47	1, 297	.227	.005
Social Distancing	0.06	1, 297	.807	< .001
COVID-19 Worry	**10.78**	**1, 297**	**.001**	**.035**
Extraversion	**6.79**	**1, 297**	**.010**	**.022**
Closeness				
Size^	**10.34**	**1.40, 414.49**	**< .001**	**.034**
Medium	**6.27**	**1, 297**	**.013**	**.021**
Size × Medium^	0.50	1.56, 463.78	.513	.002
Age	3.36	1, 297	.068	.011
Gender	0.51	2, 297	.602	.003
Survey Date	0.02	1, 297	.880	< .001
Social Distancing	0.01	1, 297	.939	< .001
COVID-19 Worry	**4.38**	**1, 297**	**.037**	**.015**
Extraversion	**3.12**	**1, 297**	**.014**	**.020**
Enjoyment				
Size^	**10.56**	**1.46, 433.02**	**< .001**	**.034**
Medium	3.65	1, 297	.057	.012
Size × Medium^	1.87	1.63, 483.41	.162	.006
Age	2.29	1, 297	.131	.008
Gender	0.15	2, 297	.862	.001
Survey Date	0.08	1, 297	.777	< .001
Social Distancing	2.01	1, 297	.157	.007
COVID-19 Worry	1.81	1, 297	.179	.006
Extraversion	**7.71**	**1, 297**	**.006**	**.025**

Note. Bolded values indicate significant effects. For brevity, only the interaction effect between size and medium is reported. ^ Mauchly's test for sphericity was significant and the Hyunh-Feld correction was used. CI = confidence interval.

Table B.3

Descriptive Statistics and Correlations (Study 2)

Variable	1	2	3	4	5	6	7	8	9	10	11
1. Self-disclosure	—										
2. Responsiveness	.62***	—									
3. Closeness	.61***	.63***	—								
4. Enjoyment	.55***	.59***	.69***	—							
5. Age	.04	.07^	.05	.06	—						
6. Isolating	.03	.00	.02	.06	.18***	—					
7. Wearing a Mask	.08^	.07^	.03	.06	.02	.26***	—				
8. Social Distancing	.09*	.05	.05	.11**	.19***	.39***	.64***	—			
9. COVID-19 Worry	.07^	.03	.07^	.08^	.05	.27***	.49***	.51***	—		
10. Extraversion	.28***	.17***	.22***	.16***	.13***	-.15***	-.03	-.04	.05	—	
11. Close Other	.24***	.18***	.50***	.42***	-.09*	.10*	-.05	-.04	.01	-.01	—
M	4.87	3.73	4.37	4.50	31.04	44.90	66.81	57.08	43.00	3.14	0.59
SD	1.64	1.02	1.75	1.68	11.87	31.71	33.36	34.58	32.83	0.75	0.49
Minimum	1	1	1	1	18	0	0	0	0	1.33	0
Maximum	7	5	7	7	68	100	100	100	100	5.00	1

Note. *** $p < .001$, ** $p < .01$, *$p < .05$, ^ $p < .10$.

Table B.4

ANOVA Results with Covariates (Study 2)

	F	df1, df2	*p*	η_p^2
Self-disclosure				
Size	**15.16**	**1, 598**	**< .001**	**.025**
Medium	0.02	1, 598	.899	< .001
Size × Medium	**4.28**	**1, 598**	**.039**	**.007**
Age	0.00	1, 598	.947	< .001
Gender	0.06	2, 598	.943	< .001
Mask Wearing	0.63	1, 598	.428	.002
Social Distancing	3.28	1, 598	.071	.005
COVID-19 Worry	0.09	1, 598	.763	< .001
Extraversion	**58.15**	**1, 598**	**< .001**	**.089**
Close Other	**36.49**	**1, 598**	**< .001**	**.058**
Responsiveness				
Size	**31.28**	**1, 598**	**< .001**	**.050**
Medium	**4.43**	**1, 598**	**.036**	**.007**
Size × Medium	**7.31**	**1, 598**	**.007**	**.012**
Age	1.30	1, 598	.254	.002
Gender	0.37	2, 598	.693	.001
Mask Wearing	2.35	1, 598	.126	.004
Social Distancing	0.34	1, 598	.558	.001
COVID-19 Worry	0.64	1, 598	.423	.001
Extraversion	**23.40**	**1, 598**	**< .001**	**.038**
Close Other	**15.01**	**1, 598**	**< .001**	**.024**
Closeness				
Size	**3.96**	**1, 598**	**.047**	**.007**
Medium	1.12	1, 598	.289	.002
Size × Medium	3.16	1, 598	.076	.005
Age	1.22	1, 598	.269	.002
Gender	0.80	2, 598	.450	.003
Mask Wearing	0.32	1, 598	.570	.001
Social Distancing	1.12	1, 598	.290	.002
COVID-19 Worry	0.16	1, 598	.689	< .001
Extraversion	**41.26**	**1, 598**	**< .001**	**.065**
Close Other	**202.66**	**1, 598**	**< .001**	**.253**
Enjoyment				
Size	0.01	1, 598	.916	< .001
Medium	**13.29**	**1, 598**	**< .001**	**.022**
Size × Medium	**10.52**	**1, 598**	**.001**	**.017**
Age	2.12	1, 598	.146	.004
Gender	1.44	1, 598	.237	.005
Mask Wearing	0.13	2, 598	.719	< .001
Social Distancing	**7.64**	1, 598	**.006**	**.013**
COVID-19 Worry	0.10	1, 598	.758	< .001
Extraversion	**22.59**	**1, 598**	**< .001**	**.036**
Close Other	**117.22**	**1, 598**	**< .001**	**.164**

Note. Bolded values indicate significant effects. For brevity, only the interaction effect between size and medium is reported.

Table B.5

ANOVA Results with Covariates Except Close Other for Closeness (Study 2)

	F	df1, df2	*p*	η_p^2
Size	**6.85**	**1, 599**	**.009**	**.011**
Medium	**11.82**	**1, 599**	**< .001**	**.019**
Size × Medium	**6.66**	**1, 599**	**.010**	**.011**
Age	0.01	1, 599	.913	< .001
Gender	0.35	2, 599	.704	.001
Mask Wearing	0.05	1, 599	.829	< .001
Social Distancing	1.27	1, 599	.260	.002
COVID-19 Worry	0.54	1, 599	.464	.001
Extraversion	**35.23**	**1, 599**	**< .001**	**.056**

Note. Bolded values indicate significant effects. For brevity, only the interaction effect between size and medium is reported.

