RUNNING HEAD: Psychological distance in emotion attribution

# Happier than thou?
# A self-enhancement bias in emotion attribution

Desmond C. Ong, Noah D. Goodman, and Jamil Zaki
Department of Psychology, Stanford University

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Address Correspondence to:

Desmond C. Ong
Department of Psychology
Stanford University
Stanford, CA 94305
dco@stanford.edu

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## ABSTRACT

People tend to judge themselves as exhibiting above average levels of desirable traits—including competence, kindness, and life satisfaction—but does this self-enhancement extend to emotional responses? Here, we explore this question by having people attribute emotions to themselves and others following simple gambles. We demonstrate that people display an *emotional self-enhancement bias* that varies with the context of the emotion-eliciting situation. People judge themselves as experiencing more positive emotional reactions on average, and they also believed that others' emotions are more sensitive to gamble outcomes, such that people judge others to experience stronger negative affect in response to negative outcomes (Study 1). This self-enhancement bias further tracks social distance, such that people attribute less positive and more negative emotion to more dissimilar, as compared to more similar others (Study 2). People also predict less favorable emotional states for themselves and others experiencing events *in the future*, as compared to the present (Study 3), suggesting that this attribution bias extends across multiple dimensions of psychological distance. Broadly, these data suggest that people exhibit self-enhancement in emotion attribution, but do so in subtle ways that depend on situational and social factors.

**Keywords**: Emotion; Attribution; Inference; Self-enhancement; Psychological distance

People tend to view themselves in an unrealistically positive light. For instance, individuals judge themselves to be more competent, kind, attractive, and likely to succeed than the average person (Blaine & Crocker, 1993; Dunning, Meyerowitz, & Holzberg, 1989; Mezulis, Abramson, Hyde, & Hankin, 2004; Taylor & Brown, 1988). This tendency to *self-enhance* occurs when people make global evaluations of their personal attributes or *traits*, relative to that of the average person (Brown, 1986; Chambers & Windschitl, 2004; Hughes & Zaki, 2015). Here, we examined whether such self-enhancement extends to people's judgments of *emotional states*.

Emotional states are discrete, momentary responses that arise in response to motivationally relevant events, such as arguing with a friend or winning an award (e.g., Barrett, Mesquita, Ochsner, & Gross, 2007; Ortony, Clore, & Collins, 1988). At least two properties of emotions differentiate them from the traits typically examined in self-enhancement research. First, emotional states are relatively transient, often lasting on the order of seconds. Traits like one's competence and kindness, by contrast, remain constant over much longer timescales. Second, emotional states are context-dependent—they arise in response to specific events—while traits are generally construed as global evaluations that are less susceptible to contextual variation. Traits might thus be uniquely amenable to self-enhancement. Consider a typical "self-enhancement experiment", where individuals are asked to make abstract, generalized attributions, like judging their own trustworthiness compared to that of the average person. When making these judgments, people can flexibly draw on favorable information about themselves (e.g., Dunning et al, 1989), and need not be limited to specific instances of trustworthy behavior. This also applies to work in which people report higher levels of life satisfaction and other items of

Subjective Well-Being (SWB) than their peers (Lykken & Tellegen, 1996; Vautier & Bonnefon, 2008; Wojcik & Ditto, 2014)[1].

By contrast, making a judgment of an emotional state requires considering the context in which the emotion arises (e.g., Ong, Zaki, & Goodman, 2015; Zaki & Ochsner, 2011). The context-sensitivity of emotion inference, or *affective cognition*, sets up multiple predictions about how people might self-enhance for emotional states. On the one hand, judgments about one's own emotional states could approximate self-enhancement over traits like trustworthiness and SWB (Lykken & Tellegen, 1996; Wojcik & Ditto, 2014). In this case, individuals would *globally* self-enhance—attributing more positive emotions to themselves, versus others, irrespective of how "good" or "bad" a situation is. This prediction is represented in **Figure 1a**.

A second prediction focuses on people's sensitivity to situation features, and emerges from research on *affective forecasting*—people's predictions about their future emotional responses. Work in this domain reliably demonstrates that people overestimate the extent to which discrete events, such as winning the lottery or breaking a limb, will impact their emotional states (Gilbert, Pinel, Wilsom, Blumberg, & Wheatley, 1998; Wilson & Gilbert, 2003). This phenomenon arises in part because of a *focalism bias* that characterizes people's over-reliance on a "focal" event when predicting emotional responses (Schkade & Kahneman, 1998; Wilson, Wheatley, Meyers, Gilbert, & Axsom, 2000).

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[1] At least in English, laypeople and scientists alike often do not distinguish between "happy" emotional states, "happy" moods, a temperamentally "happy" individual, or global "happiness with life". In this manuscript, we use "happy" to refer to the emotional state, while we consider items like "happiness with life" as part of SWB (e.g. Lykken & Tellegen, 1996; Vautier & Bonnefon, 2008; Wojcik & Ditto, 2014).

The focalism bias characterizes people's forecasts about their own future emotions, but it could also affect their judgments about *others'* emotions. Recent theories describe both temporal distance—e.g., predictions about the future—and social distance—e.g., inferences about others—as two sub-types of a more general "psychological distance" (Maglio, Lieberman, & Trope, 2013; Parkinson, Liu, & Wheatley, 2014; Trope & Liberman, 2010). Both social and temporal distance alters people's inferences and decision-making in similar ways (e.g., Bar-Anan, Liberman, Trope, & Algom, 2007; Pronin, Olivola, & Kennedy, 2008; Soderberg, Callahan, Kochersberger, Amit, & Ledgerwood, 2015; Tamir & Mitchell, 2011). For example, when deciding how much time to contribute to help tutor fellow schoolmates, students offered less help if they thought they would help "this week", than if they thought they would help "next semester". Similarly, students themselves offered less help as compared to how much they thought other freshmen would help (Pronin et al, 2008). Returning to emotion judgments, we predict that focalism—which affects judgments about emotions across temporal distance—might similarly apply to judgments about emotions across social distance. People have less information to draw from when drawing inferences about either socially or temporally distant events, and thus might suffer from a focalism bias in both cases. If this is the case, people should over-emphasize the emotionally relevant features of a situation when making judgments about others' emotions, as compared to their own. Thus, we may predict that social distance would increase people's sensitivity towards situation features, as represented by **Figure 1b**. For instance, individuals might overestimate the emotional impact that winning an award, or getting into an argument, might have on others, as compared to themselves.

The previous two predictions—self-enhancement and increased sensitivity—while seemingly different, are not mutually exclusive. In fact, they offer potentially complementary predictions about the nuances of affective cognition. Global self-enhancement, from studies on social comparison, is thought to stem from underlying *motivational* forces, and can be operationalized as a "difference in the means" (or intercepts; **Fig. 1a**). Increased sensitivity, from studies on affective forecasting, is framed as part of the *cognitive process* of reasoning about emotions, and can be operationalized as a "difference in the slopes" (**Fig. 1b**). A third possibility is that emotional state attributions across social distance could exhibit *both* a global self-enhancement and an increased sensitivity bias. That is, people might globally attribute more positive emotional reactions to themselves as compared to others, and simultaneously exhibit an increased sensitivity to outcomes when rating others' emotional responses. If this is the case, then we might expect the disparity between self- and other-oriented emotion attributions to be especially marked as outcomes become more negative, as represented by **Figure 1c**. Under this *contextualized self-enhancement* model, there may be little to no difference between self and other attributions of emotions following positive events: people would feel similarly positive as they think others will when good things happen. But following a negative event, people's self-attributions would be more positive (less negative) than their attributions of others' emotions following that same negative event. This prediction is also consistent with previous research suggesting that self-enhancement might serve to buffer against negative, self-threatening information, and hence be strongest in negative situations (e.g. Alicke & Sedikides, 2009; Taylor & Brown, 1988).

