

Social Sharing of Emotions and Bivalent Emotions

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I thank everyone who spends time reading this thesis to learn about my research. This thesis is not as well-integrated as it would otherwise have been because of the unexpected interruptions caused by COVID-19 to my original plans for research focusing entirely on bivalent emotions. Considering that the pandemic had stirred intense negative emotions in people, there would be difficulties collecting reports of bivalent emotional experiences (and purely positive ones for comparisons) from participants in naturalistic settings. Therefore, I chose to put this line of research on hold and adapted to those unprecedented times by turning to an investigation of social sharing of COVID-related problems (Projects 1 and 2). After the pandemic started settling down, I returned to the topic of bivalent emotions after reading about the more recent body of research on effects of bivalent emotions on cognition. These recent studies sparked new ideas, so I not only resumed my original work on bivalent emotions within the framework of social sharing (Project 3) but also launched another project to act on my new interest in the cognitive effects of bivalent emotions in intergroup contexts (Project 4), which I believed would have even more profound implications on real-world issues—particularly, prejudice, discrimination, and dehumanisation.

Please also note that Study 1 constitutes a part of the manuscript titled “Social Sharing of Emotion During the Collective Crisis of COVID-19” which has been submitted for publication. The manuscript is available as a pre-print on the Open Science Framework (OSF) at <https://osf.io/9p3wh/>. Other studies presented in this thesis will also be submitted for publication in due time.

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ABSTRACT

This thesis has two objectives: Investigating effects of sharing one's own emotions with other people across different stressors and exploring bivalent emotions in interpersonal and intergroup contexts. Chapter One presents a general introduction to the theoretical foundation which my pursuit of the two lines of research stems from. Chapter Two presents Project 1, an online study (Study 1) that captured social sharing behaviour during a relatively early stage of the COVID-19 pandemic using prospective questionnaires. Results suggested that social sharing was associated with emotion-improving effects. Chapter Three presents Project 2, which involved two online experimental studies confirming that interpersonal encouragement of reconstrual, rather than of recounting, generally yielded more positive effects across stressors varying in time span (interpersonal conflicts vs. COVID-19 news; Study 2A) or induced specific emotion (angry vs. regretful conflicts; Study 2B). Chapter Four, switching the thesis's focus to questions relevant to bivalent emotions, presents Project 3, an online study (Study 3) which explored motives for and affective outcomes of sharing this kind of emotional experience using prospective questionnaires. Results showed that motives and outcomes associated with sharing bivalent emotions were similar to those associated with sharing negative emotions. Chapter Five presents Project 4, an online experimental investigation of bivalent emotions (Study 4) that moved beyond into the framework of social cognition. Despite the new insights into the dehumanisation literature, this study failed to provide evidence that presenting outgroups' bivalent, rather than univalent, emotions helped improve intergroup relations. Finally, Chapter Six presents a general discussion about the main findings from the above four projects,

the limitations observed throughout my DPhil research, and the implications and future research directions indicated by the research reported in this thesis.

CHAPTER ONE

General Introduction

1.1 Abstract

In this chapter, I present the literature that lays the theoretical foundation for my two lines of research: Social sharing of emotions and bivalent emotions. For the former, I review Omarzu's (2000) disclosure decision model and other prior investigations of social support and emotion regulation, which have provided evidence opposing to the lay belief that simply venting helps alleviate emotional distress by highlighting the critical role of identifying and fulfilling sharers' needs in determining the regulatory success of social sharing. For the latter, I review relevant research which has offered initial views on the functions of bivalent emotions based on Chaiken's (1980) heuristic-systematic model, Festinger's (1957) cognitive dissonance theory, and Schwarz and Clore's (1983) affect-as-information model.

People are happy when receiving a present, excited when seeing an old friend, angry during an argument, sad about losing a loved one, etc. These different reaction patterns are called “emotions,” which according to the American Psychological Association (APA), involve “experiential, behavioural and physiological elements” (APA Dictionary of Psychology, n.d.). Although emotions are often intuitively considered internal and personal reactions, in most cases they are caused by other people (Parkinson, 1996), just as demonstrated in the above examples. Emotions also have social consequences: Once an emotion—positive or negative—is experienced, it is communicated to other people 80-95% of the time (i.e., *social sharing of emotion*; for a review, see Rimé, 2009). This phenomenon has two defining characteristics: The emotion is recalled in a “socially shared language,” and an addressee is present “at least at the symbolic level” (Rimé et al., 1991, p. 438). Given the obvious importance of emotions in people’s social lives, this thesis aims to investigate the processes and consequences of the phenomenon of social sharing of emotions. In particular, I took advantage of the unprecedentedly challenging circumstances due to the impact of the deadly coronavirus COVID-19 pandemic during my studies and focused on the sharing of negative, especially crisis-related, emotions.

Another objective of this thesis is to address a third kind of emotional experience besides positive and negative: *Bivalent*, wherein both positive and negative emotions are felt (also known as “emotional ambivalence”; Larsen et al., 2001). For example, people may feel happy when receiving a present but sad too when realising that it is from a loved one who has recently deceased. Although there is a growing body of research on bivalent emotions, most of the work has focused on proving that they exist in not only laboratory (e.g., Larsen & Stastny, 2011; Pan et al., 2016) but also naturalistic (e.g., Hostler & Berrios, 2021; Larsen et al., 2001) settings, how to measure

them (for a review, see Kreibig & Gross, 2017), as well as their intrapersonal impact (e.g., Fong, 2006, on creativity; Hostler & Berrios, 2021, on judgments). My research, on the other hand, is interested in the social aspect of bivalent emotions. Specifically, I explore the reasons for, and processes involved in, sharing bivalent emotions, as well as the impact of perceiving other people's bivalent emotions on one's own cognitions about those people.

1.2 Social Sharing of Emotions

When an emotion is shared, its related mental images, body sensations, and subjective feelings are re-experienced (Rimé, 2009; Rimé et al., 1991). This explains why people are so eager to share their positive emotions. Duprez et al. (2015) collected participants' motives for discussing joyful episodes using their own developed Social Sharing Motive Scale (SSMS-39) and reported that a desire to revive pleasantness (i.e., capitalisation) and to elicit empathy/attention (i.e., social integration) seems to underly this sharing behaviour. Indeed, prior research has shown that sharing positive experiences not only enhances the sharer's well-being and positive affect but also strengthens the social bond between the sharer and the listener when the latter responds enthusiastically (e.g., Gable et al., 2004; Gable & Reis, 2010). However, in the case of negative emotions (which are the focus of the first part of my thesis), if social sharing reactivates them and leads people to an unpleasant state, then why do people still share them?

Duprez et al. (2015) found that compared to positive episodes, negative ones are discussed more frequently because people want to find meaning and seek attitudes and actions from others that help alleviate distress (see also Harlow & Cantor, 1995). In other words, people allow themselves to feel bad again during interpersonal exchange in

the hope of making sense of their experience and receiving support that will help them feel better in the longer run. Unlike sharing of positive experiences, evidence supporting the emotion-improving effects of sharing of negative experiences is equivocal.

Although some studies show benefits of discussing negative episodes such as bringing interactants closer to each other (for reviews, see Collins & Miller, 1994; Rimé, 2009) and promoting the feeling of social integration (for a review, see De Rivera & Páez, 2007), others argue that social sharing intensifies the discussed negative emotions and leads to higher levels of mental rumination (Kanyangara et al., 2007; for a review, see Rimé et al., 1998). Therefore, it appears that there are other factors that determine whether social sharing is an effective emotion regulation process. What might they be?

Indeed, despite the lay belief that “verbalising an emotional memory can transform it and that after verbalisation, this memory would lose a significant part of its emotional load” (i.e., emotional venting; Zech et al., 2004, p. 8), researchers have found no empirical evidence that a reduction in emotional intensity follows the *mere* act of talking about an emotional experience (for a review on studies using questionnaires and autobiographic recall, see Rimé et al., 1998; for studies using the diary design, see Rimé et al., 1994, as cited in Zech et al., 2004, p. 10, as an example; for experimental studies, see Zech & Rimé, 2005, as an example; for a meta-analysis on clinical studies, see Van Emmerik et al., 2002). If the answer to our question does not reside in the “sharing” part of social sharing, then it is probably linked to the “social” part.

Omarzu’s (2000) disclosure decision model proposed that self-disclosure (i.e., verbally communicating one’s personal information—in the context of this thesis, emotional experiences—to another person) is only adaptive if it is effective in fulfilling one’s need (e.g., for self-clarification or social validation) and if there is an appropriate listener available. An appropriate listener is, according to Horowitz et al. (2001),

someone who can provide the right social support that satisfies the distressed person's need. Complementing these propositions, previous studies have generally indicated that distress is more likely to decrease when people engage in supportive social interactions and increase when social interactions are unsupportive (e.g., Lepore, 1992; Major et al., 1997; Manne et al., 1997). In other words, the adaptiveness of social sharing depends on whether the sharer's need is correctly identified and fulfilled. Before further examining this logic, two concepts—social support and emotion regulation—should be introduced in greater depth, so that social sharing of emotions can be better understood in relation to them.

1.2.1 Social Support

Social support is defined as “provision of psychological and material resources intended to benefit an individual's ability to cope with stress” (Cohen, 2004, p. 676). Similarly, Schwarzer et al. (2004) formulates this construct as “resources provided by others, as coping assistance or as an exchange of resources” (p. 160). Various types of social support are documented in the literature. Within the context of social sharing of emotions, two types of support that listeners may offer have been distinguished by Rimé (2009): *Socio-affective* support, which offers comfort and validation; and *cognitive* support, which encourages alterations in cognitions about an emotional experience (i.e., reappraisal).

Considering that distressed individuals' ultimate goal in sharing their emotions is to feel better about what happened, cognitive support, which changes how people see things instead of simply tuning in people's feelings, should theoretically be the more helpful type of support that listeners can provide. Indeed, many studies have demonstrated that cognitive support is associated with more long-term recovery and socio-affective support is associated with temporary alleviation of emotional distress

(e.g., Brans et al., 2014; Lepore et al., 2004; Nils & Rimé, 2012; Tamir, 2009).

However, cognitive support sometimes may be ineffective in the long run because problems may be persistent and intractable.

When sharing a negative experience, it is natural for sharers to describe “what happened” at the beginning of the conversation and inevitable that they relive some of the unpleasant feelings associated with the event. This process may lead to rumination (i.e., repetitive thinking about the negative features of an upsetting event; Skinner et al., 2003), so once the context is established, listeners ought to intervene and provide support. In the case of cognitive support, listeners cue sharers to adopt an alternative perspective and see the event unfold again. Although sharers still need to somewhat ruminate about the event to come up with a way to reappraise it, their negative affect should eventually de-escalate as they reach closure (i.e., the feeling of psychological completion). However, when problems are persistent and intractable, it is likely that sharers have difficulties changing their view of the problems and just keep on talking about the problems with their listeners (i.e., co-rumination; Rose, 2002).

On the other hand, in the case of socio-affective support, listeners provide “responses that offer help, support, comfort, consolation, legitimization, attention, bonding, and empathy” which are “well-suited to alleviating [sharers’] insecurity, anxiety, helplessness, loneliness, and so forth” (Rimé, 2009, p .75). Although this type of support does not modify sharers’ appraisal of the negative situation as cognitive support can do, it still has cognitive effects that help alleviate sharers’ distress. For example, comforting and validating feedback provided by listeners often communicates that sharers’ responses to an unpleasant experience make sense and are understandable (Linehan, 1993), improving sharers’ emotional self-efficacy (i.e., perceived capability to self-regulate; Witkowski, 2017), which in turn may increase their coping potential

(i.e., perceived capability to deal with the aversive situation; Lazarus, 1991). It is also likely that the comforting and validating feedback alone already distracts sharers from the emotional stimulus and occupy their attention with the fact that someone acknowledges their feelings for what they are. Therefore, despite not being equivalent, socio-affect support seems to share some qualities in common with distraction, which according to Skinner et al. (2003), refers to active attempts to deal with an unpleasant reality by re-orienting people to an alternative pleasurable activity and blocking emotional processing of the reality. In their experiments, Sheppes and colleagues (2011) found that people in high-intensity negative situations display a stronger preference for distraction to reappraisal as their coping strategy than do those in low-intensity situations. Therefore, in face of problems that are difficult to deal with (and possibly are highly negative), cognitive support may not be as helpful as it normally would be, and sharers may be more motivated to seek socio-affective support which can make them feel less threatened by the negative emotional stimulus temporarily. One of the focuses of my first line of research is to investigate the role and effectiveness of cognitive support in helping sharers pursue regulatory success across different problems.

1.2.2 Emotion Regulation

Emotion regulation refers to the ability to exert control over one's own emotional state (Gross, 2001) through different strategies, such as reappraisal and distraction (e.g., Ayduk & Kross, 2010; McRae et al., 2010). Reappraisal, which aims to change one's appraisal of a situation (Gross, 1998a), is one of the regulatory strategies that have received the most research attention, and hence is a focus of the first line of my research. Individuals can deploy the emotion regulation process in solitude. For example, when people express their emotions in a diary, they often imagine that they are writing to a different self (Apgar, 1997), write in the past tense (Polanyi, 1982), and

focus their attention on the broader picture of what happened (Meier, 2002). Such a writing process facilitates spontaneous self-distancing and leads people to display less negative affect when reflecting on the experience again in the future (see Ayduk & Kross, 2010, Study 3). Similarly, Pennebaker and colleagues (2001, 2007) documented a number of emotional and even physical health improvements following expressive writing. In the context of social interactions, people have an actual instead of imaginary listener. More relevant to the focus of this thesis, emotion regulation can be deployed during social interaction (i.e., *interpersonal emotion regulation*). It should be noted that, according to Zaki and Williams (2013), if an interpersonal episode occurs without the sharer's pursuit of a goal to alter her affective state, it does not constitute "regulation" but "modulation".

Zaki and Williams (2013) also proposed that the adaptiveness of interpersonal regulation can be response-dependent—that is, whether the sharer feels better after sharing her emotions depends on the listener's feedback. Prior research has indeed showed that the sharer's positive affect is intensified more when the listener responds enthusiastically (Gable & Reis, 2010) whereas negative affect is dampened when the listener responds supportively (Nils & Rimé, 2012). Particularly in the case of negative affect, the listener's supportive behaviours can signal to the sharer that it is safe to reconsider the shared event because she does not have to face it alone but instead has access to both parties' psychological resources (Zaki & Williams, 2013). This acknowledgement, as a result, can make the negative event appear less threatening (Lazarus & Folkman, 1984) and facilitate alterations of the sharer's responses to the event (e.g., Coan, 2011; Lepore et al., 1993). Even when the listener is unable to provide support that directly alleviates the sharer's distress, the latter can still find the

sharing rewarding by recognising that someone else is “on the same page” as herself (e.g., Klucharev et al., 2009; Zaki et al., 2011).

Interpersonal regulation can also be adaptive without requiring the listener to respond in any particular way (i.e., response-independent; Zaki & Williams, 2013): As people communicate their emotions to listeners, they often “[refine] appraisals of those emotions” in order to find the more “fine grained” way to label their affective states and assess the causes underlying those states (p. 806). Such labelling, accompanied by assessment, has been found to reduce the ambiguity of affective states and facilitate coping (e.g., Kircanski et al., 2012; Kross et al., 2005; Pennebaker, 1997). Therefore, even when the listener fails to provide appropriate feedback, social sharing that contains these two psychological ingredients can still promote regulatory success.

In sum, people initiate social sharing in times of distress to seek support (socio-affective or cognitive) from someone else. Once a supportive environment is established, or at least when labelling and assessment are produced, sharing is a useful act that promotes emotion regulation.

1.2.3 Overview of Projects 1 and 2

There are numerous stressors in people’s daily lives. Is social sharing consistently effective in regulating emotions across these stressors? To answer this question, Project 1 observed social sharing behaviour during the collective crisis of COVID-19, which was declared as a “pandemic” by the World Health Organization on 11th March 2020. Prior research conducted in the aftermath of major collective crises has yielded convergent evidence that social sharing is a typical response to these negative events. For example, 99% of respondents still expressed a need to talk about their emotions even one week after the 2004 Madrid bombings (Rimé et al., 2010), and the number of French-language tweets produced by users living in France more than

doubled on the day of the Paris terrorist attacks on 13th November 2015 (Garcia & Rimé, 2019). The previously investigated crises were usually, if not always, acute—that is, they started and ended within the same day (although their impacts obviously persisted for much longer), meaning that social sharing took place *after* the central event had ended. By contrast, the COVID-19 pandemic was a chronic, enduring event, so social sharing was initiated *during* this crisis. Therefore, it is worth examining the processes of social sharing and its effects on sharers' emotional well-being in the face of this unprecedented stressor.

In Project 2, I zoomed in on the main mechanism that has been proposed to underlie the adaptiveness of social sharing: Cognitive support provided for sharers that promotes reconstrual—a sub-type of reappraisal. To my knowledge, Lee and his colleagues (2020) were the first to experimentally investigate the effects of sharing in dyadic settings by manipulating reconstrual. However, they only focused on acute interpersonal conflicts, so it is unclear that whether the benefit of this chatting style also applies when discussing more chronic and intractable stressors. Even when the same type of stressor is in question, it may lead to different kinds of emotional response. For example, within the domain of interpersonal conflicts, people can be angry with a friend because of what the friend has said or done or be regretful for what they themselves have said or done during an argument. Therefore, this project further tested whether the effects of reconstrual are moderated by the specific emotion experienced by the sharer.

So far, I have proposed that the effects of social sharing may depend on the type of stressor in terms of its time span or the emotion it induces. Although negative experiences are the most common source of distress, bivalent emotional experiences can also elicit discomfort because they reflect a violation of consistency (Cacioppo et al.,

1997; Priester & Petty, 1996). I will elaborate more on the nature and the social aspect of these bivalent emotional states in the next section.

1.3 Bivalent Emotions

Researchers have paid most of their attention to univalent (purely positive or negative) emotions given the prevalence of this kind of emotional experience in people's daily lives. Using the experience sampling method, Watson and Stanton (2017) recruited two groups of participants who were asked to rate their current mood on the expanded version of the Positive and Negative Affect Schedule (PANAS-X; Watson & Clark, 1999). The authors then standardised all ratings ($N = 12,788$) on a within-subjects basis and collapsed them across participants. Based on this combined sample, the authors reported that although people do not experience any significant emotions 42.2% of the time, they experience single significant positive or negative emotion or multiple significant positive or negative emotions more than half of the time (52.7%). On the other hand, a mix of positive and negative emotions is experienced only 5.1% of the time. An emotion is considered "significantly" experienced when it is rated as 3 or higher out of 5 (for similar approaches, see Peterson & Janssen, 2007; Riediger et al., 2009). Despite them not being as prevalent as univalent emotional episodes, research on bivalent episodes is still warranted to complete understanding of the affective side of people's lives. In particular, this thesis focuses on two unique features of bivalent emotions: Their inherent feelings of conflict and cognitive flexibility function.

1.3.1 Feelings of Conflict

The experience of bivalent emotions can manifest in two ways: Simultaneously (i.e., feeling opposing emotions concurrently; Larsen et al., 2001; Larsen & McGraw, 2011) or sequentially (i.e., feeling opposite emotions in rapid alternations; Filipowicz et

al., 2011; Sinaceur et al., 2013). Despite the difference in their time course, Rothman and Melwani (2017) argue that these two forms of bivalent emotions share a similar internal experience, in that, for example, they are correlated with each other (Spencer-Rodgers et al., 2010b) and triggered by emotionally complex events (Larsen & McGraw, 2011). More importantly, both forms are accompanied by feelings of conflict (Thompson et al., 1995): When opposing emotions are experienced simultaneously, people report feeling conflicted (Aaker et al., 2008); when opposing emotions are experienced in rapid alternations, conflict is created by overlaps of emotions as one emotion emerges on top of the residue of another emotion (Carrera & Ocejja, 2007; Pe & Kuppens, 2012). Given all these similarities, I treat simultaneous and sequential bivalent emotions as equivalent in this thesis.

According to Festinger (1957), when people hold two inconsistent thoughts (i.e., cognitive dissonance), they experience conflict, which gives rise to uncomfortable tensions that need to be addressed in some manner. Drawing upon this classic work focusing on conflict between cognitive elements, researchers have extended their investigations to situations in which people experience emotional or affective inconsistency. Williams and Aaker (2002) found that similar to cognitive dissonance, bivalent emotions, violating internal consistency, produce a third independent emotion of discomfort (see also Cacioppo et al., 1997; Priester & Petty, 1996). Therefore, although both bivalent and negative emotions induce unpleasantness, they are two distinctive affective states in that the former is accompanied by felt conflict which typically does not arise from the latter.

1.3.2 Cognitive Flexibility

Bivalent emotions serve a cognitive flexibility function at the individual level. Chaiken's (1980) heuristic-systematic model (HSM) posits that people process

information heuristically (i.e., superficially assessing the situation by assimilating it to existing knowledge) or systematically (i.e., effortfully and rationally taking all information pertaining to the situation into consideration). The former is the default strategy whereas the latter is only activated when people fail to reach their “sufficiency threshold,” which is the point at which people feel sufficiently confident about their conclusion given the relative importance of the decision that they are making. In simple, safe situations, the threshold is set low so that people can reach it effortlessly using heuristic processing. However, in times when people view the situation as problematic and potentially threatening, their sufficiency threshold is raised, triggering more careful and deep processing of information (for a review, see Böhner et al., 1995). According to Schwarz and Clore’s (1983) affect-as-information model, negative emotions give rise to such a perception of one’s environment and consequently activate systematic processing (e.g., Gasper & Clore, 2002).

Drawing on the same model, Rothman and Melwanie (2017) proposed that bivalent emotions alert people to complex and contradictory elements in their environment, driving them to assess the effectiveness of their current thinking and behaviour in response to the situation and broaden their scope of attention to include divergent perspectives. Relatedly, the inherent conflict associated with bivalent emotions should motivate people to seek out further information that will allow them to develop a more balanced approach that enables resolution of their felt conflict. Integrating these cognitive and motivational functions into the HSM, bivalent emotions should raise people’s sufficiency threshold and lead them to engage in more careful and thorough thinking that helps them close the gap between that threshold and their current judgmental confidence level. Indeed, previous studies have suggested that people experiencing bivalent emotions engage in more systematic processing (Jonas et al.,

1997; Petty & Cacioppo, 1986), such as reappraising the causes of their emotions (Zanna & Cooper, 1974), if the event is of high personal relevance. In this thesis, I extend these functional views of bivalent emotional states to the interpersonal and intergroup levels to supplement the social sharing and dehumanisation literature.

1.3.3 Overview of Projects 3 and 4

In Project 3, I investigated bivalent emotions within the context of social sharing of emotions. By Rothman and Melwanie's (2017) logic, it seems that individuals experiencing bivalent emotions would search for and welcome cognitive support from other people that challenges their current way of thinking. To my knowledge, no empirical work has been done on why people discuss their bivalent emotional experiences with other people. In fact, although Rimé (2009) emphasises that social sharing develops naturally following the experience of an emotion regardless of its valence, research on social sharing has always focused on univalent emotional states (positive or negative) and neglected bivalent emotional states (positive and negative). Therefore, it is not known whether people would feel any urge to talk about their bivalent emotions. I attempted to take the first step towards filling this knowledge gap by recording the parameters of and reasons for sharing bivalent emotions using prospective questionnaires. I also examined the emotional outcomes of this sharing and compared them to those obtained from sharing univalent emotions. In my research, I was more interested in examining emotional and sharing episodes that occurred within a given interval in the future (e.g., in the next seven days) than those that occurred in the past (e.g., in the last month). Therefore, unlike retrospective questionnaires which ask participants to recall an episode that happened before they take part in a study, my prospective questionnaires instructed participants not to report an old episode but to

wait for one to naturally take place. This method allowed me to capture participants' thoughts and feelings as soon as possible after they emerged.

In addition to influences on people's information processing, there is indirect evidence suggesting that bivalent emotions indicate a "complex" mind. Grossmann et al. (2019) presented initial observational evidence that individuals who report more bivalent emotions in situations involving conflict and decision-making are more likely to be perceived as "wise" by their peers. In their experimental studies, Prati and Giner-Sorolla (2018) also found that perceiving disliked outgroups' (i.e., social groups with which people do not identify) bivalent emotions, rather than univalent emotions, improved participants' evaluations of members of those groups. Given that outgroups—especially the extreme outgroups which are stereotyped as the "lowest of the low" (Fiske et al., 2002)—are subject to prejudice and even dehumanisation, Project 4 built upon these previous findings and continued examining the cognitive effects of bivalent emotions on intergroup relations.

CHAPTER TWO

Project 1: Social Sharing of Emotion During the Collective Crisis of COVID-19¹

2.1 Abstract

Does sharing negative experiences related to an unfolding collective crisis make people feel better? The present study used prospective questionnaires to collect data from the United Kingdom (UK) and the United States (US) between June and September 2020 to investigate the processes and emotional outcomes of social sharing about COVID-related events. Results showed that participants generally perceived their feelings about an event to have improved when that event was shared but perceived relatively less improvement when the shared event was a news story (especially one shared in-person) than when it was a personal experience. Furthermore, there was a selective attenuating effect of sharing on participants' negative feelings about their personal experience. Overall, the study suggested that social sharing is linked with emotional recovery and therefore appears to be an adaptive response to a persistent collective crisis.

¹ This chapter adapts material from a manuscript titled “Social Sharing of Emotion During the Collective Crisis of COVID-19” which has been submitted for publication. The manuscript is available as a pre-print on OSF at <https://osf.io/9p3wh/>.

It is a common response for people to talk about their emotions with other people once those emotions are experienced (i.e., *social sharing of emotions*; Rimé et al., 1991; for a review, see Rimé, 2009). Following the World Health Organization's (WHO) declaration that the outbreak of the deadly coronavirus COVID-19 constituted a “pandemic” on 11th March 2020, social sharing tended to focus on hopes and fears in relation to this collective crisis. The present research took advantage of these unprecedented circumstances and investigates the processes and outcomes of social sharing in the face of an enduring stressor that has a collective impact.

Social sharing is commonly observed in times of distress. An early study by Schoenberg et al. (1975) found that 88% of bereaved people expressed a need to talk in response to the acute stressor presented by the death of their loved ones. Similarly, Mitchell and Glicksman (1977) reported that approximately 80% of cancer patients undergoing radiotherapy expressed the need to “discuss their situation more fully” with someone (p. 64) in response to this more chronic, personal stressor. More recent studies conducted in the aftermath of major collective traumas have obtained concordant results. For example, in a study conducted in the days immediately following the September 11 attacks on the World Trade Center in 2001, 98% of the interviewed American adults stated that talking with others helped them to regulate their emotions (Schuster et al., 2001). Another study investigating the aftermath of the 2004 Madrid bombings reported that 99% of respondents were still talking about their emotions one week after the event (Rimé et al., 2010).

People talk about their negative emotions because they want to make use of the interpersonal context to vent, find meaning, and seek social support that helps them feel better (Duprez et al., 2015; Harlow & Cantor, 1995). There is, however, mixed evidence as to whether this emotion-recovery goal can really be achieved by social sharing:

While some previous studies have suggested that social sharing leads to a number of positive outcomes (for reviews, see Collins & Miller, 1994; De Rivera & Paez, 2007; Rimé, 2009), other studies have shown the opposite effects (Kanyangara et al., 2007; for a review, see Rimé et al., 1998).

Rimé et al.'s (2010) study of the 2004 Madrid bombings directly assessed both positive and negative consequences of social sharing in response to a collective crisis and reported mixed results: The extent of initial social sharing (one week after the event) was positively associated with higher emotional arousal in relation to the terrorist attacks and higher levels of mental rumination three and eight weeks after the event. However, initial social sharing was also found to predict increased positive affect, reduced feelings of loneliness and stronger perceptions of social support in those weeks. Therefore, despite positive psychosocial effects, talking about a traumatic event seemed to simultaneously incur the cost of reactivating and reinforcing negative emotions. More recently, Garcia and Rimé (2019) focused on social sharing on Twitter in response to the Paris terrorist attacks on 13th November 2015. Results showed that the number of French-language tweets produced by users living in France more than doubled on the day of the attacks. There was also an associated rise in the use of negative-affect terms and a significant drop in the use of positive-affect terms, suggesting a collective experience of negative emotion. However, the use of solidarity-related terms was found to peak one day after the attacks, potentially indicating positive psychosocial effects associated with expressing negative emotions on social media.

Most (if not all) of the collective traumatic events that have been investigated had a very short duration. For example, the Paris terrorist attacks (Garcia & Rimé, 2019) and the Madrid bombings (Rimé et al., 2010) both started and ended within the same day (although their consequences obviously persisted for much longer). This

means that the social sharing investigated in studies of these events typically took place *after* the central shared event had ended. By contrast, the COVID-19 pandemic was a global event that had endured for many months and that still persisted at the time when people started discussing it. Given the prevalence of social sharing in communities affected by traumatic events (e.g., Rimé et al., 2010; Schuster et al., 2001), I conducted an online study which aimed to add to the existing empirical work by tackling the main question of whether sharing negative emotions *during* an unfolding crisis serves the purpose of emotional recovery. Because previous investigations did not directly assess changes in both positive and negative affect and compare in-person and remote social sharing, I included measures in my study that helped address these limitations.

2.2 Study 1

I used prospective questionnaires to collect self-reports of participants' experienced feelings before and after sharing negative pandemic-related events, the nature of the event they discussed during social sharing, and their choice of communication channels for social sharing. In particular, I instructed participants to report on their sharing of two different types of COVID-related negative events, one involving a personal experience and the other involving a news story, to facilitate comparison of responses to personally and collectively experienced events. I targeted participants residing in either the UK or the US, two Western countries whose COVID-19 timelines had been similar when we started conducting the present research (June 2020). Cross-national comparisons allowed an initial assessment of whether effects generalised across broadly individualistic societies.

Mixed effects of sharing have been reported in previous studies investigating acute collective crises (Garcia & Rimé, 2019; Rimé et al., 2010). The present study

assessed whether similar effects operate during a chronic crisis, or whether the balance between positive and negative effects of sharing differs because of the crisis's continuing nature. One possibility was participants who shared COVID-related events would tend to display intensified negative affect and weakened positive affect after sharing than those who did not share as a consequence of repeatedly revisiting COVID-related unpleasant feelings and thoughts. An alternative possibility was that those who talked about COVID-related events would tend to display weakened negative affect and intensified positive affect as a result of the social support they solicited and the appreciation that they were not alone in facing this unprecedented situation. In other words, my study addressed the question of whether sharing COVID-related events led to emotional worsening or emotional improvement compared to not sharing.

I also expected the extent of emotional effects to differ depending on whether the shared event was personally experienced by the participant or was a news story that potentially impacted the participant (sharer) and listener to a similar extent (i.e., collective experience). In my view, personal experiences are typically shared with others in order to solicit some form of social support (e.g., sympathy or advice; Duprez et al., 2015), whereas news stories are shared because they are perceived to be equally interesting or important to the other person. Consequently, there is likely to be more interpersonal emotion regulation by the listener in the case of personal experiences.

My secondary analyses addressed whether the effects of sharing differed across different communication methods. I suspected that emotions would be regulated to a lesser extent after remote than in-person sharing, partly because remote interaction limits access to a wide range of signals (e.g., shared environment, olfaction, touch), and partly because real-time face-to-face interaction permits more dynamically attuned responsiveness to presented concerns (for a review, see Parkinson, 2008).

2.2.1 Methods

i) Participants

At the time when this study was conceived, no researchers to my knowledge had conducted a study using a similar design. Therefore, I used the default value for each parameter when computing the power analysis for this study. G*Power (version 3.1; Faul et al., 2009) was the software used for all power analyses presented in this thesis. For the present study, in order to detect a medium f^2 , which is .15 according to the G*Power 3.1 manual (2021, p. 33), with 95% power and 5% alpha using a linear multiple regression (fixed model, R^2 deviation from zero) containing four predictors, a total sample size of 129 was required.

I recruited participants using the Prolific platform (Peer et al., 2017) between 5th June 2020 and 3rd September 2020. All participants gave their informed consent prior to their inclusion in the study. The sample size was larger than what my power analysis recommended because I expected to see substantial participant attrition following completion of each stage of data collection. I was also planning to conduct follow-up studies to create a longer time series of data, so I expected that not all participants in this study would agree to be contacted in the future or still agree to take part in the follow-up studies when subsequently contacted.

During data collection, I requested 80 participants who already showed signs of severe psychological distress (scoring ≥ 30 out of 50 on the Kessler Psychological Distress Scale; also known as the K10; Andrews & Slade, 2001) in the pre-screening stage to withdraw from participation out of ethical concerns: Because I was not trained to offer any professional clinical support after participants recalled details of unpleasant events that they had recently experienced, the study potentially added further mental burden to those who were already experiencing significant distress.

After completing data collection, six further participants who did not report either a COVID-related personal experience ($N = 4$) or a COVID-related news story ($N = 2$) were excluded from analysis, as were 11 participants whose reported news stories were in fact their acquaintance's personal experiences. After these exclusions, the final sample included 285 participants (152 female, 130 male, 3 stating 'other'; $M_{\text{age}} = 33.50$, $SD_{\text{age}} = 11.35$), with 144 living in the UK and 141 in the US since the outbreak of the pandemic.

ii) Design

This study had a 2 (Country: UK vs. US) x 2 (Event: personal experience vs. news story) x 2 (Time: before the 24-hour interval – T1 vs. after the 24-hour interval – T2) mixed design, with the first independent variable being a between-subjects factor and the latter two within-subjects factors. This design was used in analyses of positive and negative affect, which were two of the main dependent variables. The other main dependent variable was overall perceived affect change which was measured only at T2 and was consequently analysed using the same design but without the within-subjects factor of time. It should be noted that Country was an extraneous variable in all analyses that concerned participants' affect. When a model including Country (full model) provided a significantly better fit than a model excluding it (reduced model), I kept this variable as a covariate in the model. Otherwise, this variable was not investigated, and the reduced model was used.

iii) Procedure

Participants who passed the pre-screening received a link to the Part 1 questionnaire, in which they provided demographic information.

During the seven days after submitting the Part 1 questionnaire, participants needed to complete two prospective Part 2 questionnaires²: In one (2a) they reported a negative COVID-related personal experience (e.g., “A family friend has tested positive with COVID-19, but I couldn’t go to visit her due to lockdown.”) as soon as possible after it happened, and in another (2b) they reported a negative COVID-related news story (e.g., “I saw an article stating that the death rate had now gone over 40,000.”) as soon as possible after they learnt about it. The personal experience needed to be a first-person event which took place within the 7-day window and which participants had direct involvement in, while the news story was specified as an event that had collective impact (i.e., not only affecting the news bearer and the participant but the community) and was reported to, rather than experienced first-person by, participants within the given time frame (e.g., something that they read about in the newspapers, local news they learnt from a colleague). Being told about an acquaintance’s own personal experience (e.g., a friend told me that her colleague tested positive for COVID) was not acceptable. After reporting each event, participants also record their feelings during it (T1).

Twenty-four hours after participants had completed the second of their two Part 2 questionnaires, they received a link to the Part 3 questionnaire. There were two sections in this questionnaire, whose presentation order was counterbalanced across participants, respectively asking about details of their first discussion(s) about each event and their current emotional state (T2). Based on evidence that initial sharing of emotional experiences occurs on the same day in 60% of cases (Gable & Reis, 2010), I

² The order of completion depended on whether participants had a negative personal experience or heard about a negative news story first. 216 of them reported a personal experience first and 69 reported a news story first. Baseline (T1) positive and negative emotion ratings for personal experiences were not significantly affected by order of completion, but ratings for news stories were (see [Appendix A.1](#) for further details). The mean temporal gap between completion of the 2a and 2b questionnaires was 155 minutes.

set the interval between Part 2 and Part 3 to 24 hours so that over half of the participants would have a chance to engage in social sharing and could report the events and their sharing in as detailed a manner as possible while this information was still fresh in their minds, thereby minimising recall bias. At the end of this questionnaire, participants were invited to leave their email address for future contact about follow-up studies. Those who did not accept our invitation were then given further information about what we were researching and thanked. For those who did, debriefing was postponed, avoiding interference with data collection at the follow-up stage.

iv) Measures³

In the Part 1 questionnaire, I asked about participants' age, gender, and country of residence since the outbreak of the pandemic.

In the Part 2 sub-questionnaires (T1), I asked participants to briefly describe the event they were reporting, then measured their positive and negative emotions separately by asking them "During this experience, how [positive/negative] were your feelings?" using 6-point Likert scales ranging from 0 ("Not at all") to 5 ("Extremely").

In the Part 3 questionnaire (T2), participants completed a separate section for each of the two specified events. For each event, they first answered either "yes" or "no" to the question "Did you discuss the event with anyone else during the 24 hours after it happened?". They then responded to the question "In general, how do you feel about the event right now?" by rating on a 7-point Likert scale running from -3 ("Much more negative) to +3 ("Much more positive"), with 0 representing "more or less the same". This provided my measure of how much better or worse participants were feeling at T2 than they had been feeling at T1 (i.e., perceived affect change). Those who

³ There were additional measures at each stage that were not part of the primary focus of the present research and thus are not reported here. To see the full list of these measures, please refer to the [Appendix A.5](#).

did not engage in any discussions were asked to briefly explain why they did not discuss the event during the period. For those who discussed the event, they were asked to specify how many times they had talked about the event within the 24 hours⁴ and which of these options (in-person conversation, audio call, video chat, text messages, social media posts, traditional media, emails, or other) best represented the format of their first discussion. At the stage of data analysis, I categorised participants who stated that they had an in-person conversation as “in-person” sharers while those who chose other options as “remote” sharers. Participants also completed a 4-item scale⁵ that assessed their level of satisfaction with the initial support-provider’s response using 1 (“Not at all”) to 7 (“Very much”) rating scales (adapted from Pauw et al., 2017). Finally, a subset of the participants also responded to the question “How [positive/negative] are your feelings about the event right now?” in a similar manner as they did in Part 2⁶.