Table B.6

Direct Effects for the Moderated Mediation Model (Study 2)

Pathway	*b*	*SE*	*p*	95% CI
Self-disclosure (*SD*)				
Size → *SD*	**-0.53**	**.13**	**< .001**	**[-0.78, -0.27]**
Medium → *SD*	-0.04	.13	.735	[-0.30, 0.21]
Size × Medium → *SD*	**-0.61**	**.26**	**.021**	**[-1.12, -0.09]**
Perceived Responsiveness (*PR*)				
Size → *PR*	**-0.27**	**.06**	**< .001**	**[-0.40, -0.14]**
Medium → *PR*	**-0.16**	**.06**	**.013**	**[-0.28, -0.03]**
Size × Medium → *PR*	**-0.26**	**.13**	**.044**	**[-0.51, -0.01]**
Self-disclosure → *PR*	**0.37**	**.02**	**< .001**	**[0.33, 0.41]**
Closeness (*C*)				
Size → *C*	0.18	.11	.092	[-0.03, 0.38]
Medium → *C*	**-0.24**	**.10**	**.018**	**[-0.45, -0.04]**
Size × Medium → *C*	-0.11	.21	.585	[-0.52, 0.29]
Self-disclosure → *C*	**0.37**	**.04**	**< .001**	**[0.30, 0.45]**
Responsiveness → *C*	**0.71**	**.07**	**< .001**	**[0.58, 0.84]**
Enjoyment (*E*)				
Size → *E*	**0.37**	**.10**	**.001**	**[0.16, 0.57]**
Medium → *E*	**-0.49**	**.10**	**< .001**	**[-0.69, -0.29]**
Size × Medium → *E*	**-0.42**	**.21**	**.044**	**[-0.82, -0.01]**
Self-disclosure → *E*	**0.31**	**.04**	**< .001**	**[0.23, 0.39]**
Responsiveness → *E*	**0.68**	**.07**	**< .001**	**[0.55, 0.80]**

Note. Bold values indicate significant effects. Effects-coding was used for the size variable (-0.5 = *dyad*, 0.5 = *group*) and medium variable (-0.5 = *in-person*, 0.5 = *video chat*). Closeness and enjoyment were examined in separate models but are presented in the same table for brevity. CI = confidence interval.

Table B.7

Descriptive Statistics and Correlations (Study 3)

Variable	1	2	3	4	5	6	7	8	9	10	11
1. Self-disclosure	—	.92***	.70***	.68***	-.03	.03	.11	.21**	-.07	-.02	.13
2. Responsiveness	.76***	—	.65***	.70***	.02	.06	.10	.13	-.07	.04	.06
3. Closeness	.62***	.54***	—	.73***	.06	-.14	.20*	.47***	-.08	-.06	.25**
4. Enjoyment	.60***	.60***	.64***	—	.19*	-.12	.14	.23**	-.11	-.09	.33***
5. Size	-.10***	-.06***	-.13***	.04*	—	.03	-.06	-.05	-.22**	-.07	.10
6. Medium	-.02	-.01	-.16***	-.09***	.06**	—	-.11	-.27***	.25**	.07	-.24**
7. Weekend	.03	.04*	.13***	.08***	.05**	-.10***	—	.17*	-.10	.02	-.11
8. Close other	.26***	.17***	.55***	.31***	-.16***	-.25***	.13***	—	-.11	-.19*	.23**
9. Age									—	-.04	-.11
10. COVID-19 Worry										—	-.14
11. Extraversion											—
M	5.40	5.50	5.21	5.12	2.60	0.09	0.28	0.88	19.46	1.87	3.42
SD within	1.54	1.42	1.72	1.56	2.29	0.29	0.45	0.32			
SD between	0.94	0.88	0.91	0.81	0.94	0.15	0.12	0.16	1.17	1.04	0.71
Minimum	1	1	1	1	1	0	0	0	18	1	1.17
Maximum	7	7	7	7	11	1	1	1	24	5	4.92
ICC	.34	.35	.24	.23							

Note. Correlations above the diagonal reflect between-person correlations; correlations below the diagonal reflect within-person correlations. ICC = intraclass correlation coefficient. *** $p < .001$, ** $p < .01$, * $p < .05$, ^ $p < .10$.

Table B.8

Between-person and Within-person Main Effects of Conversation Size and Medium with Covariates (Study 3)

	Self-disclosure			Perceived Responsiveness			Closeness			Enjoyment		
Fixed Effects	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI
Between-person (Level 2)												
Size	-0.02 (.08)	.811	[-0.18, 0.14]	0.03 (.08)	.708	[-0.12, 0.18]	0.10 (.07)	.185	[-0.04, 0.24]	**0.16 (.07)**	**.018**	**[0.03, 0.28]**
Medium	0.72 (.57)	.211	[-0.38, 1.81]	0.53 (.53)	.317	[-0.49, 1.55]	-0.00 (.50)	.995	[-0.97, 0.96]	-0.24 (.45)	.593	[-1.12, 0.64]
Weekend	0.62 (.67)	.360	[-0.68, 1.92]	0.63 (.62)	.318	[-0.58, 1.83]	**1.22 (.59)**	**.039**	**[0.10, 2.35]**	**1.08 (.54)**	**.046**	**[0.05, 2.12]**
Close Other	0.96 (.55)	.085	[-0.10, 2.02]	0.37 (.51)	.469	[-0.61, 1.35]	**2.29 (.48)**	**< .001**	**[1.37, 3.21]**	0.36 (.44)	.410	[-0.47, 1.20]
Age	-0.03 (.07)	.712	[-0.18, 0.08]	-0.00 (.07)	.998	[-0.13, 0.13]	0.04 (.06)	.518	[-0.08, 0.16]	0.02 (.06)	.686	[-0.09, 0.13]
Gender	0.26 (.18)	.145	[-0.08, 0.61]	**0.37 (.17)**	**.029**	**[0.05, 0.69]**	0.28 (.16)	.073	[-0.02, 0.58]	0.14 (.14)	.315	[-0.13, 0.42]
COVID-19 Worry	-0.00 (.08)	.978	[-0.15, 0.14]	0.03 (.07)	.683	[-0.11, 0.16]	0.02 (.07)	.705	[-0.10, 0.15]	-0.01 (.06)	.841	[-0.13, 0.10]
Extraversion	0.16 (.12)	.174	[-0.06, 0.38]	0.07 (.11)	.487	[-0.13, 0.28]	**0.21 (.10)**	**.040**	**[0.01, 0.40]**	**0.35 (.09)**	**< .001**	**[0.18, 0.52]**
Within-person (Level 1)												
Size	**-0.05 (.01)**	**.002**	**[-0.07, -0.02]**	**-0.03 (.01)**	**.027**	**[-0.06, -0.00]**	**-0.07 (.02)**	**< .001**	**[-0.10, -0.03]**	**0.05 (.02)**	**.005**	**[0.02, 0.08]**
Medium	0.17 (.09)	.059	[-0.01, 0.34]	0.03 (.08)	.693	[-0.13, 0.19]	-0.07 (.09)	.434	[-0.24, 0.10]	-0.02 (.09)	.805	[-0.21, .16]
Weekend	-0.03 (.05)	.485	[-0.13, 0.06]	0.03 (.05)	.454	[-0.06, 0.13]	**0.19 (.05)**	**< .001**	**[0.09, 0.29]**	0.05 (.05)	.320	[-0.05, 0.16]
Close Other	**1.32 (.08)**	**< .001**	**[1.16, 1.48]**	**0.79 (.08)**	**< .001**	**[0.64, 0.93]**	**2.88 (.08)**	**< .001**	**[2.72, 3.04]**	**1.61 (.09)**	**< .001**	**[1.44, 1.78]**