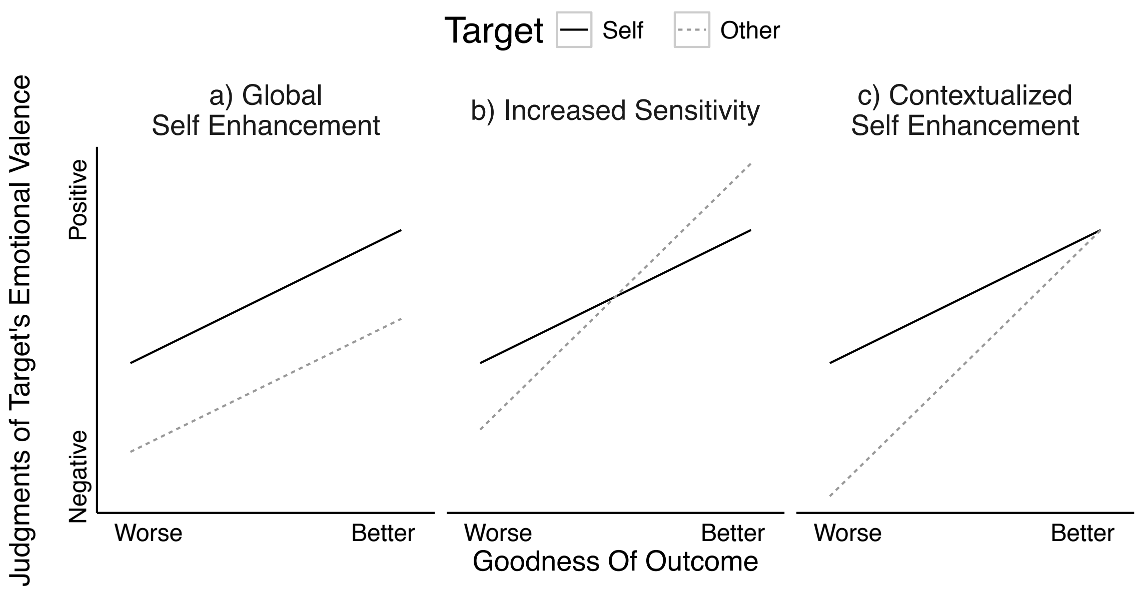


Figure 1. Predictions. On the vertical axis are attributions of emotional valence following an outcome: negative valence at the bottom and positive valence towards the top. On the horizontal axes are how "good" or "bad" the outcome is. Overall, emotional valence attributions should increase with the "goodness" of the outcome. Predictions for attributions to oneself are given in solid lines, while attributions to others are given in dashed lines. (a) Predictions from a context-independent global self-enhancement bias. (b) Predictions from an "increased sensitivity" account: attributions to others are more sensitive to changes in the outcome. (c) Combining both (a) and (b) to get *contextualized self-enhancement*.

In this work, we test these three plausible models of affective cognition by leveraging a paradigm we developed to examine emotion attribution under tightly controlled circumstances (Ong, Zaki, & Goodman, 2015). In this paradigm, participants view another person playing a monetary gamble, and then infer the emotions they believe that person feels in response to the outcome of the gamble. Here, we extend this paradigm by comparing participants' inference about another person's emotions to inferences about their own emotions after playing the gamble. This affords at least two methodological advantages. First, we can vary the parameters of the gambles that participants observe, which allowed us to vary the "goodness" of the outcome (the horizontal axes in Fig. 1) in a systematic fashion. Classic theories argue that people's subjective utility tracks not the absolute value of an outcome, but rather the extent to which it exceeds expectations (Kahneman & Tversky, 1979; Mellers, Schwartz, Ho, & Ritov, 1997). In particular, we varied the *prediction error*, or extent to which participants won more or less money than would have been expected based on the gamble parameters. Indeed, we found in previous work that prediction errors in this paradigm strongly predicted people's attributions about others' emotions (Ong et al., 2015).

Secondly, this paradigm allowed us to carefully control the form of judgment people made through a between-participants approach. In studies of self-enhancement, individuals are typically asked to directly compare themselves to others ("Compared to the average person, how X are you?"; e.g., Dunning et al., 1989; Lykken & Tellegen, 1996; Wojcik & Ditto, 2014). This approach has faced methodological criticisms (Klar & Giladi, 1999), for example, on the grounds that it induces a socially desirable motive for participants to self enhance (Chambers & Windschitl, 2004). Scientists can circumvent this problem by having participants make separate judgments about themselves versus others (Vautier & Bonnefon, 2008). This so-called "indirect method" provides a more conservative and precise estimate of self-enhancement (Chambers & Windschitl, 2004). Here, to model biases in emotion inference, we take this method a step further, and have different groups of participants make judgments only about themselves, or about others in a between-participants fashion.

**Overview of the Current Work**

Across three studies, we examined how people attribute emotions to themselves, or to others, following simple gambles. We find evidence for contextualized self-enhancement in emotion attribution consistent with the combination of biases described in **Figure 1c**. In Study 1, participants observed another person make simple monetary gambles, or imagined playing those same gambles themselves. Participants attributed more positive and less negative emotions to themselves as compared to others, especially in response to negative outcomes. In Study 2, we manipulated the social closeness between the participants and the target, and demonstrate that the strength of emotion attribution bias varies parametrically with social distance (Murray & Holmes, 1997; Suls, Lemos, & Stewart, 2002).

In Study 3, we further probed the idea that emotion attribution might vary similarly across both social and temporal distances. In particular, we combined these two types of distances by asking participants to make attributions about themselves or another person, either now or in the future. This 2-by-2 factorial design indeed revealed that temporal distance affected emotion attribution in a similar manner to social distance: Participants attributed more positive and less negative emotions to targets (themselves or others) in response to gambles made *now*, as opposed to gambles in the future. Broadly, our data speak to a nuanced self-enhancement bias in the attribution of emotional states that (i) varies by situation context, and (ii) varies along multiple dimensions of psychological distance.

## Methods and Results

All code to replicate our experiments and reproduce our analyses, and all data, can be found at: https://github.com/desmond-ong/emotionPsychDistance

### Study 1: Self and Other emotion attributions

Study 1 examined participants' attributions of emotions to themselves and another person following a hypothetical monetary gamble. The simple nature of the paradigm allowed us to systematically vary features of the scenario (e.g., how much the target wins in a particular gamble) and examine the resulting emotion attribution.

**Methods.** We recruited 200 participants (81 female; mean age=34.5 years) online using Amazon's Mechanical Turk (AMT), and randomly assigned them to an *Other* (N=98) or a *Self* (N=102) condition. The *Other* condition is a replication of the first experiment of Ong et al. (2015).

*Other condition*. On each trial, participants watched a fictional character spin a wheel containing three monetary payoffs of differing amounts. Participants were asked to imagine that the character had won the amount of money associated with the part of the wheel on which they had landed (much like in the gameshow "Wheel of Fortune"; see Fig. 2 for an illustration).

Outcomes on each trial were selected, without replacement, from a larger set of 50 different scenarios. Each scenario comprised winning a particular amount of money on a particular wheel. The wheels were designed to de-correlate the amount won on a particular spin from the expected value—the averaged reward that one should expect from the wheel[2]. This allowed us to vary how positive or negative the outcome was, relative to the expectation of the wheel, and examine participants' emotion attribution as a function of these outcomes. All values on the wheels are greater than or equal to $0: our previous work (Ong et al., 2015) demonstrated that spins with negative prediction errors are interpreted as negatively valenced events, even if the gamble has a positive payoff (This follows previous work on reference-dependent utility, e.g., Kahneman & Tversky, 1979).

After observing the outcome of a spin, participants judged the extent to which they believed the character would experience each of eight emotions (*Happiness, Sadness, Anger, Surprise, Disgust, Fear, Contentment,* and *Disappointment*), using 9 point Likert scales. Each participant completed 10 trials. The names of the characters were randomized on every trial.