2.2.2 Results

i) Likelihood of Sharing

Out of 285 participants, 142 (49.82%) shared both their negative COVID-related personal experience and a news story within the 24-hour interval, 52 (18.25%) shared only the personal experience, 43 (15.09%) shared only the news story, and 48 (16.84%) shared neither.

⁴ I tested whether sharing frequency impacted perceived affect change for exploratory purposes. Results showed that perceived affect change scores for personal experiences did not depend on how frequently each event was shared. However, change scores for news stories did. Specifically, I found that the more often participants talked about both events, the greater the emotional improvement they perceived for the news story. Please see the [Appendix A.2](#) for more details.

⁵ The four items of the satisfaction scale were “To what extent [did you like the person’s response/did the person’s response make you feel better/do you think the person’s response was suitable/did you feel supported by the person’s response]?”. A Cronbach’s alpha test showed that the scale produced by combining these items had good internal consistency ($\alpha = .88$).

⁶ The two independent emotion measures were presented only in the latter half of data collection to 133 of my participants due to an oversight.

Two-way mixed ANOVAs assessing the effects of sharing and event type on T1 emotion ratings showed that shared events did not significantly differ from unshared events in associated levels of negative ($p = .15$) or positive ($p = .69$) emotion. T1 negative ($p = .89$) and positive ($p = .20$) ratings also did not significantly differ between personal experiences and news stories. Additionally, the interaction between sharing and event type was non-significant for negative emotion ($p = .34$) and positive emotion ($p = .66$)

I also performed chi-square tests to check whether participants were relatively more likely to share either kind of event in either the UK or US. No significant effects were detected ($ps > .08$).

ii) Perceived Affect Change

I ran a multivariate regression to check the overall effects of sharing versus not sharing on improvement in affect. The full model that included country as a covariate failed to provide a significantly better fit ($p = .07$), so the reduced model that excluded the covariate was used. In the reduced model, participants' binary response (yes or no) to whether they had shared the personal experience within the 24-hour interval was one of the predictors, and their response to whether they had shared the news story was the other predictor. The model tested for main effects of these two predictors and their two-way interaction on two outcome variables: Participants' perceived affect change for the personal experience and their perceived affect change for the news story. I set the reference level of each of the predictors to "no," so that each intercept in the output represented the mean perceived affect change score for each event when both events were not discussed.

Results showed that when neither event was shared, the mean perceived affect change scores for *personal experience* did not differ significantly from zero (i.e., "more

or less the same”; $p = .53$). However, the sharing of the personal experience had a significant main effect on the change score ($b = 0.57$, $SE = 0.25$, $p = .02$, 95% CI [0.08, 1.06]), such that participants who shared the personal experience alone perceived their affect about this event as having improved more than those who shared neither of the two events. Specifically, the mean change score was significantly greater than zero ($t(51) = 3.71$, $p < .001$, $d = .51$). On the other hand, the main effect of the sharing of the news story was not significant ($p = .27$), meaning that participants who shared the news story alone did not perceive their affect differently from those who shared neither event. The interaction between the sharing of the personal experience and that of the news story was also not significant ($p = .64$), suggesting that the effect of the sharing of either event did not depend on the sharing of the other event. These findings remained the same after controlling for participants’ positive and negative emotion ratings for the personal experience at T1 (see [Appendix A.3](#) for further details). In those additional analyses, T1 positive emotion was a significant positive predictor and T1 negative emotion was a significant negative predictor of perceived affect change.

Similarly, when neither event was shared, the mean perceived affect change scores for *news story* did not differ significantly from zero ($p = .15$). However, the sharing of the news story had a significant main effect on the change score ($b = 0.62$, $SE = 0.26$, $p = .02$, 95% CI [0.12, 1.13]), such that participants who shared the news story perceived their affect about this event as having improved more than those who did not share. Specifically, the mean change score was significantly greater than zero ($t(42) = 2.11$, $p = .04$, $d = .32$). The main effect of the sharing of the personal experience was not significant ($p = .48$), meaning that participants who shared the personal experience alone did not perceive their affect differently from those who shared neither event. The interaction between the sharing of the personal experience and that of the news story

was also not significant ($p = .30$), suggesting that the effect of the sharing of either event did not depend on the sharing of the other event. These findings remained the same after controlling for participants' positive and negative emotion ratings for the news story at T1 (see [Appendix A.3](#) for further details). Table 1 presents the means and SDs of change scores for each event as a function of whether the two events were shared.

Table 1

Means and Standard Deviations of Perceived Affect Change for Personal Experience and News Story as a Function of Whether the Two Events Were Shared

Variable	Neither event		Only personal		Only news		Both events	
	shared		experience shared		story shared		shared	
	(n = 48)		(n = 52)		(n = 43)		(n = 142)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Perceived affect								
change for <i>personal</i>	0.10	1.15	0.67	1.31	0.40	1.26	0.81	1.24
<i>experience</i>								
Perceived affect								
change for <i>news</i>	-0.25	1.19	-0.08	1.23	0.37	1.16	0.21	1.25
<i>story</i>								

Note. $N = 285$.

Given that participants' emotions about a negative experience seemed to improve as long as that particular event was shared regardless of whether the other event was shared, I focused on the subset of participants who shared *both* events ($N = 142$) to check whether the extent of affect improvement due to sharing differed across event types. A paired samples t-test showed that perceived affect change score for personal experiences ($M = 0.81$, $SD = 1.24$) were significantly higher than for news

stories ($M = 0.21$, $SD = 1.25$; $t(141) = 5.79$, $p < .001$, $d = .49$), indicating that participants perceived greater emotional improvement after sharing their personal experience than after sharing a news story.

iii) In-Person vs. Remote Sharing

Using the same subset of participants who shared both events ($N = 141$)⁷, I next tested whether perceived affect change about shared events depended on the communication method. A majority talked about their events with someone else in-person ($N_{personal\ experience} = 95$, 67.38%; $N_{news\ story} = 100$, 70.92%) rather than remotely ($N_{personal\ experience} = 46$, 32.62%; $N_{news\ story} = 41$, 29.08%). Again, the full model of a multivariate regression that included country as a covariate failed to provide a significantly better fit ($p = .25$), so the reduced model that excluded the covariate was used. The reduced model contained two predictors: The method of sharing the personal experience (in-person or remote) and that of sharing the news story. The model tested for main effects of these two predictors and their two-way interactions on participants' perceived affect change. I set the reference level of each of the predictors to "remote," so that each intercept in the output represented the mean perceived affect change score for each event when both events were discussed remotely.

Results showed that when both events were shared remotely, the mean perceived affect change scores for *personal experience* was significantly greater than zero ($t(24) = 3.10$, $p = .01$, $d = .62$). The method of sharing the personal experience and that of sharing the news story did not have significant main effects or interactions ($ps > .33$), meaning that participants who shared either or both of the events in-person did not perceive their affect differently from those who shared both events remotely. These

⁷ One participant who failed to specify how the discussions were conducted was excluded for this particular analysis.

findings remained the same after controlling for participants' positive and negative emotion ratings for the personal experience at T1 (see [Appendix A.4](#) for further details).

Table 2

Means and Standard Deviations of Perceived Affect Change for Personal Experience and News Story as a Function of the Sharing Methods for the Two Events

Variable	Both events		Only personal		Only news story		Both events	
	shared remotely		experience		shared in-person		shared in-	
	(n = 25)		shared in-person		(n = 21)		person	
			(n = 16)				(n = 79)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Perceived affect								
change for <i>personal</i>	0.80	1.29	1.19	0.91	0.57	1.57	0.81	1.20
<i>experience</i>								
Perceived affect								
change for <i>news</i>	0.56	1.58	0.25	1.18	-0.19	1.17	0.19	1.16
<i>story</i>								

Note. *N* = 141.

On the other hand, when both events were shared remotely, the mean perceived affect change scores for *news story* did not significantly differ from zero ($p = .09$). However, the method of sharing the news story had a significant main effect on the change score ($b = -0.75$, $SE = 0.37$, $p = .04$, 95% CI [-1.48, -0.02]), such that the change score was significantly lower when participants shared the news story in-person and the personal experience remotely than when they shared both events remotely. The main effect of the method of sharing the personal experience was not significant ($p = .44$), meaning that participants who shared the personal experience in-person and the news story remotely did not perceive their affect differently from those who shared both events remotely. The interaction between the method of sharing the personal experience

and that of sharing the news story was also not significant ($p = .17$), suggesting that the effect of the method of sharing either event did not depend on the method of sharing the other event. These findings remained the same after controlling for participants' positive and negative emotion ratings for the news story (see [Appendix A.4](#) for further details). Table 2 presents the means and *SDs* of change scores for each event as a function of the sharing methods for the two events.

iv) Emotion Ratings at T1 and T2

As indicated earlier, a subset of our participants ($N = 133$) rated their positive and negative emotions again in the Part 3 questionnaire (T2). To complement the results obtained from the measure of perceived affect change, which was a one-time assessment, I focused on this subset of participants and performed a series of linear multiple regressions to check whether social sharing predicted rated positive and negative emotions at T2 after controlling for the effects of the same-valenced emotion at T1. Again, country was included in all regression models as a covariate but was removed from most of the models because it failed to improve fit indices ($ps > .37$). There was only one instance in which country yielded significant better fit and was kept, which I will present below. In all models (full or reduced), sharing of the personal experience (yes or no) and sharing of the news story were the predictors. The reference levels of these predictors were set to “no”. Each model tested for main effects of these two predictors and their two-way interactions on a single outcome variable, which was rated positive or negative emotion ratings for one of the events at T2, while checking if the same-valenced emotion ratings for the same event type at T1 (the second covariate) had any effects on the outcome variable.

Negative emotion ratings. The sharing of the personal experience had a significant main effect on T2 negative emotion ratings for the same event ($b = -0.75$, SE

= 0.35, $p = .03$, 95% CI [-1.44, -0.06]), such that participants who shared only the personal experience rated their negative emotion about this event lower at T2 than those who shared neither event. The sharing of the news story, on the other hand, did not have a significant main effect ($p = .18$), meaning that participants who shared only the news story did not rate their negative emotion about the personal experience differently at T2 from those who shared neither event. However, there was a significant interaction between the sharing of the personal experience and that of the news story ($b = 1.03$, $SE = 0.49$, $p = .04$, 95% CI [0.06, 2.00]), indicating that the effect of the sharing of either event depended on the sharing of the other event. Specifically, participants rated their negative emotion about the personal experience higher at T2 when they shared both events than when they shared only one of the events. Finally, the control variable of T1 negative emotion was a significant positive predictor of T2 negative emotion ($b = 0.35$, $SE = 0.12$, $p = .004$, 95% CI [0.12, 0.58]).

As for T2 negative emotion ratings for the news story, I did not find any significant main effects of the two predictors or their interactions ($ps > .31$). However, similarly, the control variable of T1 negative emotion was a significant positive predictor of T2 negative emotion ($b = 0.29$, $SE = 0.11$, $p = .01$, 95% CI [0.07, 0.51]). Table 3 presents the means and *SDs* of T2 negative emotion ratings for each event as a function of whether the two events were shared.

Positive emotion ratings. There were no significant main effects of the sharing of the personal experience and that of the news story on T2 positive emotion ratings for the personal experience ($ps > .08$). The two-way interaction was also not significant ($p = .64$). However, the control variable of T1 positive emotion was a significant positive predictor of T2 positive emotion ($b = 0.34$, $SE = 0.11$, $p = .003$, 95% CI [0.12, 0.56]).

Table 3

Means and Standard Deviations of T2 Negative and Positive Affect About Personal Experience and News Story as a Function of Whether the Two Events Were Shared

Variable	Neither event was shared (<i>n</i> = 25)		Only personal experience was shared (<i>n</i> = 31)		Only news story was shared (<i>n</i> = 19)		Both events were shared (<i>n</i> = 58)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Personal experience								
T2 negative affect	2.76	1.13	2.06	1.59	2.26	1.05	2.57	1.37
T2 positive affect	1.60	1.08	2.03	1.47	1.89	1.45	2.17	1.14
News story								
T2 negative affect	2.84	1.18	3.26	1.55	2.74	1.15	2.69	1.47
T2 positive affect	1.44	1.12	1.13	1.34	2.05	0.97	1.93	1.31

Note. *N* = 133.

As for T2 positive emotion ratings for the news story, again, I did not find any significant main effects of the two predictors or their interactions ($ps > .13$). Also, the control variable of T1 positive emotion did not have any significant effects on T2 positive emotion ($p = .33$). Notably, for this particular analysis, I used the full model which included country as a covariate because this model ($AIC = 438.78$) yielded significantly better fit than the reduced model ($AIC = 441.11$; $p = .04$). Specifically, country had a significant main effect ($b = -0.45$, $SE = 0.22$, $p = .04$, 95% CI [-0.89, -0.02]), such that participants in the UK ($M = 1.98$, $SD = 1.19$) rated their positive

emotion about the news story higher at T2 than those in the US ($M = 1.46$, $SD = 1.30$). Table 3 presents the means and SD s of T2 positive emotion ratings for each event as a function of whether the two events were shared.

v) Satisfaction with Initial Support Provision

Because prior research has showed that distress is more likely to be alleviated when people engage in more supportive social interactions (e.g., Lepore, 1992; Major et al., 1997; Manne et al., 1997), I considered that it was worth checking the relationship between participants' satisfaction with the feedback from their initial support-provider and their affect.

In order to examine as much data as possible, I used the subset of participants who shared *both* events ($N = 142$). These participants reported relatively high levels of satisfaction with the feedback from the initial support-provider with whom they shared the personal experience ($M = 5.55$, $SD = 1.15$) and the news story ($M = 5.25$, $SD = 1.20$) on the 7-point scale assessing this variable. A paired samples t-test showed that the reported level of satisfaction was significantly higher for personal experiences than for news stories ($t(141) = 3.28$, $p = .001$, $d = .25$). Performing Pearson correlation tests, I found that reported satisfaction showed significant positive correlations with rated *perceived affect change* for both personal experiences ($r(142) = .29$, $p < .001$) and news stories ($r(142) = .22$, $p = .01$).

To complement the above results, I focused on the smaller subset of participants who rated their positive and negative emotions again at T2 ($N = 133$). This time, I performed partial correlation tests which measured the relationship between satisfaction with initial support provision and T2 emotion for each event, with the effect of the same-valenced emotion at T1 removed. Results showed that for both events, reported satisfaction was significantly positively correlated with T2 positive emotion (personal

experience: $r(89) = .25, p = .02$; news story: $r(77) = .32, p = .01$). However, although reported satisfaction was significantly negatively correlated with T2 negative emotion for news stories ($r(77) = -.31, p = .01$), it was not significantly correlated with T2 negative emotion for personal experiences ($p = .12$)

2.2.3 Discussion

Overall, the social sharing of pandemic-related negative experiences seemed to result in improvement in participants' reported feelings. When participants talked about an event that had been bothering them, they tended to report greater improvement in affect about that particular event afterwards, supporting my hypothesis that social sharing was associated with emotional recovery. Moreover, I found that the effects of sharing were stronger for emotions related to personal experiences than for those related to collectively experienced events (i.e., news stories). This finding is consistent with my prediction that the extent of emotional recovery would differ depending on the nature of the shared event. A possible explanation is that more interpersonal emotion regulation is provided by the listener when a personal experience rather than a news story is shared because the typical motive for sharing the former kind of event is soliciting social support while that for sharing the latter kind of event is informing or exchanging views. News stories relating to COVID-19 also often focused squarely on broader aspects of the crisis, which only provide the background context for the participant-relevant factors that dominate most COVID-related personal experiences. It is thus possible that the crisis becomes more salient when COVID-19 news is discussed, making it more difficult for people to reach closure than when a personal experience is discussed.

My more fine-grained emotion measures showed that social sharing seemed to affect negative emotions more than positive emotions and that these effects related to personal experiences specifically, such that talking about the event alleviated

participants' unpleasant feelings more than not talking. However, those who talked about both their personal experience and news story rated their negative emotions about the former event as higher than did those who talked only about their personal experience. As discussed earlier, the motives for sharing different event types vary, so any emotional benefits gained from sharing a personal experience may be undermined by sharing a news story, which typically does not trigger emotion regulation and may actually reinforce negative perceptions of the crisis.

Some of the benefits of talking about negative personal experiences may depend on intrapersonal processes relating to sense-making and coming to terms with what has happened (Pennebaker & Beall, 1986) rather than interpersonal processes relating to the solicitation of social support. However, my finding that affect change was greater and post-sharing negative emotion was lower when participants were more satisfied with the support that they had received is consistent with interpersonal mediation of my reported effects. It is also not clear why sense-making would operate more strongly after discussion of personal experiences than after discussion of news stories.

I also found between-country differences in participants' positive emotion ratings for their reported news story at T2 after controlling for T1 positive emotion about the same kind of event, which suggested that our UK participants tended to recover from negative news stories about COVID-19 quicker than did our US ones. Although a number of contextual factors could have contributed to this finding, one possible explanation is that compared to people in the UK, those in the US had less trust in their government during the study period. Several studies have demonstrated the crucial role of institutional trust in buffering against negative well-being outcomes during the COVID-19 pandemic (e.g., Matsushima et al., 2021; Lee, 2022). During the study period, the number of new cases fell and stayed steady in the UK whereas cases in

the US continued to increase (see Unruh et al., 2022, Figure 1, for the statistics). Around the same time, the killing of George Floyd in the US sparked outrage and protests across the country. These two incidents might have profoundly undermined Americans' trust in government, which in turn fuelled feelings of powerlessness and helplessness and impaired subjective well-being (Lindström, 2008). Such negative feelings and the perception that the government was incapable of protecting its people might have eventually led these individuals to find it more difficult to regulate their emotions about COVID-19 news. That being said, my findings do not suggest that the effects of sharing differed more generally between the geographical locations I studied.

Against my expectations, a majority of our participants initially shared their negative experiences in-person rather than remotely during a time when social-distancing rules were in effect. In hindsight, this finding was in line with what Choi and Toma (2014) reported in their study: Highly intense negative events are more likely to be shared in face-to-face settings where the communication partner can offer a full range of nonverbal cues of support and full attention. The mode of communication did not alter the improving effects of sharing on emotions related to personal experiences probably because any type and extent of social support were appreciated when social interaction was limited. Interestingly, I found that participants' perceived affect change scores for news stories were lower when sharing of that event type was done in-person than when it was done remotely (although there was no significant improvement in either case), which was contrary to our prediction. As people typically want to exchange views with, rather than seek support from, the listener when encountering a collective event, the face-to-face context may lead to greater potential for emotion transmission across the two parties (for a review, see Parkinson, 2011) because they were both impacted (and likely troubled) by the event.

It is also worth noting that participants who shared their personal experience or a news story did not report significantly different levels of pre-sharing negative or positive emotion than those who did not share. Further, focusing on responses to reported personal experiences, T1 negative emotion was a significant negative predictor of perceived affect change, whereas T1 positive emotion was a significant positive predictor. This means that participants who felt worse about the personal experience while it was happening experienced less rather than more improvement in their affective state, which seems to be the opposite of what explanations of my results in terms of regression to the mean (i.e., a statistical phenomenon that an atypical high or low outcome is likely to return to a baseline level over time; e.g., Barnett et al., 2005) or hedonic adaptation (i.e., the tendency of humans to restore baseline level of happiness after positive or negative events which lead to a subsequent increase in positive or negative affect; e.g., Frederick & Loewenstein, 1999) would predict. I therefore believe that it is more likely that the reported pattern of results reflects the consequence of sharing rather than the consequence of differences in pre-sharing affect.

A distinctive feature of this research is that I conceptualised and measured positive and negative emotions as two separate dimensions (Cacioppo & Berntson, 1994) rather than as opposite ends of the same continuum (Russell & Carroll, 1999) in both studies. As noted by Keyes (2007) and Payton (2009), the absence of negative aspects in the realm of mental health does not entail the presence of positive aspects. With emerging evidence that people may simultaneously feel happy and sad in certain situations (e.g., on a graduation day; Larsen et al., 2001), I concur that positive and negative emotions are not always perfectly inversely related and thus should be assessed separately. Furthermore, prior crisis research has documented mixed effects of sharing (Garcia & Rimé, 2019; Rimé et al., 2010), so it made sense for me to continue

examining the sharing's effects on positive and negative emotions separately in the present study. The reported differences in effects on self-reported positive and negative emotions justified my decision to treat them separately. For example, I found that participants' positive emotion ratings for their personal experience at T2 were not dependent on whether the event was shared, but their negative ratings were. This may suggest that talking about what happened helps relieve emotional load but is not yet sufficient to make people see the event in a more positive light, which is a marker of reappraisal (Webb et al., 2012)—a cognitive form of emotion regulation. If I had only assessed changes in negative emotion only, I would have drawn inaccurate conclusions and failed to detect this potentially interesting effect. However, it should be noted that the absence of significant effects on positive emotion ratings might be a consequence of the reduced sample size providing T2 data for this analysis.

Because my intention was to investigate the consequences of naturalistic sharing, I did not directly manipulate whether or not participants shared or how frequently they shared. Therefore, any conclusions about sharing's causal impact need to be tempered as a consequence. For example, it is possible that less negative emotions promote sharing rather than vice versa, or that happier individuals tend to spend less time alone and more time socializing (Diener & Seligman, 2002). However, neither of these associations could easily explain the findings across both of our studies, given that there was no evidence that participants were happier about the events that they shared than about those that they did not. Of course, it would have been possible to randomly assign half of the participants to share their negative experiences and the other half not to in order to substantiate the causal process, but checking whether participants really adhered to these instructions would have been challenging. It would also raise ethical issues, as some participants might be forced to disclose their negative experiences when

they preferred keeping them to themselves, and some might be prohibited from disclosing negative experiences when they would want to during the study period.

My other ethical decision of excluding highly distressed participants in the pre-screening stage also raised an issue regarding generalisability. Specifically, it is difficult to apply the findings from the present study to clinical populations. An example of those populations which has been widely studied is individuals suffering from depression. Depressive cognition often includes rumination (Nolen-Hoeksema et al., 2008; Watkins, 2008) because these individuals are commonly under the belief that perseverating on negative information helps them solve problems (Papageorgiou & Wells, 2001). They also tend to selectively direct their attention towards self-relevant information (Wisco, 2009). Given that a successful conversation typically requires one party to devote cognitive resources to understanding the other party's perspective, depressed individuals may be more reluctant than healthy individuals to actively disclose themselves and solicit support. In the absence of a support-provider, depressed individuals may not be able to come up with an alternative approach to solving the problem more effectively, so they may end up ruminating more and feeling worse across time. Even if they manage to engage in social sharing somehow, the emotion-improving effect of sharing may not be as strong as observed in the present sample because depressive cognition is less flexible (Channon, 1996). This is a characteristic that likely hinders reappraisal, especially when the problem is chronic and intractable. Future research should recruit both non-clinical and clinical populations and compare their sharing tendencies, processes, and outcomes across different kinds of stressor to form a more thorough understanding of the role of social sharing in people's lives.

In hindsight, I should have measured participants' rumination about their negative experiences at both timepoints to learn more about the effects of social sharing

on mental well-being. This information would have also offered some insights into the adaptiveness of sharing in clinical settings. The only rumination measure in the present study was one-time (in the Part 1 questionnaire; see [Appendix A.5.1](#)). However, considering that this measure concerned the COVID-19 pandemic in general instead of the specific events (personal experience and news story) reported by participants, analysing it would not be very informative. Because rumination was not discussed here, I did not include a rumination measure in my later projects pertaining to social sharing (i.e., Projects 2 and 3) to maintain a tight focus on emotion.

Although the present study provides convergent evidence that sharing tends to improve participants' affect, the results may not generalise to other chronic stressors, given the unique features of the COVID-19 pandemic. One of these features is restricted social interaction, which may have made the mere act of interacting with someone emotionally beneficial in its own right. Moreover, this research was conducted at the early stages of the pandemic, so how sharing's effects may have evolved over the full span of the crisis remains unknown: People may have gotten used to the "new normal" of restricted social contact and consequently stopped benefitting from talking about the crisis. Alternatively, they may have come to feel distressed by the unexpected persistency of the pandemic and thus relied on sharing to their regulate emotions to an even greater extent.

My interpretations of the present findings have emphasized the chronic, collective nature of the stressors presented by the COVID-19 pandemic, but it is not clear whether similar effects would also apply in the case of chronic personal stressors including illnesses that are not experienced on a societal and collective level. It is certainly possible that the commonly experienced risks associated with COVID-19

made the task of offering empathic support more straightforward than it would have been if the chronic stressor had been more idiosyncratic.

In sum, the present study investigated social sharing related to a collective crisis initiated when the crisis was unfolding, instead of after it. The use of prospective questionnaires allowed me to record the processes and outcomes of people's sharing in a naturalistic setting, with the issues of recall bias being minimised as much as possible. I believe that this work contributes to the social sharing and emotion regulation literature by demonstrating that reaching out to and conversing with others about one's own negative emotions during times of distress appears beneficial to mental well-being. In the next chapter, I will present Project 2 wherein I experimentally manipulated the process of social sharing—specifically, the different chatting styles that promote or suppress provision of cognitive support—and compared effects of sharing between different stressors.

CHAPTER THREE

Project 2: Effects of Chatting Styles on Sharers' Emotions During Online Conversations

3.1 Abstract

Prior social sharing research suggests that cueing sharers to focus on emotionally arousing details of a negative experience (i.e., recounting) often increases negative affect, while cueing them to think about things differently (i.e., reconstrual) leads to emotional recovery. Extending these findings, I tested whether reconstrual was consistently more effective than recounting in improving emotions induced by different kinds of negative experiences (interpersonal conflicts vs. COVID-19 news in Study 2A; angry vs. regretful conflicts in Study 2B). In addition, Study 2B compared effects of two different forms of reconstrual (stimulus-reappraisal and perspective-taking). Both studies used pre-programmed questions from a simulated online support-provider to encourage participants to either recount or reconstrue their experience. Results showed that reconstrual generally had more beneficial effects than recounting and that the benefits of different kinds of reconstrual depended on the kind of emotional experience sharers had. Specifically, perspective-taking was especially effective in regulating anger whereas stimulus-reappraisal tended to improve feelings about regretful events.

Imagine that you are concerned about an argument you had with your best friend a while ago: What would you do to make yourself feel better? You might find a quiet place and try to work through your feelings on your own (e.g., through expressive writing; Pennebaker et al., 2001, 2007), or you might talk to someone else about the experience (i.e., social sharing of emotion; Rimé et al., 1991). In either case, the outcomes may differ depending on the way that you approach the task of emotion regulation. The present project assesses whether different ways of discussing unpleasant events in online conversations bring about different emotional consequences.

Is reflecting on negative experiences always helpful? Different studies suggest different answers to this question: While some researchers propose that self-reflection indeed improves emotions by leading people to mentally represent the experience in less negative terms (e.g., Monson et al., 2006; Pennebaker & Chung, 2007; Wilson & Gilbert, 2008), others imply that self-reflection can backfire because it makes negative feelings and thoughts associated with the experience accessible and leads people to ruminate on these undesirable details (e.g., Ayduk & Kross, 2008; Nolen-Hoeksema et al., 2008; Smith & Alloy, 2009).

In their review, Kross and Ayduk (2017) proposed that the effectiveness of self-reflection in coping with emotional distress depends on the kind of perspective people adopt. Specifically, a psychologically *immersed* perspective often leads people to *recount* the emotionally arousing details of their experiences and to be caught up in those details, thereby hindering objective reasoning about what happened and resulting in maladaptive self-reflection. On the other hand, a psychologically *distanced* perspective which allows people to “take a step back” is more likely to make them *reconstrue* their experiences, thereby facilitating constructive reasoning about what happened and resulting in adaptive self-reflection. Reappraisal is the process of

“changing how [people] appraise the situation [they] are in to alter its emotional significance” (Gross & Thompson, 2007, p. 14). Uusberg et al. (2019) referred to reconstrual as a reappraisal strategy which involves “changing the situational construal” (p. 6). As noted by Kross and Ayduk (2017), reconstrual often operates in ways that foster meaning-making (i.e., focusing on “why” rather than “what”) and closure. In sum, different types of self-perspective (immersed or distanced) have been suggested to promote different ways of mentally representing unpleasant past experiences (recounting or reconstruing), which in turn results in different psychological outcomes. The self-regulatory benefits of psychological distance emerged in the early emotion regulation literature (e.g., Gross, 1998b; Lazarus & Alfert, 1964). More recent studies have presented supporting evidence for the associations between a self-distanced perspective and less distress (e.g., Kross & Ayduk, 2008; Schartau et al., 2009) and between a self-immersed perspective and negative emotional reactivity (e.g., Bushman, 2002; Glynn et al., 2002; Kross & Ayduk, 2008). Further, Kross and Ayduk (2008) found that self-distancing leads people to report less distress in the long term compared to not only self-immersion but also distraction.

Drawing on the self-reflection literature, Lee and his colleagues (2020) turned to (computer-mediated) dyadic settings and examined whether prompting people to reconstrue—instead of recounting—when they are sharing their negative experiences with someone else leads to less negative affect as it does when people are self-reflecting. In their study, an experimenter served as the support-provider who followed a predefined script while interacting with participants via an online instant messenger. While participants who were cued to recount a recent interpersonal conflict about which they were still upset felt significantly worse after the online chat than at baseline, manipulated reconstrual buffered against increased negative affect. In other words,

previous findings showing different emotional effects for recounting and reconstrual seem to extend to online conversations.

3.2 The Present Research

Given that emotional episodes are frequently followed by social sharing (80-95% of the time; for a review, see Rimé, 2009), it is important to investigate the emotional outcomes of different chatting styles. Many researchers have conducted related studies, such as Nils and Rimé (2012) and Pauw et al. (2022) who compared the effects of receiving socio-affective support to the effects of receiving cognitive support. Lee et al. (2020), on the other hand, compared recounting (neither socio-affective or cognitive support) to reconstrual (a type of cognitive support), which are the focus of the present research.

I aimed to extend Lee et al.'s (2020) investigation by addressing two questions: First, is reconstrual consistently more adaptive than recounting when the sharer is discussing different kinds of distressing experience? Because Lee et al. (2020) only investigated acute interpersonal conflicts, I wanted to find out whether their findings also applied when discussing more chronic and uncontrollable concerns that have a collective impact. Prior research has suggested that emotion-focused coping may be more useful than problem-focused coping when encountering relatively uncontrollable stressors (e.g., a loved one's illness) because emotions, rather than the situation, are what people have more control of in this context (e.g., Folkman, 1984; Lazarus, 1993; Troy et al., 2013). Reappraisal, which involves people changing a stressor's construal but not its objective level of threat, is considered a type of emotion-focused coping strategies (Lazarus & Folkman, 1984), so the regulatory success of reconstrual reported in Lee et al.'s (2020) study may be more apparent for uncontrollable stressors that are

experienced collectively for a long time (e.g., the COVID-19 pandemic). However, another possibility is that in face of such kind of stressor, reappraisal may lose its beneficial effects: Even if people successfully come up with a new interpretation of the problem, they are living in an environment which constantly exposes them to unpleasant emotional stimuli in the environment (e.g., other people's negative affect, everyday bad news). Being always reminded of the problem as it is, people may find it difficult to really change their view of the circumstances and thus may be less susceptible to its benefits.

Second, does reconstrual consistently outperform recounting when discussing concerns that lead to different kinds of emotional response? For example, Pauw et al. (2017) found that angry individuals tend to dislike support that targets their cognitions whereas regretful individuals are more positive about it. Therefore, reconstrual may be more suited for regulating regret than anger. One possible reason for this is that regret leads people to attribute blame to themselves (Wetzer et al., 2007; Zeelenberg & Breugelmans, 2008) whereas anger leads them to attribute blame to an antagonist (Smith & Kirby, 2000; Wu & Wang, 2017), so regretful individuals are more amenable to tactics that help them feel better about what has been done. Furthermore, regret often involves irreversible decisions made in the past (Wetzer et al., 2007; Zeelenberg & Breugelmans, 2008), so by the same logic as presented above, emotion-focused coping, or reappraisal, which encourages people to change their emotions rather than the situation (of which people do not have control given that the outcome is already settled) should be more useful for this emotion than for anger. On the other hand, anger does not engender a strong sense of personal responsibility for why things have turned out bad. Also, individuals experiencing this emotion are highly motivated to take antagonistic action (e.g., Ellsworth & Smith, 1988; Frijda, 1986), so they probably prefer to maintain

their other-blame appraisals and consequently find it difficult to reappraise the blameworthiness of the target of their anger.

Like Lee and colleagues (2020), I used online conversations to manipulate chatting styles partly for reasons of experimental control but also partly because of the increasing importance of remote interactions in the provision of social support by peers (Lenhart et al., 2015) and mental health professionals (Glueckauf et al., 2018). Indeed, the recent COVID-19 pandemic has moved a large proportion of people's lives online, causing many psychotherapists to use communication technology to continue providing their services in accordance with social distancing guidelines. Online therapies have become an accepted necessity, making it increasingly important to understand the consequences of talking about various kinds of problem online using different conversational styles. In my two online studies, I therefore examined the effects of chatting styles involving recounting and reconstrual on affective responses to different kinds of event (Study 2A), and to interpersonal events that induced different kinds of emotion (Study 2B).

3.3 Study 2A

This study investigated whether reconstrual is consistently more effective than recounting at regulating emotions about two different kinds of negative event. At the time this research was conducted, the world was still living under the threat of COVID-19, and I took advantage of this unique context by comparing the effects of different chatting styles on affective responses to news related to the crisis and the effects on affective responses to interpersonal conflicts, similar to those investigated in Lee et al.'s (2020) earlier study.

I hypothesised that reconstruing alleviated negative affect to a greater extent than recounting as in Lee et al.'s (2020) study. Further, I hypothesised that the kind of negative event would moderate the effects of chatting style. Drawing on prior research (e.g., Folkman, 1984; Lazarus, 1993; Troy et al., 2013), reconstrual, which involves altering cognitions about the situation to reduce its emotional impact, should be more suited for coping with COVID-19 news, which was a more uncontrollable stressor than was an interpersonal conflict. However, given the enduring and collective nature of the pandemic, an alternative prediction was that reconstrual lost its effectiveness in regulating emotions associated with pandemic-related news to which people were constantly exposed and operated relatively more successfully in the case of interpersonal conflicts.

In their study, Lee et al. (2020) reported that reconstruing participants scored higher on closure than recounting ones, so I included a similar measure of closure in order to assess whether similar effects operated across both kinds of event sampled in my study. I also assessed whether perceived closure mediated the effects of chatting style on reported affect in secondary analyses. Finally, because research on the therapeutic alliance (e.g., Martin et al., 2000) has suggested that support outcomes depend on how much the support-provider is liked, I also followed Lee et al.'s (2020) lead by measuring participants' perceptions of their chat companion's warmth and tested whether these warmth perceptions moderated the effects of chatting style.

3.3.1 Methods

i) Participants

Because the present study had a similar design to Lee et al.'s (2020), I referred to their reported effect size ($\eta_p^2 = .08$) of the interaction between chatting style and time, which was their main finding, when deciding the parameters for my power analyses

using G*Power (version 3.1; Faul et al., 2009). To detect a similar medium-to-large-sized effect ($f = .29$) in the present study with 95% power and 5% alpha for a within-between interaction (time x chatting style) using a repeated measures ANOVA (N of groups = 2, N of measurements = 2), 42 participants were needed. However, to detect a medium-to-large-sized effect with the same power level and alpha for the interaction between two between-subjects factors of chatting style and event using an ANOVA (numerator $df = 1$, N of groups = 4), 158 participants were needed.

I recruited 160 participants (116 female, 43 male, 1 stated ‘other’; $M_{\text{age}} = 22.61$, $SD_{\text{age}} = 6.64$) via the Prolific platform (Peer et al., 2017) between 19th August 2021 and 20th October 2021. All participants were adult undergraduate students currently residing in the UK. To be eligible to take part in this study, participants needed to still feel concerned about a recent piece of COVID-related news (e.g., “COVID cases are gradually back on the rise and a winter lockdown is possible.”) and a recent interpersonal conflict that was not related to the pandemic (e.g., “My friend keeps cancelling our plans but always has time for her other friends.”), be willing to talk about either of the events, and be committed to completing the two-part study within the specified time frame. The questions that the simulated support-provider used to prompt participants to recount or reconstrue their interpersonal conflict were similar to those in Lee et al.’s (2020) study, which focused on topics about the sharer and the other person involved in the interpersonal conflict. I made some adjustments to the questions for the COVID news condition because sharers rarely have direct interactions with those involved in news stories. Consequently, I changed the focus of the relevant questions in this condition to participants’ interaction with those around them who were also aware of the news. This change also led me to introduce the inclusion criterion that

participants needed to have discussed their negative experiences with someone else before taking part in this study.