Note. Bold values indicate significant effects. Regression coefficients and their respective confidence intervals are reported. Level 1 predictors are person-mean centered. Level 2 variables are sample-mean centered. Dummy coding was used for the Medium (0 = *in person*, 1 = *video chat*), Weekend (0 = *weekday*, 1 = *weekend*), Close Other (0 = *not present*, 1 = *present*), and Gender (0 = *male*, 1 = *female*) variables. We did not have enough participants who identified as a non-binary gender (n = 2) to make comparisons, so they were excluded from analyses. CI = confidence interval.

Table B.9

Between-person and Within-person Interaction Effects of Conversation Size and Medium with Covariates (Study 3)

	Self-disclosure			Perceived Responsiveness			Closeness			Enjoyment		
Fixed Effects	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI	*b* (*SE*)	*p*	95% CI
Between-person (Level 2)												
Size	-0.01 (.09)	.888	[-0.18, 0.15]	0.05 (.08)	.533	[-0.10, 0.20]	0.11 (.07)	.141	[-0.04, 0.25]	**0.18 (.07)**	**.009**	**[0.05, 0.31]**
Medium	0.77 (.58)	.191	[-0.35, 1.88]	0.53 (.53)	.317	[-0.40, 1.68]	0.05 (.51)	.919	[-0.93, 1.03]	-0.17 (.46)	.717	[-1.06, 0.73]
Size × Medium	-0.22 (.53)	.679	[-1.24, 0.80]	-0.37 (.49)	.453	[-1.32, 0.57]	-0.18 (.46)	.699	[-1.07, 0.72]	-0.29 (.42)	.491	[-1.10, 0.52]
Weekend	0.62 (.69)	.366	[-0.68, 1.91]	0.62 (.63)	.325	[-0.59, 1.83]	**1.22 (.59)**	**.041**	**[0.09, 2.34]**	1.05 (.54)	.053	[0.02, 2.09]
Close Other	0.95 (.55)	.089	[-0.11, 2.01]	0.32 (.52)	.535	[-0.66, 1.31]	**2.25 (.48)**	**< .001**	**[1.33, 3.17]**	0.29 (.44)	.509	[-0.55, 1.13]
Age	-0.03 (.07)	.688	[-0.17, 0.11]	-0.00 (.07)	.956	[-0.13, 0.13]	0.04 (.06)	.529	[-0.08, 0.16]	0.02 (.06)	.670	[-0.08, 0.13]
Gender	0.24 (.19)	.191	[-0.11, 0.60]	0.33 (.17)	.056	[0.00, 0.66]	0.26 (.16)	.105	[-0.04, 0.57]	0.11 (.15)	.443	[-0.17, 0.39]
COVID-19 Worry	-0.00 (.08)	.972	[-0.15, 0.14]	0.03 (.07)	.674	[-0.10, 0.16]	0.03 (.07)	.698	[-0.10, 0.15]	-0.01 (.06)	.865	[-0.12, 0.10]
Extraversion	0.16 (.12)	.175	[-0.06, 0.38]	0.08 (.11)	.459	[-0.13, 0.29]	**0.21 (.10)**	**.038**	**[0.02, 0.40]**	**0.35 (.09)**	**< .001**	**[0.18, 0.53]**
Within-person (Level 1)												
Size	**-0.05 (.01)**	**.003**	**[-0.07, -0.02]**	-0.03 (.01)	.061	[-0.06, 0.00]	**-0.06 (.02)**	**< .001**	**[-0.10, -0.03]**	**0.05 (.02)**	**< .001**	**[0.02, 0.09]**
Medium	0.17 (.09)	.058	[-0.01, 0.34]	0.03 (.08)	.704	[-0.13, 0.19]	-0.07 (.09)	.398	[-0.25, 0.10]	-0.02 (.09)	.812	[-0.21, .16]
Size × Medium	-0.02 (.04)	.521	[-0.09, 0.05]	**-0.14 (.03)**	**< .001**	**[-0.21, -0.08]**	**-0.12 (.04)**	**.001**	**[-0.19, -0.05]**	**-0.18 (.04)**	**< .001**	**[-0.25, -0.11]**
Weekend	-0.03 (.05)	.485	[-0.13, 0.06]	0.03 (.05)	.492	[-0.06, 0.12]	**0.19 (.05)**	**< .001**	**[0.09, 0.29]**	0.05 (.05)	.348	[-0.05, 0.15]
Close Other	**1.32 (.08)**	**< .001**	**[1.16, 1.48]**	**0.74 (.08)**	**< .001**	**[0.59, 0.89]**	**2.84 (.08)**	**< .001**	**[2.68, 3.00]**	**1.55 (.09)**	**< .001**	**[1.38, 1.72]**

Note. Bold values indicate significant effects. Regression coefficients and their respective confidence intervals are reported. Level 1 predictors are person-mean centered. Level 2 variables are sample-mean centered. Dummy coding was used for the Medium (0 = *in person*, 1 = *video chat*), Weekend (0 = *weekday*, 1 = *weekend*), Close Other (0 = *not present*, 1 = *present*), and Gender (0 = *male*, 1 = *female*) variables. We did not have enough participants who identified as a non-binary gender (n = 2) to make comparisons, so they were excluded from analyses. CI = confidence interval.

