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A Double-Edged Sword? Unpacking the Effects of Rumination on Emotional Clarity

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Abstract

Rumination, or thinking passively and repetitively about one's distress, and low emotional clarity, or not understanding one's emotions, are risk factors for psychopathology. It has been suggested that people attempt to increase emotional clarity by ruminating, but whether ruminating works to help or harm emotional clarity in the moment is unknown. In $N = 74$ adults, following an idiographic negative mood induction, we experimentally manipulated rumination and two comparison conditions – distraction and mindfulness – to assess their effects on negative emotion, subjective and implicit indices of emotional clarity, and self-insight. Manipulation checks showed that conditions produced a pattern of distinct experiences theoretically consistent with each response style. Compared to comparison conditions, rumination was less effective in alleviating negative emotion. However, all conditions produced similar effects on emotional clarity and self-insight. Whereas each condition failed to influence subjective emotional clarity, they increased implicit clarity and perceived self-insight. Results underscore the importance of incorporating multiple measures of emotional clarity and suggest that, compared to other cognitive emotion response styles, rumination may function as a double-edged sword that keeps one entrenched in negative emotion but without impairing implicit emotional clarity and self-insight. Findings may have implications for why people ruminate despite its negative impact on well-being.

Keywords: rumination, repetitive negative thinking, emotional clarity, self-insight

Introduction

Rumination is a cognitive style characterized by passively and repetitively thinking about the causes, consequences, and meaning of one's feelings and problems (Nolen-Hoeksema et al., 2008). Nolen-Hoeksema's (1991) Response Styles Theory proposes that rumination perpetuates distress by enhancing the effects of depressed mood on negative thinking, impairing effective problem solving, interfering with instrumental behavior, and undermining social support. A large body of evidence shows that rumination maintains or exacerbates negative mood and predicts the onset, maintenance, and worsening of depression (Nolen-Hoeksema et al., 2008). Rumination is a risk factor for a range of psychopathologies and helps explain their comorbidity (Ehring & Watkins, 2008; McEvoy et al., 2013). A key need, then, is to understand why people ruminate despite its negative consequences. This question can be answered in part by examining perceived or actual benefits of ruminating (e.g., Lyubomirsky & Nolen-Hoeksema, 1993).

Early work has suggested that people ruminate in an understandable but ultimately counterproductive attempt to make meaning (Papageorgiou & Wells, 2001), including to identify how they feel (Vine, et al., 2014). Dysphoria and depression are associated with a stronger need to understand intrusive thoughts (Watkins, 2004) and beliefs that ruminating will help people understand themselves

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and their problems (Papageorgiou & Wells, 2001; Watkins & Moulds, 2005). Emotional clarity is the perceived ability to identify one's emotions (Salovey et al., 1995), which may help regulate emotions, with implications for psychological health (Gratz & Roemer, 2004; Vine & Aldao, 2014; Vine & Marroquín, 2018). Participants low in emotional clarity, compared to those higher in clarity, engage in more ruminative-like thinking following a negative mood induction (Salovey et al., 1995). Low emotional clarity also prospectively predicts rumination among people intolerant of ambiguity, suggesting that people unclear about their emotions ruminate more if they need these answers (Vine et al., 2014).

But does ruminating help people gain emotional clarity? Assessing whether ruminating bolsters emotional clarity has implications for understanding how rumination is reinforced. Consistent with early work discussed above, rumination might produce an elevation in emotional clarity, but this has not been examined experimentally. Experimental support comes from research on a similar construct – perceived self-insight, or the degree to which people rate their characterological level of insight into the self, others, and the world (e.g., "*I have a deep understanding of people*", "*I understand myself;*" Lyubomirsky & Nolen-Hoeksema, 1993). In this study, the authors compared dysphoric and non-dysphoric college students on ratings of negative mood (i.e., sadness and depression) and perceived self-insight following a rumination or distraction response manipulation. Relative to the distraction condition, dysphoric and non-dysphoric participants induced to ruminate reported greater self-insight despite dysphoric participants also reporting prolonged negative mood. Rumination thus led to greater perceived self-insight *regardless* of dysphoria status and *despite* prolonging negative mood in dysphoric participants (Lyubomirsky & Nolen-Hoeksema, 1993). To the extent that understanding one's emotions is akin to perceived self-insight, ruminating might similarly enhance emotional clarity.