In the interest of protecting participants' mental well-being, I assessed their baseline levels of psychological distress using the K10 (Andrews & Slade, 2001) at the beginning of the study. A total of 43 participants showed signs of severe distress (scoring ≥ 30 out of 50) and thus were warned of the risks of proceeding and given a chance to withdraw. One of them decided to withdraw their participation so was not included in my final sample. Regardless of their final decision, a follow-up email was sent to all those reporting above-threshold distress the next day listing several sources of advice or help that were free and readily available.

ii) Design

The main analyses used a 2 x 2 x 2 mixed design with Time (T1: before chat vs. T2: after chat) as a within-subjects factor and Chatting Style (recount vs. reconstrue) and Event (interpersonal conflict vs. COVID news) as between-subjects factors. The main dependent variables were separate positive and negative emotion ratings and general perceived affect ratings, all measured twice.

iii) Procedure

The study had two parts. In the Part 1 questionnaire, after providing their informed consent and basic information (e.g., age, gender), participants were randomly assigned to identify (depending on experimental condition) either a non-COVID-related interpersonal conflict they had recently experienced or a piece of COVID news they had recently learnt about to talk about with a supposed research assistant in a text-based online chat the following day. The statements delivered by this research assistant, named Alex, were in fact pre-programmed (but participants were not told of this fact). To strengthen the impression that participants would be interacting with a human

partner, I informed them that the chat room only operated between 10am and 6pm during the research assistant's office hours and asked them to select a time slot within that window that worked best for them for a live chat on the following day. Subsequent to participants' submission of the Part 1 questionnaire, a link to the Part 2 questionnaire was sent to them automatically via email.

Once participants had clicked the link to the Part 2 questionnaire on the following day, they were asked to take a few moments to recall the negative event they had been assigned to talk about and to rate their current feelings about this event (T1). They were also asked how many times they had already talked about the event with other people. Next, I presented a list of instructions about what participants should and should not say during the chat. For example, whenever they wanted to refer to the other person involved in the event, they were told to use "X" instead of that person's name ostensibly for reasons of confidentiality, but in fact to allow me to use pre-scripted questions referring to this person. After participants had agreed explicitly to follow these instructions, they were directed to the chat, whose structure was adapted from Lee et al. (2020).

The chat began with a short "ice-breaking" conversation to help participants feel comfortable. Following the icebreaker, Alex, the supposed human partner, instructed them to briefly describe the negative event they intended to share. According to Lee et al. (2020), this "warm-up" phase was necessary to naturally "establish a context for support provision to take place" (pp. 372-373). Next, Alex randomly prompted participants to recount or reconstrue the event by asking five scripted questions (adapted from Lee et al., 2020; see [Appendix B.1](#) for my verbatim questions). For example, in the conflict condition, recounting participants were asked "Can you tell me what happened exactly from start to finish?" whereas reconstruing were asked "Can you tell

me why it's been concerning you...?”. My decision to pre-program the support-provider’s responses was intended to keep all aspects of the chat (except for the manipulation of chatting style) constant, minimising the chance that our results might be confounded by human factors, such as different word choices and typing speeds of real people delivering questions.

After participants had finished the chat and were disconnected from the chat room, they rated their current feelings about the event (T2) and their closure levels, as well as their perceptions of Alex. Finally, participants were thanked and debriefed about the deception and the reasons for it.

iv) Measures⁸

Previous sharing. At the beginning of the Part 2 questionnaire, participants were asked to indicate approximately how many times they had talked about the assigned event with others prior to starting the study. I used this measure to check that participants had previously discussed the event as the instructions required.

Pre- vs. post-chat affect. In the Part 2 questionnaire, before participants were directed to the chat (T1), they rated their general affect about the event by answering the question “In general, how do you feel about the [conflict/news] right now?” using a -50 (“Very bad”) to +50 (“Very good”) scale, with 0 representing “Neutral”. They rated the same item again immediately after the chat (T2)⁹.

⁸ There were additional measures in the Part 1 questionnaire that were not part of the primary focus of the present research and thus are not reported here. To see the full list of these measures, please refer to the [Appendix B.3.1](#).

⁹ In addition to the general affect measure, I asked participants to separately rate their positive and negative emotions by answering two questions “How [positive/negative] are your feelings about the [conflict/news] right now?” on 7-point Likert scales ranging from 0 (“Not at all”) to 6 (“Extremely”) in case they were experiencing bivalent emotions (Larsen et al., 2001) about the event. However, ratings on the positive and negative items were moderately and significantly negatively correlated at baseline (T1; $r(158) = -.52, p < .001$), suggesting that participants’ emotional response were mainly univalent (i.e., varying only in degree of pleasantness/unpleasantness). Additionally, I averaged these two baseline ratings to create a single index and found it to be strongly and significantly positively correlated with T1 general affect ratings ($r(158) = .76, p < .001$), so I decided to focus only on the general affect measure in my main analyses.

Content analyses. I used the dictionary-based Linguistic Inquiry and Word Count 2015 (LIWC2015; Pennebaker et al., 2015) to measure participants' use of certain categories of words in their responses to the five scripted questions during the chat. Following coding instructions used by Lee et al. (2020) for human judges, I specifically focused on between-condition differences in usage of *insight*- (e.g., “think”) and *cause*-related (e.g., “because”) words working on the assumption that reconstrual helps people gain a new perspective and understand the underlying reasons behind their experience, whereas recounting simply allows people to describe their experience. LIWC-coded insight and cause scores thus allowed me to check that my manipulation of chatting style had the intended effects on participants' written contributions to the conversation with the simulated support-provider.

Closure. After the chat, I used Lee et al.'s (2020) single-item measure of closure which asked participants to rate how much they agreed with the statement: “As I was talking to the research assistant about the [conflict/news], I had a realisation that led me to experience a sense of closure” on a 1 (“Strongly disagree”) to 7 (“Strongly agree”) scale.

Perceived warmth of support-provider. I used Lee et al.'s (2020) two items to measure participants' perceptions of their chat companion by asking them: “How much did you like the research assistant?” and “How close did you feel to the research assistant?” on 1 (“Not at all”) to 7 (“Very much”) scales. As in Lee et al.'s (2020) study, these two ratings showed a strong and significant positive correlation ($r(158) = .71, p < .001$), so I averaged them to create a single index of participants' perceptions of the support-provider's warmth.

Perceived responsiveness of support-provider. Participants also answered the question “At any stage during the procedure, did you doubt whether the research

assistant was really following the conversation with you?” using a rating scale running from 1 (“Did not doubt it at all”) to 7 (“Very much doubted it”). This item was designed to assess the extent to which participants were convinced that the support-provider was responding to their specific comments without directly providing clues that the support-provider might be simulated. The higher the rating, the less convinced participants were. This measure allowed me not only to assess participants’ perceptions of the naturalness of their interaction in general but also to check whether any effects of our manipulation of chatting style were moderated by the sense that the support-provider was not directly responding to their problem or their written comments about it.

3.3.2 Results

i) Allocation to Conditions

Two participants were excluded from analysis because their perceived general affect ratings at T2 were extremely high ($> 3 SD$ from the mean), resulting in a final sample of 158. Please note that all measures reported in this thesis underwent an outlier check before being analysed: If the z-score of a rating was less or greater than $3 SD$ from the mean, it was identified as an outlier. The *Ns* for each condition were:

Conflict/Recount = 39, Conflict/Reconstrue = 40, News/Recount = 40,

News/Reconstrue = 39.

ii) Manipulation Checks

All participants indicated that they had discussed the event prior to the study as required by instructions. The mean frequency of previous sharing was 3.39 times ($SD = 3.86$ times, skewness = 4.93, median = 2.5 times).

I performed a series of two-way ANOVAs to check whether usage of LIWC-coded insight- and cause-related words depended on the chatting style being used or the type of event being shared. Results showed that the main effect of event on usage of

insight-related words was significant ($F(1, 786) = 6.41, p = .01, \eta_p^2 = .01$), indicating that they were used more often in participants' responses when they were chatting about an interpersonal conflict ($M = 4.94, SD = 4.61$) than when they were chatting about a news story ($M = 4.14, SD = 4.27$). The main effect of chatting style was, on the other hand, not significant ($p = .48$). A significant two-way interaction ($F(1, 786) = 7.92, p = .01, \eta_p^2 = .01$) indicated that the effect of event depended on chatting style.

Specifically, a Tukey's HSD test revealed that insight-related words were used more for conflicts than news when both events were reconstructed ($p < .001$) and not when either or both of the events were recounted ($ps > .06$).

Turning to usage of cause-related words, I only found a significant main effect of chatting style ($F(1, 786) = 9.65, p = .002, \eta_p^2 = .01$), suggesting that these words were used more often when participants reconstructed ($M = 3.79, SD = 3.61$) than when they recounted ($M = 2.94, SD = 3.96$), regardless of which event they were chatting about. Neither the main effect of event nor its interaction with chatting style was significant ($ps > .44$). Taken together, the content analyses confirmed that reconstructing participants were more motivated to understand the causes of their experience than were recounting ones, so my manipulation of chatting style was generally successful.

iii) Main Analyses

A 2 (time) x 2 (chatting style) x 2 (event) mixed ANOVA revealed a significant main effect of time on general affect ratings ($F(1, 154) = 6.34, p = .01, \eta_p^2 = .04$), showing that rated general affect after the chat was more positive than before it. There was also a significant main effect of event ($F(1, 154) = 10.26, p = .002, \eta_p^2 = .06$), such that rated general affect about the conflict was more positive than that about the news. The main effect of chatting style was not significant ($p = .68$). The two- and three-way

interactions were also not significant ($ps > .06$). Table 4 presents the means and *SDs* of general affect ratings at each timepoint as a function of chatting style and event.

Table 4

Means and Standard Deviations of General Affect and Closure as a Function of Chatting Style and Event

Variable	Interpersonal conflict				News story			
	Recount		Reconstrue		Recount		Reconstrue	
	(n = 39)		(n = 40)		(n = 40)		(n = 39)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
General affect								
T1	-10.72	18.52	-11.18	16.88	-19.03	17.05	-18.59	15.94
T2	-10.46	19.29	-4.33	19.28	-15.68	19.84	-17.38	18.39
Closure	3.74	1.41	4.33	1.61	3.10	1.32	3.54	1.60

Note. $N = 158$.

iv) Secondary Analyses

Closure. The mean rating of closure was 3.68 ($SD = 1.54$). I performed a two-way ANOVA to assess whether levels of closure depended on event and chatting style. There were significant main effects of both event ($F(1, 154) = 9.11, p = .003, \eta_p^2 = .06$) and chatting style ($F(1, 154) = 4.63, p = .03, \eta_p^2 = .03$), indicating that closure was generally higher in the conflict than news condition, and after participants reconstrued rather than recounted. The two-way interaction was not significant ($p = .76$). The means and *SDs* of closure ratings as a function of chatting style and event are reported in Table 4.

Given the significant main effect of *event* on participants' general affect ratings in the main analyses, I further examined whether this effect was mediated by closure. I applied the lavaan package (version 0.6.12; Rosseel, 2012) in R to fit the regression

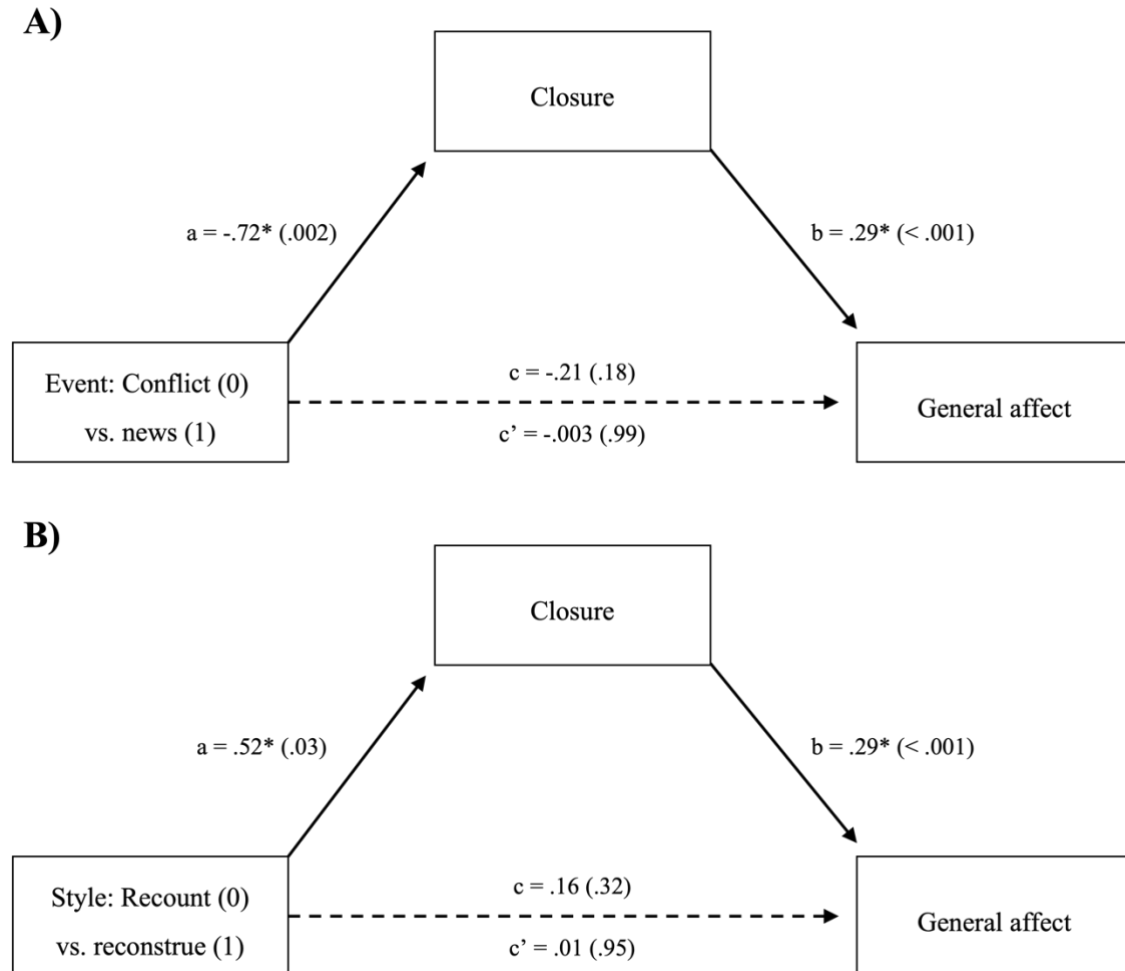
equations representing the total effect, the indirect effect, and the direct effect for statistical mediation analysis (Judd & Kenny, 1981) using structural equation modelling (SEM). As noted by Vuorre and Bolger (2018), one of the methods of addressing mediation in experiments where participants are measured twice (e.g., pre- vs. post-treatment score) is to use “change scores between experimental conditions” (p. 2129). In their methodological paper which presents an example of conducting mediation analyses using SEM, Rijnhart et al. (2017) computed standardised residual change scores for their within-subjects mediator and outcome variable in order to “take into account the baseline values of these variables” (p. 131). In the present study, my outcome variable, general affect, was also measured twice, so I followed Rijnhart et al.’s (2017) procedures and used standardised residual change scores of participants’ general affect as my outcome variable. My mediator, closure, on the other hand, was only measured once, so I kept it as it was in my mediation analyses.

My mediation analyses constructed 95% confidence intervals around the indirect effect of event (which I dummy coded, such that conflict appeared as 0 and news as 1), via the mediator of closure, on participants’ general affect. The number of bootstrap samples was set to 5000.

I found evidence for indirect-only mediation (see Figure 1A; for more details of different types of mediation, see Hair et al., 2021, p. 142): While event did not have a direct effect on general affect ($p = .99$), it had a significant indirect effect via closure on this emotion measure. Specifically, compared to participants who discussed a news story, those who discussed an interpersonal conflict experienced a greater sense of closure ($b = -0.72$, $SE = 0.24$, $p = .002$, 95% CI $[-1.18, -0.24]$), which led to a greater increase in general affect over time ($b = 0.29$, $SE = 0.05$, $p < .001$, 95% CI $[0.20, 0.39]$).

Figure 1

The Mediating Role of Closure in the Effects of Manipulations of Event and Chatting Style on General Affect



Note. The *c* path coefficients in panels A and B respectively represent the total effect of event and that of chatting style on general affect, while the *c*-prime path coefficients refer to the direct effects. The *a* and *b* path coefficients refer the indirect effects via closure. The value in each bracket represents the exact *p* value of each path, **p* < .05. Significant paths are illustrated using solid lines, and insignificant ones are illustrated using dashed lines.

Next, I tested whether closure mediated the effect of *chatting style* (which I dummy coded, such that recounting appeared as 0 and reconstrual as 1) on participants' general affect. Although I did not find any significant effects of chatting style in the main analyses, I had predicted that closure would mediate the effect of chatting style on

participants' emotions prior to conducting this study and therefore decided to test this mediational hypothesis in any case.

Again, I found evidence for indirect-only mediation (see Figure 1B): Compared to recounting participants, those who reconstructed experienced a greater sense of closure ($b = 0.52$, $SE = 0.24$, $p = .03$, 95% CI [0.05, 0.98]), which led to a greater improvement in general affect over time ($b = 0.29$, $SE = 0.05$, $p < .001$, 95% CI [0.20, 0.39]). The direct effect of chatting style on general affect was not significant ($p = .95$).

Perceived warmth of support-provider. The mean rating of perceived warmth was 4.31 ($SD = 1.37$). A two-way ANOVA showed that neither event nor chatting style had a significant main effect on this variable ($ps > .22$). The two-way interaction was also not significant ($p = .55$). To test whether perceived warmth had a main effect on general affect or interacted with any of the three predictors in the 2 x 2 x 2 model used in the main analyses (reduced model), I added this variable to the model to create a full model. However, results showed that the full model did not significantly improve fit ($p = .11$).

Perceived responsiveness of support-provider. The mean rating on the responsiveness item was 3.99 ($SD = 2.02$). Again, adding this variable to the 2 x 2 x 2 analysis of general affect ratings did not significantly improve model fit ($p = .31$).

3.3.3 Discussion

Study 2A did not replicate Lee et al.'s (2020) finding that reconstruction had a more positive impact on affect than recounting (my first hypothesis). However, reconstruction did lead to a greater sense of closure than recounting, in line with Lee et al.'s (2020) results. There was also a significant indirect effect of reconstruction on affect via closure, suggesting that emotional recovery can be achieved using reconstruction which promotes the feeling of psychological completion.

One possible reason for my failure to replicate Lee et al.'s (2020) finding that reconstrual led to relatively greater affect improvement than recounting is my use of a pre-programmed rather than experimenter-adjusted script during the online conversation. However, the fact that perceived responsiveness of the chat companion had no moderating effect suggests that participants' affect ratings did not depend on the extent to which they were convinced that the other person was responding to their specific comments. I therefore believe that a more likely explanation is that the reconstrual style adopted by the participants may not have consistently involved the most effective means of regulating the particular emotions they experienced in relation to the events they described during the online chat. To assess this possibility, Study 2B compared two more targeted forms of reconstrual applied to two different kinds of emotional experience.

My findings also failed to support my second hypothesis that the effect of chatting style would depend on the type of event that was shared. Instead, my main analyses showed that participants consistently rated their emotions about the interpersonal conflict as more positive than their emotions about the COVID news and that their emotions improved after the chat irrespective of which event they talked about and how they talked about it.

My secondary analyses found an unpredicted significant effect of event type on closure, with greater closure associated with discussion of interpersonal conflicts than with discussion of COVID news. Closure also mediated the effect of event on affect, with interpersonal conflicts, rather than COVID news, leading to greater closure and consequently more positive feelings. The most likely explanation for this finding is that participants were less able to attain a feeling of psychological completion about COVID news because of the collective and continuing nature of the crisis (at the time of data

collection). Finally, perceived warmth did not significantly moderate any of the effects on participants' affect. In other words, the extent to which participants liked and felt close to the simulated support-provider seemed to make no difference to the emotional outcome of the chat.

3.4 Study 2B¹⁰

Study 2A found no significant differences in the effects of recounting and reconstrual across event types, so Study 2B focused exclusively on the domain of interpersonal conflicts and tested whether the effects of my manipulation of chatting style depended on the specific emotion involved in the situation using a similar design to Study 2A. Although the perceived responsiveness of the support-provider did not significantly moderate reported effects in Study 2A, I still created a more realistic-looking online messaging platform (e.g., showing typing dots, enabling chat bubbles) for Study 2B to try to minimise any potential credibility issues that might influence our interpretation of the results.

As in Study 2A, I hypothesised that both forms of reconstrual improved sharers' affect to a greater extent than recounting in general. However, the beneficial effects of reconstrual would probably depend on sharers' specific emotional response: Anger would be generally less susceptible than regret to alleviation by reconstrual because the former emotion is characterised by appraising someone else as accountable for their negative affect (Smith & Kirby, 2000; Wu & Wang, 2017) and an antagonistic action tendency (e.g., Ellsworth & Smith, 1988; Frijda, 1986), meaning that sharers experiencing this emotion would probably be reluctant to reconsider their antagonist's

¹⁰ Study 2B was conducted with the help of Chloe Pavey, who was an undergraduate student at University of Oxford and has now graduated. Conrado Eiroa-Solans, now at University of California, Berkeley, also contributed to this study by assisting in the designing and programming of the online simulated chat.

blameworthiness. Furthermore, although I hypothesised that reconstrual was generally more effective in regulating regret than anger, *stimulus-reappraisal* (i.e., reinterpreting the context or cause of the emotion by, for example, considering the experience in a more positive light; Webb et al., 2012) might work particularly better than *perspective-taking* (i.e., adopting a different outlook on the emotional event by, for example, imagining being a fly on the wall; Webb et al., 2012) because regretful individuals usually appraise themselves as accountable for an irreversible bad outcome (Wetzer et al., 2007; Zeelenberg & Breugelmans, 2008). This means that they would wish to come to terms with their unchangeable past mistakes and would thus probably benefit more from using stimulus-reappraisal that helps them “identify a silver lining” (for a review, see Gilovich & Medvec, 1995) than from using perspective-taking that simply asks them to think about what happened in the third person.

3.4.1 Methods

i) Participants

As in Study 2A, I wanted to detect a medium-to-large-sized effect ($f = .29$) in the present study with 95% power and 5% alpha for a within-between interaction (time x chatting style) using a repeated measures ANOVA (N of groups = 3, N of measurements = 2). In order to achieve this, G*Power (version 3.1; Faul et al., 2009) recommended that 51 participants were needed. I also referred to Pauw et al.’s (2017) first study which reported a significant interaction between a between-subjects factor of emotion and a within-subjects factor of support type and obtained an effect size (η_p^2) of .09. Therefore, to detect a similar medium-to-large-sized effect ($f = .30$) with 95% power and 5% alpha for the interaction between two between-subjects factors of event emotion and chatting style using an ANOVA (numerator $df = 2$, N of groups = 6), 175 participants were needed.

177 UK-based undergraduate students (130 female, 43 male, 4 stated ‘other’; $M_{\text{age}} = 23.58$, $SD_{\text{age}} = 6.96$) were recruited from the University of Oxford’s Research Participation Scheme (RPS; $N = 15$) and the Prolific platform (Peer et al., 2017; $N = 162$) between 16th December 2021 and 18th January 2022. Similar to Study 2A, I required all participants to have recently experienced two separate situations with a friend about which they still felt angry (e.g., “My roommate ate my leftovers without asking.”) in one case and regretful (e.g., “I regret that comment I made to my friend.”) in the other case. Participants also had to be willing to talk about these events and be committed to completing the two-part study within the specified time frame¹¹. Again, I warned the 61 participants who showed signs of severe distress on the K10 (Andrews & Slade, 2001) of the risks of continuing their participation at the beginning of the study. All of them decided to continue their participation so were included in our sample.

ii) Design

Study 2B’s main analyses used a 2 x 3 x 2 mixed design with Time (T1: before chat vs. T2: after chat) as a within-subjects factor and Chatting Style (recount vs. stimulus-reappraisal vs. perspective-taking) and Event Emotion (anger vs. regret) as between-subjects factors. In addition to Study 2A’s general affect item, Study 2B also included anger and regret ratings as dependent variables at T1 and T2 to allow me to assess effects on the specific emotions associated with the emotional events that I was comparing.

iii) Procedure

Study 2B’s procedure was similar to Study 2A’s: In the Part 1 questionnaire, I randomly assigned participants to identify either an angry or a regretful recent situation

¹¹ Given that many of Study 2A’s participants missed their next-day chat appointment, I decided to offer extra leeway in this study by allowing participants to complete all study activities within six consecutive days. I contacted all participants who failed to engage in Part 2 using Prolific’s messaging tool and asked them to rearrange their chat appointment.

involving a friend to talk about in the next stage of the study. In the Part 2 questionnaire, participants were randomly assigned to the recount, stimulus-reappraisal, or perspective-taking condition. The chat length was shortened to include only three, instead of five, scripted questions (see [Appendix B.2](#) for my verbatim questions). Questions for recounting participants were similar to those in Study 2A. An example of a question intended to encourage stimulus reappraisal was “Can you try to describe to me a more positive way of interpreting the situation?”, and an example of a question intended to encourage perspective-taking was “Can you describe the situation by imagining that you are a fly on the wall observing how things unfold rather than being directly involved?”. Participants’ emotions about the event were measured before (T1) and after (T2) the chat.

iv) Measures¹²

Pre- vs. post-chat affect. The general affect measure was the same as in Study 2A¹³. The additional measures of anger and regret (“How much [anger/regret] do you feel about the event right now?”) using 0 (“Not at all”) to 6 (“Extremely”) scales were presented to participants both before and after the chat. The anger and regret measures at baseline (T1) did not show a strong intercorrelation ($r(177) = -.11, p = .13$), supporting my decision to assess these emotions separately.

Content analyses. In Study 2B, I again used LIWC (Pennebaker et al., 2015) scores to assess usage of insight- and cause-related words in participants’ responses to

¹² There were additional measures in the Part 1 questionnaire that were not part of the primary focus of the present research and thus are not reported here. To see the full list of these measures, please refer to the [Appendix B.3.2](#).

¹³ As in Study 2A, I included separate positive and negative emotion measures. However, the moderate and significant negative correlation between the two items at baseline (T1; $r(177) = -.59, p < .001$) again suggested that participants mainly experienced univalent rather than bivalent emotions. I also averaged these two baseline ratings to create a single index and found it to be significantly (though weakly) positively correlated with T1 general affect ratings ($r(177) = .31, p < .001$), so I continued focusing only on the general affect measure in my main analyses.

chat questions. I expected the two reconstrual conditions to promote more usage of insight- and cause-related words compared to the recounting condition.

Study 2B also included more direct manipulation checks of the different chatting styles: I presented participants with three questions, each corresponding to the intended effects of one of the conditions (recount: “During the chat, I mainly focused on describing the details of the situation”; stimulus-reappraisal: “During the chat, I focused on trying to reinterpret the situation in a more positive way”; perspective-taking: “During the chat, I focused on imagining how my situation would look from a different perspective”).

Secondary measures¹⁴. As in Study 2A, I measured participants’ frequency of sharing prior to the study, sense of closure, perceptions of the support-provider’s warmth and responsiveness after the chat. The single index of participants’ warmth perceptions was again created by averaging the likeability and closeness ratings given their strong and significant positive correlation ($r(177) = .61, p < .001$).

3.4.2 Results

i) Allocation to Conditions

The *N*s for each condition were: Anger/Recount = 28, Anger/Stimulus-Reappraisal = 30, Anger/Perspective-Taking = 30, Regret/Recount = 29, Regret/Stimulus-Reappraisal = 30, and Regret/Perspective-Taking = 30.

ii) Manipulation Checks

I performed a series of independent samples *t*-tests to check whether rated anger and regret at T1 (prior to the chat) depended on event emotion. Results showed that participants in the anger condition rated their anger about the event ($M = 3.42, SD =$

¹⁴ Study 2B’s secondary measures (except the one regarding previous sharing) used 0 (“Not at all”) to 6 (“Extremely”) rating scales instead of the 1 to 7 scales used in Study 2A. To facilitate clearer comparison between the results of the two studies, the means presented in this Results section are recoded by adding 1 scale point to each.

1.62) significantly higher than those in the regret condition ($M = 1.96$, $SD = 1.65$), $t(175) = 5.95$, $p < .001$, $d = .89$. Similarly, participants in the regret condition rated their regret about the event ($M = 3.76$, $SD = 1.51$) significantly higher than those in the anger condition ($M = 1.85$, $SD = 1.73$), $t(175) = 7.83$, $p < .001$, $d = 1.18$. Therefore, participants were successfully led to report an interpersonal situation with the intended emotion as the dominant one, confirming the success of the emotion manipulation.

I also performed 2 (event emotion) x 3 (chatting style) ANOVAs on the three self-report checks of the chatting style manipulation. There were no significant effects of event emotion or chatting style on participants' self-reports of recounting ($ps > .40$). As for stimulus-reappraisal ratings, chatting style had a significant main effect ($F(2, 171) = 3.31$, $p = .04$, $\eta_p^2 = .04$) but event emotion did not ($p = .79$). Bonferroni-corrected pairwise comparisons indicated that participants focused more on reinterpreting the situation in a positive light in the stimulus-reappraisal condition ($M = 4.28$, $SD = 1.71$) than in the recount condition ($M = 3.49$, $SD = 1.55$; $p = .03$). There were no significant differences between the stimulus-reappraisal and perspective-taking conditions ($p = .98$) or the perspective-taking and recount conditions ($p = .39$). The main effect of chatting style was qualified by a significant interaction with event emotion ($F(2, 171) = 4.35$, $p = .01$, $\eta_p^2 = .05$), indicating that the difference in reported stimulus reappraisal between the stimulus-reappraisal and recount conditions was only significant when the shared conflict involved regret ($p = .003$) and not when it involved anger ($p = 1.00$). Finally, neither the main effect of event emotion on reported perspective-taking ratings nor its interaction with chatting style was significant ($ps > .78$). However, there was a significant main effect of chatting style ($F(2, 171) = 3.68$, $p = .03$, $\eta_p^2 = .04$), suggesting that participants in the perspective-taking condition ($M = 4.85$, $SD = 1.49$) focused more on adopting a new perspective on the situation than

those in the recount condition ($M = 4.05$, $SD = 1.79$; $p = .03$), but not than those in the stimulus-reappraisal condition ($p = .24$). There was also no significant difference between the recount and stimulus-reappraisal conditions ($p = .91$).

As in Study 2A, I used LIWC-coded insight- and cause-related words as an additional check of the chatting style manipulation, after excluding three participants whose responses during the chat were not formatted in a way that permitted processing by the software (leaving a corrected sample size of 174). There were no significant effects of event emotion or chatting style on usage of LIWC-coded insight-related words ($ps > .08$). As for cause-related words, I found only a significant main effect of chatting style ($F(2, 516) = 5.29$, $p = .005$, $\eta_p^2 = .02$). Bonferroni-corrected pairwise comparisons indicated that compared to the recount condition ($M = 2.23$, $SD = 3.15$), these words were used more often in the perspective-taking ($M = 3.36$, $SD = 3.41$; $p = .005$) and stimulus-reappraisal ($M = 3.14$, $SD = 3.62$; $p = .04$) conditions. There was no difference between the two reconstrual conditions ($p = 1.00$).

Taken together, these findings suggest that my manipulation of chatting style was broadly successful. Each of the two forms of reconstrual was distinguishable from recounting on the relevant dimension, and both reconstrual conditions were associated with greater use of cause terms than recounting.

iii) Main Analyses.

General affect ratings. A 2 (time) x 3 (chatting style) x 2 (event emotion) mixed ANOVA revealed a significant main effect of time on general affect ($F(1, 171) = 13.96$, $p < .001$, $\eta_p^2 = .08$), indicating that participants generally felt more positive about the shared event after the chat than before it, regardless of chatting style and event emotion. The main effects of chatting style and event emotion were not significant ($ps > .13$). Also, none of the two-way interactions were significant ($ps > .12$). However, there was

a significant three-way interaction ($F(2, 171) = 3.55, p = .03, \eta_p^2 = .04$) which suggested that the effect of time operated differently for different combinations of manipulated chatting style and event emotion. To decompose this three-way interaction, I computed simple time x chatting style two-way interactions at each level of event emotion (for the origin of the phrase “simple two-way interaction,” see Kassambara, n.d.), but none of these interactions was significant ($ps > .09$). Table 5 presents the means and *SDs* of general affect ratings at each timepoint as a function of chatting style and event emotion.

Anger and regret ratings. Next, I analysed effects on participants’ reported levels of anger and regret. I directly compared ratings of these two emotions by adding “rated emotion” as another within-subjects independent variable in the original $2 \times 3 \times 2$ model. The resulting four-way mixed ANOVA showed that time had a significant main effect on participants’ emotion ratings ($F(1, 171) = 14.90, p < .001, \eta_p^2 = .08$), again meaning that participants generally felt less negative (i.e., less angry and less regretful) after the chat than before it. The main effects of chatting style, event emotion, and rated emotion were not significant ($ps > .16$). None of the two-way interactions were significant ($ps > .30$), except for the interaction between event emotion and rated emotion ($F(1, 171) = 130.92, p < .001, \eta_p^2 = .43$). Bonferroni-corrected pairwise comparisons revealed that angry conflicts were generally associated with more rated anger than regret ($p < .001$) whereas regretful conflicts were generally associated with more rated regret than anger ($p < .001$). This two-way interaction was qualified by a significant three-way interaction between event emotion, rated emotion, and time ($F(1, 171) = 4.97, p = .03, \eta_p^2 = .03$). To decompose this three-way interaction, I computed simple time x rated emotion two-way interactions at each level of event emotion, but none of the interactions was significant ($ps > .11$).

Table 5

Means and Standard Deviations of General Affect, Rated Anger, and Rated Regret at Each Timepoint as a Function of Chatting Style and Event Emotion

Variable	Angry event					
	Recount		Stimulus-reappraisal		Perspective-taking	
	(n = 28)		(n = 30)		(n = 30)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
General affect						
T1	-18.18	16.05	-7.33	21.34	-20.70	14.88
T2	-14.64	18.05	-10.77	20.87	-9.40	19.59
Rated anger						
T1	3.21	1.81	3.30	1.47	3.73	1.60
T2	2.61	1.62	3.10	1.54	2.37	1.45
Rated regret						
T1	1.79	1.69	2.30	2.04	1.47	1.36
T2	1.82	2.04	1.40	1.33	1.90	2.01
Variable	Regretful event					
	Recount		Stimulus-reappraisal		Perspective-taking	
	(n = 29)		(n = 30)		(n = 30)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
General affect						
T1	-15.34	20.13	-16.40	18.60	-14.70	15.35
T2	-9.03	18.95	-1.73	22.26	-7.60	17.01
Rated anger						
T1	2.03	1.68	2.23	1.72	1.60	1.54
T2	1.69	1.65	1.50	1.50	1.67	1.65
Rated regret						

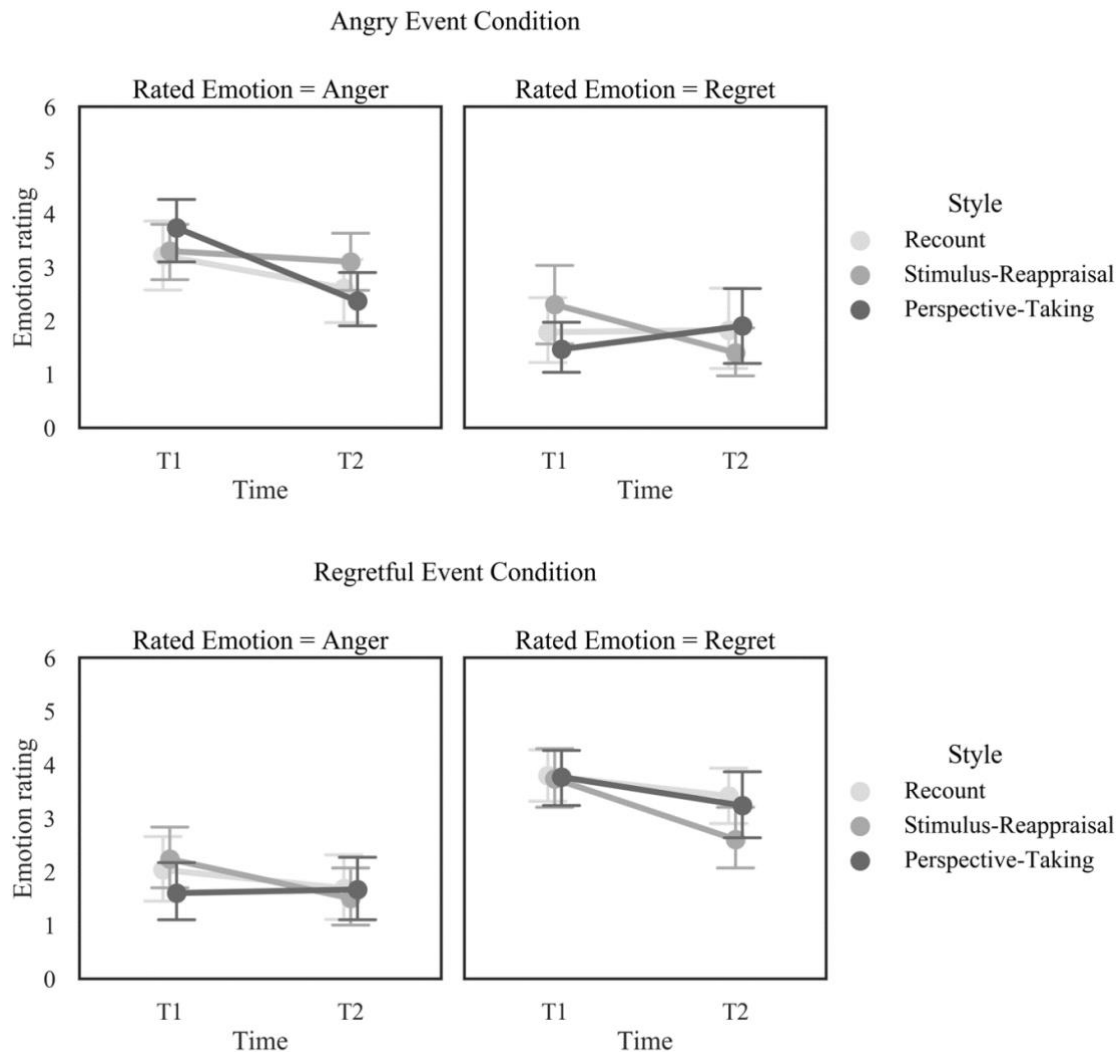
T1	3.79	1.37	3.73	1.60	3.77	1.59
T2	3.41	1.42	2.60	1.67	3.23	1.79

Note. $N = 177$.