Table B.10

Within-person Simple Slopes for the Effect of Conversation Size at Different Levels of Medium Controlling for Additional Covariates (Study 3)

	-1SD				+1SD			
Outcome	*b*	*SE*	*p*	95% CI	*b*	*SE*	*p*	95% CI
Self-disclosure	**-0.04**	**.02**	**.030**	**[-0.07, -0.004]**	**-0.05**	**.02**	**.002**	**[-0.08, 0.02]**
Responsiveness	0.01	.02	.619	[-0.03, 0.04]	**-0.06**	**.02**	**< .001**	**[-0.10, -0.03]**
Closeness	-0.03	.02	.101	[-0.07, 0.01]	**-0.09**	**.02**	**<.001**	**[-0.13, -0.06]**
Enjoyment	**0.10**	**.02**	**<.001**	**[0.06, 0.14]**	0.01	.02	.632	[-0.03, 0.04]

Note. Simple slopes of conversation size at two levels of medium: 1 SD above and below the mean. -1SD represents in-person conversations among participants who had 26% video chats; +1SD represents video chat conversations among participants who had 74% video chats. Analyses are controlling for weekend, close other, age, gender, COVID-19 worry, and extraversion. Bold values indicate significant effects. CI = confidence interval.

Table B.11

Within-person Direct and Indirect Effects for the Serial Mediation Model (Study 3)

Pathway	*β*	*SE*	*p*	95% CI
Self-disclosure (*SD*)				
Size → *SD*	**-.08**	**.02**	**< .001**	**[-.11, -.04]**
Medium → *SD*	.03	.02	.095	[-.01, .06]
Size × Medium → *SD*	-.02	.02	.433	[-.05, .02]
Weekend → *SD*	-.02	.02	.391	[-.05, .02]
Close Other → *SD*	**.30**	**.02**	**< .001**	**[.26, .33]**
Perceived Responsiveness (*PR*)				
Size → *PR*	-.01	.01	.566	[-.04, .02]
Medium → *PR*	-.01	.01	.328	[-.04, .01]
Size × Medium → *PR*	**-.08**	**.01**	**< .001**	**[-.11, -.05]**
Weekend → *PR*	.02	.01	.119	[-.01, .05]
Close Other → *PR*	-.01	.02	.755	[-.03, .03]
SD → *PR*	**.65**	**.01**	**< .001**	**[.63, .67]**
Closeness (*C*)				
Size → *C*	**-.04**	**.01**	**.003**	**[-.06, -.01]**
Medium → *C*	**-.03**	**.01**	**.026**	**[-.05, -.003]**
Size × Medium → *C*	**-.04**	**.01**	**.001**	**[-.07, -.02]**
Weekend → *C*	**.07**	**.01**	**< .001**	**[.04, .09]**
Close Other → *C*	**.41**	**.01**	**< .001**	**[.38, .43]**
SD → *C*	**.34**	**.02**	**< .001**	**[.30, .37]**
PR → *C*	**.17**	**.02**	**< .001**	**[.14, .20]**
Size → *SD* → *C*	**-.03**	**.01**	**< .001**	**[-.04, -.01]**
Size → *PR* → *C*	-.001	.002	.566	[-.01, .003]
Size → *SD* → *PR* → *C*	**-.01**	**.002**	**< .001**	**[-.01, -.004]**
Total Effect	**-.04**	**.01**	**.003**	**[-.06, -.01]**
Enjoyment (*E*)				
Size → *E*	**.12**	**.01**	**< .001**	**[.09, .15]**
Medium → *E*	-.02	.01	.236	[-.05, .01]
Size × Medium → *E*	**-.06**	**.02**	**< .001**	**[-.09, -.03]**
Weekend → *E*	.02	.01	.124	[-.01, .05]
Close Other → *E*	**.18**	**.02**	**< .001**	**[.15, .21]**
SD → *E*	**.30**	**.02**	**< .001**	**[.26, .34]**
PR → *E*	**.31**	**.02**	**< .001**	**[.27, .34]**
Size → *SD* → *E*	**-.02**	**.01**	**< .001**	**[-.03, -.01]**
Size → *PR* → *E*	-.002	.004	.565	[-.01, .01]
Size → *SD* → *PR* → *E*	**-.02**	**.004**	**< .001**	**[-.02, -.01]**
Total Effect	**.12**	**.01**	**< .001**	**[.10, .15]**

Note. Standardized regression coefficients, standard errors, and 5,000 bootstrapped confidence intervals are reported. Size is person-mean centered, such that larger values indicate a larger-than-average conversation for a given participant. We controlled for conversation medium, size*medium, weekend, and close other for all pathways at the within- and between-person levels. We also controlled for size at the between-person level. Closeness and enjoyment were examined in separate models but are presented in the same table for brevity. CI = confidence interval; *SD* = Self-disclosure; *PR* = Perceived Responsiveness.

Supplemental Figures

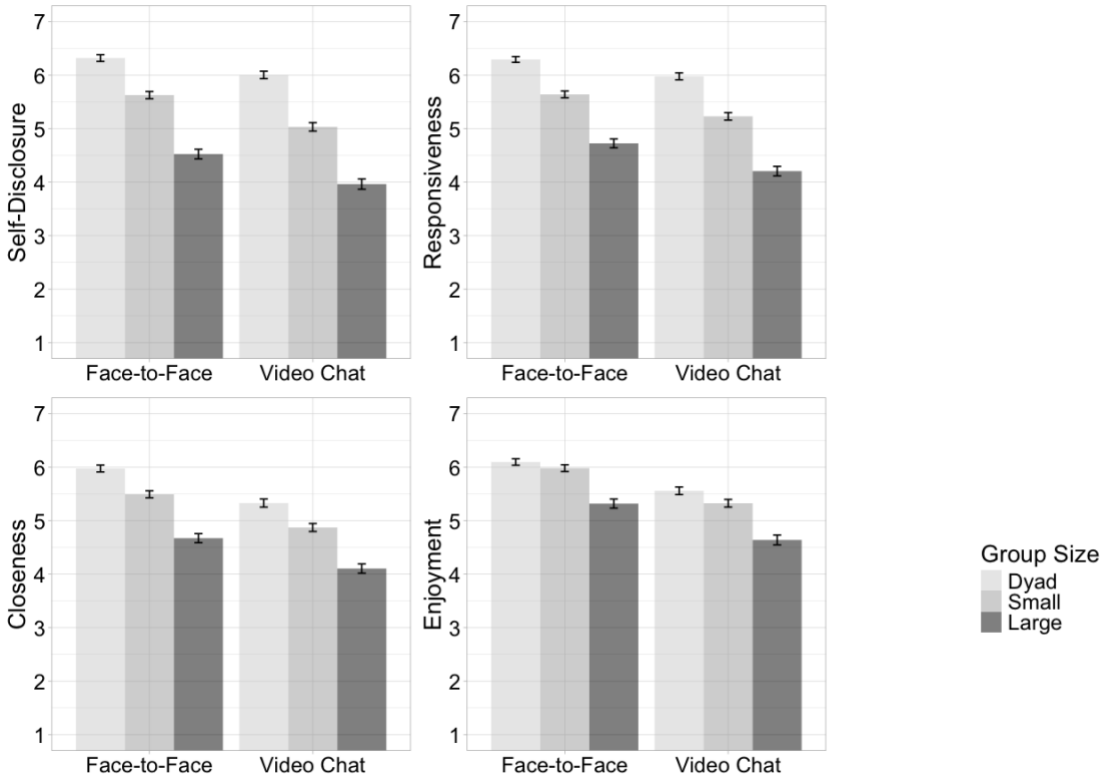


Figure B.1. Means and Standard Errors by Conversation Size and Medium (Study 1).