S*elf condition*. Participants imagined *themselves*, instead of a fictional character, playing the gambles. Participants clicked a button to spin the wheel, and after observing the outcome of the spin, rated how they felt along the same eight emotions used in the *Other* condition. The gambles were hypothetical, and participants were not paid any additional money based on the outcomes of the gambles. Like the *Other* condition, participants completed 10 trials.

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[2] The expected value is calculated using a standard definition. For each sector of the wheel, we take the product of the notational reward and the probability of obtaining that reward (i.e., the size of the sector relative to the whole wheel). The expected value of a wheel is the sum of these products.

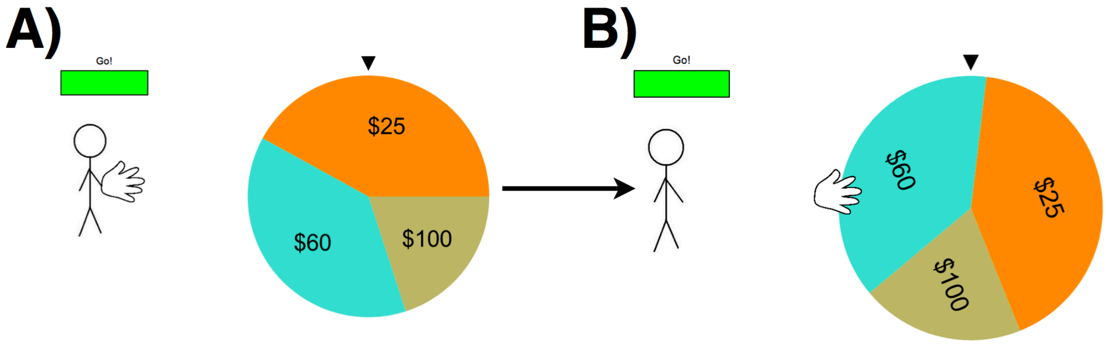


Figure 2. Illustration of the experimental paradigm used in Studies 1-3. In the *Other condition*, participants are shown a stick figure character as above, playing the wheel and winning a certain amount (i.e., Fig. 2A and 2B). In the *Self* condition, the stick figure character is removed and participants are asked to imagine themselves playing the game. Study 2 uses the same sequence as the *Other* condition in Study 1. Study 3 replicates Studies 1 and 2 and extends them by adding two *Future* conditions, where participants only see the wheel *before* it is spun and prospectively rate how they or another target would feel if it were to land on one of the outcomes (i.e., Fig. 2A only).

**Results.** We operationalized the utility of the outcome by calculating a prediction error (PE) for each outcome, as in our previous work with this paradigm (Ong et al, 2015). PE signals, here, how positive or negative each gamble outcome was relative to the wheel's expected value (**PE = win – expected value**). In essence, a high (positive) PE means that an outcome was better than expected, whereas a low (negative) PE means that an outcome is worse than expected (Holroyd & Coles, 2002; Kahneman & Tversky, 1979; Sutton & Barto, 1998). We constructed linear mixed-effects models, with PE and its interactions with a condition (*Other* versus *Self*) dummy variable as fixed effects, and random intercepts by participant and scenario. We estimated eight separate models, one for each of the eight emotions.

Figure 3 displays data for *Happy* and *Disappointment* judgments as a function of (i) condition (*Other* versus *Self*), and (ii) Prediction Error (PE). In both conditions, PE positively predicts happiness judgments, and negatively predicts disappointment judgments. This means, unsurprisingly, that participants judge both themselves and others as feeling more happiness and less disappointment after receiving better, as compared to worse, monetary outcomes.

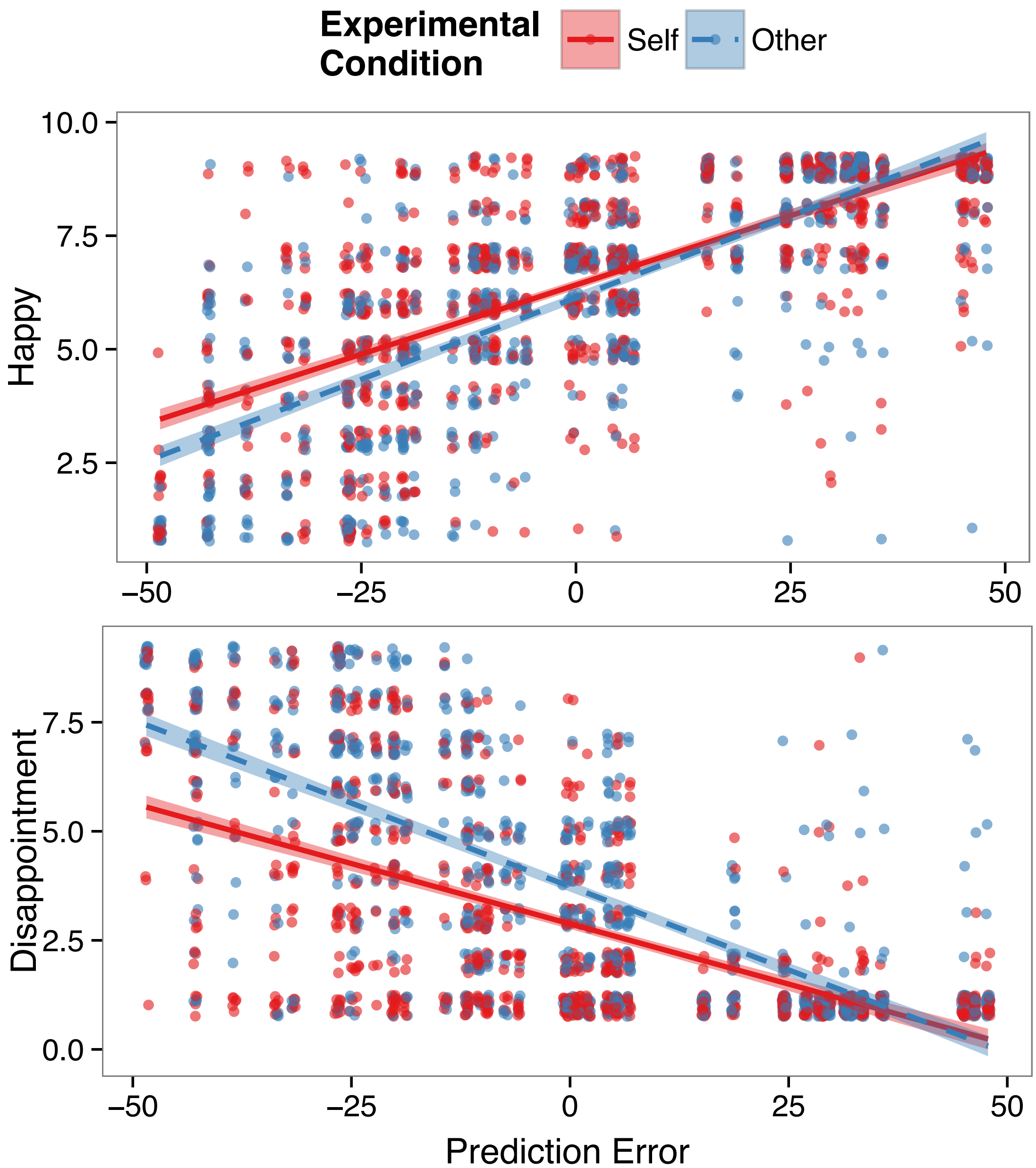


Figure 3. Participants' attributions of happiness and disappointment to themselves (in solid, red lines) and others (dashed, blue lines) as a function of PE. Data points are jittered for visibility, with best linear fits overlaid, with a shaded 95% confidence region.