Finally, the significant three-way interaction was qualified by a significant four-way interaction between event emotion, rated emotion, time, and chatting style ($F(2, 171) = 3.68, p = .03, \eta_p^2 = .04$; see Figure 2). To decompose this four-way interaction, I computed simple rated emotion x chatting style x time three-way interactions at each level of event emotion. The simple three-way interaction was significant in the angry event condition ($F(2, 85) = 4.47, p = .01, \eta_p^2 = .10$) but not in the regretful event condition ($p = .60$). To decompose the simple three-way interaction in the angry event condition, I further ran simple time x rated emotion two-way interactions at each level of chatting style. The interaction was significant in the perspective-taking condition ($F(1, 29) = 8.95, p = .006, \eta_p^2 = .24$) but not in the recounting ($p = .28$) and stimulus-reappraisal ($p = .26$) conditions. Analysis of simple simple main effects (for the origin of this phrase, see Kassambara, n.d.) revealed that perspective-taking led to a significant reduction in rated anger about angry events ($p < .001, \eta_p^2 = .38$), but there was no significant effect of time on rated regret about the same event type ($p = .40$). Table 5 presents the means and *SDs* of anger and regret ratings at each timepoint as a function of chatting style and event emotion.

Figure 2

Four-Way (Event Emotion $\times$ Rated Emotion $\times$ Time $\times$ Chatting Style) Interaction



Note. The left panel in each row presents mean anger ratings and the right panel mean regret ratings. Both panels show the respective change in the ratings from T1 to T2 when each of the three chatting styles was applied (recount: the lightest lines; stimulus-reappraisal; perspective-taking: the darkest lines). Error bars represent 95% confidence intervals (CI).

iv) Secondary Analyses

Previous sharing. Unlike in Study 2A, there was no requirement for prior sharing of the negative event before participation. However, only 25 participants (14.12%) reported not having discussed the shared event in advance of the study. The overall mean frequency of previous sharing was 3.62 times ($SD = 4.11$ times, skewness

= 2.53, median = 3 times), which was not significantly different from the mean obtained in Study 2A ($p = .47$).

Closure. The mean rating of closure was 3.79 ($SD = 1.57$), which was not significantly different from the mean obtained in Study 2A ($p = .37$). A two-way ANOVA of closure ratings yielded no significant effects ($ps > .19$). Because closure did not depend on my manipulations, it is unlikely to be a mediator of significant effects on other dependent measures. Therefore, mediation analyses were not conducted.

Perceived warmth of support-provider. Perceived warmth was not significantly affected by my manipulations of chatting style or event emotion ($ps > .16$). The mean rating was 4.43 ($SD = 1.36$), which was not significantly different from the mean obtained in Study 2A ($p = .23$). As in Study 2A, I tested whether perceived warmth had a main effect on general affect, rated anger, and rated regret or interacted with any of the three predictors in the $2 \times 3 \times 2$ model used in the main analyses (reduced model) by adding this variable to the model (full model). However, results showed that the full model failed to yield significantly better fit than the reduced model ($AIC = 1386.5$; $ps > .22$), except for when rated regret was the outcome ($AIC = 1383.7$; $p = .01$).

Specifically, perceived warmth did not have a significant main effect on rated regret ($p = .19$), but it was involved in a significant four-way interaction ($b = -1.33$, $SE = 0.64$, $p = .04$, 95% CI $[-2.54, -0.12]$). Post-hoc tests further revealed that the difference between levels of rated regret at T2 for regret- and anger-inducing conflicts in the perspective-taking condition was greater when the support-provider was perceived as warmer ($b = 0.87$, $SE = 0.34$, $p = .01$, 95% CI $[0.18, 1.56]$). In other words, perceptions of support-providers as warm were associated with relatively higher levels of post-chat regret about regret-inducing conflicts only when the chat encouraged perspective-taking.

Perceived responsiveness of support-provider. The mean rating on the responsiveness item was 3.78 ($SD = 2.11$), which was not significantly different from the mean obtained in Study 2A ($p = .19$). Adding this variable to the $2 \times 3 \times 2$ analyses of general affect, rated anger, and rated regret did not significantly improve model fit ($ps > .05$).

3.4.3 Discussion

Consistent with Study 2A, I found that cause-related words were used more in both the stimulus-reappraisal and perspective-taking conditions than in the recount condition, suggesting that these two reconstrual styles were effective in promoting meaning-making. However, unlike Study 2A, Study 2B provided evidence that certain forms of reconstrual outperformed recounting in terms of its effectiveness in regulating certain emotional responses.

My first hypothesis that both forms of reconstrual would improve sharers' affect to a greater extent than recounting was not supported, given the general reduction in negative affect following sharing in all chatting style conditions. Nevertheless, there was some support for my second hypothesis that the beneficial effects of reconstrual would depend on sharers' specific emotional response. Specifically, one of the reconstrual styles, perspective-taking, was found to be especially effective in alleviating sharers' reported anger induced by an angry interpersonal situation. This is contrary to my expectation that anger would be less susceptible than regret to alleviation as a result of reconstrual but somewhat reflects the success of my more targeted manipulation of different forms of reconstrual combined with my instructions to focus on specific kinds of interpersonal conflict inducing either regret or anger.

An explanation for perspective-taking being especially suited for regulating anger is that this style facilitated a more objective perspective that helped sharers

distance themselves from the unpleasant stimulus (the wrongdoing of another person). Moser et al. (2017) found that third-person self-talk results in less negative affect than first-person self-talk, suggesting that using names and third-person pronouns to refer to the self leads people to think about themselves as if they are someone else, thereby facilitating emotion regulation. Such self-distancing may therefore enable angry sharers particularly to see things from a different perspective (potentially the perspective of their antagonist), thereby reducing their other-blame appraisals (e.g., Smith & Lazarus, 1993) and antagonistic action tendency (e.g., Ellsworth & Smith, 1988) and consequently their anger. On the other hand, stimulus-reappraisal, which offers a more positive perspective that helps sharers see the bright side of what has happened, did not lead to an extra reduction in anger probably because angry sharers were not prepared to reconsider whether what their antagonist had done was a bad thing, so they could be encouraged to find good in the bad and feel less angry as easily.

I found no evidence for my prediction that stimulus-reappraisal would be more effective than perspective-taking in regulating regret. Rather, both reconstrual styles seemed to work equally well. Gilovich and Medvec (1995) have proposed that the strategy of identifying a silver lining facilitates the alleviation of regret. Therefore, stimulus-reappraisal likely enabled regretful sharers to use this strategy to convince themselves that the decisions they made in the past, though irreversible, are not entirely bad, thereby reducing their self-blame appraisals (e.g., Wetzler et al., 2007). On the other hand, perspective-taking likely reduced reported regret by allowing the sharers to gain more objectivity when reviewing their mistakes.

If the above analysis is correct, then the different effects I found for angry and regretful conflicts reflect the fact that a wide range of chatting styles can alleviate regret but a more restricted set can alleviate anger. However, it should be noted that there is a

caveat to the interpretations of the results of the present study: The three questions used in the stimulus-reappraisal and perspective-taking conditions to prompt participants to reconstrue their experience during the chat were not pre-tested, so the finding that perspective-taking caused a further reduction in reported anger in the anger condition may not be completely explained by participants' usage of that strategy. As shown in the manipulation checks, participants' levels of engaging in stimulus-reappraisal and perspective-taking were not different across the two reconstrual conditions. The difference in reported stimulus reappraisal between the stimulus-reappraisal and recount conditions was only significant when the shared conflict involved regret and not when it involved anger. Therefore, my choice of questions for the two reconstrual conditions may not have been sufficiently well targeted to bring about the intended effects of those styles to a sufficient degree, resulting in inadequate support for my hypotheses.

In my secondary analyses, I found that closure levels were not significantly different across chatting style conditions, failing to replicate our findings in Study 2A and Lee et al.'s (2020) finding that participants who reconstrued experienced greater closure than those who recounted. This may reflect the fact that reconstruing participants were more reluctant to report a sense of closure for events involving either regret or anger than for events that elicited more general negative affect (the focus of Study 2A).

Perceived warmth of the support-provider did not increase the emotion-improving effect of the conversation as it did in Study 2A. Its only significant effect was to increase the relative level of rated regret in the regret condition compared to the anger condition when the chat encouraged perspective-taking. One possible explanation for this effect is that participants who were attempting to see things from a more objective perspective felt more able to express their regretful feelings (and less willing

to express anger) with a warmer support-provider, and this effect then generalized to their subsequent rated regret on the subsequent rating scale. Finally, perceived responsiveness of the support-provider did not have any moderating effects on our emotion measures, consistent with our finding in Study 2A.

3.5 General Discussion

Lee et al. (2020) reported an experimental investigation of the effects of recounting and reconstrual chatting styles in computer-mediated communication about acute interpersonal conflicts. The present research advanced on their study by assessing whether similar effects of these chatting styles applied when discussing more chronic concerns (e.g., relating to the collective health crisis of COVID-19; Study 2A), and by comparing the effects of two specific forms of reconstrual with those of recounting on different negative emotions often induced by interpersonal conflicts (Study 2B).

Across both of my studies, reconstrual produced more beneficial effects than recounting. In Study 2A, reconstrual increased the sense of closure more than recounting did. Reconstrual also promoted meaning-making by encouraging the use of cause-related words to a greater degree than recounting, regardless of the nature of the shared concern. Additionally, Study 2B showed that reconstrual based on perspective-taking is especially effective in alleviating anger induced by angry events, perhaps because it encourages an objective perspective that helps people reduce their other-blame appraisals. Based on these findings, it seems that a generic instruction to reconstrue (as used in Study 2A) is not more sufficient than recounting to reduce negative affect across all kinds of emotional situations. Instead, cueing sharers to use specific forms of reconstrual (as in Study 2B) seems to better address the unique appraisals associated with each negative emotion, leading to alleviation of that emotion.

Given that cognitive support is associated with long-term recovery (Nils & Rimé, 2012; Tamir, 2009), future research should extend the range of manipulated chatting styles and assess their effects on a wider range of emotional experiences to identify the most suitable form of reconstrual for various kinds of negative emotions.

The specific appraisals associated with a particular emotion may also be relevant to how easily closure is reached. In Study 2A, general reconstrual cues may have led to increased closure because the shared events elicited general negative affect rather than particular emotions. By contrast, Study 2B targeted anger and regret specifically but did not find any difference in closure levels across chatting styles. This implies that although sharers are more able to recover from events eliciting anger or regret using the appropriate reconstrual style than using recounting, they are still reluctant to report that the event is resolved. Specifically, angry sharers may still have found it difficult to let go of their grievance because the harm done to them had not yet been rectified, and regretful sharers may still have found it difficult to put the mistake completely behind them because its negative outcome could no longer be rectified.

A major limitation of the present research is the absence of a control condition which would have allowed a direct comparison of the emotional effects between shared and non-shared negative experiences. Given that this project was conducted starting from August 2021, one month after the UK government removed most legal limits on social contact, participants might have already started interacting with a wider range of other people in person and perceived social sharing of any kind of event with a virtual listener less appealing than it might be when social distancing measures were still being enforced. In other words, there might be no difference in sharers' affect regardless of whether sharing took place. This might help explain why I did not find a stronger emotion-regulatory effect of reconstrual than of recounting in Study 2A. Although one

of the reconstrual styles, perspective-taking, did lead to greater induction in anger than did recounting in Study 2B, it should be noted that I gave participants a more specific instruction to reconstrue their experience in a certain way instead of giving a generic instruction to reconstrue. Therefore, it is possible that after the removal of social distancing measures, people benefitted from chatting with a virtual listener particularly when the chat encouraged them to use specific forms of reconstrual that addressed the distinctive natures of different emotions and the specific appraisals associated with them.

Another limitation of the present research arises from its absence of analysis of the text provided by participants during the chat. In both Studies 2A and 2B, reconstruing sharers did not experience greater affect improvement than recounting ones, failing to replicate Lee et al.'s (2020) finding. Although I collected participants' subjective ratings of their stimulus-reappraisal and perspective-taking levels in Study 2B, there were still no indications that participants did engage in reconstrual. Those ratings may only reflect what participants believed they were doing, but not what they were actually doing. A direct approach to addressing this issue would be conducting a qualitative text analysis: Recruiting two human judges, who are blind to the chatting style conditions, to view each participant's responses to the prompt questions and assess whether the participant was reappraising their experience (and whether they were using stimulus-reappraisal or perspective-taking) or simply recounting it.

I believe that my work has important implications for online (particularly text-based) mental health services. Lee et al. (2020) used confederates to ask scripted questions to cue participants to recount or reconstrue their negative experience. My use of pre-programmed responses allowed me to eliminate uncontrolled factors resulting from the use of human confederates (e.g., typing speed, choice of word), thereby

ensuring that conversation outcomes were not a consequence of differences in the delivery of questions during the chat. Despite its lack of nonverbal cues to facilitate successful support provision (Jakubiak & Feeney, 2017), the present research showed that participants generally reported emotional improvement following an online chat, adding support to previous studies that have demonstrated positive effects of remote interactions (e.g., Kessler et al., 2009; Morris et al., 2015). Moreover, the fact that participants' emotions still improved even when they felt that the support-provider was not really responding to their comments provided further evidence regarding the advantages of using a chatbot as an impactful tool in online therapies (for a review, see Vaidyam et al., 2019). Specifically, although more advanced technology is needed to enable chatbots to offer more dynamically attuned responses, they can be applied in the initial sessions to help people cope with their negative experience by prompting them to discuss the event using a chatting style that works most effectively for the specific emotion involved. Given that online mental health care is more accessible and affordable than the in-person version, more research should be carried out to examine and enhance the benefits of chatbots in clinical settings. Future research should also compare the effects of different modes of provision of social support (e.g., text-based, video-mediated, and in-person conversations).

To conclude, the present research generally confirms that reconstrual is a more adaptive way to discuss negative experiences than recounting in computer-mediated communications. Furthermore, my findings contribute to the social sharing and emotion regulation literature by offering further insights into the limits and moderators of previous findings: First, reconstrual appears effective in regulating emotions not only when people are concerned about an interpersonal conflict but also when they are concerned about a collective crisis. Second, the benefits of reconstrual can be

maximized when the more appropriate form is identified and used. More specifically, my results suggest that perspective-taking is especially suited for regulating anger induced by angry events. In the next chapter, I will turn to an underexplored kind of emotional experience, bivalent emotions, and investigate social sharing of these emotions and its consequences.

CHAPTER FOUR

Project 3: Social Sharing of Bivalent Emotions in Daily Life

4.1 Abstract

What motivates people to share their bivalent emotional experiences, which are characterised by the feelings of conflict, and how do they feel after sharing? Are these motives and affective outcomes of sharing bivalent emotions different from those of sharing univalent emotions (purely positive or negative)? The present project investigated the underexplored topic of bivalent emotions in an interpersonal context and tackled these questions using prospective questionnaires. Results showed that the primary motives for sharing bivalent experiences include finding meaning and seeking emotional and cognitive support, which are similar to the motives for sharing negative experiences. I also found preliminary evidence that sharing bivalent experiences seems to help alleviate sharers' negative affect and felt conflict but have no impact on positive affect, resulting in lower levels of ambivalence. The same reduction in negative affect and felt conflict also seem to follow sharing of negative experiences, accompanied by an increase in sharers' positive affect. By contrast, sharing positive experiences is associated with no affect change.

When people experience an emotion, they immediately feel the need to share it with someone else and actually do so in most of the cases (Rimé et al., 1991; for a review, see Rimé, 2009). Prior research has extensively investigated sharing motives, processes, and consequences of univalent emotions, given the prevalence of this kind of emotional experience (Watson & Stanton, 2017). However, emotions are complex in that they can manifest in a bivalent form, when people simultaneously feel opposing emotions (Larsen et al., 2001; Larsen & McGraw, 2011) or sequentially feel opposite emotions in rapid alternations (Filipowicz et al., 2011; Sinaceur et al., 2013). Despite them not being as prevalent as univalent emotional episodes, it is still important to explore bivalent episodes in order to complete understanding of the affective side of people's lives.

Different measurement methods have been used to estimate the intensity of bivalent emotional experiences (for a review, see Berrios et al., 2015). A common one, inspired by previous works on attitudinal ambivalence (e.g., Priester & Petty, 1996), is using the minimum value between positive and negative emotion ratings (i.e., MIN index; Schimmack, 2001; Williams & Aaker, 2002): If a participant gives a rating of 4 for happiness and 2 for sadness, the MIN index will give a value of 2, and if either of the emotions is rated as 0, the MIN index will be 0, indicating that bivalent emotions are not felt because the experience would be purely positive or negative. This method requires researchers to measure positive and negative emotions independently instead of using a bipolar scale with positive and negative endpoints, which can yield the “midpoint” problem. That is, researchers will be unable to tell whether a midpoint rating reflects the fact that participants are feeling ambivalent and trying to do justice to both their positive and negative evaluations (Kaplan, 1972; Thompson et al., 1995) or that they are feeling neutral (neither positive nor negative) about a stimulus.

Prior research has documented that people share their positive experiences to capitalise on their pleasantness and as a means to reach out to other people and bond with them (e.g., Duprez et al., 2015; Gable et al., 2004; Gable & Reis, 2010). As for negative experiences, despite the risk of reviving the unpleasantness, people share them to make better sense of what happened and alleviate distress (e.g., Duprez et al., 2015; Harlow & Cantor, 1995). Why people share their bivalent emotional experiences, which consist of both pleasant and unpleasant feelings about a stimulus (Larsen et al., 2001), however, is unclear. In fact, even the most basic question of whether people share this kind of experience has not been investigated previously.

Drawing upon Festinger's (1957) work on cognitive dissonance, Williams and Aaker (2002) found that bivalent emotional experiences, wherein people experience internal inconsistency, lead to a third independent emotion of discomfort, which in turn elicits the desire for consistency and clear action tendencies (see also Cacioppo et al., 1997; Priester & Petty, 1996). To fulfil this desire, people search for appropriate coping strategies (Folkman et al., 1986). Notably, if the event is of low personal relevance, people may simply focus on only their positive or negative evaluations of it to resolve the conflict (Nordgren et al., 2006). However, if the event is of high personal relevance, they are more likely to engage in more systematic processing (Jonas et al., 1997; Petty & Cacioppo, 1986), such as reappraising the causes of their bivalent emotions (Zanna & Cooper, 1974).

Rothman and Melwanie (2017) built upon Schwarz and Clore's (1983) affect-as-information model to develop an account of the cognitive function of bivalent emotions: As previously noted, HSM proposes that people process information in two broad ways: Heuristically or systematically (Chaiken, 1980). Based on Bohner et al.'s (1995) review, people use heuristic processing when their confidence in their judgment about the

situation reach or surpass their sufficiency threshold, which is set low by default when the environment is simple and safe. However, when negative emotions are experienced, this signals that the situation is problematic and potentially threatening, resulting in raising of the sufficiency threshold and triggering more careful and deep processing of information (see also Gasper & Clore, 2002). According to Rothman and Melwanie (2017) bivalent emotions signal complex and contradictory elements in the environment, resulting in expanded cognitive boundaries which enhance susceptibility to alternative perspectives. The mechanism underlying this process is likely to be the raising of the sufficiency threshold resulting from the perception that the situation is complicated. Indeed, previous studies have found that people experiencing bivalent emotions engage in more systematic processing (Jonas et al., 1997; Petty & Cacioppo, 1986), such as reappraising the causes of their emotions (Zanna & Cooper, 1974), if the event is of high personal relevance. Relatedly, Rothman and Melwanie (2017) proposed that the bivalent emotional state has a motivational function: Its inherent conflict should drive people to gather and integrate alternative perspectives to reach a balanced consideration of the situation.

Integrating these views into the framework of the functions of social sharing, people probably share their bivalent emotions to alleviate discomfort resulted from violation of consistency and to gain new insights from other people that help revise their current appraisals about the situation. In other words, people may share bivalent emotions for generally similar reasons to those that motivate them to share negative emotions.

4.2 Pilot Study

While there is evidence that people experience bivalent emotions in naturalistic settings (e.g., Hostler & Berrios, 2021; Larsen et al., 2001), no one has checked how often this happens and how intense these emotions generally are. Because I planned to ask participants to report an emotional experience that naturally took place within a given time frame in the main study, it was important to first assess the frequency of experiencing each type of emotions of my interest (positive, negative, and bivalent) in order to determine how long that time frame should be. This pilot study therefore aimed to collect data on people's most recent emotional experience of these three kinds before establishing the design parameters for the main study. I also collected data on how intense these three types of emotional experience generally are in daily life.

4.2.1 Method

i) Participants

To detect a medium-sized effect ($f = .25$) of emotion type on event occurrence and MIN index with 95% power and 5% alpha using a one-way repeated measures ANOVA (N of groups = 1, N of measurements = 3), G*Power (version 3.1; Faul et al., 2009) recommended that a minimum sample size of 43 was needed. However, I expected that some participants would fail to report three independent emotional experiences, so I decided to recruit a larger number of participants.

All participants were recruited via the Prolific platform (Peer et al., 2017) on 19th December 2019. One participant stated that their bivalent emotional experience took place on 24th December 2019, which was five days later than the recruitment day. Because a future date of the occurrence of a reported experience could not be used to compare with the recruitment date for calculating how recently the corresponding emotion type was last felt, I excluded this participant from data analysis. Two other

participants were excluded because they did not report three independent emotional experiences, and two more were also excluded because they completed the study in a non-English language. The final sample size was 95 (53 female, 42 male; $M_{\text{age}} = 27.42$, $SD_{\text{age}} = 8.59$).

ii) Procedure

After providing informed consent, participants were presented with examples that corresponded to the three specified kinds of emotional experience. They then completed three question blocks, each enquiring about the details of one of the three emotional experiences, in random order. In each block, participants were asked to recall their most recent positive/negative/bivalent emotional experience—even if it seemed trivial or insignificant—and describe what happened and how they felt. They then stated the month and day when this experience took place and rated how positive and negative their feelings were during the experience. Finally, they indicated how often they generally felt bivalent emotions, before being debriefed and thanked.

iii) Measures

The date of the occurrence of each emotional experience that participants reported was used to calculate how recently the corresponding emotion type was felt by them. I also included a complementary measure that asked participants to indicate their generally frequency of experiencing bivalent emotions (daily, every 2-3 days, every 4-5 days, weekly, every two weeks, or less often than every two weeks).

Participants were asked to rate their positive and negative feelings during each experience on separate 6-point Likert scales ranging from 0 (“Not at all”) to 5 (“Extremely”). The lower rating was used as the MIN index that reflects the intensity of bivalent emotions.

4.2.2 Results and Discussion

The mean difference in dates between the reported emotional experience and the recruitment day was 38 days for positive ($SD = 75.11$ days, skewness = 2.85, median = 9 days), 48.88 days for negative ($SD = 79.15$ days, skewness = 2.11, median = 12 days), and 57.95 days for mixed ($SD = 80.48$ days, skewness = 1.65, median = 17 days; see Figure 3). A Friedman test¹⁵ showed that date differences were not statistically different across emotion type ($p = .09$). There was a wide range of occurrence dates for all three types of emotional experience, such that some participants reported experiences from more than nine months ago. One possible explanation is that participants were motivated to produce “good” data for the study due to social desirability pressures (Gawronski & De Houwer, 2014) and therefore focused on significant rather than the most recent, but relatively trivial, experiences. Alternatively, it is possible that only particularly intense and memorable events appeared salient to participants at the time of reporting due to availability biases (Tversky & Kahneman, 1973).

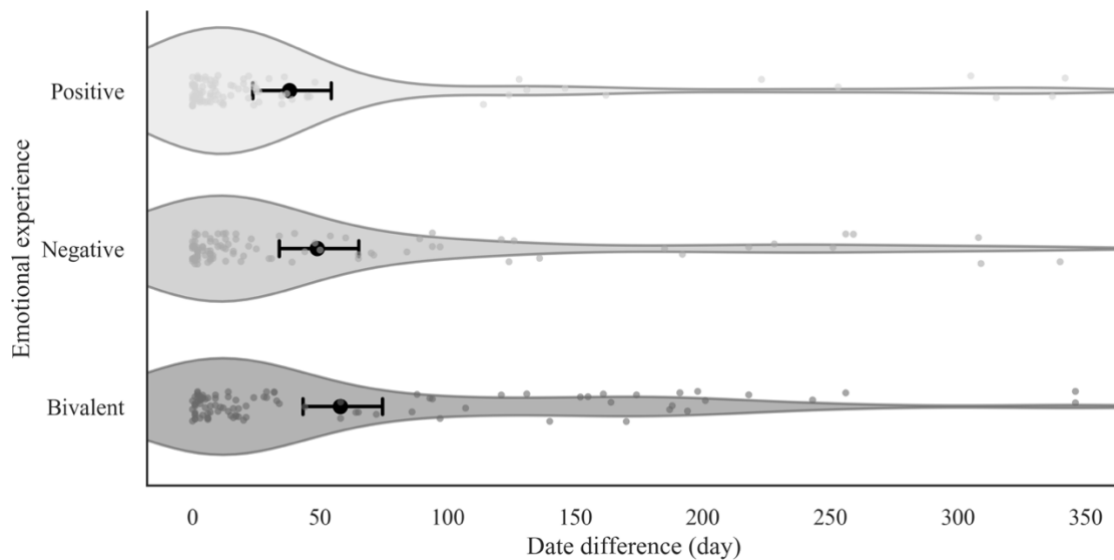
Given the skewed distributions of date differences, the medians (positive: 9 days; negative: 12 days; bivalent: 17 days) may be more meaningful than the means. In other words, 50% of the reported positive and negative emotional experiences took place within the past two weeks whereas 50% of bivalent emotional experiences took place slightly more than two weeks ago. However, this estimate of emotion frequency still was exceeded by the one provided by participants when directly asked how often they felt bivalent emotions in general. 74.74% of participants indicated that they experienced these emotions more frequently than on a weekly basis. Because previous evidence has suggested that univalent emotions are more prevalent than bivalent

¹⁵ The non-parametric version of one-way repeated measures ANOVA was used because my data were not normally distributed.

emotions (Watson & Stanton, 2017), it seems likely that participants actually experienced purely positive and purely negative emotions even more frequently than on a weekly basis. Therefore, it seems that a 7-day window would be ideal for participants in the main study to naturally experience their assigned type of emotions and report on it.

Figure 3

Mean Differences in Dates Between the Occurrence of Each Type of Emotional Experience and the Recruitment Day



Note. Each datapoint is illustrated in dots. Error bars represent 95% confidence intervals (CI).

Paired samples t-tests confirmed that participants felt significantly more positive ($M = 4.53$, $SD = 0.63$) than negative ($M = 0.29$, $SD = 0.52$) about their reported positive emotional experiences ($t(94) = 44.46$, $p < .001$, $d = 4.56$), and significantly more negative ($M = 4.37$, $SD = 0.81$) than positive ($M = 0.43$, $SD = 0.68$) about their reported negative emotional experiences ($t(94) = -29.26$, $p < .001$, $d = -3.00$). In both cases, I found no significant correlations between remoteness of experience and rated feelings ($ps > .09$). As for bivalent emotional experiences, participants rated their positive feelings ($M = 3.24$, $SD = 1.05$) as significantly more intense than their negative feelings

($M = 2.73$, $SD = 1.08$; $t(94) = 2.97$, $p = .004$, $d = .30$). Notably, there was a significant positive correlation between positive ratings and remoteness of this type of experience ($r(95) = .21$, $p = .04$), suggesting that bivalent emotional experiences that occurred longer ago were associated with higher positive ratings. However, the correlation between negative ratings of remoteness of this type of experience was not significant ($p = .59$).

Additionally, a one-way repeated measures ANOVA revealed that the MIN index was significantly different between emotion types ($F(2, 188) = 264.77$, $p < .001$, $\eta_p^2 = .74$). Bonferroni-corrected pairwise comparisons indicated that the MIN index for bivalent emotional experiences ($M = 2.31$, $SD = 0.87$) was significantly greater than that for positive ($M = 0.28$, $SD = 0.47$; $t(94) = 19.92$, $p < .001$, $d = 2.87$) and negative ones ($M = 0.41$, $SD = 0.61$; $t(94) = 16.15$, $p < .001$, $d = 2.51$), confirming that participants who reported a bivalent emotional experience felt more ambivalent than those who reported a univalent one. The MIN indices for the two univalent experiences were not significantly different from each other ($p = .23$). A limitation of this pilot study is its lack of a measure that directly assesses feelings of conflict, which have been suggested to uniquely accompany bivalent emotions (Thompson et al., 1995; Williams & Aaker, 2002). My main study addressed this limitation.

In sum, my results suggested that bivalent emotions are felt in daily life and are moderately intense in general. However, it should be noted that bivalent emotional experiences are more subject to a memory decay effect than are univalent ones (Aaker et al., 2008). Therefore, it is likely that participants in this pilot study remembered their bivalent emotional experiences as less intense than they originally were, especially when their “most recent” experience turned out to be more than two months old on average.

4.3 Study 3

This main study used a prospective rather than retrospective methodology to collect data on the details of social sharing of positive, negative, and bivalent emotions. More precisely, I asked participants to report an emotional experience that happened within the next few days, instead of asking them to report on the most recent one that happened before the study. My primary interest was to explore the reasons for sharing bivalent emotions using Duprez et al.'s (2015) Social Sharing Motives Scale (SSMS) and assess the affective outcomes of such sharing in comparison to those of sharing of univalent emotions.

Duprez et al. (2015) developed the 39-item SSMS directly based on the reasons that their participants recalled for sharing positive or negative episodes. The scale contains seven categories of sharing motives: Clarification/meaning (CM; gaining an understanding of an emotional experience and making a meaning out of it), rehearsing (R; reliving and even amplifying an emotion), venting (VE; hoping to reduce the emotional weight associated with an experience), arousing empathy/attention (AEA; eliciting listeners' sympathy), informing and/or warning (IW; using one's own experience to benefit other people), assistance/support and comfort/consolation (HC; pursuing emotional support), and advice/solutions (AS; pursuing intellectual and/or practical support). The authors reported that R and AEA motives are stronger for positive than negative emotions whereas CM, VE, HC, and AS motives are stronger for negative than positive emotions. IW, on the other hand, is equally rated as a sharing motive by people feeling positive and those feeling negative. I expected these results to be replicated in this study.

Although results from my pilot study revealed that bivalent experiences are rated as less negative than negative ones, bivalent experiences inherently produce additional feelings of conflict (e.g., Cacioppo et al., 1997; Priester & Petty, 1996; Williams & Aaker, 2002), which are distressing and possibly give rise to a desire for VE and HC as in the case of negative experiences. Both negative and bivalent emotions have also been suggested to stimulate systematic processing, though the underlying mechanisms are not exactly the same: Negative emotions signal that the situation is problematic and potentially threatening (for a review, see Bohner et al., 1995), thereby driving people to engage in more careful and deep processing of information (e.g., Gasper & Clore, 2002). On the other hand, bivalent emotions indicate that the situation is changing and contains contradictory elements, thereby driving people to expand their cognitive boundaries by considering a broader set of alternatives (for a review, see Rothman & Melwanie, 2017). Drawing from these previous works, it seems that although both kinds of emotions elicit the same need for cognitive clarity to form a better approach towards the environment, bivalent emotions are associated with a greater need for other people's perspectives to improve their accuracy of interpreting the situation. Indirect evidence can be found in Rees et al.'s (2013) work, which showed that bivalent emotional states lead to higher receptivity to alternative perspectives than univalent ones. Therefore, I predicted that bivalent emotions would involve the same desire for CM and AS as in the case of negative emotions but that the desire for AS would be stronger in the former case.

I also compared the affective outcomes of sharing different kinds of emotions. First, I expected participants experiencing positive emotions to capitalise on their pleasant episodes during social sharing (e.g., Duprez et al., 2015; Gable et al., 2004; Gable & Reis, 2010) and consequently report increased positive feelings. Second,

participants experiencing negative emotions should recover from their unpleasant episodes as a result of social sharing (e.g., Duprez et al., 2015; Harlow & Cantor, 1995) and consequently report increased positive feelings and decreased negative feelings. Finally, I hypothesised that participants experiencing bivalent emotions would resolve the felt conflict inherent in their ambivalent episodes (e.g., Folkman et al., 1986; Rothman & Melwanie, 2017; Williams & Aaker, 2002) and consequently report increased positive feelings and decreased negative feelings and felt conflict. This means that the intensity of their ambivalence, which is reflected by the MIN index, should decrease over time, as these episodes became generally more positive.

4.3.1 Method

i) Participants

I planned to perform one-way ANOVAs to test whether participants experiencing the three different types of emotions (between-subjects factor) differed in their sharing motives. To detect a medium-sized effect ($f = .25$) with 95% power and 5% alpha, G*Power (version 3.1; Faul et al., 2009) recommended that a sample size of 252 was needed. Given that my last attempt (Study 1) at recruiting a large sample to complete a multiple-part study was costly and time-consuming, this time I set the desired power level to 80%, which is more usual in social psychology. Updating this parameter in the power analysis, a minimum sample size of 159 was required. I also wanted to examine if there was any change in participants' feelings from before to after social sharing took place for the different emotion types. To detect a medium-sized effect ($f = .25$) with 80% power and 5% alpha for a within-between interaction (time x emotion type) using a repeated measures ANOVA (N of groups = 3, N of measurements = 2), a minimum sample size of 42 was required. Thus, 159 participants would have been sufficient for this study. However, I expected that not every participant would have

the chance to initiate social sharing during the 24 hours after reporting an experience, so I aimed to continue recruiting participants until I had 159 participants who indicated that they had shared their emotions.

Only 136 of the 212 participants who were recruited reported sharing in time for the completion of this chapter. The 212 participants were recruited via the Prolific platform (Peer et al., 2017) between 24th August 2022 and 11th November 2022. All of them were adults, able to read and write English proficiently, and willing to commit themselves to completing all three parts of the study within nine consecutive days. Because the present research primarily investigated why people share different kinds of emotions and the consequences of sharing, I focused on the subset of participants who discussed their emotional experience with someone else during the 24-hour interval. 64.15% of the total sample engaged in social sharing ($N = 136$; 56 female, 78 male, 1 stated ‘other’, 1 stated ‘prefer not to say’; $M_{\text{age}} = 31.69$, $SD_{\text{age}} = 11.64$), consistent with the previous finding that approximately 60% of the emotional experiences are first shared on the actual day that the event happened (Gable & Reis, 2010).

ii) Design

This study had a 3 x 2 mixed design with Emotion Type (positive vs. negative vs. bivalent) as the between-subjects factor and Time (before vs. after a 24-hour interval) as the within-subjects factor. My main dependent variables were ratings for each of the seven categories of sharing motives, which were collected only after the 24-hour interval, as well as ratings for positive and negative feelings, which were measured at the time when participants reported their emotional experience (T1) and 24 hours later (T2).

iii) Procedure

This study had three stages. In Stage 1, after providing their informed consent and demographic information (e.g., age, gender), participants were informed of which kind of emotional experience they were randomly assigned to report in Stage 2. They were given seven days (including the day when Stage 1 happened) to report the experience and instructed that the experience had to take place within those seven days after they completed Stage 1. Also, participants were told that their reported experience should involve feeling two emotions (e.g., positive: “I won a competition and will be attending the award ceremony very soon. I feel so proud and excited.”; negative: “I had to cancel my trip because my doctor just called me and urged me to see her at the hospital. I feel so disappointed and anxious.”; bivalent: “I am so excited that I’m about to start a new job that I’ve been dreaming of very soon. However, I’m also anxious because the job requires me to move to a new city that I’ve never been to before.”). The instruction to report on mixed (monovalent or bivalent) emotional experiences was intended to ensure that my results were driven by the combination, rather than quantity, of emotions that were felt. Subsequent to participants’ submission of the Stage 1 questionnaire, an invitation to the Stage 2 questionnaire was sent to them manually via Prolific’s messaging tool.

In Stage 2, I first asked participants to indicate which kind of emotional experience they were assigned to report to confirm that they had paid attention to the result of random assignment in Stage 1¹⁶. Those who failed to pass this check were

¹⁶ Please note that this check was not implemented until the first 31 participants had already been recruited. These participants’ descriptions of their experience were checked manually to ensure that the reported emotions matched the assigned type. Ten of these participants did not pass this check and were also excluded from the study. Such a high proportion of participants failing to report the correct kind of experience may be attributed to the fact that I implemented random assignment of emotion conditions at the beginning of the study (instead of at the end in the updated version), which probably caused participants to have easily forgotten which condition they were assigned to in Stage 1. Also, three participants did not proceed to Stage 3 because they indicated that they did not experience the target emotions within the given time frame.

excluded from the study ($N = 18$). In most cases, those who passed the check proceeded to reporting a personal experience of the assigned emotion type by describing the experience in terms of what happened and what emotions they felt at the time. Following this writing task, they rated how eager they had been to share their experience and how positive, negative, and conflicted they felt during the experience (T1). However, 16 participants who reported not experiencing the target emotion for their condition during the allotted 7-day time frame were not asked to proceed to Stage 3.