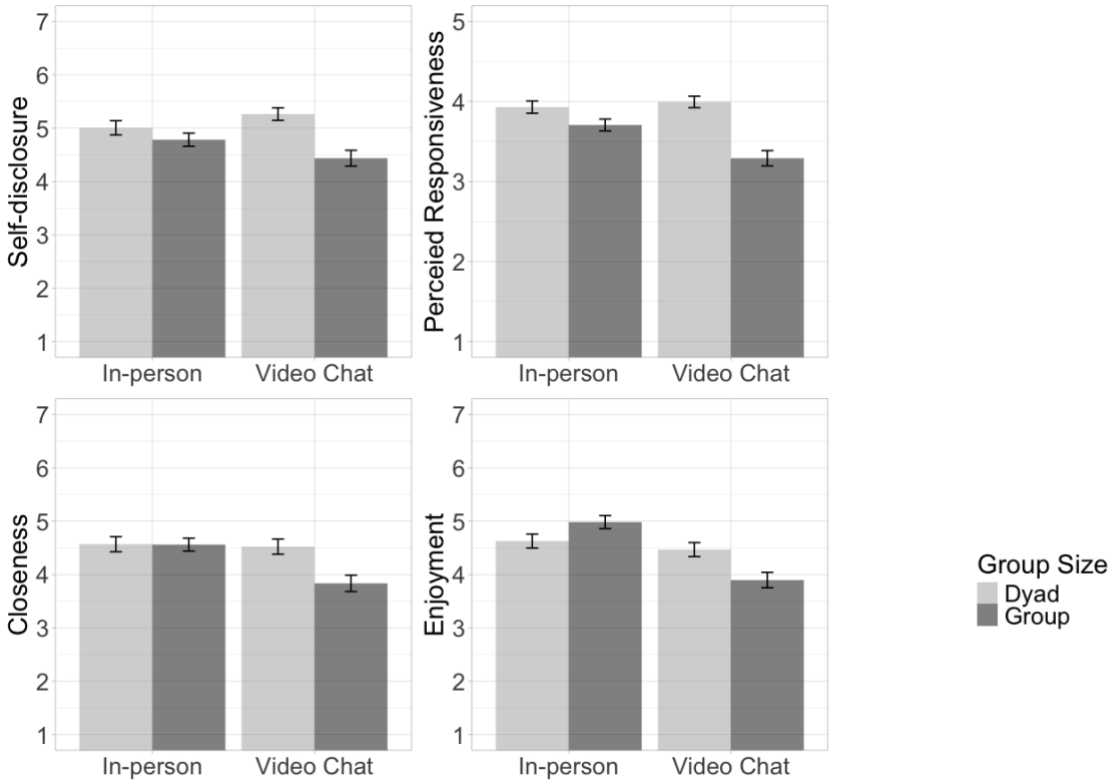


Figure B.2. Means and Standard Errors by Conversation Size and Medium (Study 2).

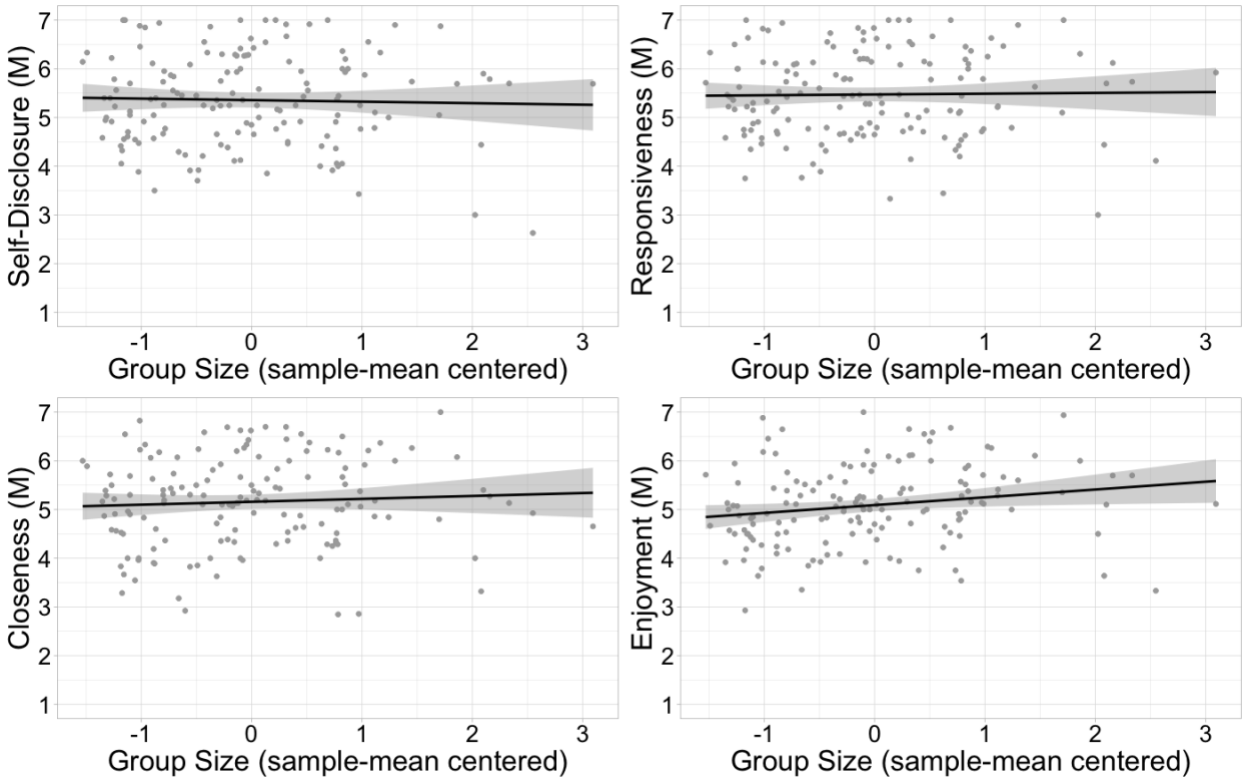


Figure B.3. Between-person Fixed Effects of Conversation Size (Study 3). Each data point represents a participant. Data points are jittered. M = mean