Two key findings emerged from our analysis. First, when controlling for PE, participants attribute less happiness ($b$ = -0.307 with 95% CI: [-0.596, -0.019], $t(196) = -2.09$, $p = 0.04$; see Table 1 for full statistics) to others than to themselves. The simple effect of distance can be interpreted as the difference between the *Other* and *Self* conditions when the target wins an amount equal to the expected value of the wheel (i.e., at PE=0). This means that, on average, participants attribute 0.31 points less happiness (on a 9 point scale) to others than to themselves after winning the expected amount. Participants also tended to attribute more *sadness*, *anger*, *disgust,* and *disappointment* ($p$'s<0.001), to others as compared to themselves for an expected reward (see Table 1 for full results). The self-enhancement simple effect was not significant for judgments of *contentment* ($p$=0.29). Overall though, assessing the pattern of results across all of the emotions, the simple effect results are consistent with a self-enhancement bias (Fig. 1a), whereby participants self-enhance by attributing more positive emotions and less negative emotions to themselves as compared to others.

We next explored reward sensitivity, or the slope of the graphs in Figure 3. We find that social distance *increases* sensitivity to the prediction error of the outcome. The interactions of *Self* versus *Other* condition with PE are significant and positive for both *happiness* and *contentment* ($p$'s<0.001; Table 1). In particular, this suggests that participants weigh the outcome PE *more* when making judgments about others' emotions than when making judgments about themselves. A unit change in the goodness of the outcome affects participants' judgments for others more than it affects participants' judgments for themselves (as in Fig. 1b). The interactions are also significant and in the predicted directions for negative emotions: *sadness*, *anger*, *disgust*, and *disappointment* (*p*'s<0.001). These interactions demonstrate an increased sensitivity to changes in the situation valence, and are similarly robust across both positive and negative emotions. The linear mixed-effects models provided good fits to the data, explaining between 51% to 76% of the variance (see Table 1 for individual model $R^2$).

Overall, the combination of both the interaction (steeper slope) and the simple effect (group differences at zero PE) imply that the self-enhancement effect is contextualized, and is more pronounced at negative PE, i.e., when targets lose more relative to the expected value. Participants think that they would react much more positively than others when reacting to negative gamble outcomes (negative PE), and this difference diminishes when outcomes are better than expected (positive PE; Fig. 1c).

| | Simple Effect of Distance | | | Reward Sensitivity | | | |
|---|---|---|---|---|---|---|---|
| Emotion | b [95% CI] | t | p | b [95% CI] | t | p | $R^2$ |
| Happy | -.307 [-.596, -.0185] | -2.09 | .038* | .0148 [.0107, .0190] | 7.07 | <.001*** | .76 |
| Content | .260 [-.216, .736] | 1.07 | .29 n.s. | .0157 [.0103, .0210] | 5.75 | <.001*** | .69 |
| *Positive emotion composite* | -.0245 [-.355, .306] | -.15 | .89 n.s. | .0154 [.0112, .0195] | 7.31 | <.001*** | .76 |
| Sad | .643 [.336, .949] | 4.11 | <.001*** | -.0237 [-.0282, -.0193] | -10.5 | <.001*** | .63 |
| Anger | .511 [.221, .801] | 3.46 | <.001*** | -.0195 [-.0231, -.0159] | -10.6 | <.001*** | .58 |
| Disgust | .447 [.190, .703] | 3.41 | <.001*** | -.0159 [-.0198, -.0121] | -8.05 | <.001*** | .51 |
| Disappoint-ment | .877 [.559, 1.195] | 5.40 | <.001*** | -.025 [-.0301, -.0199] | -9.65 | <.001*** | .69 |
| *Negative Emotion Composite* | .620 [ .367, .873] | 4.80 | <.001*** | -.0211 [-.0245, -.0178] | -12.4 | <.001*** | .69 |
| Surprise | .748 [.323, 1.173] | 3.45 | <.001*** | -.0046 [-.0105, .0014] | -1.51 | .13 n.s. | .56 |
| Fear | .033 [-.159, .225] | 0.33 | .74 n.s. | -.0015 [-.0033, .0003] | -1.63 | .11 n.s. | .63 |

Table 1. Study 1 results. Each row indicates the results of one model predicting one emotion (or emotion composite, described in text). Left: Coefficients on the simple effect of condition (i.e. the contrast *Other-Self* at PE=0) in the mixed models. Right: Reward sensitivity, or the coefficient on the Condition x PE interaction term in the mixed models. (n.s. not significant, * p<.05, ** p<.01, *** p<.001). We calculated model $R^2$ using the definition of "conditional $R^2$" provided by Nakagawa and Schielzeth (2013), which describes the proportion of variance explained by both the fixed and random effects.

For a rough estimate of the effect size in meaningful quantities, consider the coefficients on the simple effects and interaction terms in the model predicting *disappointment* (coefficients = 0.88 and -0.025 respectively; Table 1). On average, participants tended to attribute 0.88 points more disappointment, on a 9 point Likert scale, to a random other than to themselves, if both had won an amount equal to the expected value of the gamble—this is the simple effect. For every additional dollar "lost" (in our paradigm), participants' attributions of disappointment to others increases 0.03 units *more* per dollar, as compared to their attributions of disappointment to themselves—this is the difference in slopes. Concretely, if both the average participant and a stranger won $35 less than the expected value, then the participant would attribute 0.88 + (-0.025)*(-35) = 1.76 more points of disappointment to this stranger than to themselves, or almost double the simple effect. Conversely, this other-versus-self difference would decrease in magnitude as the PE becomes less negative and more positive. However, we caution against extrapolating the results to large, positive PEs beyond what we measured. Importantly, we do not expect the self-enhancement to significantly "flip" in sign at extreme positive values of PE, given the theoretical motivation for this effect, although future work could verify this via empirically examining even larger, positive PEs. Finally, we stress that these specific numerical magnitudes of the effects apply within our current paradigm, and is meant more to illustrate a general, intuitive understanding of the results.

For completeness, we report the results over surprise and fear, though we had no *a priori* hypotheses regarding these emotions from our prior work (Ong et al., 2015). Looking at the simple effects, we find that participants attributed more *surprise* to others as compared to themselves ($p<0.001$), but no difference in *fear* ($p=0.74$). The interactions of self versus other with PE did not significantly predict surprise and fear judgments ($p$'s>0.11).

Finally, because the results of Study 1 suggested one pattern of results for the positive emotion judgments, and a second pattern for the negative emotions, we created two composite variables to minimize the number of comparisons in the studies we report later in this paper. We averaged the positive (*happiness*, *contentment*) and negative (*sadness*, *anger*, *disgust*, and *disappointment*) emotion attributions into two composites. We verified that the results for the emotion composites are consistent with the individual emotions (Table 1). The simple effect for the positive emotion composite is not significant, although the simple effect for the negative emotion composite, and the increased sensitivity for both positive and negative emotion composites, appear to be strong, robust effects. We argue that the lack of statistical significance for the simple effect of the positive emotions by itself is not a disconfirmation of our hypotheses, as simple effects have to be interpreted together with higher-order interactions. In addition, the results for the positive emotions should be interpreted together with the complementary results for the negative emotions. For the remaining studies in the paper, we will use the emotion composites as the unit of analysis, and we present the results on all the individual emotions in the Supplemental Material.

**Study 2: Social Distance**

Study 1 demonstrates that people attribute more favorable emotion states to themselves than to others following a simple gamble; their judgments for others were also more sensitive to changes in the reward. In Study 2, we examine how this bias varies with social distance, by having participants draw inferences about the emotions of targets who systematically varied in social distance from the participants.

**Methods.** We recruited 300 participants (122 female; mean age=33.6 years) online using Amazon's Mechanical Turk (AMT). In the first part of the experiment, participants learnt about a "previous participant" in our experiment with whom they had been paired. Participants were told that they would first get to know a little about their partner (hereafter, the *target*). Participants answered a series of personality questions from a ten-question subset of the Big Five Inventory (BFI-10; e.g., "I see myself as someone who is reserved"; Rammstedt & John, 2007). For simplicity, we adapted the BFI-10 such that participants answered each item as a True/False binary item instead of on a Likert scale. After each question, participants were told that the target had given either the same or a different answer as them. As an attention check, participants then indicated the answer that the target had given. Sixty-two participants answered one or more of these attention checks incorrectly; these participants' data were excluded from further analysis.