Alternatively, rumination could impair emotional clarity. This would be consistent with trait-level associations between tendencies to ruminate and habitual emotional clarity, which are inversely correlated (Salguero et al., 2013; Vine et al., 2014). This association may reflect an inhibitory process at the state level, whereby ruminating interferes with the clarity of emotion experience. For example, scrutinizing and exhaustively labeling one's emotions – a behavior that shares properties with rumination – leads to lower emotional clarity, suggesting that a mixture of many concurrent negative emotions may be more difficult to understand than fewer negative emotions (Vine et al., 2019). The revisiting of unpleasant past experiences resulting from rumination also might produce more intense and prolonged negative emotions that are difficult to understand. Finally, as the cognitive avoidance (Borkovec et al., 2004) and contrast avoidance models of worry (Newman & Llera, 2011) and depressive certainty hypothesis (e.g., Lavendar & Watkins, 2004; Miranda et al., 2023; Yook et al., 2010) have all suggested, people may implicitly use repetitive negative thinking to protect themselves from the possibility of even more unpleasant emotional experiences. Accordingly, some research shows that rumination is related to experiential avoidance (Cribb et al., 2006; Giogio et al., 2010; Liverant et al., 2011; Moulds et al., 2007). If rumination facilitates avoidance of emotion, essentially helping people keep their emotions at arm's length, we speculated that it could prevent a deeper or greater level of confrontation that may be needed to form a clear emotional representation. These effects together may seem at odds with the evidence suggesting that rumination fosters self-insight (Lyubomirsky & Nolen-Hoeksema, 1993). However, such a discrepancy could be explained by dissociability of perceived versus implicit measures. For instance, self-reported emotional clarity was only weakly related to a performance-based index using response times (Lischetzke et al., 2005, 2011). Rumination could thus either improve or impair emotional clarity. Understanding the direction of this association could provide insight into why individuals are motivated to ruminate, and ultimately inform interventions to help interrupt this process.

We sought to examine the effects of an experimental rumination manipulation on subjective (i.e., self-report) and implicit (i.e., response time to emotion ratings) emotional clarity. We incorporated subjective and implicit measures of emotional clarity to avoid known self-report biases (Robinson & Clore, 2002) and to potentially discover dissociation between the impacts of rumination on subjective

and implicit emotional clarity. We also attempted to replicate prior work on the effects of ruminating on perceived self-insight. We compared rumination to two comparison conditions: distraction and mindfulness. Distraction, a common comparison to rumination, was conceived by the Response Styles Theory as style of responding to negative mood that can relieve negative emotion in the short-term (Nolen-Hoeksema et al., 2008). Given the lack of previous research on distraction and emotional clarity, we considered this research question exploratory. Mindfulness is present-focused and non-judgmental awareness and acceptance of mental states (Brown et al., 2007). We selected mindfulness as a comparison condition because, like rumination, it is self-focused, but unlike rumination, it is theorized to alleviate negative mood and increase emotional clarity (Roemer et al., 2015). Experimental mindfulness manipulations have improved mood, compared to unfocused attention and distraction tasks (see Roemer et al., 2015 for a review). Of the studies comparing experimentally induced rumination to distraction and mindfulness, some (Huffziger & Kuehner, 2009; Singer & Dobson, 2007) but not all (Kuehner et al., 2009) found that mindfulness and distraction elicit comparable improvements in mood.

Together, this led to us to four hypotheses regarding the effects of different response styles following an idiographic mood induction. First, we expected that relative to the distraction and mindfulness conditions, those in the rumination condition would exhibit higher negative emotion, replicating past work. Second, to replicate Lyubomirsky and Nolen-Hoeksema (1993), we expected that rumination would increase perceived self-insight relative to the distraction condition. Examination of effects of mindfulness on perceived self-insight or its relative difference from rumination and distraction were exploratory. Third, we expected that both distraction and mindfulness would increase emotional clarity. We also sought to examine the effect of rumination on emotional clarity, but given competing hypotheses in the literature, we did not have a specific prediction about this direction of this change or its relative difference from other conditions.

Method

Participants and Procedure

Participants were recruited in the summer of 2013 using paper flyers posted both on a university campus and in the surrounding community (e.g., at bus stops, coffee shops) in a small, northeast U.S. city. Eighty-five participants completed the study, but data for some were excluded for inattentiveness (n = 4)[1] and nonresponse to the mood induction (n = 7),[2] leaving 74 participants in the final sample. The final sample did not differ from excluded participants on educational status or baseline levels of outcome variables (ps range from .219-.965), but excluded participants were older (p = .011) and less likely to be female (p = .015) and White (p=.034). The final sample was disproportionately female (70.27%) and White (70.27%). Mean age was 25.27 (SD = 6.58, range = 18-43; see Table 1). Participants were compensated $10.