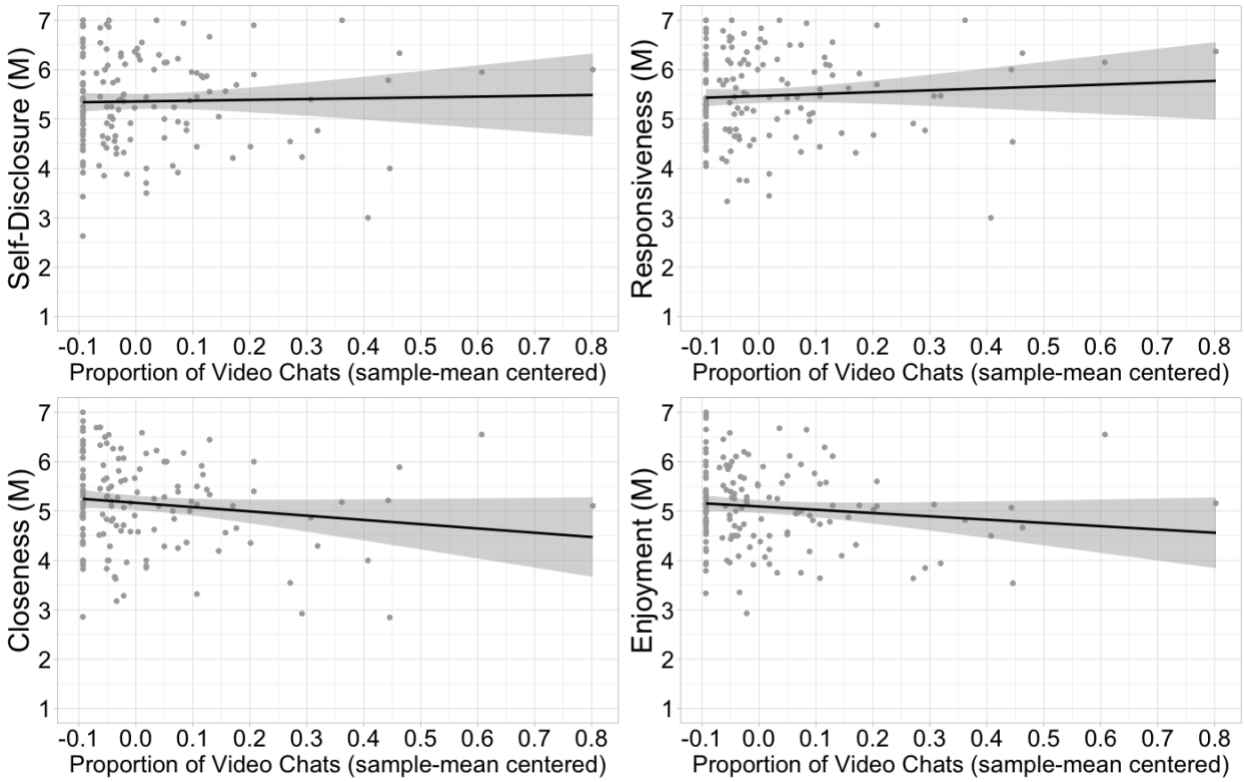


Figure B.4. Between-person Fixed Effects of Conversation Medium (Study 3). Each data point represents a participant. Data points are jittered. M = mean

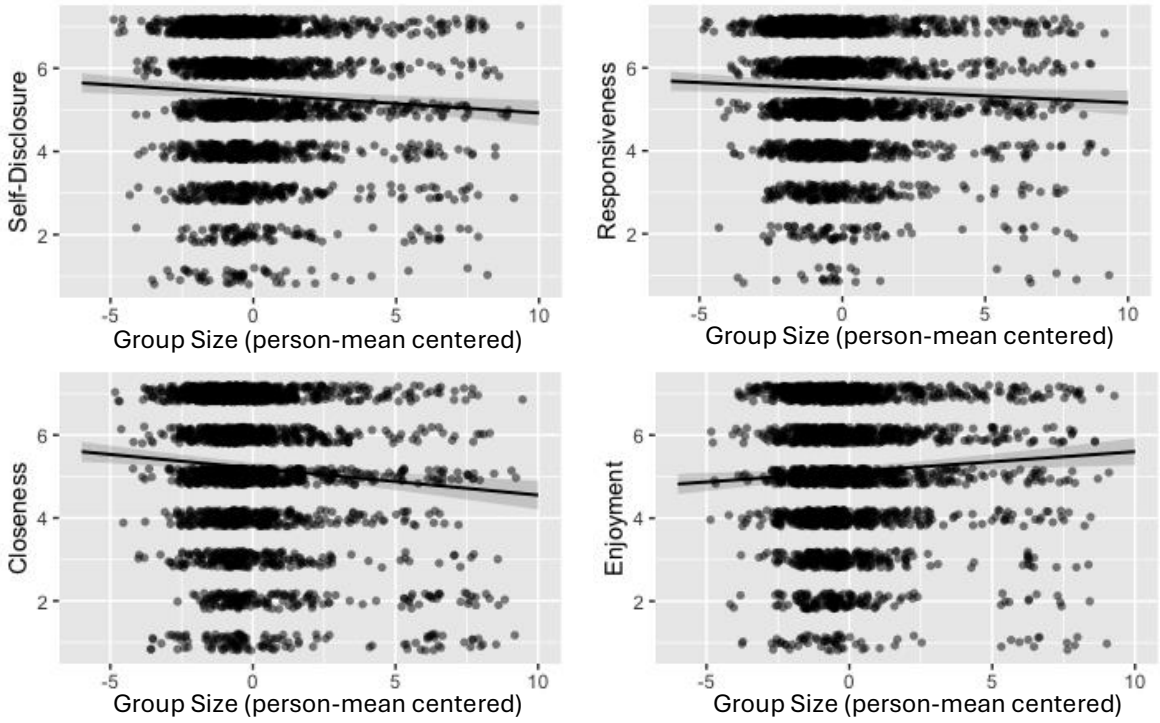


Figure B.5. Within-person Fixed Effects of Conversation Size (Study 3). Each data point represents a conversation. Data points are jittered.