In actual fact, we generated the target's answers to the BFI to vary the perceived social distance separating the target and participant. We did this by varying the number of personality questions that the target answered similarly to the participants. In particular, participants encountered targets who answered 1, 3, 5, 7, or 9 questions (out of a total of 10) in the same way participants themselves had answered. Thus, a participant who encountered a target who answered 9 questions similarly to the participant would experience *low social distance* from (i.e., high similarity with) that target. Conversely, a participant who encountered a target who answered 1 question similarly to the participant would experience *high social distance* from (i.e., low similarity with) that target. All participants answered the BFI-10 in the same order, but saw the order of similar and dissimilar feedback in a random order. Note that each participant saw only one target, and consequently only one social distance: Social distance was a between-participant manipulation.

After answering the BFI-10, participants then saw a summary statement: they and the target had an approximately 10%, 30%, 50%, 70%, or 90% similarity in personality. As a manipulation check, participants then filled out a continuous version of the Inclusion of Other in Self scale (IOS; Aron, Aron, & Smollan, 1992), to indicate how much they felt their self-concepts and that of the target overlapped. They also reported on how similar they felt to, how much they liked, and how much they empathized with the target.

The second half of the experiment was similar to the *Other* condition in Study 1 (Fig. 2). Participants watched the target play ten rounds of the same wheel game as in Study 1, attributing emotions to the target after each round.

**Results.** Manipulation checks confirmed that the manipulated social distance increased participants' subjective distance from the target. As expected, the number of questions targets and participants answered similarly tracked participants' sense of overlap with ($r$=0.60, $t(236)$=11.43, $p$<0.001), similarity to ($r$=0.76, $t(236)$=18.22, $p$<0.001), liking of ($r$=0.46, $t(236)$=7.91, $p$<0.001), and empathy for the target ($r$=0.46, $t(236)$=8.06, $p$<0.001).

Next, we modeled the results in a similar fashion as Study 1. We created two composite variables by averaging the positive and negative emotion ratings. Results for individual emotions are consistent with the results for the emotion composites, and we report detailed results for the individual emotions in the Supplemental Material. We fit mixed-effects linear models separately for the positive emotion composite and the negative emotion composite. We used the PE of the gambles that the target played, as in Study 1, and its interaction with manipulated social distance as fixed effects. We modeled social distance as a continuous variable[3].

As targets became more socially distant from (i.e., less similar to) the participants, participants attributed more negative emotions to the target ($b$=0.055 [0.009, 0.101], $t(232)=2.33$, $p=0.020$), but not significantly different positive emotions ($p=0.34$; Fig. 4, left panel). This is a simple effect, and so can be interpreted as the effect of distance when the target won an amount equal to the expectation of the gamble. We also observe significant interactions of social distance with PE (Fig. 4, right panel) for both positive emotions ($b$=0.0011 [0.0005, 0.0018], $t(232)=3.35$, $p<0.001$) and negative emotions ($b$=-0.0012 [-0.0018, -0.0006], $t(232)=-3.90$, $p<0.001$). This means that participants' judgments of others' emotions become more sensitive to changes in the outcome as those targets became more socially distant. The models explained a large proportion of the variance in participants' emotion attributions (positive composite model conditional $R^2$=.76; negative composite conditional $R^2$=.71; using the definition from Nakagawa & Schielzeth, 2013). The results of Study 2 replicate that of Study 1: the weak, non-significant result of the simple effect of positive emotions as well as the robust simple effect for negative emotions, and the interactions for both composites. We note that the regression coefficient effect sizes in Study 2 are weaker than the effect sizes in Study 1,

[3] We tested for, but did not find, non-linear (i.e., quadratic) effects of social distance, so we report the model with only a linear social distance term.

and this is correspondingly because the social distance manipulation is subtler and weaker in Study 2 (see Supplemental Materials for more details). That said, despite the subtler manipulation, both the simple effects and the interactions replicate the pattern documented in Study 1: social distance affects emotion judgments least at positive levels of PE, and more at negative levels of PE.

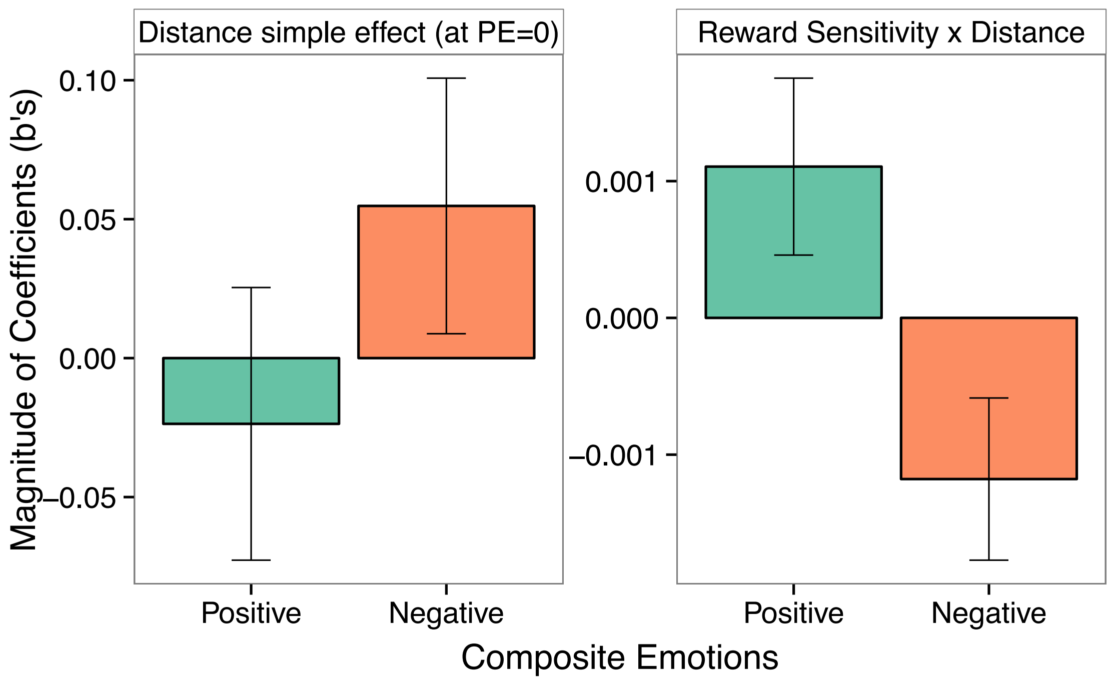


Figure 4. Study 2 results. Left: Simple effect (i.e., the effect of social distance at PE=0). Right: Reward sensitivity across social distance (i.e., interaction of social distance with PE). Error bars represent 95% Confidence Intervals.

The results of Study 2 demonstrate that in addition to rating themselves as experiencing more desirable emotion than a stranger, people also judge socially close, as compared to distant targets to feel more similarly to them. People's judgments also increase in sensitivity to the situation outcome for distant, as compared to socially close, targets. Studies 1 and 2 thus jointly provide consistent evidence for a social-distance-moderated bias in emotional reasoning.

**Study 3: Combining social and temporal distance**

Studies 1 and 2 showed evidence for an emotion attribution bias that varies with social distance. In particular, the increased sensitivity that people show when making judgments about others' emotions (Studies 1 and 2) looks similar to the focalism bias that people display when making judgments about their future emotions (e.g. Wilson et al, 2000). Indeed, previous work shows that both social and temporal distances affect cognition in similar ways (e.g., Bar-Anan et al., 2007; Kim, Zhang, & Li, 2008; Pronin et al., 2008; Tamir & Mitchell, 2011). Following this reasoning, we should also expect that the observed emotion attribution bias should similarly apply to judgments about targets in the future. We designed Study 3 to test this by varying both social and temporal distance.