An invitation to Stage 3 was sent to participants after a further 24 hours. As in Project 1, I set the interval between this and the last stage to 24 hours to allow social sharing to naturally occur and to allow me to collect participants' sharing details when they were still fresh. Once the invitation was sent, participants had another 24 hours to complete the questionnaire. Participants who reported sharing their experience were asked to indicate how many times they had discussed it within the 24-hour interval. Then, they reported the reasons for sharing the experience by completing the abridged version of SSMS described below. Participants who did not share their experience were asked to briefly explain why they did not discuss it with anyone. All participants were again required to rate their current eagerness to share and feelings about the reported experience (T2). Finally, they were debriefed and thanked.

iv) Measures¹⁷

Previous sharing. At the beginning of Stage 3, participants were asked to enter the approximate number of times they had discussed their reported experience during the 24-hour interval. Because this chapter focuses on the subset of participants who

¹⁷ There were additional measures at each stage that were not part of the primary focus of the present research and thus are not reported here. To see the full list of these measures, please refer to the [Appendix C.2](#).

engaged in social sharing, this measure served as a tool to confirm that each experience had been discussed previously.

Sharing motives. Because the Stage 3 questionnaire included a large number of measures, I decided to abridge the original SSMS to shorten the time participants needed to complete the study. Based on the results obtained from Duprez et al.'s (2015) factor analysis (p. 772-774), I chose the three items that best represent each category to create an abridged version of the scale (see [Appendix C.1](#)). However, because the original scale was developed in French, some items did not convey an accurate meaning after being translated into English (e.g., “touching him/her”). Also, some of the items under each motive category appeared to share a highly similar meaning (e.g., “be supported” and “receive support” under advice/solutions). Therefore, I avoided using these problematic items in the abridged version of SSMS. Cronbach's alpha tests showed that the internal consistency of each subscale of the resulted SSMS is as follows: CM ($\alpha = .79$, original $\alpha = .73$), R ($\alpha = .63$, original $\alpha = .70$), VE ($\alpha = .59$, original $\alpha = .73$), AEA ($\alpha = .76$, original $\alpha = .63$), IW ($\alpha = .61$, original $\alpha = .74$), HC ($\alpha = .78$, original $\alpha = .68$), and AS ($\alpha = .80$, original $\alpha = .64$). Most of the subscales have acceptable internal consistency, except for the VE one. Participants who indicated that they shared their emotional experience with someone else during the 24-hour interval were asked to rate how much they agreed with each item representing why they initiated social sharing on a scale ranging from 1 (“Not at all”) to 7 (“Very much”). Ratings for the three items under each motive category were averaged to produce a mean for comparison between emotion conditions.

Pre- vs. post-interval affect. In Stage 2 (T1), participants rated how positive and negative their feelings were during the reported experience on the same separate 6-point scales as used in the pilot study. The positive and negative ratings were then used to

compute the MIN index which reflects the intensity of bivalent emotions (for a review, see Berrios et al., 2015). In addition, they rated their felt conflict¹⁸ during the experience using a 0 (“Not at all conflicted”) to 7 (“Very conflicted”) scale (adapted from Aaker et al., 2008). All participants rated the same items after the 24-hour interval in Stage 3 (T2).

Eagerness to share. In Stage 2 (T1), all participants were asked to rate how much they wanted to discuss their reported experience with other people immediately after it happened using a 0 (“No at al”) to 5 (“Extremely”) scale. Then, in Stage 3 (T2), they used the same scale to indicate how much they wanted to discuss the experience with other people at the moment. This item was included in my secondary analyses.

4.3.2 Results

i) Allocations to Conditions

Focusing on the 136 participants who shared their experience, the *Ns* for each emotion condition were: Positive = 46, Negative = 45, Bivalent = 45¹⁹

ii) Manipulation Checks

All non-excluded participants indicated that they had discussed the event prior to Stage 3 of the study. On average, participants discussed their emotional experience 3.09 times (*SD* = 6.48 times, skewness = 10.19, median = 2 times) within the 24 hours immediately after the occurrence of the experience. A one-way ANOVA showed that

¹⁸ Participants were also asked to rate their felt tension at both timepoints (see [Appendix C.2.2](#) for details of the corresponding questions). Because of the filler question that asked for a binary response, not every participant provided a rating for this variable at T1 and T2. In fact, across both timepoints, only 109 observations contained a rating for this variable. Furthermore, using these observations, a Pearson correlation test showed that felt tension had a significant (but weak) positive relationship with felt conflict ($r(109) = .39, p < .001$). Given that felt tension was measured as a supplementary variable, I decided not to combine it with felt conflict.

¹⁹ Using the full sample ($N = 212$), a chi-square test showed that tendency to share did not depend on the kind of reported emotional experience ($p = .52$). Among the remaining 76 (38.85%) participants who did not share their experience during the 24-hour interval, the *Ns* for each emotion condition were: Positive = 20, Negative = 29, Bivalent = 27.

frequency of sharing was not significantly different between emotion conditions ($p = .32$).

Paired samples t-tests showed that at baseline (T1), participants felt significantly more positive ($M = 4.65$, $SD = 0.48$) than negative ($M = 0.087$, $SD = 0.28$) about their reported positive emotional experiences ($t(45) = 47.27$, $p < .001$, $d = 6.97$)²⁰, and significantly more negative ($M = 4.09$, $SD = 0.90$) than positive ($M = 0.36$, $SD = 0.91$) about their reported negative emotional experiences ($t(44) = -17.43$, $p < .001$, $d = -2.60$)²¹, consistent with my pilot study's results. As for bivalent emotional experiences, participants rated their positive feelings ($M = 3.56$, $SD = 1.16$) as significantly more intense than their negative feelings ($M = 2.96$, $SD = 1.15$; $t(44) = 2.19$, $p = .03$, $d = .33$)²², again replicating my pilot study's results. Furthermore, a one-way ANOVA revealed that the MIN index was significantly different between emotion conditions ($F(2, 133) = 148.07$, $p < .001$, $\eta_p^2 = .69$). Bonferroni-corrected pairwise comparisons indicated that participants who reported a bivalent emotional experience felt more ambivalent ($M = 2.40$, $SD = 0.91$) than those who reported a positive ($M = 0.09$, $SD = 0.28$; $p < .001$) or negative ($M = 0.31$, $SD = 0.76$; $p < .001$) one as expected. However, the MIN indices for the two univalent experiences were not significantly different from each other ($p = .20$). These results suggested that bivalent experiences were distinct from univalent ones in that the former elicited greater ambivalence than did the latter. In sum, I confirmed that my manipulation of emotion condition was successful.

²⁰ Independent samples t-tests showed that participants did not provide significantly different ratings at T1 for their negative affect about positive experiences that were shared from those that were not ($M = 0.10$, $SD = 0.31$; $p = .87$). However, they rated their positive affect significantly higher for shared positive experiences than for non-shared ones ($M = 4.20$, $SD = 0.70$; $t(64) = 3.05$, $p = .003$, $d = .82$).

²¹ Independent samples t-tests showed that participants did not rate shared negative experiences significantly differently from non-shared ones at T1 in terms of both positive ($M = 0.31$, $SD = 0.66$; $p = .82$) and negative ($M = 4.00$, $SD = 0.71$; $p = .65$) affect.

²² Similarly, shared bivalent experiences were not rated significantly differently from non-shared ones at T1 in terms of both positive ($M = 3.33$, $SD = 1.14$; $p = .43$) and negative ($M = 3.07$, $SD = 0.96$; $p = .65$) affect.

iii) Main Analyses

Sharing motives. I performed a series of one-way ANOVAs to test whether participants in each emotion condition rated each of the seven sharing motives differently. Results showed that ratings for all motives, except for informing and/or warning (IW; $p = .92$), depended on emotion condition. Table 6 presents the correlations between the motive variables, and Table 7 presents the means and SDs of motive ratings across emotion conditions. Mean motive ratings in all emotion conditions are also illustrated in Figure 4.

Table 6

Intercorrelations Between Motive Variables in the Abridged Version of SSMS

Motive	1	2	3	4	5	6	7
1. CM	—						
2. R	.13	—					
3. VE	.51***	.12	—				
4. AEA	.24**	.74***	.07	—			
5. IW	.27**	.33***	.12	.36***	—		
6. HC	.58***	-.02	.52***	.10	.26**	—	
7. AS	.62***	.03	.31***	.15	.24**	.66***	—

Note. CM = Clarification/Meaning; R = Rehearsing; VE = Venting; AEA = Arousing empathy/attention; IW = Informing and/or warning; HC = Assistance/Support and comfort/consolation; AS = Advice/Solutions. $N = 136$. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table 7

Means and Standard Deviations of Motive Ratings Across Emotion Conditions

Motive	Positive ^{a,b}		Negative ^{a,c}		Bivalent ^{b,c}		^a p	^b p	^c p
	$(n = 46)$		$(n = 45)$		$(n = 45)$				
	M	SD	M	SD	M	SD			
CM	2.96	1.54	4.05	1.60	3.99	1.67	**	**	1.00

R	4.88	1.10	3.28	1.28	3.30	1.24	***	***	1.00
VE	4.34	0.97	5.39	1.37	4.81	1.38	***	.19	.14
AEA	3.68	1.49	2.34	1.27	2.44	1.49	***	***	1.00
IW	4.20	1.15	4.27	1.54	4.15	1.27	—	—	—
HC	3.22	1.57	4.46	1.52	4.22	1.59	***	**	1.00
AS	3.27	1.71	4.24	1.66	4.61	1.52	*	***	.85

Note. CM = Clarification/Meaning; R = Rehearsing; VE = Venting; AEA = Arousing empathy/attention; IW = Informing and/or warning; HC = Assistance/Support and comfort/consolation; AS = Advice/Solutions. $N = 136$. * $p < .05$; ** $p < .01$; *** $p < .001$.

Rehearsing and arousing empathy/attention were rated as significantly different between emotion conditions (R: $F(2, 133) = 26.36$, $p < .001$, $\eta_p^2 = .28$; AEA: $F(2, 133) = 12.64$, $p < .001$, $\eta_p^2 = .16$). Bonferroni-corrected pairwise comparisons indicated that participants who shared a positive experience rated these two as their reasons for sharing significantly more strongly than those who shared a negative or bivalent experience ($ps < .001$). However, these two motives were not rated significantly differently between the negative and bivalent emotions ($ps = 1.00$).

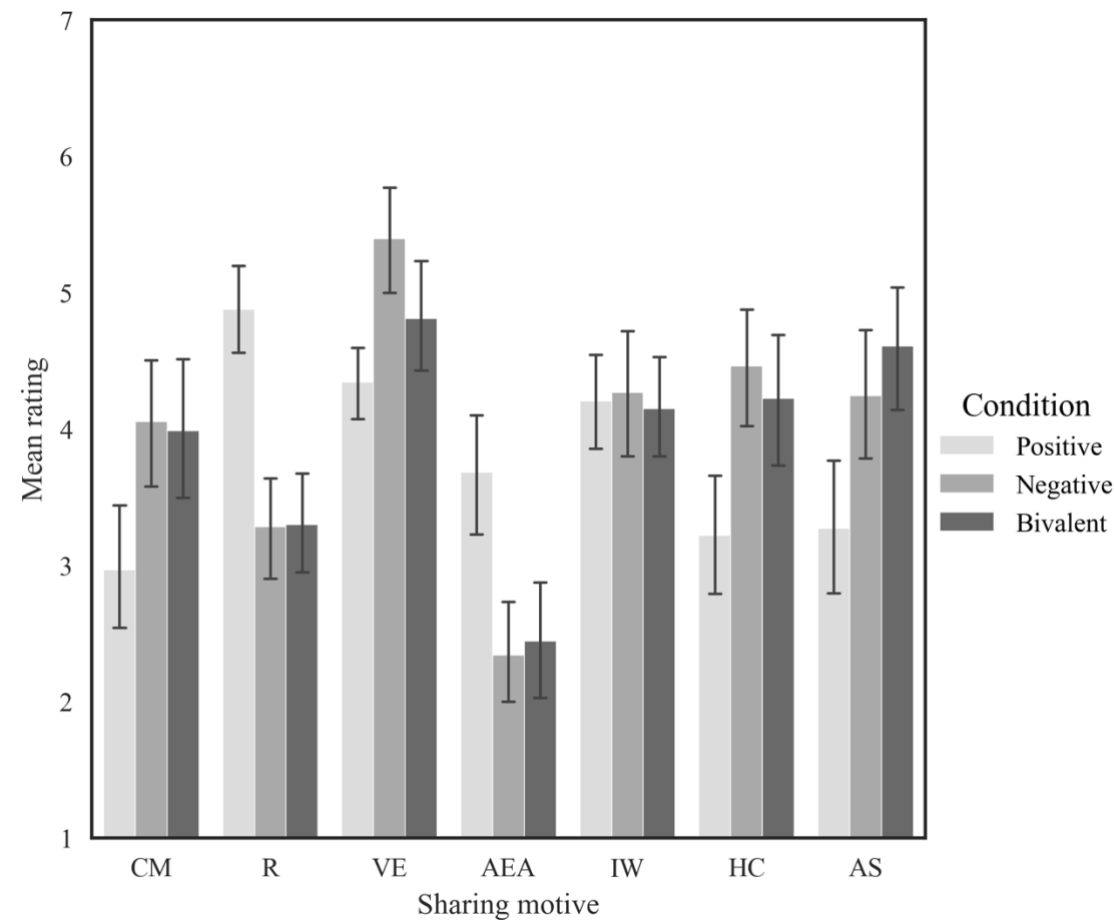
Clarification/meaning, assistance/support and comfort/consolation, and advice/solutions were also rated as significantly different between conditions (CM: $F(2, 133) = 16.98$, $p = .002$, $\eta_p^2 = .09$; HC: $F(2, 133) = 19.84$, $p < .001$, $\eta_p^2 = .11$; AS: $F(2, 133) = 21.89$, $p < .001$, $\eta_p^2 = .11$), but in the opposite direction: Participants who shared a negative or bivalent experience rated these three as their reasons for sharing significantly more strongly than those who shared a positive experience ($ps < .02$). These three motives were not rated significantly differently between the negative and bivalent emotions ($ps > .85$).

Additionally, venting (VE) was rated significantly differently between conditions ($F(2, 133) = 12.63$, $p < .001$, $\eta_p^2 = .11$). However, the difference was

significant only between the positive and negative conditions ($p < .001$), such that participants rated this motive as their reason for sharing much more strongly in the negative than in the positive condition. Participants who reported a bivalent experience did not rate this motive significantly differently compared to their counterparts in the other two conditions ($ps > .14$).

Figure 4

Illustration of Mean Motive Ratings Across Emotion Conditions



Note. CM = Clarification/Meaning; R = Rehearsing; VE = Venting; AEA = Arousing empathy/attention; IW = Informing and/or warning; HC = Assistance/Support and comfort/consolation; AS = Advice/Solutions. Error bars represent 95% confidence intervals (CI).

Pre- vs. post-interval affect. A series of two-way mixed ANOVAs was performed to examine the effects of emotion condition and time on participants’

positive affect, negative affect, MIN index, and felt conflict. Table 8 presents the means and SDs of these emotion measures at each timepoint across emotion conditions.

Table 8

Means and Standard Deviations of Positive Affect, Negative Affect, MIN Index, Felt Conflict, and Eagerness to Share at Each Timepoint Across Emotion Conditions

Variable	Positive (<i>n</i> = 46)		Negative (<i>n</i> = 45)		Bivalent (<i>n</i> = 45)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Positive affect						
T1	4.65	0.48	0.36	0.91	3.56	1.16
T2	4.52	0.62	1.07	1.30	3.16	1.38
Negative affect						
T1	0.09	0.28	4.09	0.90	2.96	1.15
T2	0.04	0.21	2.91	1.35	2.09	1.33
MIN index						
T1	0.09	0.28	0.31	0.76	2.40	0.91
T2	0.04	0.21	0.76	0.88	1.49	0.92
Felt conflict						
T1	0.37	0.97	3.42	2.33	4.29	1.74
T2	0.17	0.53	2.20	1.95	2.62	1.79
Eagerness to share						
T1	3.89	1.18	3.33	1.54	3.62	1.37
T2	2.33	1.51	1.44	1.34	1.80	1.38

Note. *N* = 136.

First, the main effect of time on participants' positive affect about their shared experience was not significant ($p = .51$). However, there was a significant main effect of emotion condition ($F(2, 133) = 230.40, p < .001, \eta_p^2 = .78$). A Tukey's HSD test

indicated that all pairwise comparisons were significant ($ps < .001$), such that participants generally rated their positive affect as the highest in the positive condition, as the second highest in the bivalent condition, and as the lowest in the negative condition. This effect was qualified by a significant interaction with time ($F(2, 133) = 13.13, p < .001, \eta_p^2 = .17$): Bonferroni-corrected pairwise comparisons showed that participants' positive ratings increased significantly over time only when the shared experience was negative ($p = .002$). The ratings did not change when the shared experience was positive ($p = .67$) or bivalent ($p = .06$).

As for negative ratings, there was a significant main effect of time ($F(1, 133) = 60.13, p < .001, \eta_p^2 = .31$), meaning that participants generally tended to feel less negative over time. There was also a significant main effect of emotion condition ($F(2, 133) = 206.99, p < .001, \eta_p^2 = .76$), but this time in the reverse direction: Participants generally rated their negative affect as the highest in the negative condition, as the second highest in the bivalent condition, and as the lowest in the positive condition. Again, all pairwise comparisons were significant ($ps < .001$). These two main effects were qualified by a significant two-way interaction ($F(2, 133) = 14.30, p < .001, \eta_p^2 = .18$), such that negative ratings decreased significantly over time only when the shared experience was bivalent or negative ($ps < .001$). The ratings did not change over time when the shared experience was positive ($p = 1.00$), probably because of a floor effect.

Turning to the MIN index, I found a significant main effect of time on the MIN index ($F(1, 133) = 4.64, p = .03, \eta_p^2 = .03$), such that it generally tended to decrease over time. Similar to negative affect, there was a significant main effect of emotion condition ($F(2, 133) = 138.60, p < .001, \eta_p^2 = .68$), such that the MIN index was the highest for bivalent experiences, intermediate for negative ones, and the lowest for positive ones. All pairwise comparisons were significant ($ps < .001$). These two main

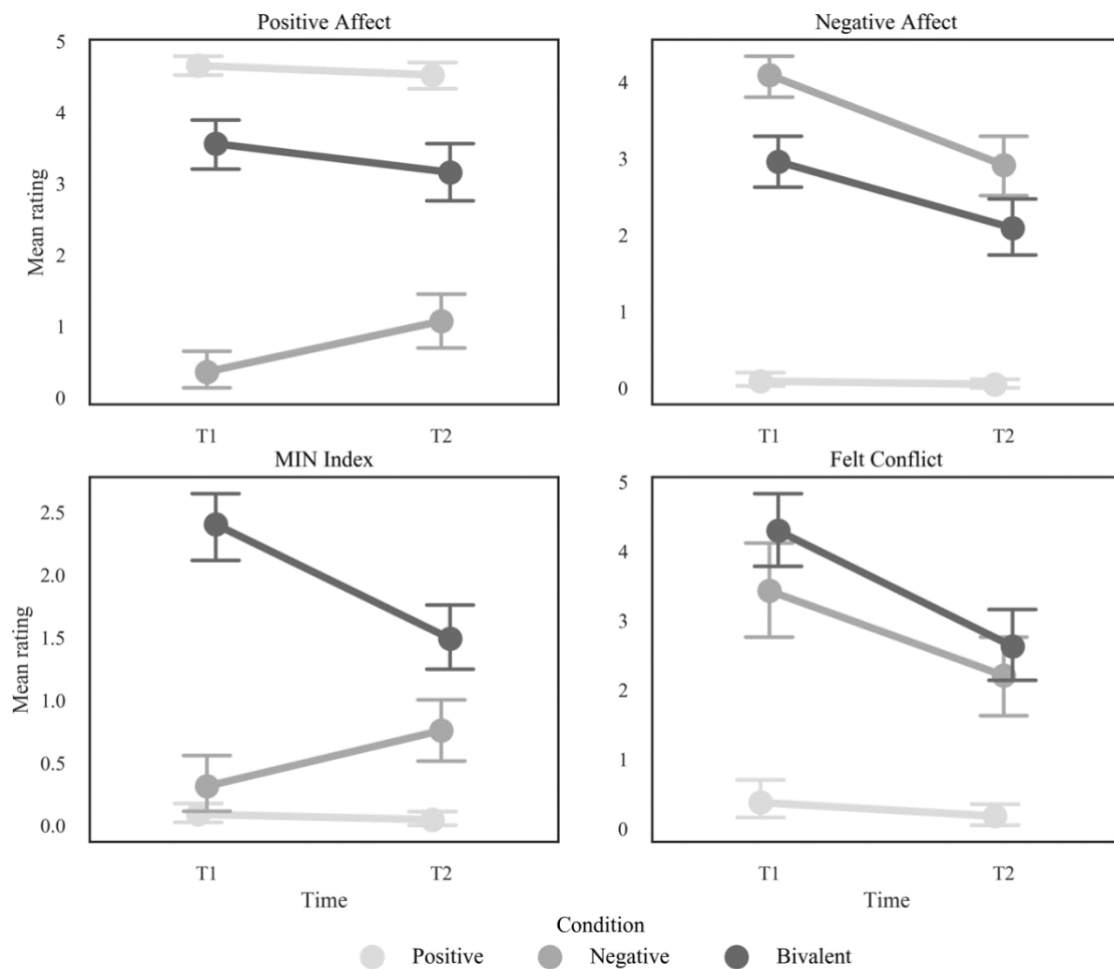
effects were qualified by a significant two-way interaction ($F(2, 133) = 25.05, p < .001, \eta_p^2 = .27$), such that the MIN index changed significantly over time only when the shared experience was either bivalent or negative. Specifically, the MIN index decreased for bivalent experiences ($p < .001$) but increased for negative ones ($p = .03$). On the other hand, the MIN index for positive experiences did not change significantly over time ($p = 1.00$), again probably due to a floor effect.

Results for the felt conflict measure²³ were similar to those for the negative affect measure. There was a significant main effect of time ($F(1, 133) = 60.31, p < .001, \eta_p^2 = .31$), suggesting that felt conflict generally tended to decrease over time. There was also a significant main effect of emotion condition ($F(2, 133) = 59.63, p < .001, \eta_p^2 = .47$), indicating that felt conflict was the highest for bivalent experiences, intermediate for negative ones, and the lowest for positive ones. All pairwise comparisons were significant ($ps < .04$). More importantly, there was a significant interaction between time and emotion condition ($F(2, 133) = 10.89, p < .001, \eta_p^2 = .14$), such that felt conflict only decreased for negative and bivalent experiences ($ps < .001$) but did not change significantly for positive experiences ($p = .69$). The two-way interactions for all four emotion measures are illustrated in Figure 5.

²³ A one-way ANOVA showed that felt conflict at T1 was significantly different between emotion conditions ($F(2, 133) = 193.41, p < .001, \eta_p^2 = .48$). Bonferroni-corrected pairwise comparisons showed that participants in the positive condition ($M = 0.37, SD = 0.97$) felt less conflicted than those in the negative ($M = 3.42, SD = 2.33$) and bivalent ($M = 4.29, SD = 1.74$) conditions ($ps < .001$). Interestingly, felt conflict for negative experiences was not significantly different from that for bivalent ones ($p = .15$).

Figure 5

Temporal Change in Mean Ratings for Different Emotion Measures Across Emotion Conditions



Note. Error bars represent 95% confidence intervals (CI).

iv) Secondary Analyses

Shared vs. non-shared events. Although the group of participants who did not share their experience and the group of those who shared were of unequal sizes, it was still worth performing three-way mixed ANOVAs using Sharing as the third IV to see if my results reported above were different between shared and non-shared events.

For positive affect, all significant effects were the same as reported above in the main analyses. The only additional finding is that there was a significant main effect of sharing ($F(1, 206) = 4.07, p = .05, \eta_p^2 = .02$), such that participants rated their positive affect generally higher for their shared events ($M = 2.90, SD = 1.93$) than their non-

shared ones ($M = 2.47$, $SD = 1.70$). The three-way interaction was not significant ($p = .26$).

For negative affect, the MIN index, and felt conflict, again, all significant effects were the same as reported above in the main analyses. There were no additional findings. Also, none of the three-way interactions was significant ($ps > .07$).

Eagerness to share. Among participants who discussed their experience during the 24-hour interval, eagerness to share was rated significantly differently across emotion conditions ($F(2, 133) = 4.57$, $p = .01$, $\eta_p^2 = .06$). Bonferroni-corrected pairwise comparisons indicated that participants in the positive condition were more eager to talk about their experience with other people than were those in the negative ($p = .01$) condition. There was no significant difference in eagerness to share between the positive and bivalent conditions ($p = .29$) or between the negative and bivalent conditions ($p = .60$). However, a significant main effect of time ($F(1, 133) = 165.11$, $p < .001$, $\eta_p^2 = .55$) suggested an overall decrease in eagerness to share from T1 to T2. There was no significant interaction between time and emotion condition ($p = .59$). The means and *SDs* of eagerness to share at each timepoint across emotion conditions are reported in Table 8.

4.3.3 Discussion

The present research implemented Duprez et al.'s (2015) SSMS in a prospective study designed to assess people's reasons for sharing different kinds of emotions: Positive, negative, and bivalent. The study also examined and compared the affective outcomes of social sharing between these different emotions.

Results showed that participants who discussed their positive experience were more likely to be motivated by a desire to revive such an event and let listeners share their pleasantness than those who discussed their negative experience, in line with

Duprez et al.'s (2015) original findings and findings from studies of capitalisation (Gable et al., 2004; Gable & Reis, 2010). On the other hand, participants who discussed their negative experience were more likely to be motivated by a desire to vent, find meaning, and seek distress-buffering support—both emotional and cognitive—from listeners than those who discussed their positive experience, again consistent with previous findings (Duprez et al., 2015; Harlow & Cantor, 1995).

Finding meaning and seeking emotional and cognitive support were also rated as stronger reasons for sharing bivalent emotions than positive emotions, supporting the theory that people experiencing bivalent emotions are motivated to look for clarity and new insights that help them reappraise the situation (e.g., Rothman & Melwanie, 2017; Williams & Aaker, 2002). Of course, future research is needed to clarify whether sharing motives depend specifically on the conflicting nature of bivalent emotions or simply reflect the fact that these emotions have a negative aspect which is what sharers mostly want to cope with. However, contrary to expectation, participants in the bivalent condition, who were supposed to be especially receptive to alternative perspectives (Rees et al., 2013) and thus be even more eager to seek advice and suggestions from other people on forming clearer action tendencies, did not rate the motive of AS significantly differently from those in the negative condition. Prior research on bivalent emotions has often compared different characteristics (e.g., Williams & Aaker, 2002, on felt conflict; Rees et al., 2013, on receptivity to other people's opinions) across only two unmixed opposite-valenced emotions (usually happiness and sadness). In the present study, participants in each condition instead reported on their experiences of two mixed univalent or bivalent emotions. Therefore, one explanation for the unexpected finding is that participants in the negative condition who were asked to report an experience involving multiple negative emotions also experienced dissonance, which heightened

their receptivity to and desire for outsiders' perspectives as in the bivalent condition during social sharing. Indeed, participants in both conditions reported similarly high levels of felt conflict about their experience at baseline. In his review, Roseman (2013) argued that each emotion has its own distinct appraisal pattern and associated action tendency (see also Frijda et al., 1989). For example, people may get angry at their friend who they perceive as having wronged them, then say or do something in retaliation that they regret, thereby producing a mixture of these two negative emotions. In this scenario, people may experience internal inconsistencies because on one hand, anger directs blame and aggression at the other person (Smith & Kirby, 2000; Wu & Wang, 2017) and on the other hand, regret directs blame at oneself and prompts corrective actions (Wetzer et al., 2007; Zeelenberg & Breugelmans, 2008). Even when the different negative emotions involved in a reported experience share more compatible appraisals and action tendencies, certain emotions may still elicit an especially strong desire for cognitive support. For example, Pauw et al. (2017) found that angry people prefer validation to advice and reappraisal whereas regretful people do not show this preference. Future research can experimentally manipulate the kinds of emotions people experience to examine whether emotions of the same valence with conflicting appraisals or support preferences are associated with greater felt conflict and different sharing motives from those with more matching appraisals.

Because bivalent emotions give rise to additional feelings of discomfort (Williams & Aaker, 2002), I hypothesised that people in this emotional state would have an urge to vent and rate this motive as equally important as those in a negative state did. However, this was not the case. In fact, based on participants' affective ratings at T1, negative experiences induced conflict as much as bivalent ones did, indicating that previous researchers' evidence that felt conflict uniquely accompanying opposite-

valence emotions (e.g., Thompson et al., 1995; Williams & Aaker, 2002) is not unequivocal, as same-valence emotions with opposing appraisals may also elicit dissonance as discussed earlier. Thus, the difference in the need for venting between these two kinds of emotions may simply be attributed to their different levels of negativity. Negative experiences were evaluated as predominantly negative by participants at T1, while bivalent ones were relatively more ambivalent, with their positivity outweighing their negativity. Given that the motive of venting has been suggested to be associated with negative emotional episodes (Duprez et al., 2015), it makes sense that this motive is not rated as a very important motive for bivalent episodes which are perceived as relatively more positive than negative. In future research, it would be worth examining whether type of sharing motive moderates effects of sharing for each kind of emotional experience. I did not run this analysis because there were too many sharing motives in the present research which I would need to treat each of them separately rather than looking at the overall pattern. One way to address this concern is to further simplify Duprez et al.'s (2015) SSMS by grouping similar motives together, as in Bucich and MacCann's (2019) work which proposed three motives: Recovery (i.e., gaining new perspectives or advice), relief (i.e., venting or arousing empathy), and bonding (i.e., strengthening social bonds).

There were a few unexpected observations too when I was investigating the affective outcomes of sharing different kinds of emotions. First, I predicted an increase in positive affect in the positive condition due to capitalisation (Gable et al., 2004; Gable & Reis, 2010). Such an increase was not observed, mostly because the positive ratings at baseline were already close to the maximum value on the scale, leading to a potential ceiling effect. However, I found that positive affect decreased for non-shared events in the positive condition. This suggests that participants might not be able to

enjoy the pleasantness of an event to the full without sharing it, serving as indirect evidence that sharing facilitates capitalisation. Second, I found a decrease in negative affect and felt conflict in the bivalent condition as expected but did not observe any change in positive affect. The MIN index of this emotion type was also found to decrease over time as expected, reflecting a reduction in ambivalence as the negative elements of a shared bivalent experience lost their weight. This suggests that people tend to resolve internal inconsistencies by attenuating their negative evaluations of the event rather than amplifying their positive evaluations, even though the positivity of their bivalent experience tends to be greater than its negativity. Prior research has suggested several approaches people may take to simplify their complex states (for a review, see Rothman et al., 2017). For example, accentuating either their positive or negative evaluations (Pratt & Doucet, 2000). This approach seems to be the one that participants in my study took when coping with their bivalent emotions. Qualitative studies should be conducted in future to analyse the content of social sharing of bivalent emotions in order to directly examine how different aspects of this kind of experience are prioritised during verbal exchange and how exactly people reach a simplified view of their shared experience.

On the other hand, affective outcomes of social sharing in the negative condition completely matched my predictions, such that participants in this condition showed a decrease in their negative affect and felt conflict (similar to that shown by those in the bivalent condition) and an increase in their positive affect at T2. Notably, these participants were the only ones who displayed an increase in positive affect after the 24-hour interval, suggesting that compared to sharing in the bivalent condition, sharing in the negative condition might be more effective in boosting positive affect. This finding, however, may be subject to regression towards the mean (e.g., Barnett et al., 2005),

given that reported positive affect at T1 was the lowest for negative experiences. However, this explanation is undermined by the fact that there was no change in reported positive affect for positive experiences even though the corresponding ratings were the highest (hence also extreme) at T1. Because regression towards the mean not happening in one emotion condition does not guarantee that it does not happen in another, I propose another plausible explanation for the increased positive affect observed in the negative but not bivalent condition: While people experiencing bivalent emotions probably aim to cope with their negative evaluations of the event, those experiencing negative emotions probably aim to not only relieve their negative feelings but also make themselves feel better. These different goals may in turn determine the specific type of coping strategy that people seek during social sharing. For example, people sharing a bivalent experience may express the need for a more objective way to form a more balanced view of what happened (i.e., reappraising by perspective taking) whereas those sharing a negative experience may ask for a more positive way to reinterpret things (i.e., reappraising the emotional stimulus; Webb et al., 2012). It may be worth investigating further whether cognitive support, though equally wanted in both conditions, is actually more preferable in certain forms across emotions.

As discussed above, some of the unexpected findings observed in the negative condition may be explained by the conflicting nature of the specific multiple emotions that were reported by participants. Therefore, a major limitation of the present research is that I controlled the number of emotions involved in each emotion condition but not the appraisals associated with each of the reported emotions, which makes it difficult to directly compare and interpret results obtained from the negative and bivalent conditions. Another limitation is that the present research did not directly manipulate the occurrence of social sharing, so it is difficult for me to conclude that the observed

affective outcomes were due to sharing. Although my secondary analyses suggested that the emotional effects over time did not differ between shared and non-shared events, there is a caveat to the conclusion that these effects were attributed to the simple passage of time instead of sharing too because of the unequal cell sizes for these two sharing conditions. The smaller size of the non-sharing group probably led to loss of power of the comparison between the two sharing groups, causing the interactions between sharing and time and between sharing, time, and emotion condition in my secondary analyses to be not significant. My rationale for not manipulating sharing in the first place was to maintain the naturalistic nature of the present research as much as possible and to have this chapter maintain its focus on how different the sharing motives and outcomes are between emotion kinds. Also, there are ethical issues associated with manipulating whether participants share their real-life emotional experiences. Prior research has documented a high prevalence of social sharing (80-95%; for a review, see Rimé, 2009), so many participants who were originally inclined to share would probably have needed to suppress that urge if they were assigned to the no-sharing condition. On the other hand, a smaller number of participants might initially have preferred to keep their emotions secret but would have been forced to disclose them if they were assigned to the sharing condition. Finally, my research only looked into what happened relatively soon after an emotional experience has taken place, so the results regarding motives for and outcomes of sharing may no longer apply after a longer time has passed.

Still, I believe that the present research has important theoretical implications: Broadly, it helps fill the gap in emotion research by presenting novel knowledge related to bivalent emotions, which are a relatively underexplored topic. More specifically, it enriches the social sharing literature by showing that people communicate not only their

univalent emotions (Rimé et al., 1998) but also their bivalent emotions after experiencing them and by shedding light on the causes underlying sharing of bivalent emotions and how different the affective outcomes are between sharing of univalent and bivalent emotions. Also, the present research adds new insights into the emotion regulation literature in terms of what people experiencing different kinds of emotions want to achieve via social sharing. More specifically, my results imply that the primary goal of people experiencing bivalent emotions is to resolve internal inconsistencies by focusing on their negative evaluations and alleviating their impact and that the goal of people experiencing negative emotions is to make themselves feel not only less bad but also better. Therefore, although both bivalent and negative emotions are associated with a desire for cognitive support, different coping strategies may be needed to optimise the effect of social sharing in real life. Finally, given that I successfully replicated Duprez et al.'s (2015) findings respectively for positive and negative emotions using the abridged version of their SSMS, the reliability of this scale for assessing sharing motives is supported.

In sum, I used prospective questionnaires to assess the motives for sharing bivalent emotions in daily life and compare these motives to those for sharing univalent emotions. This work is the first to explore bivalent emotions in an interpersonal context. It contributes to the social sharing and emotion regulation literature by showing that people share a bivalent experience for similar reasons (finding meaning, seeking emotional and cognitive support) to those for sharing an upsetting experience that involves multiple negative emotions. Also, my results suggest that different affective outcomes seem to accompany sharing of different kinds of emotions. Specifically, discussing a positive experience maintains the high levels of positive affect and low levels of negative affect, discussing a negative experience reduces negative affect and

increases positive affect, and discussing a bivalent experience alleviates negative affect but has no impact on positive affect. In the next chapter, I will present Project 4 wherein I explored the cognitive effects of bivalent emotions by attempting to use these emotions as a tool to improve intergroup relations.

CHAPTER FIVE

Project 4: Effects of Perceiving Extreme Outgroup Members' Bivalent Emotions on Dehumanisation

5.1 Abstract

People tend to consider groups with which they do not identify as inferior to their own group, often resulting in prejudice against these outgroups. One form of intergroup prejudice is denying outgroup members' ability to experience uniquely human (UH) emotions, although there have been critiques that people do not actually perceive those members as less human but simply prefer their ingroup to outgroups. Given evidence that bivalent emotional experiences require cognitive complexity and are positively evaluated, I tested the effectiveness of a recently proposed strategy of presenting outgroup members' bivalent emotions in reducing dehumanisation (or social preference). I also compared the effects between an unfamiliar and a heavily stigmatised outgroup. Results showed that the strategy had no effects on intergroup relations and that unfamiliar outgroups are not evaluated as any different from average humans. However, this work shed light on a new perspective on explaining hostility towards heavily stigmatised outgroups: Socially undesired emotions, are ascribed more strongly to these groups than socially desired ones, especially when these emotions are non-UH, implying the operation of both social preference and dehumanisation.