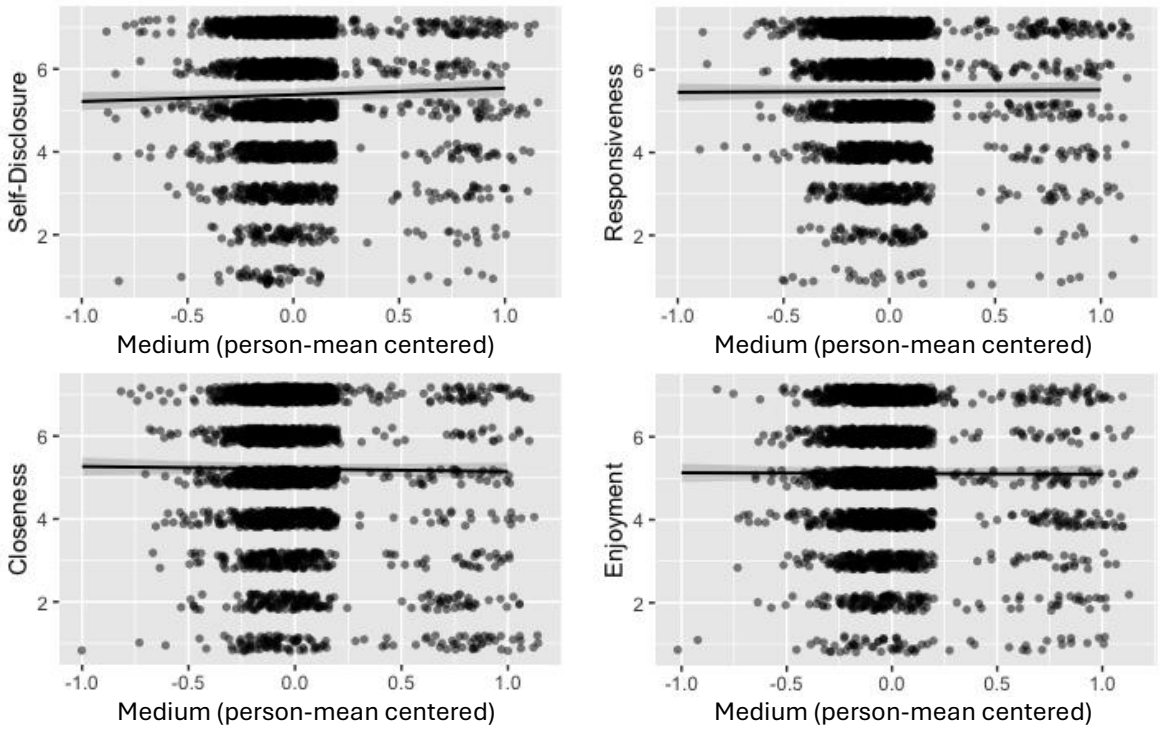


Figure B.6. Within-person Fixed Effects of Conversation Medium (Study 3). Each data point represents a conversation. Positive values indicate video chat conversations; negative values indicate in-person conversations. Data points are jittered.

APPENDIX C

PAPER 3 SUPPLEMENTAL MATERIALS

Past Ghosting Behavior and Experience as Moderators

Study 1

As exploratory analyses, we examined if previously ghosting a former partner or having been ghosted by a former partner moderated the association between the need for closure and ghosting intentions. To do so, we ran separate linear regressions on ghosting intentions with previous ghosting target and previous ghosting initiator behaviors included in the models with the need for closure and the appropriate interaction term. There was not a main effect of being a previous target of ghosting on ghosting intentions, $b = 0.43$, $SE = 0.56$, $p = .437$, nor was there a significant interaction with the need for closure, $b = -0.17$, $SE = 0.12$, $p = .151$ (model: $F(3, 549) = 8.13$, $p < .001$, $R^2 = .04$). Additionally, there was not a main effect of previously ghosting a former partner on ghosting intentions, $b = -0.24$, $SE = 0.50$, $p = .635$, nor was there a significant interaction with the need for closure, $b = -0.19$, $SE = 0.11$, $p = .084$ (model: $F(3, 549) = 45.62$, $p < .001$, $R^2 = .20$).

Study 2

As exploratory analyses, we examined if previously ghosting a former partner or having been ghosted by a former partner moderated the association between the need for closure and ghosting intentions. To do so, we ran separate linear regressions on ghosting intentions with previous ghosting target and previous ghosting initiator behaviors included in the models with the need for closure and the appropriate interaction term. There was not a main effect of being a

previous target of ghosting on ghosting intentions, b = -0.08, SE = 0.70, p = .911, nor was there a significant interaction with the need for closure, b = -0.04, SE = 0.15, p = .768 (model: $F(3, 407) = 2.02$, p = .111, R^2 = .01). Additionally, there was not a main effect of previously ghosting a former partner on ghosting intentions, b = -0.78, SE = 0.64, p = .223, nor was there a significant interaction with the need for closure, b = -0.07, SE = 0.13, p = .619 (model: $F(3, 407) = 30.31$, $p < .001$, R^2 = .18).

Study 3

As an exploratory analysis, we examined if the interaction between the need for closure and condition on needs satisfaction depended on whether they had ghosted someone in the past. To do so, we conducted an analysis of covariance (ANCOVA) on needs satisfaction with need for closure, condition, past ghosting behavior (Yes/No), and the corresponding interaction terms included in the model. The overall model was significant, $F(11, 533) = 136.72$, $p < .001$, but the main effects were not significant for the need for closure, $F(1, 533) = 2.75$, p = .098, η_p^2 = .005, condition, $F(2, 533) = 0.79$, p = .924, $\eta_p^2 < .001$, or past ghosting behavior, $F(1, 533) = 0.92$, p = .338, η_p^2 = .002. Further, the interaction was not significant between condition and past ghosting behavior, $F(2, 533) = 1.13$, p = .323, η_p^2 = .004, or past ghosting behavior and the need for closure, $F(1, 533) = 0.34$, p = .541, η_p^2 = .001, but remained significant for the need for closure and condition, $F(2, 533) = 3.52$, p = .030, η_p^2 = .013. Finally, the three-way interaction between the need for closure, condition, and past ghosting behavior was not significant, $F(2, 533) = 0.77$, p = .463, η_p^2 = .003, suggesting that the interaction between the need for closure and condition did *not* depend on whether participants had ghosted someone else in the past (see Figure C.1).