**Methods.** We recruited 400 participants online using Amazon's Mechanical Turk (AMT), and randomly assigned them to one of four conditions in a 2 (*target*: *Other* vs. *Self*) x 2 (*time frame*: *Now* vs. *Future*) design.

The *Other,Now* (N=100) and *Self,Now* (N=100) conditions were identical to the *Other* and *Self* conditions in Study 1, and provide a direct replication. In the two corresponding *Future* conditions (N=100 for *Self,Future*; N=100 for *Other,Future*), participants were shown the same wheels but did not actually see gambles take place. Instead, participants were asked to forecast how the target (*Self* or *Other*) would feel in the future if the wheel were to land on one of the payoff amounts. They were only asked about one possible outcome ("How do you think [you / other character's name] will feel if the wheel landed on **$X**?"). Concretely, participants were only shown the initial position of the wheels, and the wheels were never spun. For example, in the *Self,Future* condition, a participant on a trial could be shown the initial wheel in Figure 2A and be asked to predict how they would feel if the wheel were to land on $60. After the participant made emotion judgments for just one possible outcome, the experiment would immediately move on to the next trial.

**Results.** We followed the same analysis procedure as in the first two studies. We created a positive emotion composite and a negative emotion composite; the results for the individual emotions, given in the Supplemental Material, are consistent with the emotion composites reported here. Here, we have a 2x2 design and hence 2 condition dummy variables, one for the "*Other-Self*" contrast (social distance) and one for the "*Future-Now*" contrast (temporal distance). We included, as fixed effects, all the interactions between the three predictors: PE, a dummy variable for the social distance condition, and the corresponding dummy variable for the temporal distance condition. As in our previous studies, we included random intercepts by participant and scenario. We estimated two separate models, one for the positive emotion composite and one for the negative emotion composite.

Similar to Studies 1 and 2, we again find results consistent with contextualized self-enhancement (Fig. 5, left panel). There is a significant simple effect of social distance on the negative emotions. When the target won an amount equal to the expected value, participants tended to attribute more negative emotions to others than to themselves ($b$=0.655 [0.425, 0.885], $t(378)$=5.58, $p$<0.001), but not less positive emotions ($p$=0.12). In addition, there is also a significant simple effect of temporal distance. Participants attributed more negative emotions ($b$=0.426 [0.183, 0.668], $t(378)$=3.44, $p$<0.001) and less positive emotions ($b$=-0.329 [-0.632, -0. 026], $t(378)$=-2.13, $p$=0.034) to targets in the future, as compared to targets who already know the results of the gamble. The two-way interactions between the *Other-Self* and *Future-Now* dummy variables were not significant for either the positive or negative emotion composites ($p$>.7).

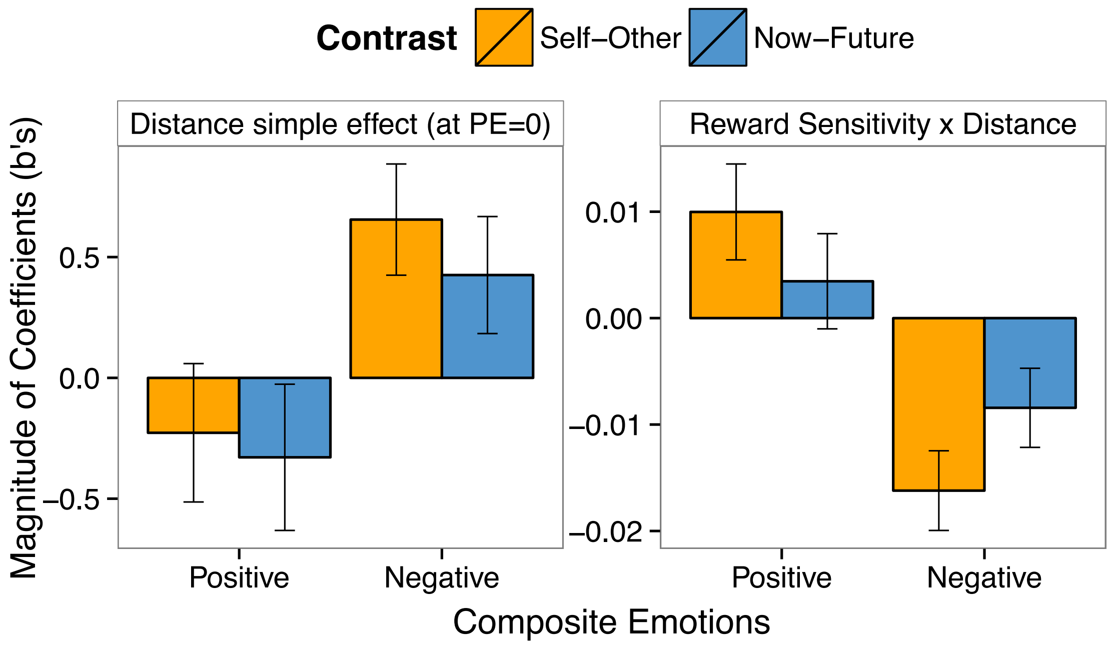


Figure 5. Study 3 results. Left: Simple effect by distance (i.e., the effect of the distance manipulations at PE=0). Right: Reward sensitivity across psychological distance (i.e., the interactions of the two distance manipulations with PE). Error bars represent 95% Confidence Intervals.

Next, we examine the interactions with PE. Again, similar to Studies 1 and 2, we find that, for social distance, there is a significant interaction with PE for positive ($b$=0.010 [0.0055, 0.015], $t$(378)=4.34, $p$<0.001) and negative emotions ($b$=-0.016 [-0.020, -0.012], $t$(378)=-8.49, $p$<0.001). We observe a similar pattern for temporal distance for negative emotions ($b$=-0.0084 [-0.012, -0.005], $t$(378)=-4.44, $p$<0.001), although the interaction with positive emotions is not significant ($b$=0.003, $p$=.13). These interactions reflect a pattern similar to the one documented in Studies 1 and 2:

Participants rate themselves, as compared to others, and targets now, as compared to in the future, as experiencing less negative and more positive emotions following relatively unfavorable outcomes. When prediction errors become more positive, this bias shrinks, suggesting that observers reason that, when good things happen, everyone is similarly positive.

Finally, we were interested in how both dimensions of psychological distance together might influence the emotion attribution bias. For example, the effect of psychological distance could be *super-additive* across both dimensions, whereby the combined effect of the temporal-and-social distance manipulations would be stronger than the sum of the individual manipulations. Conversely, the effect of distance could be *sub-additive*, whereby the combined effect would be less than the sum of the individual effects. To investigate this, we examined the highest-order interactions in our model. The three-way (*PE* x *social* x *temporal*) interaction is significant for positive emotions ($b$=-0.0090 [-0.0154, -0.0026], $t(378)=-2.75$, $p=0.006$) and marginally significant for negative emotions ($b$=0.0048 [-0.0005, 0.0101], $t(378)=1.78$, $p=0.076$). These three-way interactions are opposite in sign to the two-way (*PE* x *social* / *PE* x *temporal*) interactions (e.g., the coefficient for the three-way interaction for positive emotions is less than zero, while the corresponding two-way interactions are both greater than zero). This difference in sign suggests that the effects of distance are *diminishing* across the two dimensions. That is, the marginal effects of distance taper off when *both* social *and* temporal distance are present; Put another way, the effect of psychological distance on the emotion attribution bias is *sub-additive* across multiple dimensions of distance. This is consistent with at least two previous studies that also found diminishing returns across two dimensions of psychological distance when people make choices about distant others (Kim et al., 2008; Pronin et al., 2008). We expand on this point in the general discussion.