During the Holocaust, Nazi propaganda likened the Jews to rats. Towards the end of the 20th century, Hutus in Rwanda started genocidal killings of Tutsis, whom they called cockroaches. Whenever members of one social group are considered “less than human” by members of another social group, dehumanisation is at work. This phenomenon is still found in today’s society. For example, there is neural evidence that the medial prefrontal cortex (mPFC), a brain region that is normally recruited when viewing humans, is not activated when viewing images of individuals suffering from drug addiction or homelessness (for a review, see Harris & Fiske, 2009).

According to Tajfel and Turner’s (1979) social identity theory, people have a natural tendency to put themselves and other individuals into different groups (social categorisation). By grouping individuals with similar features together and separating individuals with different features from each other, people can make sense of the social environment more easily. They can categorise themselves as part of a certain group and identify with that group (ingroup) to gain a sense of belonging to the social world (social identification). The ingroup then naturally has a need to compare favourably with another group with which they do not identify with (outgroup) to maintain their own self-esteem and positive distinctiveness (social comparison). In order to fulfil this need, the ingroup ascribes better qualities more strongly to themselves than to the outgroup. For example, the ingroup tends to reserve humanity, a fundamental essence, more for themselves than other groups. When humanness is fully denied to the outgroup, the outgroup members are seen as non-human creatures (Haslam, 2006; Smith, 2011). Dehumanisation can also operate in less blatant forms, involving the ingroup seeing outgroup members as *somewhat* less human by believing that they possess mental states (Harris & Fiske, 2006), UH character traits (dual model; Haslam, 2006), or UH emotions (infracommunication; Leyens et al., 2000, 2001) to a lesser extent.

The present study particularly focused on *infrahumanisation*, which has been shown to reliably co-occur with blatant dehumanisation (Kteily et al., 2015). Furthermore, it has been widely investigated in recent research (for a review, see Vaes et al., 2012) and has been linked to reduced prosocial behaviour towards the outgroup (Cuddy et al., 2007; Vaes et al., 2003).

UH emotions, also known as secondary emotions, are one of the most prominent characteristics of the human essence (Leyens et al., 2000). This kind of emotions (e.g., shame, remorse) has been suggested to be cognitively complex because it requires a view of the self, the past, or the future. On the contrary, non-UH emotions, or primary emotions (e.g., anger, happiness), are simply reactions to immediate stimuli in the environment and thus are experienced by both humans and other animals (for a review, see Demoulin et al., 2004; Ekman, 1992). In their research, Leyens et al. (2001) found that the ingroup attributes a higher capacity to feel UH emotions, both positive and negative, to themselves than to the outgroup. This finding has been replicated in subsequent research across explicit and implicit measures (Boccatto et al., 2007; Paladino et al., 2002), and across intergroup contexts, such as different religious and racial groups (Banton et al., 2020) and even minimal groups (Simon & Gutsell, 2020).

Besides secondary emotions, there is evidence suggesting that feeling bivalent emotions (Larsen et al., 2001) also requires advanced cognitive capacities—particularly, the capabilities needed for evaluating the same event from different perspectives and integrating conflicting feelings (Labouvie-Vief, 2015)—and thus constitutes another form of uniquely human emotional experience. Indeed, Larsen et al. (2007) observed that as with secondary emotions, children develop a better conceptual understanding of bivalent emotions and are more likely to actually experience them as they grow older. In addition, bivalent emotions have been linked to “wise reasoning,” which involves a

number of beneficial cognitive processes, such as acknowledgement of the possibility of multiple outcomes and making less biased judgments (Grossmann et al., 2019). More relevant to the present research topic, these authors observed that individuals who report more bivalent emotions are more likely to be perceived as “wise” by their peers, compared to those who report less.

If cognitive complexity, and even human uniqueness, can be inferred from the ability to feel bivalent emotions, then showing that outgroup members possess this ability may help boost their human status, improving general attitudes towards them. Prati and Giner-Sorolla (2018) tested this hypothesis and found generally consistent evidence across studies in support of it. In particular, participants attributed more UH emotions, showed less prejudice, and were more willing to increase social proximity to outgroup members described as experiencing bivalent emotions (e.g., anger and pleasure, fear and happiness) than to those described as experiencing univalent emotions (e.g., happiness and pleasure, fear and anger). These authors purposefully used non-UH emotions to create their bivalent emotional pairs because, if participants were told that the outgroup members experienced UH emotions, the authors would not be able to test their hypothesis by asking participants to rate the extent to which the outgroup members could feel UH emotions. More notably, expressions of UH emotions from outgroups are typically not believed or recognised and sometimes even backfire by eliciting more negative reactions (for a review, Vaes et al., 2012; Wohl et al., 2012).

In recent years, more and more researchers have raised questions about the explanatory power of the construct of dehumanisation. As Manne (2016) mentioned in her critique, outgroup members can be brutally mistreated while still being perceived as humans. For example, the terms that appeared in Nazi anti-Jewish propaganda (e.g., corrupt, treacherous) make sense only when applied to humans but not animals or

machines. Therefore, people do not have to lose their sense of shared humanity to see outgroup members as inferior and inflict harm on them. Relatedly, Over (2020, 2021) argues that it is actually possible for some uniquely human qualities to be attributed more strongly to the outgroup than the ingroup. Her hypothesis is supported by Enock et al.'s (2021a, 2021b) experimental research. In their (2021b) study, these authors discovered that while prosocial emotions are indeed ascribed to the outgroup less strongly than to the ingroup, antisocial emotions—even UH ones—are ascribed to them more strongly. This effect was consistently found across seven different intergroup contexts (e.g., criminals vs. non-criminals, minimal outgroup vs. minimal ingroup) and is probably better explained by a social preference account, namely people's tendency to favour their own group over the outgroup (Tajfel & Turner, 1979; Taylor & Doria, 1981).

The key distinction between infrahumanisation theory and the social preference account lies in the former's exclusive emphasis on the humanness of attributed emotions: As long as an emotion is classified as UH, it should be associated more with the ingroup than with the outgroup, even when the emotion is unpleasant to experience. Although prior research has consistently supported this prediction (e.g., Cortes et al., 2005; Leyens et al., 2001), it has omitted the fact that emotions are not only individual but also social experiences (Parkinson, 1996). That is, besides valence (positive vs. negative), emotions can also vary in terms of their prosociality (prosocial vs. antisocial). Some emotions can be pleasant to experience but are not kind to other people (e.g., *schadenfreude*), and some can be unpleasant to experience but are not unkind to other people (e.g., guilt). Enock et al.'s (2021b) work was the first to address this issue and discovered that intergroup ascriptions of emotions are qualified by their prosociality instead of humanness, thereby supporting the social preference account and not

infracommunication theory. With this in mind, it seems important to revisit Prati and Giner-Sorolla's (2018) work and re-examine the reported effect of their manipulation. Specifically, does presenting bivalent emotions really reduce infracommunication or simply social preference?

Another objective of the present study was to compare any effects of the bivalent-emotion manipulation on participants' perceptions of a *normal* outgroup and an *extreme* outgroup. In the context of the present study, I defined a normal outgroup as a group which UK participants generally do not identify with but do not express hostility towards. On the other hand, an extreme outgroup is a group which, according to the stereotype content model (SCM; Fiske et al., 2002), is typically perceived as low on both warmth and competence (i.e., the "lowest of the low"), thereby eliciting disgust. When an outgroup threatens one's worldview, is disliked, and is alienated, it is particularly likely to be infracommunicated (Leyens, 2009). Additionally, disgust is an emotion that has been associated with moral violations and aggression from the perceiver (Haidt et al., 1997). Taken together, infracommunication (or social preference) should be stronger for extreme than normal outgroups. Prati and Giner-Sorolla (2018) tested their manipulation on three kinds of outgroups and found a significant effect on emotion ascriptions for a fictitious species (aliens) and an unfamiliar group (Uzbeks) but not for a familiar but disliked group (Muslims to Italian participants), which implied that presenting bivalent emotions may be less effective when infracommunication (or social preference) is stronger. Therefore, I included a normal and an extreme outgroup to directly assess whether the previous authors' reported effects extend to the latter. If they do, then my findings suggest that providing information about outgroup members' emotional complexity represents a useful strategy for improving intergroup relations even when they are at their worst.

In sum, the present research re-examined the effects of perceiving outgroup members' bivalent emotions to assess: 1) Whether this manipulation improved intergroup attitudes as it did in Prati and Giner-Sorolla's (2018) study, 2) whether such improvement was better explained by reduced infrahumanisation or reduced social preference, and 3) whether such improvement was observed for both a normal and an extreme outgroup.

5.2 Pilot Study

A classic example of a “lowest of the low” extreme outgroup is drug addicts (Harris & Fiske, 2006), who are often seen as not deserving social support or empathy (Corrigan et al., 2009). Harris and Fiske (2009) even showed that when viewing images of these individuals, people's brain (mPFC) does not recognise them as humans. Sumnall et al. (2021) also reported that blatant dehumanisation and infrahumanisation of drug addicts, especially those who use heroin, are relatively greater than for other potentially stigmatised groups. All this evidence supported my decision to use drug addicts as the target extreme outgroup.

As for my target normal outgroup, Uzbeks, who were previously used in Prati and Giner-Sorolla's (2018) Experiment 2, seemed a good choice because they are a little-known population. Using the same outgroup as Prati and Giner Sorolla (2018) also allowed me to establish a direct connection to their work and facilitated a more direct comparison between my and their results, given that these authors reported significant improvement in intergroup relations following the manipulation when this group was the target.

To confirm that my chosen extreme outgroup was more stigmatised than my chosen normal outgroup by UK participants, I conducted this pilot study to measure

participants' baseline perceived warmth and competence of these two groups. I expected drug addicts to be rated lower than Uzbeks on both dimensions, as members of an extreme group are supposed to be.

5.2.1 Methods

i) Participants

To detect a medium-sized effect ($d = .50$) with 80% power and 5% alpha using a paired-sample two-tailed t-test, G*Power (version 3.1; Faul et al., 2009) recommended that a sample size of 34 was needed. I therefore recruited 34 participants (16 female, 17 male, 1 stated 'other'; $M_{age} = 40.71$, $SD_{age} = 16.92$) via the Prolific platform (Peer et al., 2017) on 7th April 2022. All participants were adults, UK nationals, and able to read and write English proficiently.

ii) Procedure

Once informed consent was obtained, participants were asked to rate their knowledge of and views on different groups of individuals. Besides Uzbeks and drug addicts, I presented participants with four other groups as alternative choices in case my expectations for the two groups of my primary interest were violated: Two of the additional groups are often evaluated unfavourably by the general population (anti-vaxxers and climate change deniers) and two are ethnic groups which I believe are relatively unfamiliar to my participant population (Luxembourgers and Sammarinese). After completing all question blocks, participants were debriefed and thanked.

iii) Measures

Participants were first asked to indicate on a slider how much knowledge they already possessed about each of the presented groups from 0 ("No knowledge at all") to 100 ("A great deal of knowledge"). Then, they rated how warm they thought members of each group were on a 7-point Likert scale ranging from 1 ("Not friendly at

all/Extremely hostile”) to 7 (“Extremely friendly/Not hostile at all”). Based on these responses, participants were instructed to rate how competent they thought members of each group were in relation to their ability to provide help or inflict harm on a 7-point Likert scale (0 = “Extremely incompetent”, 7 = “Extremely competent”).

Given the positive correlation between infrahumanisation and blatant dehumanisation, I also included the corresponding scale used in previous work (Enock et al., 2021b; Kteily et al., 2015) to measure participants’ blatant dehumanisation of each presented group. Specifically, participants saw the “ascent of man” image and were asked to indicate on a slider how evolved they perceived a typical member of each group was from 0 (“Least evolved”) to 100 (“Most evolved”).

5.2.2 Results and Discussion

The means and *SDs* of all following measures for all six outgroups are presented in [Appendix D.1](#). Paired-sample *t*-tests revealed that participants possessed significantly less knowledge of Uzbeks than of drug addicts ($t(33) = -7.90, p < .001, d = 1.86$). However, Uzbeks were rated as significantly warmer ($t(33) = 7.04, p < .001, d = 1.79$) and more competent ($t(33) = 2.16, p = .04, d = .57$) than drug addicts. These results confirmed my expectation that drug addicts are more of an extreme outgroup than are Uzbeks. Specifically, although Uzbeks were a more unfamiliar population than were drug addicts, the latter were evaluated as much less warm and competent than the former²⁴.

Finally, one participant who gave an extremely low rating when assessing their blatant dehumanisation of drug addicts was considered an outlier ($< 3 SD$ from the mean) and thus excluded from the analysis of this item. With a corrected sample size of

²⁴ Since there was evidence that the two outgroups of my primary interest, Uzbeks and drug addicts, were viewed drastically different in that the latter elicited more negative attitudes despite being more known compared to the former, I decided to use them as my target normal and extreme outgroups respectively without further analysing data related to the other presented groups.

33, I unexpectedly found no difference in blatant dehumanisation between the two groups ($p = .31$), meaning that drug addicts were not more dehumanised than Uzbeks. Perhaps because the blatant dehumanisation scale is a very explicit measure, participants provided similar ratings for the two groups under the influence of social desirability pressures (Gawronski & De Houwer, 2014). However, an advantage arising from this finding is that any differences in the effects of bivalent-emotion manipulation on dehumanisation reported in the main study will not be seen as a result of initial differences in the extent of blatant dehumanisation.

5.3 Study 4

As recently shown in Enock et al.'s (2021b) work, differences in emotion ascriptions between ingroup and outgroup members seem to reflect social preference rather than infrahumanisation. I followed their procedure to develop a new version of the emotion ascription task that included terms that varied on both humanness and prosociality. If presenting bivalent emotions promotes higher ascriptions of prosocial emotions and lower ascriptions of antisocial ones to the outgroup (regardless of humanness) than when presenting univalent emotions, then there is evidence that the manipulation improves intergroup relations by attenuating social preference, adding support to Enock et al.'s (2021b) critique. In contrast, if the manipulation promotes higher ascriptions of UH emotions to the outgroup (regardless of prosociality), then Prati and Giner-Sorolla's (2018) finding is replicated, upholding infrahumanisation theory (Leyens et al., 2001). Finally, because extreme outgroups are more stigmatised than normal outgroups, I hypothesised that the effects of presenting bivalent emotions were weaker for drug addicts than for Uzbeks.

5.3.1 Methods

i) Participants

The effect size used by Prati and Giner-Sorolla's (2018) study in Experiment 2 which had Uzbeks as the target outgroup to determine the sample size for a two-group comparison was medium-to-large ($d = .64$). Therefore, to detect a medium-sized effect ($f^2 = .15$) in the present study with 80% power and 5% alpha using a linear multiple regression (fixed model, R^2 deviation from zero) containing four predictors, G*Power (version 3.1; Faul et al., 2009) recommended that a minimum sample size of 85 was required.

I recruited 88 participants (54 female, 34 male; $M_{\text{age}} = 39.06$, $SD_{\text{age}} = 14.29$) via the Prolific platform (Peer et al., 2017) between 12th and 26th May 2022. As in the pilot study, all participants were adults, UK nationals, and able to read and write English proficiently.

ii) Design

This study had a 2 x 2 x 2 x 2 mixed design with Emotion (bivalent vs. univalent) and Group (normal vs. extreme outgroup) as between-subjects factors and Humanness (UH vs. non-UH terms) and Prosociality (prosocial vs. antisocial terms) as within-subjects factors. The key dependent variable was emotional attribution.

iii) Stimuli Development

Based on the subjective ratings collected by Enock et al. (2021b), I chose 16 emotions that best fit the four emotion categories of interest: UH/prosocial, UH/antisocial, non-UH/prosocial, and non-UH/antisocial (see Table 9). Humanness ratings for emotions were as closely matched as possible between the prosocial and antisocial conditions at each level of humanness.

Among the selected UH emotions, nostalgia (85.1) scored the highest in terms of humanness on a scale of 0-100 whereas greed (74.6) scored the lowest. Among the selected non-UH emotions, jealousy (64.7) scored the highest in terms of humanness whereas hostility (53.6) scored the lowest. It should be noted that in Enock et al.'s (2021b) dataset, the lowest humanness rating collected was 48.6. In other words, all of their 54 emotions were at least perceived as being applied equally to humans and other species. On the other hand, among the prosocial emotions that I selected, empathy (86.3) scored the highest in terms of prosociality whereas nostalgia (64.6) scored the lowest. Among the selected antisocial emotions, irritation (29.1) scored the highest in terms of prosociality whereas arrogance (20.9) scored the lowest.

The emotions that I selected on the basis of this procedure were not exactly the same as those selected by Enock et al. (2021b) because some of their selected emotions (e.g., contempt, disgust) could be replaced by other emotions with more closely matched humanness and prosociality ratings. Also, some of their selected emotions were reserved for creating emotion pairs in the excerpts that I used for manipulating perceived outgroup emotions. It should be reiterated that as in Prati and Giner-Sorolla's (2018) study, I purposefully avoided using UH (secondary) emotions to imply outgroups' cognitive complexity and even human uniqueness; instead, I selected two non-UH (primary) positive emotions (happiness, tenderness) and two non-UH negative emotions (fear, sadness) to form bivalent emotional pairs used in the manipulation. Therefore, UH emotions were only used in the emotion ascription task. Participants' ratings on these emotions indicated to what extent, after being exposed to the manipulation, they believed outgroup members possessed the ability to experience complex emotions.

Table 9*Emotion Terms Included in the Emotion Ascription Task*

	Prosocial	Antisocial
UH	Nostalgia	Arrogance
	Optimism	Schadenfreude
	Humility	Scorn
	Hope	Greed
Non-UH	Love	Hostility
	Friendliness	Irritation
	Empathy	Anger
	Patience	Jealousy

Note. All emotion terms were selected based on the subjective ratings collected by Enock et al. (2021b).

iv) Procedure

After providing informed consent, participants were randomly assigned to assess either the normal outgroup (Uzbeks) or the extreme outgroup (drug addicts). They were first asked to rate how much they identified with and how much knowledge they already possessed about the group in question. Then, they read an excerpt of a news story (which was in fact made-up) revealing some basic facts about the group (see [Appendix D.2](#) for the verbatim excerpts) and rated the group on warmth and competence. These four ratings served as a manipulation check to confirm that both drug addicts and Uzbeks were outgroups to participants and that the former were more of an extreme outgroup than were the latter. I also asked participants to indicate how fixed their impression of the assigned group was.

Next, participants read the second excerpt about the outgroup (see [Appendix D.3](#) for the verbatim excerpts). For example, when assessing the extreme outgroup, the excerpt stated that “the drug addicts frequently expressed [first emotion pair]; then over

time, these emotions changed to [second emotion pair]”. Participants were randomly assigned to either the bivalent- or univalent-emotion condition. In the bivalent-emotion condition, half of the participants read that the outgroup members expressed first a mixture of two opposite-valence emotions, then a mixture of another two, whereas the other half read the same two emotion pairs but in a reverse order. In the univalent-emotion condition, half of the participants read first a pair of two positive emotions, then a pair of two negative emotions, whereas the other half read the same two pairs but in a reverse order. This manipulation was adapted from Prati and Giner-Sorolla’s (2018) study.

Following the reading task, participants needed to complete the emotion ascription task. They also responded to questions that measured their blatant dehumanisation of and prejudice towards the outgroup, as well as their perceptions of the group members’ mental complexity. To gain more insights into the effects of presenting bivalent emotions, I asked participants to rate their views of this emotional experience towards the end of study. Finally, after providing some demographic information (age, gender, ethnicity), they were debriefed and thanked.

v) Measures

Perceptions of outgroup. As in the pilot study, participants rated their knowledge and perceived warmth and competence of the assigned outgroup at the beginning of this study. Additionally, they indicated how much they identified with the group on a 1 (“Not at all”) to 7 “Extremely”) scale and how fixed their impression about the group members were on a 0 (“Not fixed at all”) to 100 (“Extremely fixed”) scale.

Emotional attribution. After reading the second excerpt, participants rated how much they thought the outgroup members possessed the ability to experience each of the 16 emotions presented in Table 9 relative to the average human population on a 1

(“Much less than the average population”) to 7 (“Much more than the average population”), with 4 representing “Neither less nor more than the average population”. This scale was adapted from Prati and Giner-Sorolla’s (2018) work.

Humanity. As in Enock et al.’s (2021b) study and my pilot study, I included the blatant dehumanisation scale in the present study to explicitly measure perceived humanity of the outgroup. In addition, I asked participants to indicate to what extent they thought that outgroup members had a complex mind on a 1 (“Not at all”) to 7 (“Extremely”) scale.

Prejudice. Prati and Giner-Sorolla (2018) reported that presenting bivalent emotions reduces prejudice against the outgroup, so I also included the prejudice scale in the present study to see if this effect could be replicated across outgroups. Participants were presented with six pairs of bipolar adjectives (warm-cold, positive-negative, friendly-hostile, trusting-suspicious, respectful-contempt, admiration-disgust) to rate on 7-point Likert scales. These adjective pairs were originally used in Wright et al.’s (1997) study.

Perceptions of bivalent emotions. Towards the end of the study, participants indicated to what extent they thought the ability to experience bivalent emotions was an indicator of a complex mind and was a socially desirable characteristic on separate 1 (“Not at all”) to 7 (“Extremely”) scales.

5.3.2 Results

i) Allocation to Conditions

None of the participants provided responses to the key dependent variable of emotional attribution that were considered outliers ($< \text{or } > 3 \text{ SD}$ from the mean). Therefore, I had the same N of 22 for each Group/Emotion condition.

ii) Manipulation Checks

The means, *SDs*, and independent samples *t*-test results of all following measures for the two outgroups are presented in Table 10. Participants did not identify strongly with either of the presented groups. In particular, their identification ratings for drug addicts were significantly lower than for Uzbeks. Although participants' knowledge of drug addicts was significantly greater than that of Uzbeks, they rated drug addicts as significantly less warm and less competent than Uzbeks. These results confirmed that participants saw Uzbeks and especially drug addicts as outgroups. Moreover, drug addicts were evaluated as more of an extreme outgroup (i.e., lower on both warmth and competence) than were Uzbeks despite being more familiar, consistent with results from the pilot study. Finally, participants indicated that their opinions about drug addicts were more unchangeable than those about Uzbeks.

Table 10

Means, Standard Deviations, and Independent Samples t-Test Results of Identification, Knowledge, Perceived Warmth and Competence, and Unchangeability of Impression for Drug Addicts and Uzbeks

Variable	Drug addicts (<i>n</i> = 44)		Uzbeks (<i>n</i> = 44)		<i>t</i> (86)	<i>d</i>
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>		
Identification	1.25	0.61	1.80	1.49	-2.25*	-.48
Knowledge	38.86	23.32	8.27	12.53	7.67***	1.63
Perceived <i>warmth</i>	3.02	1.09	4.27	1.04	-5.50***	-1.17
Perceived <i>competence</i>	3.64	1.24	4.57	1.25	-3.51***	-.75
Unchangeability of impression	39.07	22.91	12.30	19.91	5.85***	1.25

Note. *N* = 88. **p* < .05; ***p* < .01; ****p* < .001.

iii) Main Analyses

A 2 (Emotion) x 2 (Group) x 2 (Humanness) x 2 (Prosociality) mixed ANOVA of attributed emotion ratings revealed that my bivalent-emotion manipulation did not have a significant main effect on emotion attribution ($p = .40$). The main effects of group and prosociality were also not significant ($ps > .40$). However, there was a main effect of humanness ($F(1, 84) = 77.99, p < .001, \eta_p^2 = .48$), such that participants generally ascribed more non-UH than UH emotions, regardless of prosociality and manipulation, to both outgroups. This effect was qualified by a significant interaction with prosociality ($F(1, 84) = 49.01, p < .001, \eta_p^2 = .37$): Bonferroni-corrected pairwise comparisons indicated that the difference in attribution between non-UH and UH emotions was significant only when those emotions were classified as antisocial ($p < .001$) and not when they were prosocial ($p = .75$). Moreover, there was a significant interaction between prosociality and group, $F(1, 84) = 67.59, p < .001, \eta_p^2 = .45$. Specifically, antisocial emotions were ascribed more strongly to drug addicts than Uzbeks whereas prosocial ones were ascribed more strongly to Uzbeks than drug addicts ($ps < .001$). None of the other two-way interactions (group x emotion, emotion x prosociality, group x humanness, emotion x humanness) were not significant ($ps > .18$).

In addition, I observed a significant interaction between humanness, prosociality, and group ($F(1, 84) = 18.28, p < .001, \eta_p^2 = .18$; see Figure 6), which qualified the aforementioned humanness x prosociality and prosociality x group interactions. To decompose this three-way interaction, I computed simple two-way (prosociality x group) interactions (for the origin of the phrase “simple two-way interaction,” see Kassambara, n.d.) at each level of humanness. The interactions were significant in both the non-UH ($F(1, 86) = 82.82, p < .001, \eta_p^2 = .49$) and UH ($F(1, 86) = 37.02, p < .001, \eta_p^2 = .30$) conditions. Analysis of simple main effects (for the

origin of this phrase, see Kassambara, n.d.) further revealed that for non-UH emotions, those classified as antisocial in character were ascribed more strongly to drug addicts whereas prosocial ones were ascribed more strongly to Uzbeks ($ps < .001$). The same results were found for antisocial and prosocial emotions that were UH ($ps < .001$).

None of the other three-way interactions (group x emotion x prosociality, group x emotion x humanness, emotion x prosociality x humanness) were not significant ($ps > .33$). Finally, the four-way interaction was also not significant ($p = .19$). Table 11 presents the means and *SDs* of emotion attribution as a function of emotion and group.

Table 11

Means and Standard Deviations of Emotion Attribution, Humanity, Prejudice, and Perceptions of Bivalent Emotions as a Function of Emotion and Group

Variable	Bivalent			
	Drug addicts		Uzbeks	
	<i>(n = 22)</i>		<i>(n = 22)</i>	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
<i>UH emotion attribution</i>				
Prosocial	3.48	0.89	4.02	0.79
Antisocial	3.78	0.82	2.74	0.91
<i>Non-UH emotion attribution</i>				
Prosocial	3.24	0.78	4.41	0.69
Antisocial	4.98	0.86	3.36	1.07
<i>Humanity</i>				
Perceived mind complexity	4.45	1.22	4.50	1.41
Blatant dehumanisation	90.63 ⁺	12.51	89.00	12.99
Prejudice	4.91	0.91	3.23	0.92
<i>Perceptions of bivalent emotions</i>				

Mind complexity	4.91	1.27	4.73	1.49
Social desirability	4.73	1.45	4.50	1.41
Univalent				
Variable	Drug addicts		Uzbeks	
	(n = 22)		(n = 22)	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
<i>UH</i> emotion attribution				
Prosocial	3.41	0.88	4.35	0.84
Antisocial	3.76	0.73	3.09	1.01
<i>Non-UH</i> emotion attribution				
Prosocial	3.39	0.63	4.48	0.82
Antisocial	4.70	0.87	3.56	0.77
Humanity				
Perceived mind complexity	4.50	1.01	4.59	1.33
Blatant dehumanisation	90.41	12.90	93.27	11.22
Prejudice	4.72	0.58	2.98	0.89
Perceptions of bivalent emotions				
Mind complexity	5.00	1.20	4.82	1.30
Social desirability	4.86	1.39	5.18	1.30

Note. *N* = 88.

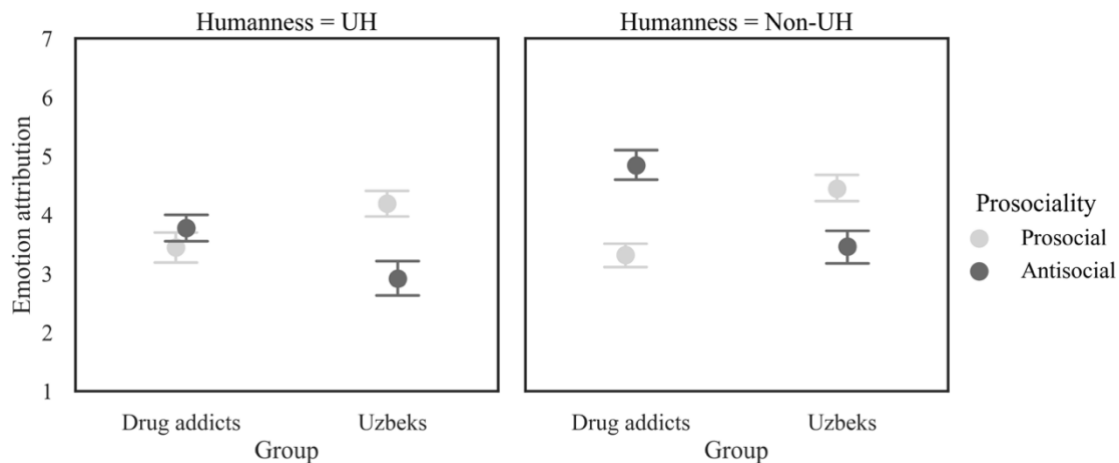
⁺ Three participants in this condition (outliers) were excluded for the analysis of blatant dehumanisation.

One-sample t-tests further confirmed that attribution ratings for antisocial emotions were significantly greater than 4 (“Neither less nor more than the average population”) when drug addicts were being assessed ($M = 4.31$, $SD = 0.97$; $t(87) = 2.95$, $p = .004$, $d = .32$) and were significantly lower than 4 when Uzbeks were being assessed ($M = 3.19$, $SD = 0.98$; $t(87) = -7.77$, $p < .001$, $d = .83$). By contrast, attribution ratings for prosocial emotions were significantly lower than 4 when drug addicts were being

assessed ($M = 3.38$, $SD = 0.79$; $t(87) = -7.36$, $p < .001$, $d = .78$) and were significantly greater than 4 when Uzbeks were being assessed ($M = 4.32$, $SD = 0.79$; $t(87) = 3.72$, $p < .001$, $d = .40$).

Figure 6

Three-Way (Humanness $\times$ Prosociality $\times$ Group) Interaction



Note. The left panel presents mean ratings for prosocial (lighter lines) and antisocial (darker lines) emotions that were classified as UH and the right panel presents mean ratings for prosocial and antisocial emotions that were non-UH. Both panels show differences in the corresponding emotion attribution ratings for drug addicts and Uzbeks. Error bars represent 95% confidence intervals (CI).

iv) Secondary Analyses

Humanity. A two-way factorial ANOVA did not show any significant effects of group or emotion on perceptions of outgroup members' mind complexity ($ps > .80$). Participants provided a mean rating of 4.51 ($SD = 1.23$) when assessing how complex the outgroup members' mind was. Similarly, neither of my manipulations had a significant impact on blatant dehumanisation of the outgroup members ($ps > .41$)²⁵. The mean rating for this measure was 90.84 ($SD = 12.30$). The means and SD s of these two measures of humanity as a function of emotion and group are reported in Table 11.

²⁵ Please note that for this particular analysis on blatant dehumanisation, I excluded three datapoints because they were considered outliers (< 3 SD from the mean), resulting in an updated sample size of 85.

Prejudice. A two-way factorial ANOVA revealed only a significant main effect of group on prejudice ($F(1, 84) = 64.49, p < .001, \eta_p^2 = .52$), such that prejudice towards drug addicts ($M = 4.81, SD = 0.76$) was generally higher than towards Uzbeks ($M = 3.10, SD = 0.90$). Neither the main effect of emotion nor its interaction with group was significant ($ps > .22$). The means and *SDs* of prejudice as a function of emotion and group are reported in Table 11.

Perceptions of bivalent emotions. Participants provided a mean rating of 4.86 ($SD = 1.30$) and 4.82 ($SD = 1.39$) when being asked how much they thought the ability to experience bivalent emotions indicated mind complexity and how much it was a socially desirable characteristic respectively. One-sample *t*-tests showed that both means were significantly greater than the midpoint (complexity: $t(87) = 6.25, p < .001, d = .67$; desirability: $t(87) = 5.54, p < .001, d = .59$), indicating that participants agreed that bivalent emotions were a relatively strong indicator of both items. A two-way factorial ANOVA did not show any significant effects of group or emotion on either of these ratings ($ps > .17$). The means and *SDs* of these two measures of perceptions of bivalent emotions as a function of emotion and group are reported in Table 11.

5.3.3 Discussion

The present research followed Prati and Giner-Sorolla's (2018) lead examining presentation of outgroup members' emotional complexity—specifically, their bivalent emotional experience—as a route to improved intergroup relations. I included a normal (Uzbeks) and an extreme (drug addicts) outgroup to further assess whether this strategy had an impact on intergroup relations, even when they are at their worst.

Results showed that presenting bivalent emotions, although being considered as an indicator of mind complexity and a socially desirable characteristic, did not lead to

any differences in emotion attribution compared to presenting univalent emotions, failing to support any of my hypotheses about the possible effects of the manipulation. The manipulation also had no impact on prejudice, such that participants' prejudice towards the extreme outgroup remained greater than that towards the normal outgroup. Additionally, I found that participants who were presented with expressions of bivalent emotions did not evaluate this kind of emotional experience more favourably than did those in the univalent-emotion condition. Because I did not include a check for this particular manipulation, it is possible that some of the participants might simply have not registered the provided information fully, which in turn prevented them from experiencing any influence of the intervention. Future researchers who wish to adopt this manipulation should add a measure that checks what kind of information participants believe they have been given about the target outgroup.

Alternatively, even if participants did register the information, the manipulation might not have been powerful enough in emphasising the complexity or desirability of the bivalent emotional experience, thereby failing to effect changes in people's views on outgroups. It is worth noting that in the original work, Prati and Giner-Sorolla (2018) did not report a significant effect of the manipulation across all of their target outgroups, so it is possible that the manipulation itself is not a reliable strategy in the first place. It is also possible that manipulation did not work at all for my version of the emotion ascription task which deviated from Prati and Giner-Sorolla's (2018) version and manipulated prosociality, instead of emotional valence, at each level of humanness. In Prati and Giner-Sorolla's (2018) version, within the category of UH, only valence was manipulated to ensure that there was a balanced number of positive and negative terms. However, three of the eight emotions used in these authors' task (compassion, optimism, and admiration; all scoring more than 70 points out of 100) were in fact rated

as highly prosocial according to Enock et al.'s (2021b) dataset, four (remorse, regret, pride, and melancholy; all scoring less than 3 points away from the midpoint) as neither prosocial nor antisocial, and only one (envy; scoring less than 30 points) as highly antisocial. In other words, there was an imbalanced number of prosocial and antisocial terms. Therefore, even if intergroup differences in emotion attribution are indeed due to social preference, the manipulation may not be the appropriate strategy to attenuate it. On the other hand, even if the differences are indeed due to infrahumanisation, the manipulation may still lead to null results because its effect has been shown to be inconsistent in the original study.

While the core of infrahumanisation theory is that UH emotions, even negative ones, are ascribed more strongly to the ingroup than outgroup (Leyens et al., 2001), that of the social preference account is that antisocial emotions, even UH ones, are ascribed more strongly to the outgroup than ingroup (Enock et al., 2021b). Given that only prosociality of the UH emotions was manipulated in the present study and not valence, my version of the emotion ascription task actually served as a more valid measure of social preference but less of infrahumanisation. In contrast, Prati and Giner-Sorolla's (2018) version manipulated only valence of the UH emotions and not prosociality, it served as a more valid measure of infrahumanisation but less of social preference. Future researchers should manipulate valence independently of prosociality in their task, so that they can directly test what better explains the intergroup differences in emotion attribution and whether Prati and Giner-Sorolla's (2018) finding that presenting outgroup members' bivalent emotions helps reduce those differences can be replicated.

The present study provided evidence that emotion attribution depended on both emotion prosociality and humanness, thereby supporting both Enock et al.'s (2021b) social preference account and Leyens et al.'s (2001) infrahumanisation theory. In

general, non-UH emotions were more likely to be ascribed to outgroups than were UH emotions (as predicted by infrahumanisation theory), but only when those emotions were antisocial and not prosocial (consistent with the social preference account). When comparing emotion attribution between two outgroups, I found that antisocial emotions, especially non-UH ones, were more likely to be ascribed to a more stigmatised outgroup than a less stigmatised outgroup, and vice versa for prosocial emotions. In sum, intergroup differences in emotion attribution were generally explained by the hypothesis that people tend to allocate less socially desirable items to a more “outcast” group, and this tendency appears to be stronger when the emotions are shared with animals than when they are uniquely experienced by humans.

Another interesting finding of the present research is that participants perceived that drug addicts experienced antisocial emotions significantly more and prosocial emotions significantly less than the average population but perceived the opposite about Uzbeks. In other words, participants not only evaluated the normal outgroup more favourably than the extreme outgroup but also considered the normal outgroup comparable to the average human population, probably even to their own group, despite not identifying with the group. This likely reflects participants’ lack of knowledge of Uzbeks: If people come across an unfamiliar group of individuals whom they have not yet formed any impressions about, they may feel no need for social comparison (Tajfel & Turner, 1979) and may even try to make sense of information about these individuals by relating them to their own group which they already know well—a cognitive process called assimilation (Di Paolo et al., 2014). Also, people may evaluate an unfamiliar group favourably due to social desirability pressures (Gawronski & De Houwer, 2014). When lacking knowledge and any kinds of impressions about an outgroup, people are uncertain about public attitudes towards the group (unlike outgroups which are clearly

negatively stereotyped), so even if they do consider the group as inferior to their own group, they may feel safer to effortfully control their evaluations by providing positive ones to prevent from looking “bad”. If prior knowledge of outgroups does alter the effects of inhumanisation and social preference, then no differences in emotion attribution should be observed when using the minimal group paradigm (Tajfel, 1970), which divides participants into two groups anonymously on the spot based on trivial criteria (e.g., the toss of a coin) that reflect nothing about the members. Although previous studies have used this method and found evidence supporting inhumanisation theory (Simon & Gutsell, 2020), they did not address emotion prosociality in their emotion ascription task, so it is worth investigating how the ingroup in the minimal group paradigm would respond to the modified version of the task used here.