Testing Relationship Type

Study 3

As an exploratory analysis, we examined if the interaction between the need for closure and condition on needs satisfaction depended on the type of relationship participants wrote about. To do so, we conducted an ANCOVA on needs satisfaction with need for closure, condition, relationship type (friendship, romantic relationship, or casually dating), and the corresponding interaction terms included in the model. The overall model was significant, $F(17, 527) = 91.29$, $p < .001$, along with the main effects for the need for closure, $F(1, 527) = 5.39$, $p = .021$, $\eta_p^2 = .010$, and condition, $F(2, 527) = 5.64$, $p = .004$, $\eta_p^2 < .021$, but not for relationship type, $F(2, 527) = 1.58$, $p = .208$, $\eta_p^2 = .006$. Further, the interaction was not significant between condition and relationship type, $F(4, 527) = 0.47$, $p = .755$, $\eta_p^2 = .004$, or relationship type and the need for closure, $F(2, 527) = 1.62$, $p = .199$, $\eta_p^2 = .006$, but remained significant for the need for closure and condition, $F(2, 527) = 6.81$, $p = .001$, $\eta_p^2 = .025$. Finally, the three-way interaction between the need for closure, condition, and relationship type was not significant, $F(4, 527) = 0.79$, $p = .530$, $\eta_p^2 = .006$, suggesting that the interaction between the need for closure and condition did *not* depend on whether participants wrote about a friendship, romantic relationship, or casual dating situation (see Figure C.2).

Supplemental Figures

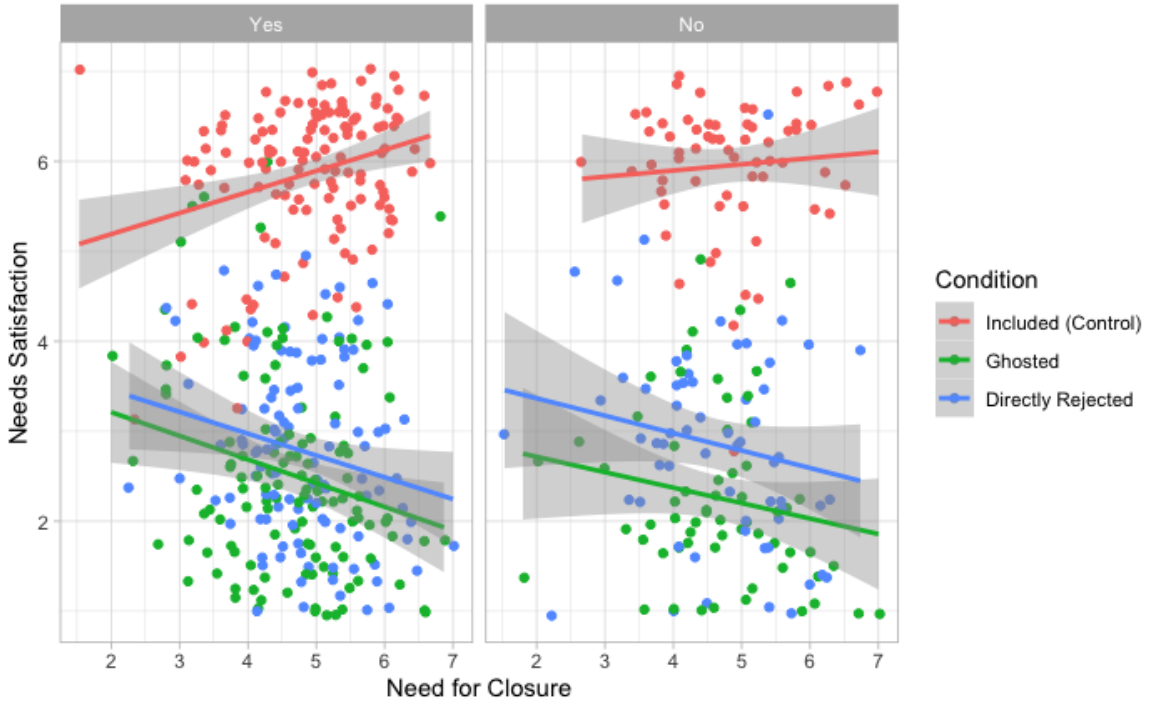


Figure C.1. Three-Way Interaction Between the Need for Closure, Condition, and Past Ghosting Behavior for Needs Satisfaction. Data points are jittered for ease of interpretation.

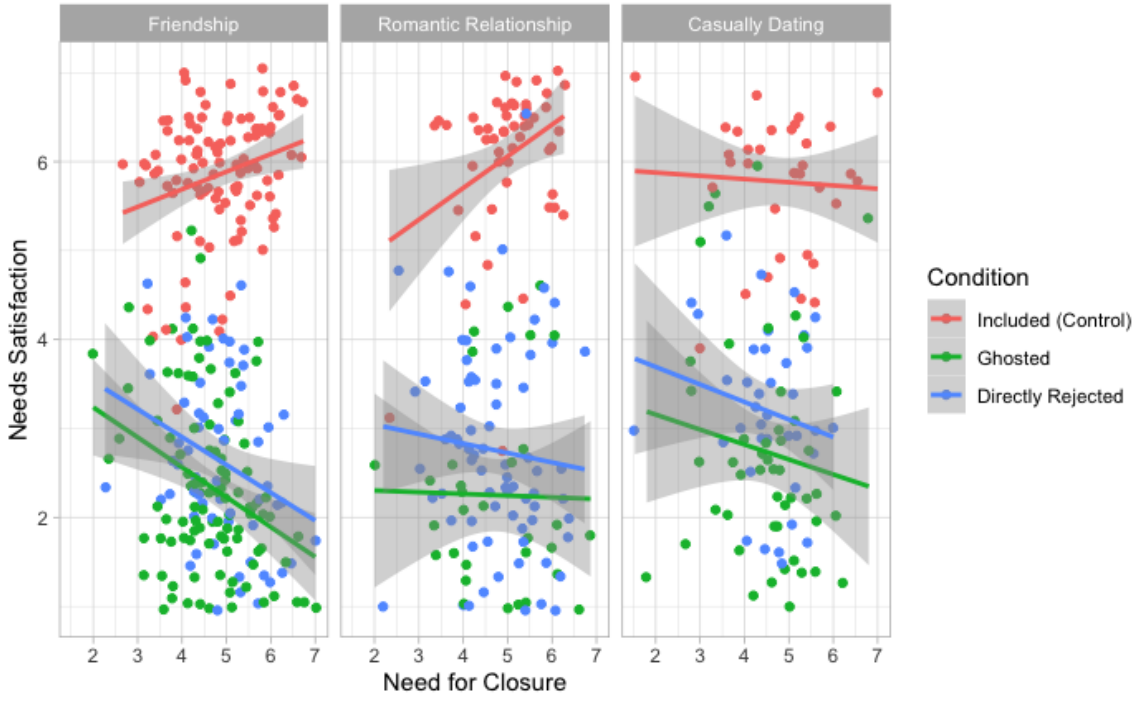


Figure C.2. Three-Way Interaction Between the Need for Closure, Condition, and Relationship Type for Needs Satisfaction. Data points are jittered for ease of interpretation.