The results of Study 3 replicate and extend the results of Studies 1 and 2. As in Studies 1 and 2, the models in Study 3 explained a large proportion of the variance in participants' emotion attributions (conditional $R^2$=.69 and .64 for the positive and negative composites respectively). The fact that our bias accumulates across both social and temporal distances is also consistent with previous work showing that dimensions of psychological distance interchangeably (and sub-additively) affect decision-making (Kim et al, 2008; Pronin et al, 2008). Importantly, our results underscore the idea that these nuanced emotion attribution processes are not limited to social judgments. Rather, this bias underlies how people fundamentally reason about emotions (Ong et al, 2015) across multiple forms of psychological distance.

## Discussion

We tested three plausible theoretical models about how people make attributions of emotions under varying levels of psychological distance. Across three studies, we found evidence for a *context-sensitive egocentric bias* in people's judgments of emotions. First, participants, on average, tended to attribute less positive and more negative emotions to others than to themselves after experiencing an emotionally relevant situation—in our study, hypothetical gambles. Second, participants' judgments of others' emotions, as compared to their own emotions, are more sensitive to the emotionally relevant features of a situation (Study 1). We found that this context-sensitivity scales with social distance: Participants attributed less favorable emotions to targets that were socially distant, as compared to targets that were socially closer (Study 2). Moreover, the bias is moderated in a similar manner by temporal distance as well. Participants judged that they (and others) in the future would similarly feel less favorable emotions after gambles, as compared to after gambles they have just played (Study 3). Despite the conservative between-participant nature of our studies, the pattern of results robustly replicated across the three studies. Our findings illustrate a complex interplay between people's tendencies to self-enhance and the context-sensitivity with which people make emotion attributions.

The two characteristics of this bias—the self-enhancement and the increased sensitivity to situation features—can be interpreted by bringing together two different literatures. First, motivated self-enhancement accounts for the overall direction of the bias: people self-enhance by attributing more favorable emotions to themselves. For example, negatively valenced events might function like a "threat" towards one's emotional well-being, and this bias may serve an adaptive purpose by allowing people to buffer their emotional health against such negative information (e.g., Alicke & Sedikides, 2009; Taylor & Brown, 1988). Second, the focalism bias studied in the affective forecasting literature accounts for the increased sensitivity to situation features. One possible explanation for this effect in our studies is that people have less information when making judgments about others or their distant future selves, such as how these others tend to react in a given situation, and therefore base their judgments more on salient contextual information in the situation. By contrast, when making judgments about themselves, people have more information: they know their own preferences and idiosyncrasies, and they might even spontaneously reappraise extreme situations. Hence, their judgments would incorporate these other factors, and would track less strongly the specific features of the situation. Future work should examine the specific cognitive processes that contribute to this sensitivity bias in emotion attribution.

The current work offers a different angle to previous work on how people make judgments of emotions across time. In addition to affective forecasting (e.g., Gilbert et al, 1998; Wilson & Gilbert, 2003), this work also relates to previous studies on what has been called *unrealistic optimism* (e.g., Weinstein, 1980). When thinking about their life in the abstract, people tend to predict that they will experience *greater* Subjective Well-Being in the distant future, as compared to the present or the near future (Heller, Stephan, Kifer, & Sedikides, 2011; Robinson & Ryff, 1999; Stephan, Sedikides, Heller, & Shidlovski, 2015). Although at first glance this may seem contradictory to our current findings, we propose that this may be resolved by considering whether such attributions are made in a global or contextualized manner. One proposed explanation of unrealistic optimism is that, when asked to predict their global SWB in the future, without additional context, people rely on abstract ideals like ideal self-concepts, hopes, and ambitions (Heller et al, 2011), and hence inflate their predictions of their future SWB. Indeed, priming people with more concrete information about their future attenuates this forecasting optimism (Robinson & Ryff, 1999). By contrast, when individuals forecast their emotions in a specific emotion-elicting context, such as how they would feel if their team wins or loses a football game, they tend to focus on the specific features of that context, and their predictions tend to be biased based on the specific context (e.g., Wilson et al, 2000). Thus, evidence points to there being different underlying cognitive processes that contribute to *global* versus *contextualized* judgments. They need not be mutually exclusive; Certain trait judgments (e.g., one's current SWB) may occur via an aggregation of a set of states (e.g., recent emotional states), and so contextualized effects might contribute to global trait judgments as well, but presumably to a weaker extent. This leaves open many questions, such as how emotional state judgments—and their associated cognitive processes and biases—may affect judgments of longer-term affect like mood, and global assessments like SWB. These are important questions in social cognition that future research should explore.

Converging evidence suggests not only that different dimensions of psychological distance are somewhat interchangeable, but also that distances can be added across dimensions (Kim et al., 2008; Pronin et al., 2008). In Study 3, we found that the effect of different dimensions of distance on emotion attribution is *sub-additive*: that is, the effect of *both* social *and* temporal distance is smaller than the sum of the effect of social distance, and the effect of temporal distance. Such sub-additivity is common across other basic psychological processes like perception: People tend to perceive brightness, loudness, an object's physical size and distance, and even economic gains and losses sub-additively (e.g., Stevens' psychophysical power law; see Kim et al, 2008 for an in depth discussion). The sub-additive perception of multiple *dimensions* of psychological distance has important implications for designing applications. For example, considering one's present situation from an emotionally distant standpoint contributes to adaptive emotion regulation (Kross & Ayduk, 2008). Adding additional dimensions of psychological distance (e.g., imagining an event in a spatially distant location) may yield diminishing results. Finally, we currently do not know how units of distance on one dimension (e.g., time) map onto distances on another dimension (e.g., social distance).

The "Now-Future" temporal distance manipulation we used in Study 3 elicited weaker behavioral effects than the "Self-Other" social distance manipulation, and one reason could be because the temporal manipulation translated to a psychologically smaller amount of perceived psychological distance as compared to the social distance manipulation. Future research should more rigorously map the relationships between these distance dimensions.

The gambling paradigm we used in our studies offers at least two advantages. First, it provides a standardized, somewhat novel situation, which facilitates comparing judgments across participants. Second, and perhaps more importantly, the nature of the paradigm allowed us to parametrically vary various parameters of the gamble. This in turn enabled us to quantitatively measure the size and context dependency of the self-enhancement bias. This also means that the paradigm is simple, and does not capture the richness of situations we encounter in everyday life. That said, we feel that the work presented here gives a firm footing from which to further examine more naturalistic scenarios, across different contexts. For example, an interesting open question is whether this emotion attribution bias varies with the social desirability of emotions in different types of situations. Negative emotions are often undesirable (as in our studies), but in some cases, people *want* to feel negative emotions (Tamir, Mitchell, & Gross, 2008; Zaki, 2014), for example, when negative emotions are goal-consistent like feeling anger or antipathy prior to a conflict. Recent work also suggests that in cases like these where negative emotions might be desirable, people might self-enhance by claiming to feel *more* negative emotions. For instance, individuals from a majority race reading about racial discrimination perpetrated by their racial group judge themselves to feel more guilt than others in their racial group (Goldenberg, Saguy, & Halperin, 2014). Individuals also judge themselves to feel more intense negative emotions than others immediately following large-scale negative events, such as national tragedies and terrorist attacks (White & Van Boven, 2012). Thus, biases in emotion attribution might not always reflect a desire to view one's self as experiencing more *positively valenced* emotions, but should flexibly tune individuals towards the belief that they experience more *desirable* emotions, irrespective of valence. This is also a natural prediction of theories of motivated cognition (e.g., Hughes & Zaki, 2015). Future work should test this prediction by varying the goal consistency of emotions across a variety of contexts.