My two measures that asked participants how much they believed the outgroup members in question had a complex mind and how evolved the members were did not show any significant differences between the two target outgroups, failing to support my predictions but replicating the results from my pilot study. Given that these measures explicitly asked how “human” people think members of an outgroup are, people were likely subject to even stronger social desirability pressures (Gawronski & De Houwer, 2014) which made them reluctant to overtly assess an outgroup as inferior to another. Such behaviour is likely to be viewed as “bad” and even condemned. Alternatively, participants might not have overtly dehumanised the extreme outgroup more than the normal outgroup because they read in the vignette that the drug addicts were interviewed at a rehabilitation centre. This information might have caused participants to infer that this particular group of drug addicts were taking actions to rehabilitate themselves and thus view them as less of a disgrace and less incompetent

than typical drug addicts. Indeed, previous research has documented that people evaluate female and male targets who violate stereotyped expectations for their gender group more positively than those who do not (Bettencourt et al., 1997), consistent with expectancy violations theory (Burgoon & Hale, 1988; Jussim et al., 1987) which predicts that positive violations lead to more favourable evaluations of the violator (see also Prati et al., 2015). Future research can directly test this theory by comparing assessments of two groups of drug addicts (or other disliked individuals), one of which is presented as acting in accordance with their negative expectations and one of which is presented as acting opposite to them. However, it is notable that humanness also played a role in attributing emotions of different levels of prosociality to outgroups. This implies that even though people were able to effortfully control their negative attitudes, they still subtly denied outgroups (especially extreme ones) uniquely human experiences.

The present research provides novel insights into the dehumanisation literature by taking a middle ground: On one hand, my results support the recent critiques (e.g., Manne, 2016; Enock et al., 2021b) that people view outgroups, particularly the heavily stigmatised ones and not those which people have no contact with previously, as inferior not by denying their ability to feel UH emotions but by assigning emotions that socially unwelcoming to them and reserving those that are socially welcoming for themselves. On the other hand, my results suggest that the originally existing proposition that outgroups are believed to experience less UH emotions holds too when intergroup relations are at their worst and when people do not perceive the need to regulate their negative attitudes, given that compared to UH antisocial emotions, non-UH ones were even more strongly ascribed to extreme outgroups. These results also show the importance of acknowledging not only the dimension of humanness but also

that of prosociality when investigating intergroup relations using the emotion ascription task.

In more practical terms, a more reliable intervention is needed to reduce social preference and infrahumanisation of strongly disliked outgroups, as both my and Prati and Giner-Sorolla's (2018, Experiment 3) works have showed that presenting outgroup members' bivalent emotions does not always yield significant results. The aforementioned idea of presenting counter-stereotypical behaviours may be worth following up on. Considering my finding that outgroups which people do not have presumptions about seem to be granted the same amount of relative humanity as average human beings, it may even more effective if people are told that the counter-stereotypical behaviours are carried out by members of an anonymous outgroup before the real identity of the group is revealed. Future research can test the effectiveness of this expectancy violation strategy in improving attitudes using vignettes, which I deem are similar to imagined contact (Miles & Crisp, 2014), then in a real-world context.

To conclude, although bivalent emotions are believed to reflect mental complexity and be a socially desirable characteristic, presenting these emotions failed to cause any cognitive effects on intergroup relations in the present study. However, I found novel evidence that extreme outgroups are ascribed more antisocial, especially non-UH, emotions and less prosocial ones to than are normal outgroups, suggesting that social preference does not fully explain the intergroup differences in emotion attribution when outgroup members are heavily stigmatised, as infrahumanisation appears to be at work too.

CHAPTER SIX

General Discussion

6.1 Abstract

In this final chapter, I summarise the main findings of each empirical study presented in this thesis. Implications are discussed in the context of research and theory regarding social sharing, social support, emotion regulation, and affect-cognition relationships. Possible directions of future research are suggested for following up on those implications. Limitations of my DPhil research in general, including the absence of control conditions in some studies, inadequate generalisability, and the unaddressed dynamic nature of emotions are also discussed. It is proposed that this thesis contributes to the understanding of human emotions in interpersonal and intergroup contexts and to two modern societal issues: The emergence of online therapy and intergroup prejudice.

Stemming from the proposition that emotions have social consequences (Parkinson, 1996), this thesis has reported research investigating social sharing of one's own felt emotions and effects of other people's expressed emotions on intergroup relations. Across four projects, negative—particularly, crisis-related—emotions (Projects 1 and 2) and bivalent emotions (Projects 3 and 4) were respectively investigated.

Prior research has shown that an emotion, regardless of its valence, is highly likely (80-95%) to be communicated to other people after it is experienced (for a review, see Rimé, 2009). This finding applies even when emotions are induced by major collective crises, such as the 2004 Madrid bombings. However, previous evidence was still equivocal as to how effective social sharing is in regulating emotions because negative outcomes have been reported in some studies following this interpersonal process (e.g., Kanyangara et al., 2007; Rimé et al., 1998; Rimé et al., 2010). Furthermore, in previous investigations, the crises were mostly (if not all) of short duration, so social sharing typically occurred after the central event had ended. Therefore, I took advantage of the COVID-19 context in Project 1 and observed the processes and consequences of sharing negative experiences related to this collective crisis whose repercussions persisted for a long time. Then, in Project 2, I conducted two experiments to examine whether different ways of chatting about an interpersonal stressor and a more chronic, collective stressor led to different affective outcomes.

Despite abundant research dedicated to the study of human emotion, researchers' attention has been mainly on univalent emotions, leaving bivalent emotions largely underexplored. I therefore launched two projects which aimed to help close this research gap by generating new knowledge related to this kind of mixed emotional state. Specifically, Project 3 presented results that shed light on the three fundamental

questions of how commonly bivalent emotions are shared in daily life, what people want to achieve by sharing these emotions, and what the affective outcomes of this sharing are. Project 4, on the other hand, extended the scope of this thesis from interpersonal to intergroup contexts and switched the focus of investigation from effects of sharing one's own emotions to those of perceiving other people's emotions.

In the below sections, I summarise the key findings of all research projects presented in this thesis and discuss them in relation to each other and the wider literature. Then, I identify the general limitations of my DPhil research and discuss the implications for applied issues. Possible avenues for future research are also suggested to address those limitations or to follow up on those implications.

6.2 Key Findings

6.2.1 Results Pertaining to Negative Emotions

i) Project 1 (Study 1)

Project 1, using prospective questionnaires, reported preliminary evidence that sharing negative experiences related to the COVID-19 pandemic—in particular, first-person events and second-hand news that impacted not only the parties involved in social sharing but also the broader community—is associated with affect improvement. This central finding suggests that social sharing generally seems an adaptive response to a collective crisis with a long time span. Specifically, people tended to feel better about an event when it was shared than when it was not. Laypeople commonly believe that venting helps dissolve the impact of an emotional memory (Zech et al., 2004). Although this project did not directly examine the content of social sharing, it showed that participants' satisfaction with their listener's feedback was positively correlated with their positive feelings about the shared events after discussing them, suggesting that the

emotion-improving effects of social sharing are better explained by the appropriate social support provided instead of the act of venting. This adds support to the previous findings that distress is more likely to alleviate when people engage in more supportive social interactions (e.g., Lepore, 1992; Major et al., 1997; Manne et al., 1997), and that venting alone actually does not lead to a reduction in emotional intensity (e.g., Rimé et al., 1998; Van Emmerik et al., 2002; Zech & Rimé, 2005).

Another important finding of Project 1 was that the extent of affect improvement appeared to be greater after discussing COVID-related personal experiences than after discussing COVID-19 news. It is likely that while the primary motive for sharing the former kind of event is soliciting social support that facilitates emotion regulation, that for sharing the latter kind is informing or exchanging views. Duprez et al. (2015), who directly assessed people's motives for sharing emotional experiences using their newly developed SSMS, reported that "informing and/or warning" was not rated as a significant sharing motive for negative (or positive) experiences. Their results were replicated in Project 3 of this thesis. However, it should be noted that in both cases, the negative events being investigated were related to personal life stressors, so the available research cannot provide evidence about whether the same sharing motives apply to events associated with stressors with a collective element. I speculate that people's main goal when sharing crisis news is not to seek support but to inform other people of the latest information worthy of concern because unlike personal experiences wherein listeners typically are an objective third party, for news stories listeners are impacted as much as sharers. If sharers assume that listeners are in a similar position to them and have the same psychological resources as they do, then they may be less likely to pursue an affect-regulatory goal during social sharing. In that case, sharers' affect is *modulated* only by the mere presence of listeners (Zaki & Williams, 2013) and not on

any active attempt on the sharers' part to solicit support from listeners, thereby explaining the weaker emotion-improving effects of sharing news stories.

Although there is a clear dividing line between the two phenomena, interpersonal modulation of affect is intimately tied to regulation, in that modulation often represents the result of an earlier regulatory goal to seek out social contact when negative events are anticipated (Zaki & Williams, 2013). Schachter (1959) demonstrated this exact phenomenon in his classic affiliation experiments: Compared to participants who were told that they were going to receive painless electric shocks, those who were going to receive painful but harmless electric shocks displayed a stronger preference for waiting with other participants who were going to receive the same shocks (see also Harlow & Zimmermann, 1959 and Taylor, 1981, for respective studies using monkeys and rats as subjects). Therefore, by the same logic, participants in Project 1 might buffer themselves from a looming stressful event that they learnt about from the news (e.g., a possible national lockdown) by seeking out contact that would reduce the need for later regulation. Some may argue that this does not explain why there was still some degree of affect improvement when the shared news was not imminent but had already happened (e.g., learning of an updated COVID-19 death toll). However, as noted in prior research, social sharing can be perceived as rewarding when sharers recognise that someone else is "on the same page" as themselves (e.g., Klucharev et al., 2009; Zaki et al., 2011). Therefore, once people inform a listener of upsetting news that is impacting both of them, the thought that they are not in it alone should become salient and make them find the sharing helpful even when they do not solicit social support.

If people are not trying to solicit support from listeners when sharing COVID-19 news, their sharing may instead involve excessive discussion about the problem or their

own negative feelings without either of the parties coming up with a solution or resolution (i.e., co-rumination; Rose, 2002; see also Rimé, 2009; Rose et al., 2007). Co-rumination has been linked to rumination and anxiety (e.g., Stone et al., 2010), so it may consequently have hindered affect improvement in Project 1. It is possible that face-to-face communication presented a further hindrance because compared to remote communication, in-person sharing allows people to access a wider range of signals (e.g., touch, posture) from their interaction partners (for a review, see Parkinson, 2008), facilitating transmission of negative emotions across the two parties (for a review, see Parkinson, 2011).

ii) Project 2 (Study 2A)

To investigate effects of sharing negative emotions more systematically, I conducted two online experiments in Project 2 which manipulated not only the type of event being shared but also the chatting style participants adopted when talking about their emotional experience. Specifically, I programmed a simulated support-provider who asked scripted questions that prompted participants to either reappraise (reconstrue) their shared emotional experience or simply recount it. One of the central findings of Study 2A, which compared interpersonal conflicts (acute personal experiences) to COVID-19 news (persistent collective experiences), is that social sharing leads to affect improvement regardless of the shared event type and the chatting style. Conflicting with this finding, Project 1 found that event type matters, as people sharing a personal experience seem to feel better than those sharing crisis news over time. Theoretically, event type should have mattered in Study 2A too, and the difference in affect improvement between shared interpersonal conflicts and shared COVID-19 news should have been even more apparent than the difference in Project 1: In Project 1, the reported personal experiences were COVID-related and thus might have

reminded participants of the persistent and intractable circumstances under which their shared event took place (even though the repercussions of the particular event did not persist), creating a barrier to closure and compromising the regulatory effect of sharing (though it was still stronger than the effect for news stories). By contrast, the personal experiences reported in Study 2A were unrelated to the pandemic, so participants should have benefitted more from sharing interpersonal conflicts than from sharing COVID-19 news. Indeed, I found that participants in this study rated their closure as greater for interpersonal conflicts than for COVID-19 news following chatting, supporting my claim that it is more difficult for people to feel “completed” about chronic problems than acute ones. Yet, there was no difference in affect improvement between the two event types as expected. This may be attributed to the drastic difference in people’s social life at different points of the pandemic: Project 1 was conducted between June 2020 and September 2020, a few months after people residing in the UK and the US had started to limit their social contacts. On the other hand, when Study 2A was conducted, most legal limits on social contact (in the UK) had just been removed, so people had opportunities to interact with a wider range of other people in person, which in turn might have reduced the emotional effects of sharing with a virtual listener.

Another central finding of Study 2A was that reconstrual did not have a more positive impact on sharers’ affect than recounting, contradicting results from prior research (e.g., Ayduk & Kross, 2010; Kross & Ayduk, 2008; Lee et al., 2020). My use of a pre-programmed rather than experimenter-adjusted script during the online chat led participants to express doubt that their chat companion was really following the conversation with them to varying degrees, but the fact that perceived responsiveness did not moderate any of the results suggests that this was not the reason for the unexpectedly similar emotional effects of the two chatting styles. Rather, this

unexpected finding may be attributed to the difference in people's social life as discussed earlier—that is, the emotional effects of sharing in general with a virtual listener might be reduced as people started to interact with other people in person again after a year of enforced social distancing measures. Still, there was evidence suggesting that reconstrual was potentially more effective in emotion regulation than recounting: First, reconstruing participants were more likely to use cause-related words during their sharing, a sign that they were engaging in meaning-making. Prior research has showed that when people are trying to make meaning out of their negative experience, they think about it from a broader perspective which often causes them to feel more psychologically distant from the event (Orvell et al., 2017), thereby reducing its emotional impact. Second, reconstruing participants experienced more closure after sharing, indicating that they had a stronger feeling that the shared problem had been resolved and therefore should have felt more relieved.

iii) Project 2 (Study 2B)

Reappraisal can come in various forms (for a review, see Webb et al., 2012). Study 2B focused only on interpersonal conflicts and examined whether the beneficial effects of reconstrual became more apparent when its more targeted forms (stimulus-reappraisal vs. perspective-taking) were applied to more specific kinds of conflicts (angry vs. regretful). This time, there was some evidence that reconstrual outperformed recounting in terms of its effectiveness in regulating sharers' emotions, likely reflecting the fact that a generic instruction to reconstrue (used in Study 2A) is relatively less sufficient than an instruction to reconstrue in a specific way (as in Study 2B) to reduce negative affect induced by different kinds of emotional situations.

Prior research has documented that despite its association with long-term emotion regulation (Nils & Rimé, 2012; Tamir, 2009), cognitive support is less

appreciated by angry or sad sharers than by regretful or worried ones (Pauw et al., 2017). While it may be true that cognitive support is not subjectively assessed equally favourably across people experiencing different emotions, Study 2B suggests that positive effects can still be produced if each emotion is matched with the appropriate form of reappraisal. Specifically, I found that perspective-taking is relatively more effective than stimulus-reappraisal in alleviating anger induced by angry conflicts. On the other hand, both stimulus-reappraisal and perspective-taking are equally suited for regulating regret. These two negative emotions are characterised by blaming but differ in the allocation of blame: Anger arises when people blame another person for threatening their interests (Smith & Kirby, 2000; Wu & Wang, 2017), so perspective-taking, which encourages an objective perspective that helps distance people from the unpleasant stimulus, may work especially well for down-regulating this emotion. Stimulus-reappraisal, which offers a more positive perspective that helps people see the bright side of what has happened, however, may not work as well because it is not easy for angry people to reconsider what their antagonist had done as a good thing. Regret, on the other hand, arises when people blame themselves for making a bad decision in the past (Wetzer et al., 2007; Zeelenberg & Breugelmans, 2008), so perspective-taking and stimulus-reappraisal can help down-regulate this emotion by allowing people to gain more objectivity when reviewing their mistakes and to find good in the bad respectively. Anger and regret also seem to differ in terms of the changeability of their repercussions: The former often prompts antagonistic action (e.g., Ellsworth & Smith, 1988; Frijda, 1986) which can influence the situation (e.g., demanding the antagonist's apologies) whereas the latter often involves irreversible outcomes of bad decisions made in the past (Wetzer et al., 2007; Zeelenberg & Breugelmans, 2008). Therefore, my results also suggest that stimulus-reappraisal may be equally appropriate for sharing a

problem which is no longer changing or changeable and a problem which is still possible to be fixed, while perspective-taking may be more appropriate for sharing the latter kind of problem.

It should be noted, though, that levels of closure were not rated as different between chatting styles or event types. This suggests that although social sharing yields a stronger regulatory effect on events eliciting anger when using perspective-taking than when using stimulus-reappraisal or recounting, perspective-taking still does not reduce sharers' reluctance to report feeling a sense of resolution.

In sum, results from Study 2A suggest that people feel better about their negative experience as long as they verbalise it, upholding the lay belief regarding the effectiveness of venting (Zech et al., 2004). However, results from Study 2B showed that how people talk about their experience seems to play a crucial factor after all in determining the adaptiveness of social sharing when their needs are met with the appropriate reappraisal strategy. This is consistent with Omarzu's (2000) disclosure decision model, which proposes that self-disclosure is beneficial only when the listener provides satisfactory support, and with Project 1's finding that people tend to rate their positive feelings higher about a COVID-related event after sharing it when they are more satisfied with their listener's feedback. Although it was the participants themselves who came up with their own alternative way of seeing things, the regulatory effects of the two reconstrual styles observed in Study 2B still appeared to be *response-dependent* (Zaki & Williams, 2013) because the simulated listener offered specific ways of reappraising participants' issues by asking different prompt questions across conditions.

As reported in previous studies, labelling one's own affective states in a more "fine-grained" way and assessing the causes underlying those states are two key

psychological ingredients that promote regulatory success as they help improve cognitive clarity and facilitate coping (e.g., Kircanski et al., 2012; Kross et al., 2005; Pennebaker, 1997). Both recounting and reconstrual involve labelling, as they ask sharers to pinpoint the source of their distress. However, unlike recounting, reconstrual instructs sharers to move on from “what” happened to “why” it happened and to see things through a new lens. Therefore, reconstrual entails both of the key ingredients that make social sharing adaptive, and such adaptiveness appears more salient when listeners provide sharers with specific rather than generic advice about the kind of reappraisal.

6.2.2 Results Pertaining to Bivalent Emotions

i) Project 3 (Study 3)

Introducing bivalent emotions to the framework of social sharing, I launched Project 3 to assess the motives for, and outcomes of, sharing bivalent emotions using a prospective study with a design similar to the one used in Project 1 and compared these motives and outcomes with those associated with sharing univalent (positive and negative) emotions. I also conducted a pilot study to obtain some basic information about bivalent emotional states, including their general frequency and intensity, which have not been documented in previous literature, and which needed to be assessed in order to lay the foundation for Study 3. The pilot study revealed that bivalent emotions were experienced more frequently than once a week by around 75% of the sampled participants, and that the intensity of their positive aspect was stronger than that of their negative aspect. The latter finding was replicated in Study 3.

The first important finding of Study 3 is that people seem to share their bivalent experiences for similar reasons to those for sharing their negative experiences: Finding meaning and seeking emotional and cognitive support. Prior research has suggested that

bivalent emotions, due to their violation of internal consistency, are inherently accompanied by feelings of conflict (e.g., Thompson et al., 1995; Williams & Aaker, 2002), which consequently elicit a desire for clarity and resolving dissonance (e.g., Cacioppo et al., 1997; Priester & Petty, 1996; Williams & Aaker, 2002). Additionally, just like negative emotions, bivalent emotions should also heighten people's sufficiency threshold (i.e., a point at which they feel confident that a task has been completed and that they can move on to other tasks; for a review, see Bohner et al., 1995) and trigger systematic processing (when the event is of high personal relevance; Jonas et al., 1997; Petty & Cacioppo, 1986). In more specific terms, while negative emotions signal that the environment is problematic and even threatening and thus drive people to re-evaluate the effectiveness of their current approach towards the environment by processing information more carefully and deeply (e.g., Gasper & Clore, 2002), bivalent emotions signal that the environment contains complex and contradictory elements and thus drive people to refine their current approach by considering a broader set of alternatives (for a review, see Rothman & Melwanie, 2017). Therefore, my finding that the motives for sharing negative and bivalent experiences are similar is likely to be explained by the highly similar needs—alleviating unpleasant feelings and improving cognitive clarity—that people have when feeling these two kinds of emotions. Of course, such speculation needs to be further examined because it is not clear whether sharing motives depend specifically on the conflicting nature of bivalent emotions or simply reflect the fact that these emotions have a negative aspect which is what sharers mostly want to cope with.

Notably, although it seems that bivalent emotions, which particularly increase people's receptivity to alternative perspectives (Rees et al., 2013), should lead to a stronger need for advice and suggestions from other people than should negative

emotions, the corresponding motive was not rated any differently between the bivalent and negative conditions in Study 3. This may be due to the conflicting nature of the experience of multiple negative emotions which, despite their consistent valence, have incompatible appraisal patterns about the situation or social support preferences. For example, angry individuals direct blame at the other person (Smith & Kirby, 2000; Wu & Wang, 2017) and display a strong dislike of advice and reappraisal (Pauw et al., 2017) whereas regretful individuals direct blame at themselves (Wetzer et al., 2007; Zeelenberg & Breugelmans, 2008) and do not dislike cognitive support (Pauw et al., 2017). The fact that participants' bivalent and negative experiences in Study 3 did not differ in levels of felt conflict supports this theory. In other words, people feeling multiple negative emotions may experience as much dissonance as those feeling opposite-valence emotions do, thereby heightening their inclination to seek other people's opinions too.

The only motive that was not shared by both negative and bivalent experiences was venting, which previous researchers have suggested is associated with negative emotional episodes (Duprez et al., 2015). Specifically, venting was considered an important motive by people feeling negative but not by those feeling bivalent. Given that these two kinds of experiences did not differ in felt conflict, the difference in the urge to vent is possibly explained by their different levels of negativity, which was higher in intensity for negative than bivalent experiences in Project 3—in fact, the positivity of bivalent experiences outweighed the negativity. These findings add new insights into research on the lay belief that venting is a useful emotion regulation strategy (Zech et al., 2004): While this belief continues to be upheld by people experiencing negative emotions, it does not seem to apply to the same extent when

people experience bivalent emotions even when this kind of experience elicits some level of negativity due to its negative component and dissonance.

Although Study 3's secondary analyses showed that affective outcomes did not differ substantially between the shared and non-shared events, this work still sheds light on the temporal changes associated with bivalent emotions. It is also important to remember that people may deploy emotion regulation strategies intrapersonally (e.g., through expressive writing; see Ayduk & Kross, 2010, Study 3; Pennebaker et al., 2001, 2007), so that the absence of social sharing is not necessarily equivalent to the absence of emotion regulation. According to my results, people tend to feel less negative and less conflicted about their bivalent experience over time but do not undergo any associated change in their positive affect. Naturally, they feel less ambivalent about the event in the meantime, as the negative component of the bivalent state becomes even weaker in comparison to its positive component. These patterns of change are similar to those characterizing the sampled negative emotions, which are also subject to a decrease in negative affect and felt conflict over time. However, in the negative condition, I also observed an increase in positive affect which was absent in the bivalent condition. This may reflect the different regulatory goals that operate in these two conditions, such that people experiencing bivalent emotions aim to cope with their negative evaluations of the event whereas those experiencing negative emotions aim not only to relieve their negative feelings but also to enhance their positive feelings. In other words, when feeling bivalent, it seems that people tend to resolve internal conflict by attenuating the negative component of their experience rather than amplifying the positive one, even though the experience is predominantly positive. Such attenuation of the negative component, which in turn makes the positive one more prominent, supports prior research that people would accentuate either their positive or negative evaluations as

one of the approaches to simplifying their complex states (Pratt & Doucet, 2000; for a review, see Rothman & Melwani, 2017).

ii) Project 4 (Study 4)

Bivalent emotions have been proposed to serve a flexibility function because people experiencing them can process multiple concepts simultaneously and adjust their thinking to the constantly changing environment. There is preliminary evidence suggesting that these emotions also seem to serve as an indicator of a “complex” mind. Prati and Giner-Sorolla (2018) showed that these emotions can be a tool that disliked outgroup members might use to improve negative attitudes towards them and even reduce intergroup inhumanisation (Leyens et al., 2000, 2001). Following these authors’ lead, my final project, Project 4, examined perceptions of bivalent emotions expressed by a normal outgroup (Uzbeks) and an extreme outgroup (drug addicts), neither of which average people in the UK identify with, and how these perceptions might help improve intergroup relations even when they are at their worst using a similar experimental design to that of Prati and Giner-Sorolla’s (2018) work. I also took the recent critiques by Enock et al. (2021a, 2021b) into consideration when designing my study, so that the study had another objective of testing whether people’s negative evaluations of outgroup members are better explained by inhumanisation theory or simply a social preference account (i.e., a tendency of favouring one’s own ingroup over the outgroup by reserving more socially desirable qualities, regardless of their humanness, for the ingroup; Tajfel & Turner, 1979; Taylor & Doria, 1981).

The first important finding of Study 4 is that presenting outgroup members’ bivalent emotions did not lead participants to evaluate the outgroup any differently from presenting their univalent (multiple positive or negative) emotions, thereby failing to replicate Prati and Giner-Sorolla’s (2018) finding that perceiving outgroup members’

emotional complexity helps improve intergroup relations. My secondary measures which assessed what people think of bivalent emotions revealed that these emotions are considered a relatively socially desirable characteristic and a relatively strong indicator of a complex mind. Supplementary results from Study 3 further showed that people are more likely to infer cognitive complexity and human uniqueness (though not social desirability) from bivalent than univalent emotions, implying that these two emotional states are indeed evaluated differently. Therefore, the explanation for the failure of expressed bivalent emotions being an effective strategy to improve intergroup relations is likely to be related to how the information about the outgroup members was delivered. It is possible that participants did not fully register the information or that the vignettes used to present the information were not reliably effective, given that the same manipulation did not work consistently across all of Prati and Giner-Sorolla's (2018) reported experiments in the first place. Alternatively, this strategy might just have failed to work on the modified emotion ascription task used in my study, which addressed both humanness and prosociality, instead of humanness and valence, of emotions.

The second central finding of Study 4 was that the observed intergroup differences in emotion attribution depended not only on infrahumanisation but also on social preference. In general, I found that non-UH emotions were indeed more likely to be ascribed to outgroups than were UH emotions, consistent with prior studies upholding Leyens et al.'s (2000, 2001) infrahumanisation theory (see also Banton et al., 2020; Boccato et al., 2007; Paladino et al., 2002). However, the fact that this tendency was stronger when the emotions were antisocial but not when they were prosocial and when the target outgroup was more disliked highlights the role of social preference (Enock et al., 2021a, 2021b) in influencing emotion attribution. Furthermore, extreme outgroups were perceived to experience antisocial emotions more frequently and

prosocial emotions less frequently than the average population whereas normal outgroups were perceived the other way around. This additional finding suggests that people evaluate extreme outgroups much less favourably than normal outgroups and average people but consider normal outgroups better than average people, probably even comparable to their ingroup. Yet, this may be due to people's lack of knowledge of the normal outgroup (Uzbeks) selected for my study: If people are unfamiliar with a group of individuals and have not yet formed any impressions about them, they may feel no need for social comparison (Tajfel & Turner, 1979) and may even try to make sense of information about these individuals by assimilating (Di Paolo et al., 2014) them to their own group which they already know well. People may also evaluate an unfamiliar group favourably under the influence of social desirability pressures (Gawronski & De Houwer, 2014). In the case of evaluating disliked outgroup members, people know how these individuals are typically viewed by the public, so it is safe for them to provide evaluations accordingly without any regulation of their prejudice towards the group. However, when lacking any kind of impression about an outgroup, people may not be sure about how to evaluate the group and may therefore effortfully control their responses by providing positive ones in order to look "good".

A final finding that is worth mentioning is that despite the intergroup differences in emotion attribution, participants did not rate the two outgroups differently when asked to rate how evolved they believed the outgroup members were and how complex these members' minds were. Again, a possible explanation is social desirability pressures (Gawronski & De Houwer, 2014), which probably becomes even stronger when it comes to more explicit measures: While participants might not be aware that the emotion ascription task was measuring their prejudice towards outgroups, it is obvious when they were being asked to assess outgroups on an evolutionary or mind complexity

scale. As a result, participants might be reluctant to overtly claim a more disliked outgroup as “less human” than a less disliked one because such kind of behaviour is likely to be viewed as “bad” and even condemned. Alternatively, drawing on expectancy violations theory (Burgoon & Hale, 1988; Jussim et al., 1987), it is possible that when people learn that extremely disliked outgroup members have positively violated stereotyped expectations (e.g., drug addicts taking actions to rehabilitate themselves), they are more willing to effortfully control their explicit negative evaluations and present positive ones.

6.3 General Limitations

6.3.1 Absence of a “No-Sharing” Condition Across Projects 1-3

Design features of many of the studies presented in this thesis limit the ability to draw strong conclusions about the effect of social sharing from the reported findings: Across Projects 1-3, I was not able to effectively disentangle the effect of sharing from the effect of the simple passage of time because of the absence of a control condition wherein no social sharing occurred. I chose not to include a “no-sharing” condition in any of my projects relating to social sharing because I was focusing on differences between sharing conditions rather than on whether sharing per se has any effects throughout my DPhil research. In fact, previous researchers (e.g., Rimé et al., 2010, who used questionnaires; Lee et al., 2020, who used an experimental design) did not seem to be particularly interested in collecting data from non-sharers too because a “no-sharing” condition was not commonly included in their studies.

Still, in Projects 1 and 3, I analysed the relatively limited data collected from participants who stated that they spontaneously did not engage in any social sharing during the given time frame by comparing them to the data collected from those who

shared to gain some insights into how different people's emotional states are between sharing and no-sharing. However, there are issues with such comparisons arising from the sole use of questionnaires in these two projects: Based on existing literature, the prevalence of social sharing occurring among people who have undergone an emotional event is high (80-95%; for a review, Rimé, 2009), so in a naturalistic setting, there should be much more reports from participants of the occurrence of social sharing than reports of zero occurrences of any kinds of discussion. Indeed, the sizes of these two groups were unequal in Projects 1 and 3. My results were therefore subject to reduced statistical power and trustworthiness. Also, the notion that not 100% of emotional experiences are shared with other people implies that social sharing is not a default response that every person has in face of an emotional stimulus. In other words, there seem to be individual differences in the propensity for sharing. For example, Baumeister and Leary (1995) found that some people prefer to keep their emotions secret to prevent their social relationships from being harmed by the revelation of certain emotional events. Therefore, the results I observed when comparing the sharing and no-sharing groups were inevitably confounded with these individual differences.

One way to balance the sizes of the sharing and no-sharing groups and address the individual differences in tendency to share is to conduct an experiment which randomly assigns participants to either of the conditions. However, as shown in Project 2, I did not do that because there would be an ethical concern that participants might have been asked to go against their wishes. Given the high prevalence of social sharing, many participants who initially wanted to share might have needed to suppress that urge if they were assigned to the no-sharing condition, which in turn might negatively impact their mental well-being (e.g., inducing frustration and rumination). On the other hand, some participants might not prefer disclosing their emotions but would be forced to do

so if they were assigned to the sharing condition, which again was likely to have a negative impact on their well-being.

6.3.2 Issue of Generalisability

i) Uniqueness of the COVID-19 Pandemic

My research also faces the issue of generalisability. In Studies 1 and 2A, I investigated social sharing of COVID-related emotions. However, the pandemic is unique in that it had an extremely long time span and led to restricted social interaction. Therefore, my findings may not apply equally to other collective crises which have a relatively short duration and/or do not profoundly impact people's social lives. This means that my research does not necessarily overturn the previous finding that discussing a crisis, such as the 2004 Madrid bombings (Rimé et al., 2010) and the 2015 Paris terrorist attacks (Garcia & Rimé, 2019), is associated with both positive and negative psychological effects; instead, it expands the scope of the crisis literature by showing that social sharing seems to yield only positive (or at least neutral) effects for this particular crisis of COVID-19 that brought the world into unprecedented circumstances. In fact, the findings of Studies 1 and 2A may not apply equally to every stage of the pandemic itself: The restrictions on social contact might have made the mere act of interacting with someone emotionally beneficial in its own right, but this attitude might have changed as the course of the crisis progressed depending on how much people had adapted to their isolated lives and how long people had lived under social distancing measures. People's attitudes towards different communication methods for social sharing might have changed too over the course of the pandemic. For example, they might have appreciated online interaction much less when they were able to socialise in-person more freely due to the removal of social distancing measures.

Although it is too late at this point to conduct longitudinal studies using questionnaires to capture the dynamics of people's sharing and affective patterns throughout the pandemic, future researchers should consider using social media data which are produced in a naturalistic setting and are more reliable than data collected using retrospective questionnaires that are often characterised by recall bias and participant attrition (see Ma et al., 2022, for details of the Twitter study conducted by another DPhil student I collaborated with). Moreover, compared to questionnaire data, social media data may reveal a more accurate picture of social sharing during this particular crisis because the Internet had been where people socialised with each other when the social distancing rules were in effect. Schelhorn et al. (2022) and Vemprala et al.'s (2021) studies are examples of using social media data to chart people's emotions over time. However, social sharing behaviour was not addressed by these authors, so future research can fill this gap.

ii) Omission of Important Moderators

In Studies 1-3 which focused on social sharing of emotions, the magnitude of emotional experiences, which is a potential moderator, was never directly examined. In Study 1, even within the category of COVID-related personal experience, the reported events appeared to vary in severity. For example, one of the participants wrote, "I found out that a close work friend of mine has died due to covid 19." Another participant, on the other hand, wrote, "A close friend of mine is infected and was taken to hospital." Although both events concerned an acquaintance catching the virus, the former would definitely have been rated as more negative than the latter as the participant was dealing with the acquaintance's death caused by the virus. Similarly, in Study 2B, when being asked to recall a recent angry interpersonal situation, a participants wrote, "My friend told my partner something very personal about me which I hadn't told him." Another

participant, on the other hand, wrote, “I was supposed to meet a friend and he ditched me without notice.” Again, it seems that the former event would have caused more anger than the latter one given that the participant’s trust was betrayed. The different levels of severity of events likely influence how people cope with their emotions and consequently how they feel. For example, prior research has suggested that people in high-intensity negative situations prefer distraction to reappraisal as their coping strategy than do those in low-intensity situations (Sheppes et al., 2011). Therefore, the positive effects of reconstruing observed in Studies 2A and 2B may be undermined when the shared event was highly aversive. There may also be differences in the results reported in Study 3 regarding motives for sharing—particularly, people who are more unhappy may not want to seek cognitive support (e.g., advice or solutions) from listeners as much as those who are less unhappy. In Study 1, I attempted to remove the effect of event severity on post-sharing affect by controlling participants’ T1 affect (i.e., subjective ratings on how they felt about an experience when it happened). A more objective approach to assessing event severity is analysing participants’ descriptions of their experiences. Future researchers should address the omission of this variable in order to complement the current understanding of social sharing.

Another variable that should have been given more attention to improve the generalisability of my findings is rumination (Skinner et al., 2003), which often accompanies mental illnesses—particularly, depression (Nolen-Hoeksema et al., 2008; Watkins, 2008). Depressed individuals, due to their inclination to rumination, may be less susceptible to the positive effects of social sharing. In Study 2A, I found that both recounting and reconstrual reduced negative affect associated with interpersonal conflicts and COVID-19 news to the same extent. However, this conclusion may not apply to clinical populations: On one hand, recounting, which prompts sharers to focus

on the unpleasant details of their experience, may reinforces rumination in depressed individuals and makes it even more difficult for them to recover from the experience. On the other hand, reconstrual, which encourages cognitive change, may not work as well for depressed individuals as for healthy individuals because depressive cognition tends to be less flexible (Channon, 1996). In particular, stimulus-reappraisal, a sub-form of reconstrual which aims to re-orient people towards positive content (see Study 2B), may have even less regulatory effects given depressed individuals' consistent negative view of the self, the world, and the future (Beck, 1967). In fact, reconstrual may backfire as it often operates in ways that allow people to make sense of their experience by asking “why” questions (Kross & Ayduk, 2017), which play a significant role in driving rumination in depressed individuals (for a review, see Watkins, 2008). According to Watkins (2008), there are two primary styles of rumination: A constructive, concrete style versus a maladaptive, abstract style. When people process information using the abstract style, they ask “why” questions (e.g., “Why did I feel this way?”). However, given their substantial self-focus (Wisco, 2009), they likely direct the meanings and implications back to themselves (e.g., “Because I am useless.”). As a result, not only does their affect fail to improve, but they also increase global negative judgements about themselves which make them feel worse. Future researchers should attend to clinical populations or at least examine the role of rumination in moderating the adaptiveness of social sharing. Investigating this variable will also help advance the current understanding of rumination-focused cognitive behaviour therapy (RFCBT; Watkins et al., 2007)—a variant of traditional CBT which aims to coach patients how to shift from a maladaptive to a constructive style of rumination.

iii) Limited Diversity of Samples

The generalisability of my findings across projects is also partly undermined by the limited diversity of my samples. In Study 1, I recruited participants from the UK and the US. In Study 2A, only those from the UK were recruited. Therefore, the affective outcomes of discussing crisis-related events in these two studies may vary when it comes to other countries, especially those in the East with a collectivistic culture. For example, given that individuals from many Eastern societies are more socially interdependent (Markus & Kitayama, 1991), they may have found social-distancing measures more difficult to adapt to than did those from an individualistic culture. As a result, the mere act of interacting with someone else might have been experienced as even more beneficial by them.