This work also raises interesting questions about the nature of the cognition and motivations that underlies this bias. Because we designed these studies as between-participants, we were not able to examine individual differences in this bias. Additional research addressing this could help scientists understand possible cognitive and motivational mechanisms, as well as potential implications, such as for mental health (Taylor & Brown, 1988). Previous studies have suggested that depressed patients may not show "better-than-average" social comparative biases (e.g., Alloy & Abramson, 1979; but see Ackermann, & DeRubeis, 1991, for a summary of challenges), and may in fact, want to regulate their emotions to feel more sadness (Millgram, Joorman, Huppert, & Tamir, 2015). Understanding biases and motivations in emotion attribution might prove to be especially important for understanding and treating mood disorders like major depression, bipolar, and the anxiety disorders. Future work should start by investigating how individual differences in patients' emotion attribution (to themselves and to others, under different contexts) might be related to risk for psychopathology, symptom severity, or potential relapse in remitted patients.

In sum, we show that the self-enhancement bias common across many aspects of social comparison extends to contextualized emotional states as well. People self-enhance by attributing more positive and less negative emotions to themselves than to others. Importantly, however, this bias varies with the situation context. In our studies, this bias varies with the valence of a given situation, such that this self-enhancement is strongest in negative situations, and is relatively attenuated in positive situations. We also demonstrate that this bias varies parametrically with social distance and across multiple dimensions of psychological distance. Our results speak to the importance of contrasting global evaluations, made outside a specific situation context, with *contextualized* affective cognition. There remain many more questions, such as how this emotion attribution bias might vary with the goal-consistency or social desirability of emotions in context. Thus, our results support the claim that people might often consider themselves "happier than thou", but this motivated bias is more nuanced and context-dependent than previously understood.

**Acknowledgements**

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**Supplemental Material**

All code and data can be found at

https://github.com/desmond-ong/emotionPsychDistance

As reported in the text, we fit mixed-effects linear models for each of the emotions, using PE and its interaction with the distance variables as fixed effects, and random intercepts by participants and trials. These models were of the form: Emotion ~ distance * PE. For Study 3, there are 2 types of distances, hence we added all interactions: Emotion ~ social_distance * temporal_distance * PE. There are 8 different emotions, across three studies, so there are 24 different models. In Table S1 we report the coefficients and p-values of the simple effects of condition, as well as the interactions of condition with PE, for all studies. The coefficients for Study 1 were reported in the main text, and are reproduced in the table to aid in comparison. We provide a short, informal discussion on the relative magnitudes of the coefficients across our studies, to show that our estimated results are consistent in magnitude across studies.

| Emotion<br><br>**Simple effects of distance** | Study 1<br>Social Distance<br>(*Other* - *Self*) | Study 2<br>Social Distance | Study 3<br>Social Distance<br>(*Other* - *Self*) | Study 3<br>Temporal Distance<br>(*Future* - *Now*) |
|---|---|---|---|---|
| Happy | -0.31 (0.04*) | -0.023 (0.27) | -0.063 (0.64) | -0.17 (0.24) |
| Content | 0.26 (0.29) (unexpected sign) | -0.025 (0.51) | -0.44 (0.031*) | -0.50 (0.022*) |
| Sad | 0.64 (<0.001***) | 0.051 (0.046*) | 0.77 (<0.001***) | 0.61 (<0.001***) |
| Anger | 0.51 (<0.001***) | 0.075 (0.005**) | 0.62 (<0.001***) | 0.40 (0.005**) |
| Disgust | 0.45 (<0.001***) | 0.048 (0.066) | 0.43 (<0.001***) | 0.12 (0.36) |
| Disappointment | 0.88 (<0.001***) | 0.046 (0.091) | 0.85 (<0.001***) | 0.54 (<0.001***) |
| Surprise | 0.75 (<0.001***) | 0.080 (0.017*) | 0.70 (<0.001***) | 0.42 (0.026*) |
| Fear | 0.033 (0.74) | 0.042 (0.038*) | 0.22 (0.012*) | 0.14 (0.13) |
| **Reward Sensitivities** | | | | |
| Happy | 0.015 (<0.001***) | 0.0011 (0.001***) | 0.0121 (<0.001***) | 0.0056 (0.017*) |
| Content | 0.016 (<0.001***) | 0.0012 (0.011*) | 0.0079 (0.011*) | 0.0014 (0.641) |
| Sad | -0.024 (<0.001***) | -0.0011 (0.003**) | -0.018 (<0.001***) | -0.015 (<0.001***) |
| Anger | -0.020 (<0.001***) | -0.0018 (<0.001***) | -0.015 (<0.001***) | -0.0087 (<0.001***) |
| Disgust | -0.016 (<0.001***) | -6.7e-4 (0.049*) | -0.011 (<0.001***) | -0.003 (0.091) |
| Disappointment | -0.025 (<0.001***) | -0.0011 (0.0085**) | -0.021 (<0.001***) | -0.0062 (0.024*) |
| Surprise | -0.0046 (0.13) | -8.5e-4 (0.065) | -0.0043 (0.15) | -0.0037 (0.22) |
| Fear | -0.0015 (0.11) | -4.5e-5 (0.75) | -0.0049 (<0.001***) | -0.0036 (0.0033**) |

Table S1: Table of regression coefficients, with p-values in parentheses. We have presented the table to facilitate comparison across studies and emotions: The top half of the table contains the coefficients for the simple effect of distance (at PE=0). The bottom half of the table contains the coefficients for reward sensitivity across psychological distance (interaction of distance with PE). The contrasts are always in a direction taking the Self (Now) as a reference point; hence the coefficients should be interpreted as the effect of increasing distance. The three-way interactions for Study 3 are omitted for clarity. (* p<.05, ** p<.01, *** p<.001)

**Comparing Study 1 to Study 2.** The coefficients for Study 1 given in Table S1 are for the "*Other – Self*" contrast. The coefficients for Study 2 are for 1 unit of 'distance', which we operationalized as answering differently 1, 3, 5, 7, or 9 personality questions, out of 10 questions. This forms a "distance scale", and we treated social distance as linearly increasing with number of personality questions answered differently (see Study 2 Methods). The "*Self*" and "*Other*" conditions from Study 1 should map somewhere onto this scale, although their exact positions are still an open research question. A possible assumption is that "*Self*" is somewhat close to a target answering 0 questions out of 10 differently from the participant (i.e., the target answered all 10 questions similarly), and a random "*Other*" is somewhat close to a target answering 10 questions out of 10 differently. Thus the Study 1 "*Other – Self*" contrast might roughly be equal to 10 units of Study 2 "distance". Under this assumption, we should, on average, observe that the coefficients for Study 1 are an order of magnitude larger (i.e., ten times larger) than the coefficients for Study 2. Indeed, this is generally the case, especially for the interactions with PE. We note that the simple distance results for Study 2 lose significance for most of the emotions. We believe that some loss of power might be expected due to the weaker social distance manipulation in Study 2, although we also think that the pattern of results should be interpreted across the multiple emotions and the multiple studies, rather than relying on individual significance values.

**Comparing Study 1 to Study 3.** We might have expected that the coefficients on Social Distance in Study 3 should be about the same magnitude as that for Study 1 as they both contain an "*Other – Self*" contrast. Note, however, that the models for the two studies are not identical, as the models for Study 3 include the effects of temporal distance as well: thus, the "social distance" coefficients roughly capture the (*Other*,*Now – Self*,*Now*) and (*Other*,*Future – Self*,*Future*) contrasts. Nonetheless, the two studies are in theory measuring the same phenomenon (effect of comparison across social distance), and so we should expect to see coefficients of comparable magnitude. Indeed, the coefficients for the negative emotions are very similar numerically across Study 1 and Study 3, although there is weaker agreement for the positive emotions.

**Comparing within Study 3.** Finally, one might compare the coefficients for Study 3 across the social and temporal conditions. As mentioned in the main text, the magnitudes of the coefficients for the temporal condition are generally smaller than the corresponding coefficients for the social condition. This suggests that in our paradigm, the effect of the "*Future*-*Now*" manipulation is weaker than the effect of the "*Other*-*Self*" manipulation. We discuss this in the main text in the General Discussion.