Culture may also influence the effects of reconstrual which were specifically investigated in Project 2. East Asians have been suggested to hold a stronger belief that reality is constantly changing (i.e., dialectical thinking) and thus expect and accept contradictions (Ji et al., 2001; Peng & Nisbett, 1999), so they may benefit even more than Westerners from reconstrual styles because they should feel more comfortable when their current cognitions about an experience are challenged. Additionally, Project 2 required all its participants to be undergraduate students, who are typically young adults. Prior research has showed that older adults benefit less from reappraisal than do younger adults (e.g., Opitz et al., 2012; Tucker et al., 2012). One reason is that people prefer to maximise positive affect in the present moment in later life (Charles & Carstensen, 2008), making reappraisal, which relies on engagement with negative information, less automatic and more effortful (Martins et al., 2018). Reappraisal may also become maladaptive for certain negative emotions as people age: For example, at an older age, regretful people may perceive that the opportunities to correct a past

behaviour are “running out,” so disengagement from those experiences may be a better approach to protecting emotional well-being (e.g., Brassen et al., 2012; Wrosch & Heckhausen, 2002). Future research should examine the role of culture and age in determining people’s susceptibility to cognitive support received during social sharing by recruiting more diverse samples.

Similarly, culture shapes people’s propensity to accept duality, which possibly moderates my findings of Projects 3-4 related to bivalent emotions. East Asians have been found to experience more bivalent emotions (Spencer-Rodgers et al., 2010a; for a review, see Goetz et al., 2008) and experience less discomfort associated with cognitive dissonance (Heine & Lehman, 1997). Considering this, sharers more exposed to East Asian cultures in Project 3 might have different needs during social sharing from those more exposed to Western cultures when dealing with their bivalent or multiple negative emotions, both of which are likely to induce feelings of conflict. Specifically, the former group of sharers, who tend to be less bothered by feelings of conflict, might not have desired listeners’ advice or suggestions about forming a simplified view of their complex experience as much as the latter. In the context of Project 4, a higher propensity to accept duality may heighten people’s sensitivity to and acceptance of outgroup members’ bivalent emotions which are believed to reflect cognitive complexity and humanness, resulting in improved evaluations of those members.

6.3.3 The Unaddressed Dynamic Nature of Emotions

All my projects, despite tackling different questions, centred on the big topic of human emotion. One of the important features of emotions, besides being social phenomena (for a review, see Parkinson & Manstead, 2015), is their dynamic nature (for a review, see Kuppens & Verduyn, 2017): Emotions are not a fleeting response to stimuli in the environment but tend to carry over from one moment to the next and are

continuously (up- or down-)regulated towards a desired state. Therefore, emotions fluctuate across time, requiring constant adjustments to the adopted regulation strategy. For example, Sheppes and colleagues (2011, 2014) found that the choice of emotion regulation strategies depends on the intensity of negative situations, such that people prefer to reappraise relatively low-intensity stimuli and to distract themselves from relatively high-intensity stimuli.

Study 2B suggested that a negative emotion seems to be more effectively regulated when the reappraisal strategy matched the needs of sharers experiencing that emotion. Specifically, I found that anger was effectively regulated by perspective-taking at the time when participants took part in the study, which was some time (approximately 24 hours) after the corresponding event had happened. Linking to my finding in Study 1 that sharers' positive affect was positively correlated with their satisfaction with listeners' feedback, the angry participants in Study 2B were probably satisfied with the listener's instruction to use perspective-taking, which in turn promoted regulatory success. However, given the dynamic nature of emotions (for a review, see Kuppens & Verduyn, 2017), these findings regarding social sharing may not apply to the full time span of an emotional state, especially when the repercussions of the corresponding event continue to unfold. For example, perspective-taking in Study 2B might not have worked well when sharers' anger was more intense at the beginning of the emotional experience. Their anger might become intense again when they were further wronged by their antagonist. In either case, perspective-taking would probably lose its positive impact because reappraisal is less preferred than distraction when facing relatively high-intensity negative situations (Sheppes et al., 2011, 2014). Similarly, the motives for sharing bivalent emotions reported in Study 3 are subject to the same caveat: Results showed that bivalent emotional experiences are associated with

less felt conflict as their negativity reduces over time, so people's regulatory needs are likely to vary too as their evaluations about the experience have changed. In sum, considering that people seem to spontaneously monitor their regulatory choice and flexibly switch between choices to accommodate their changing emotional state, future researchers should acknowledge that emotion regulation is not a "here-and-now" problem and include various regulatory stages in their investigations.

6.4 Implications for Applied Issues

Overall, the two lines of my DPhil research offer new insights into two modern societal issues: The emergence of online therapy and prejudice between groups. Researchers had long compared the efficacy of online therapies and that of in-person therapies in pre-pandemic times because many therapists were reluctant to let computers mediate their regular practices. One of their main concerns regarding online therapy was, even though empirical evidence suggests otherwise (e.g., Simpson & Reid, 2014), its supposedly reduced capacity to facilitate building a strong therapeutic alliance (for reviews, see Roesler, 2017 and Connolly et al., 2020), which has been linked to more successful support outcomes (e.g., Martin et al., 2000). However, during the outbreak of the COVID-19 pandemic, many therapists involuntarily transitioned to remote settings and as time went by, their worries about not being able to connect with patients drastically decreased (Békés et al., 2021). In terms of support outcomes, Project 2's findings show both that support-recipients generally perceive virtual support-providers as moderately warm and that the online format can successfully lead to improvement in support-recipients' affect, consistent with results from relevant pre-pandemic research (e.g., Kessler et al., 2009; Morris et al., 2015). The remote, rather than in-person, format

also appears more adaptive when people discuss second-hand negative events that have a collective impact, as shown in Project 1.

My findings suggest that disclosing one's emotions online yields positive psychological effects. Still, future research is needed to further substantiate these findings by investigating online sharing across a wider range of settings. For example, Project 2 used a virtual listener who was described as a peer who was not clinically trained, so it would be interesting to test whether it is easier for people to build an alliance with a peer listener online and consequently experience greater regulatory success than with a mental health professional. Also, future research can look into effects of online sharing in group interventions (e.g., support groups). Group interventions are often seen following a traumatic event that impacts a community (e.g., a tragic accident, a mass shooting). Given that online conversation is more accessible and affordable (and often more private) than the in-person format, the former may be more preferred by the community members. Also, as shown in Project 1, remote sharing seems a more effective approach to tackling collective, intractable problems than is in-person sharing. Taken together, it is worth testing if the positive effects of remote sharing observed presented in this thesis extend from dyadic to group settings. Finally, future research can compare different modes of remote sharing (e.g., text-based, video-mediated, audio-only) to see if support outcomes vary to different degrees when different kinds of signals that convey emotions are made available.

Furthermore, Project 2 revealed that the emotion-improving effects of online sharing are not moderated by the responsiveness of the support-provider, confirming previous findings that chatbots can be an impactful tool in clinical settings (for a review, see Vaidyam et al., 2019). That said, therapists should be cautious about their reliance on chatbots in their practices. Considering the complex and dynamic nature of human

emotions, people's regulatory needs tend to vary across time (for a review, see Kuppens & Verduyn, 2017), requiring the support-provider to flexibly adjust their current approach in order to grant them the most appropriate help needed. Such an ability to provide dynamically attuned responses is still not fully acquired by chatbots and thus may undermine support outcomes in the long run. However, recent developments in embodied Artificial Intelligence (AI) are making such interactivity a more realistic prospect in the virtual world. For example, Woebot, a therapeutic app which works on text messages, can flag expressions that indicate emotional distress (e.g., feeling sad over a breakup), verify the problem (e.g., "Oh, it looks like you're dealing with a relationship issue."), and offer corresponding advice (e.g., "There's usually more than one way to go about feeling better when our heart is hurting in a relationship."; Sachan, 2018; p. 547). Another chatbot, Wysa, is able to crack a joke after checking the user's energy levels (Sachan, 2018). Perhaps in the near future, chatbots will be "smart" enough to detect the complexity and temporal variability of human emotions and provide tuned-in responses for users as if they are talking to a real person. Future researchers should continue following up on the developments of AI and comparing the outcomes of AI-directed therapy to those of human-directed therapy in order to update on the possible application areas of chatbots in therapeutic settings.

In Project 4, I failed to find evidence for perceiving outgroup members' bivalent emotions having any effects on intergroup relations. However, given the flexibility function of bivalent emotions, an alternative approach that is worth examining is letting ingroup members personally experience bivalent emotions before having them evaluate outgroups. Stereotypes and prejudice are forms of heuristics (Bigler & Clark, 2014): People often overlook other relevant information and generate knowledge and judgments of other people simply based on their group memberships. Bivalent

emotions, on the other hand, should make people's judgmental confidence fall short of their sufficiency threshold (for the rationale of this argument, see Bohner et al., 1995) and inform people of the contradictory elements in the environment (for a review, see Rothman & Melwanie, 2017), thereby motivating them to engage in systematic processing (Jonas et al., 1997; Petty & Cacioppo, 1986) and consider alternative perspectives (Rees et al., 2013). Therefore, theoretically, compared to individuals who are in a univalent emotional state, those in a bivalent state would be more likely to suppress their stereotypical thinking and form less biased judgments about outgroups. They would also be more receptive to counter-stereotypical information about those members (e.g., their possession of emotional complexity). In a world with frequently occurring cases of discrimination based on which group people belong to, future research is warranted to examine the possibility of encouraging bivalent emotional experiences as an intervention.

6.5 Conclusions

This thesis has aimed to improve the understanding of human emotions, in both interpersonal and intergroup contexts, using online questionnaires and experiments. Through Chapters 2-4, emotions were investigated in the framework of social sharing: Project 1 suggested that people tended to feel better about their shared negative experiences related to the COVID-19 pandemic (especially when their listener provided more satisfactory feedback) than about their non-shared ones, and that such affect improvement seemed greater when the shared experience was a personal event than a second-hand news story that has a collective impact. Focusing on sharing personal events, Project 2, especially Study 2B, showed that reconstrual generally yielded more positive psychological effects than did recounting, and that giving people specific

advice on which reconstrual style (i.e., which form of reappraisal) to use for each kind of negative emotional response led to greater regulatory success than a generic instruction to reconstrue. Complementarily, I found in Study 3 that people feeling negative considered venting, finding meaning and seeking emotional and cognitive support as significantly more important motives for sharing their emotions, revealing the needs of people in this kind of emotional state. In sum, even though people commonly believe that simply venting their negative experiences reduces their emotional impact, my findings from these three projects highlight the crucial role of the responses people receive from their listeners in determining the adaptiveness of social sharing. Specifically, sharers' distress seems to be alleviated more effectively when their needs are correctly identified and fulfilled.

Through Chapters 4-5, bivalent emotions were investigated: Project 3 found that the motives for sharing this kind of experience were the same as those for sharing negative experiences, except that venting was not rated as significantly important as in the latter case. Also, both the experience of two opposite-valence emotions and that of two negative emotions induced feelings of conflict, which seemed to reduce over time. However, while people seemed to cope with their negative experiences by attenuating their negative affect and boosting their positive affect, they seemed to deal with their bivalent experiences by focusing on attenuating their negative evaluations in particular. In Project 4, I touched on the cognitive functions of bivalent emotions. Although bivalent emotions were believed to be a strong indicator of cognitive complexity and human uniqueness, presenting outgroup members' bivalent emotions failed to improve intergroup relations by causing any change to the way people attributed emotions (which was shown to be under the influence of both infrahumanisation and social preference) to those members. Still, given people's positive perceptions of and the

flexibility function of bivalent emotions, it is worth continuing to explore this kind of emotional state as a route to altering people's cognitions.

To summarise, this thesis offers new insights into research on social sharing of emotion, emotion regulation, crisis, bivalent emotions, and inhumanisation. This thesis also inspires different avenues for future research. Two of which are online therapy and intergroup prejudice, which are important issues that have profoundly impacted the world.

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APPENDICES

Appendix A

A.1. Effects of Order of Completing Part 2 Sub-Questionnaires on Baseline

Emotion Ratings

Independent samples t-tests showed that the participants' baseline positive ($M_{\text{personal experience-first}} = 1.00$, $SD_{\text{personal experience-first}} = 1.00$; $M_{\text{news story-first}} = 0.94$, $SD_{\text{news story-first}} = 0.98$) and negative ($M_{\text{personal experience-first}} = 3.86$, $SD_{\text{personal experience-first}} = 0.93$; $M_{\text{news story-first}} = 4.03$, $SD_{\text{news story-first}} = 1.00$) feelings about their personal experience were not significantly different ($ps > .20$) for those who reported a personal experience first and those who reported a news story first.

However, in the case of news stories, participants' positive emotion ratings were significantly higher ($M_{\text{personal experience-first}} = 0.77$, $SD_{\text{personal experience-first}} = 1.02$; $M_{\text{news story-first}} = 1.23$, $SD_{\text{news story-first}} = 1.27$; $t(283) = -3.09$, $p = .002$, $d = -.43$) and negative emotion ratings were significantly lower ($M_{\text{personal experience-first}} = 3.98$, $SD_{\text{personal experience-first}} = 1.05$; $M_{\text{news story-first}} = 3.62$, $SD_{\text{news story-first}} = 1.26$; $t(283) = 2.31$, $p = .02$, $d = .32$) when they reported the news story first than when they reported the personal experience first.

A.2. Relationship Between Sharing Frequency and Perceived Affect Change

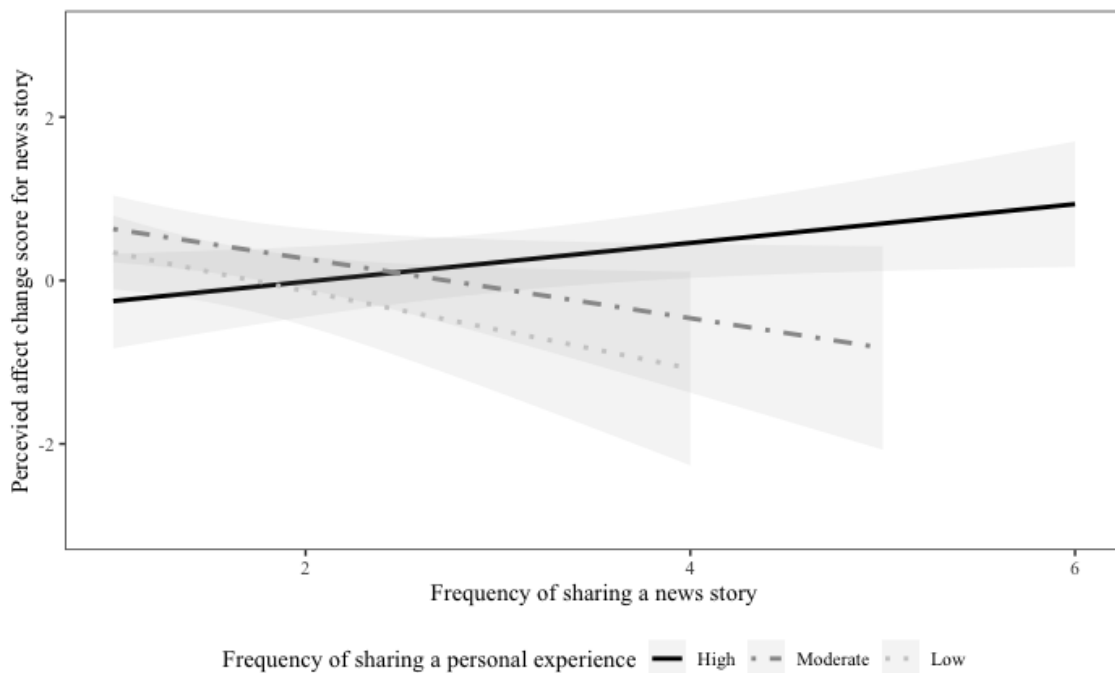
I used the subset of participants who shared both events ($N = 142$) to test if participants' frequencies of sharing their personal experience and news story during the 24-hour interval were correlated with their perceived affect change scores for each of the events.

Six participants who gave extremely large numbers as their responses to the sharing frequency questions were considered outliers ($> 3 SD$ from the mean) and

excluded from this analysis, leaving an updated sample size of 136. I ran a multivariate regression model with sharing frequency for each event as predictors and perceived affect change scores for each event as outcomes. The inclusion of country as a predictor did not provide a significantly better fit ($p = .36$), so we ran the model without it.

Figure 7

Perceived Affect Change for News Story as a Function of Frequency of Sharing the Same Event and Frequency of Sharing the Personal Experience



Note. Frequency of sharing a personal experience was a continuous variable but is presented as three categories in this figure to better visualize the interaction. Low, moderate, and high levels of sharing were respectively modelling the participants who talked about their personal experience 1 time (the minimum number; $N = 49$), 2 times (the median number; $N = 42$), and more than 2 times ($N = 45$). Confidence interval (CI) is 95%.

While no significant relationships between the predictors and perceived affect change scores for personal experiences were observed ($ps > .12$), I found a significant negative relationship between frequency of sharing a news story and perceived affect change scores for the same event ($b = -0.51$, $SE = 0.18$, $p = .005$, 95% CI $[-0.86, -0.15]$).

However, a significant interaction suggested that this relationship depended on frequency of sharing a personal experience ($b = 0.14$, $SE = 0.050$, $p = .01$, 95% CI [0.04, 0.24]). The relationship between frequency of sharing a personal experience and perceived affect changes for news story was not significant ($p = .12$). As shown in Figure 7, the more often participants talked about their news story *and* personal experience, the greater the emotional improvement they perceived for the news story.

A.3 Effects of Sharing vs. Not Sharing on Perceived Affect Change

I ran linear multiple regressions to check the overall effects of sharing versus not sharing on improvement in affect. Most of the full models that included country as a covariate failed to provide a significantly better fit ($ps > .06$), so the reduced models that excluded the covariate were used in these instances. In the reduced models, participants' binary response (yes or no) to whether they had shared the personal experience within the 24-hour interval was one of the predictors, and their response to whether they had shared the news story was the other predictor. The reference levels of these predictors were set to "no". Each model tested for main effects of these two predictors and their two-way interactions on a single outcome variable, which was perceived affect change for one of the events, while checking if T1 positive or negative emotion about the same event type (the second covariate) had any effects on the outcome variable.

Results showed that after controlling for T1 negative emotion about personal experiences, the perceived affect change score for this kind of event was significantly higher when the personal experience alone was shared than when neither event was ($b = 0.56$, $SE = 0.25$, $p = .02$, 95% CI [0.08, 1.05]). The change score when the news story alone was shared, on the other hand, did not significantly differ from that when neither

event was shared ($p = .32$). Also, the interaction between the sharing of the personal experience and that of the news story was not significant ($p = .81$), suggesting that the effect of the sharing of either event did not depend on the sharing of the other event. Notably, the control variable of T1 negative emotion was a significant negative predictor of the change score, $b = -0.19$, $SE = 0.08$, $p = .02$, 95% CI $[-0.34, -0.03]$. When T1 positive emotion about personal experiences was controlled for, similar results were observed: The change score was significantly higher when the personal experience alone was shared than when neither event was shared ($b = 0.54$, $SE = 0.25$, $p = .03$, 95% CI $[0.06, 1.03]$). The change score when the news story alone was shared, on the other hand, did not significantly differ from that when neither event was shared ($p = .31$). Also, the interaction between the sharing of the personal experience and that of the news story was not significant ($p = .74$). Finally, T1 positive emotion was a significant positive predictor of the change score, $b = 0.20$, $SE = 0.07$, $p = .01$, 95% CI $[0.06, 0.35]$.

As for the perceived affect change score for news stories, after controlling for T1 negative emotion about this kind of event, it was significantly higher when the new story alone was shared than when neither event was shared ($b = 0.58$, $SE = 0.25$, $p = .02$, 95% CI $[0.08, 1.08]$). The change score when the personal experience alone was shared, on the other hand, did not significantly differ from that when neither event was shared ($p = .44$). Also, the interaction between the sharing of the personal experience and that of the news story was not significant ($p = .32$). The control variable of T1 negative emotion about news stories was also not a significant predictor ($p = .06$). Notably, for this particular analysis, I used the full model which included country as a covariate because this model ($AIC = 924.91$) yielded significantly better fit than the reduced model ($AIC = 927.59$; $p = .03$). Specifically, we found that the change score

provided by UK participants ($M = 0.28$, $SD = 1.07$) was significantly higher than that by US ones ($M = -0.08$, $SD = 1.36$; $b = -0.31$, $SE = 0.14$, $p = .03$, 95% CI $[-0.60, -0.03]$). Similar results were yielded when T1 positive emotion about news stories was controlled for: The change score was significantly higher when the news story alone was shared than when neither event was shared ($b = 0.59$, $SE = 0.26$, $p = .02$, 95% CI $[0.09, 1.09]$). The change score when the personal experience alone was shared, on the other hand, did not significantly differ from that when neither event was shared ($p = .40$). Also, the interaction between the sharing of the personal experience and that of the news story was not significant ($p = .29$). T1 positive emotion was also not a significant predictor of the change score ($p = .28$). Again, for this particular analysis, I used the full model which included country as a covariate because this model ($AIC = 927.37$) yielded significantly better fit than the reduced model ($AIC = 929.86$; $p = .04$). Specifically, the same country difference was observed ($b = -0.31$, $SE = 0.15$, $p = .04$, 95% CI $[-0.60, -0.02]$).

A.4 Effects of In-Person vs. Remote Sharing on Perceived Affect Change

Using the same subset of participants who shared both events ($N = 141$)²⁶, I next tested whether perceived affect change about each kind of the shared events depended on the communication method after controlling for T1 emotion about the corresponding event. The full models that included country as a covariate failed to provide a significantly better fit in all cases ($ps > .14$), so the reduced models that excluded the covariate were used throughout. The reduced models contained two predictors: The method of sharing the personal experience (in-person or remote) and that of sharing the

²⁶ One participant who failed to specify how the discussions were conducted was excluded for this particular analysis.

news story. Each model tested for main effects of these two predictors and their two-way interactions on participants' perceived affect change for one of the events, while checking if T1 positive or negative emotion about the same event type (the second covariate) had any effects on the outcome variable. I set the reference level of each of the predictors to "remote," so that each intercept in the output represented the mean perceived affect change score for each event when both events were discussed remotely.

Results showed that the change score for the personal experience did not depend on whether each of the two events was shared in-person or remotely after controlling for T1 negative or positive emotion about personal experiences ($ps > .26$). T1 negative emotion was a significant negative predictor of the change score ($b = -0.23$, $SE = 0.11$, $p = .03$, 95% CI $[-0.45, -0.02]$), but T1 positive emotion did not have a significant effect ($p = .08$).

On the other hand, the change score for the news story was dependent on the communication method for sharing each event. Specifically, the change score was significantly lower when the news story alone was shared in-person than when both events were shared remotely ($b = -0.74$, $SE = 0.37$, $p = .05$, 95% CI $[-1.47, -0.02]$) after controlling for T1 negative emotion about news stories. The change score when the personal experience alone was shared in-person, on the other hand, did not significantly differ from that when both events were shared remotely ($p = .34$). Also, the interaction between the method of sharing the personal experience and that of sharing the news story was not significant ($p = .14$). Similar results were observed when T1 positive emotion about news stories was controlled for: The change score was significantly lower when the news story alone was shared in-person than when both events were shared remotely ($b = -0.75$, $SE = 0.37$, $p = .04$, 95% CI $[-1.48, -0.02]$). The change score when the personal experience alone was shared in-person, on the other hand, did not

significantly differ from that when both events were shared remotely ($p = .44$). Also, the interaction between the method of sharing the personal experience and that of sharing the news story was not significant ($p = .17$). It should also be noted that neither T1 negative nor positive emotion was a significant predictor of the change score ($ps > .15$).

A.5 Additional Measures at Each Stage of Study 1

A.5.1 Part 1

In addition to demographic information (age, gender, and country of residence), I asked participants to:

- Provide their assessment of the psychological atmosphere of their country in the context of COVID-19 using the Perceived Emotional Climate Scale (PECS; Páez et al., 1996, as cited in Rimé et al., 2017),
- Rate to what extent they were comfortable discussing their feelings with the people they were living with at the time when they took part in the study on a 0 (“Not at all”) to 5 (“Extremely”) scale²⁷,
- Indicate their education level, ethnicity, and perceived socio-economic status (using the adult version of the MacArthur Scale of Subjective Social Status; Adler et al., 2000),
- Assess their mental rumination using four items (e.g., “How frequently do images and thoughts related to COVID-19 events come involuntarily to your mind?”; adapted from Rimé et al., 2010),
- Rate how frequently they had spoken and heard about COVID-19 events and heard people close to them talking about those events in the last month on 1 (“Not at all”) to 7 (“A great deal”) scales, and

²⁷ Those who were living alone were instructed to choose the option “Not Applicable”.

- Rate how concerned they and people close to them were about COVID-19, as well as how much their own life and the lives of people close to them had been affected by COVID-19 on 1 (“Not at all”) to 7 (“A great deal”) scales.

A.5.2 Part 2

In each sub-questionnaire, participants not only described the event (a COVID-related personal experience or news story) they were reporting and rated their positive and negative emotions but also:

- Rated to what extent they felt distressed, upset, nervous, jittery, and afraid during the experience on 1 (“Very slightly or not at all”) to 5 (“Extremely”) scales, with 3 representing “Moderately” (items selected from the Positive and Negative Affect Schedule; also known as the PANAS; Watson et al., 1988), and
- Rated how much they wanted to discuss the experience with anyone else immediately after it happened on a 0 (“Not at all”) to 5 (“Extremely”) scale.

Participants were also asked to indicate through which medium (in-person conversation, audio call, video chat, text messages, social media posts, traditional media, emails, or other) they first heard about the news story they were reporting.

A.5.3 Part 3

For each event that participants indicated discussed during the 24-hour interval, I also measured:

- With how many people they discussed the experience during the interval,
- Who the first person (parent, sibling, relative, romantic partner, friend, acquaintance, or other) was that they discussed the experience with,
- Whether this first person was someone they were living with,
- How much information of each of the following kinds (details of the event itself, their feelings during the experience, and their thoughts at the moment during the

discussion) they shared with this first person during their first discussion on 1 (“Very little”) to 5 (“Very much”) scales²⁸,

- Their co-rumination levels with this first person using the abridged version of Co-Rumination Questionnaire (CRQ-9; Van der Löwe, 2013; originally developed by Rose, 2002),
- Which medium (in-person conversation, audio call, video chat, text messages, emails, social media posts, or other) they used for their first discussion, When their first discussion happened,
- How positive and negative they perceived this first person’s feelings were during their first discussion on 0 (“Not at all”) to 5 (“Extremely”) scales,
- How distressed, upset, nervous, jittery, and afraid they perceived this first person were during the first discussion on 1 (“Very slightly or not at all”) to 5 (“Extremely”) scales, with 3 representing “Moderately,”
- How distressed, upset, nervous, jittery, and afraid they felt about the experience after their first discussion on 1 (“Very slightly or not at all”) to 5 (“Extremely”) scales, with 3 representing “Moderately,”
- Whether this first person was the same person whom they first discussed the other negative event with, and if it was, whether they discussed both events with this person during the same conversation.

As for those who indicated that they did not discuss the event at all within the 24-hour interval, they were asked to rate how distressed, upset, nervous, jittery, and afraid they felt about the experience after the 24 hours since it happened.

²⁸ Participants were instructed to choose the option “Not Applicable” if they did not share the kind of information in question at all.

Appendix B

B.1 List of Questions Used for Manipulations of Chatting Style in Study 2A

B.1.1 Recounting

In the interpersonal conflict condition:

1. Can you tell me what happened exactly from start to finish? And what you felt in more detail?
2. So aside from feeling this way, what went through your mind while this was happening?
3. And what stuck in your mind the most while things were happening?
4. We were talking about what you felt - perhaps you can also tell me what X²⁹ said or did that specifically upset you the most?
5. Did you have a chance to talk to X about how you felt after what happened?

In the COVID news condition:

1. Can you tell me what you felt in detail when you found out about it?
2. So aside from feeling this way, what went through your mind when you found out about the news?
3. And what stuck in your mind the most when you found out about the news?
4. We were talking about what you felt - perhaps you can also tell me what the reaction was among people around you? For example, did they care about the news as much as you? Did they say anything about it?
5. And how did you feel about their reaction?

B.1.2 Reconstructual

In the interpersonal conflict condition:

²⁹ “X” refers to the other person(s) involved in the interpersonal conflict in question.

1. Can you tell me why it's been concerning you, looking at what happened?
2. And to think a little deeper, why do you think you felt that way?
3. Now you've told me about your feelings, but why do you think X behaved in that way?
4. We were talking about why X behaved in that way - perhaps what happened also changed the way you think about things more generally? Can you tell me about that?
5. In the overall scheme of things, if you look at the "big picture", can you make better sense of why what happened has affected you in this way?

In the COVID news condition:

1. Can you tell me why it's been concerning you, looking at what happened?
2. And to think a little deeper, why do you think you felt that way?
3. Did people around you care about the news as much as you do? And why do you think they reacted that way?
4. We were talking about the reaction among people around you - perhaps this piece of news also changed the way you think about things more generally? Can you tell me about that?
5. In the overall scheme of things, if you look at the "big picture", can you make better sense of why finding out about this news has affected you in this way?

B.2 List of Questions Used for Manipulations of Chatting Style in Study 2B

B.2.1 Recounting

1. Can you describe what happened from start to finish, just to be a little more specific? And what you felt in detail?
2. So can you tell me what stuck out the most in your mind during all this?
3. Perhaps you can also tell me in more detail about X's behaviour in the situation?

B.2.2 Stimulus-Reappraisal

1. What do you think it was about this situation that made you feel the most [anger/regret]?
2. So can you try to describe to me a more positive way of interpreting the situation?
3. Does considering an alternative interpretation make a difference to how you feel about the situation?

B.2.3 Perspective-Taking

1. Could you describe this situation from a new perspective? Let's say, imagine that you are a fly on the wall observing how things unfold rather than being directly involved.
2. So considering this different perspective, why do you think the situation unfolded the way it did?
3. Can you describe why you felt [angry/regretful] in this situation using the third person?

B.3 Additional Measures in Studies 2A and 2B

B.3.1 Study 2A

In Stage 1, in addition to demographic information (age, gender, and ethnicity), I asked participants to:

- Rate to what extent they usually focused on describing the details vs. making sense of the negative events during their conversations with people who were close to them on a 1 (“Predominantly describing the details”) to 7 (“Predominantly making sense of the events”), with 4 representing “Both, more or less equally”,
- Think about the way they usually were when talking to the people close to them about negative events and assess their co-rumination levels with those people using the abridged version of Co-Rumination Questionnaire (CRQ-9; Van der Löwe, 2013; originally developed by Rose, 2002), and
- Assess their tendency to engage in cognitive reappraisal to regulate their emotions using the Cognitive Reappraisal subscale of the Emotion Regulation Questionnaire (ERQ; Gross & John, 2003).

B.3.2 Study 2B

Same additional measures were included in Stage 1. Towards the end of Stage 2, I additionally asked the question “How beneficial do you think chatting, regardless of its format and content, is for your mental well-being under the current circumstances?” in the belief that this measure would offer insights into how the mere act of chatting helps improve people’s emotions in the midst of the collective crisis of COVID-19.

The mean rating was 4.41 ($SD = 1.63$) and was not significant different across styles and emotions ($ps > .20$), suggesting that chatting was generally perceived as moderately beneficial to their mental well-being in the pandemic situation. Adding

chatting perception to the 2 x 3 x 2 model used in our main analyses, I did not find it to have significantly moderated any effects of my manipulations on outcome variables ($ps > .10$).

Appendix C

C.1 Abridged Version of SSMS

Motive Category	Item
Clarification/Meaning (CM)	1. Better understand what happened
	2. Find meaning in what happened
	3. Clarify my feelings about what happened
Rehearsing (R)	4. Re-experience the moment
	5. Share the experience
	6. Cause surprise
Venting (VE)	7. Express my emotions
	8. Let off steam
	9. Let my emotions out
Arousing empathy/attention (AEA)	10. Generate interest
	11. Move other people
	12. Make myself stand out
Informing and/or warning (IW)	13. Inform other people about what happened
	14. Inform other people about the situation
	15. Warn other people
Assistance/Support and comfort/consolation (HC)	16. Be supported
	17. Feel I could rely on somebody
	18. Be helped
Advice/Solutions (AS)	19. Learn about other people's opinion
	20. Get an outside perspective
	21. Receive suggestions

C.2 Additional Measures at Each Stage of Study 3

C.2.1 Stage 1

In addition to demographic information (age, gender, and ethnicity), I asked participants to assess their tendency to engage in cognitive reappraisal and expressive suppression to regulate their emotions using the full version of the Emotion Regulation Questionnaire (ERQ; Gross & John, 2003).

C.2.2 Stage 2

Participants who reported a personal experience of their assigned emotion type not only described the event they were reporting and rated their positive and negative emotions and felt conflict but also responded to the question “Did you feel tense during the experience?” by providing a binary answer (“yes”/“no”). Those who answered “yes” were then asked to rate how tense they were during the experience on a 1 (“Slightly”) to 7 (“Extremely”) scale, with 4 representing “Moderately”. Questions were adapted from Ocejja and Carrera (2009).

C.2.3 Stage 3

Regardless of whether participants had shared their experience, all of them were asked to assess their positive and negative emotions, felt conflict, and felt tense the same way as they did in Stage 2. Towards the end of the study, they were additionally asked to provide their views on bivalent emotions in relative to the experience of multiple same-valence emotions by answering three questions using 1 (“Not at all”) to 7 (“Very much”) scales. One-way ANOVAs were performed to check if different ratings were provided by participants in different emotion conditions.

i) Humanness

Participants were asked “To what extent do you think the ability to experience opposite-valence mixed emotions is more uniquely human (that is, this ability is not

shared with other animals), compared to same-valence mixed emotions?”. The mean rating of this item was 4.96 ($SD = 1.48$), which was significantly greater than the midpoint of 4 ($t(211) = 9.47, p < .001, d = .65$), meaning that participants generally perceived the ability to experience bivalent emotions as more uniquely human than the ability to experience univalent emotions. There were no significant differences in ratings across emotion conditions ($p = .79$).

ii) Mind Complexity

Participants were asked “To what extent do you think the ability to experience opposite-valence mixed emotions is more of an indicator of a complex mind, compared to same-valence mixed emotions?”. The mean rating of this item was 4.75 ($SD = 1.52$), which was significantly greater than the midpoint of 4 ($t(211) = 7.13, p < .001, d = .49$), meaning that participants generally perceived the ability to experience bivalent emotions as more of an indicator of a complex mind than the ability to experience univalent emotions. There were no significant differences in ratings across emotion conditions ($p = .76$).

iii) Social Desirability

Participants were asked “To what extent do you think the ability to experience opposite-valence mixed emotions is more of a socially desirable characteristic, compared to same-valence mixed emotions?”. The mean rating of this item was 3.95 ($SD = 1.50$), which was not significantly different from the midpoint of 4 ($p = .65$), meaning that participants did not perceive the ability to experience bivalent emotions as more of a socially desirable characteristic than the ability to experience univalent emotions. There were no significant differences in ratings across emotion conditions ($p = .42$).

Appendix D

D.1 Means and Standard Deviations of Knowledge, Perceived Warmth and Competence, and Blatant Dehumanisation for Six Outgroups

Variable	Drug addicts		Anti-vaxxers		Climate change deniers	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Knowledge	45.59	31.13	50.26	26.25	32.79	28.58
Perceived warmth	2.76	1.10	2.91	1.54	2.91	1.40
Perceived competence	3.38	1.72	3.35	1.89	3.74	1.76
Blatant dehumanisation	86.12	20.69	84.32	21.09	83.26	22.66

Variable	Uzbeks		Luxembourgers		Sammarinese	
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>
Knowledge	3.88	6.44	8.18	12.03	3.26	6.92
Perceived warmth	4.68	1.04	4.97	1.09	4.71	1.09
Perceived competence	4.35	1.67	4.53	1.62	4.35	1.77
Blatant dehumanisation	89.91	17.28	94.62	10.40	92.09	15.56

Note. *N* = 34.

D.2 The First Excerpt about Each Outgroup

D.2.1 Drug Addicts

“Drug addicts are individuals who compulsively seek and use drugs. More than 0.4% people around the world suffer from drug addiction, according to a recent report released by the United Nations Office on Drugs and Crime (UNODC).”

D.2.2 Uzbeks

“Uzbeks are Central Asia’s largest population group. Although they make up over 0.4% of the world’s population, very little is known about them among people in the West.”

D.3 The Second Excerpt about Each Outgroup

D.3.1 Drug Addicts

“In order to increase knowledge about this population, interviewers from the British Broadcasting Corporation (BBC) went to a rehabilitation centre and spent several days getting to know a group of drug addicts there. The interviewers remarked on the changing emotional tone as they got to know the drug addicts. The drug addicts frequently expressed [first emotion pair]; then over time, these emotions changed to [second emotion pair].

Interviews with a psychologist, who is a long-time employer of the rehabilitation centre, confirmed that this pattern of emotions is typical of drug addicts.”

D.3.2 Uzbeks

“In order to increase knowledge about this population, interviewers from the British Broadcasting Corporation (BBC) went to Uzbekistan and spent several days getting to know a village of traditional Uzbeks, in the Namangan province to the east of the capital Tashkent. The interviewers remarked on the changing emotional tone as they

got to know the Uzbeks. The Uzbeks frequently expressed [first emotion pair]; then over time, these emotions changed to [second emotion pair]. Interviews with an anthropologist, who is a long-time resident of Uzbekistan, confirmed that this pattern of emotions is typical of the Uzbeks.”