During individual sessions, participants completed informed consent and questionnaires not included in these analyses. The experiment was then administered electronically via E-Prime with instructions presented on the computer monitor (Psychology Software Tools, Pittsburgh, PA). All participants completed the same negative mood induction followed by a response manipulation, rumination (n = 28), mindfulness (n = 24), or distraction (n = 22), to which they were randomly assigned. State-level measures of negative emotion and emotional clarity were administered at baseline

[1] To ensure data quality, we included five attention check items. Four items were embedded within trait self-report measures (e.g., "*To show that you are paying attention, please leave this item blank*") and one item was included in the experimental procedure ("*To what extent were you thinking about the topics you were instructed to think about*?"). Participants were permitted to miss one of the attention check items in the trait self-report; those who failed two or more of the attention check items in the trait self-report and/or responded "*not at all*" to the experimental attention check item were excluded for inattentiveness.

[2] Nonresponse was defined as reporting no change in negative emotions or declines in negative emotions following the negative mood induction (T1 to T2).

(T1), post-mood-induction (T2), and post-response-manipulation (T3)[3]. Afterwards, participants completed manipulation check questions and were compensated with cash or research credit.

Tasks

Mood Induction

An idiographic negative mood induction was modeled after established procedures (Rusting & Nolen-Hoeksema, 1998). Participants first identified a negative personal experience that was distressing, emotional, or overwhelming, and that felt unresolved. To avoid biasing emotional clarity ratings, these instructions deliberately did not specify target emotions. Participants typed a brief description of their memory and reported how long ago it happened (or for repeated events, its last occurrence), how resolved it felt (1 = *not at all* to 5 = *mostly or completely*), and how often they were bothered by it (1 = *almost never bothered* to 5 = *bothered 3 or more times per day*). For eight minutes, participants were then instructed to relive the experience following a series of widely spaced, auditory prompts:

> *Close your eyes if that is comfortable for you. [pause] Go back to the time and place of the experience and see the scene in your mind's eye. [pause] In your mind, see the setting where the experience took place, including the people or objects around you. [pause] Feel what it feels like to be in the middle of that scene. [pause] See the experience unfolding through your own eyes as if it were happening to you all over again. [pause] Feel what your body feels like as you relive this event and become increasingly upset. [pause] Immerse yourself in the negative sensations you are feeling in your body as the emotions get stronger. [pause] If another person is involved in this event, imagine what their voice sounds like and what they are saying to you. [pause] Let yourself react fully, as if you were actually there right now. [pause] Your sense of misery increases as you become more and more emotional... [pause] Now, open your eyes and respond to the questions on the screen before you.*

Response Manipulations

A six-minute response manipulation (rumination, distraction, or mindfulness) was delivered on the computer screen and via headphones as a series of prompts, which participants advanced at their own pace. Following closely the landmark studies on these processes (e.g., Nolen-Hoeksema & Morrow, 1993), rumination prompts were self-focused, abstract, and neutral (e.g., "*Think about why you react the way you do*"), and distraction prompts were externally focused, concrete, and neutral (e.g., "*Think about a boat slowly crossing the Atlantic*"). The mindfulness condition was adapted from Huffziger and Kuehner (2009) and contained prompts instructing participants to attend to their present physical and mental state non-judgmentally (e.g., *"Be aware of your thoughts and feelings without judging them*"), with a moment-to-moment awareness prompt ("*Consciously return your attention to your breathing*") repeated every few prompts.

Eleven debriefing items were used to assess the success of the response manipulations, each beginning with the stem "*To what extent were you…*" and followed by theoretically relevant dimensions of each response process. For instance, rumination items asked how negative, distress-focused, and past-focused participants' thinking was (see Supplemental Table S1). Participants rated each item on a Likert scale (1 = *not at all*, 7 = *very much*), and items were averaged to create rumination, distraction, and mindfulness indices. Additionally, one item assessed response fidelity in all conditions ("*To what extent were you thinking about the topics you were instructed to think about?*").

[3] Halfway through the response manipulation period, participants also completed state-level "midpoint" ratings of emotional valence, arousal, and clarity using an adapted version of the Self-Assessment Manikin (Bradley & Lang, 1994), which are not analyzed here.

Measures

Negative Emotion

Participants rated the intensity of 10 negative emotions in a fixed order (jealous, ashamed, embarrassed, guilty, sad, angry, afraid, anxious, disgusted, depressed). Items were interspersed with others not analyzed here (positive emotions, e.g., "*happy,*" and non-emotions, e.g., "*overwhelmed"*). Items were rated on a Likert-type scale (1 = *not at all*, 7 = *very much*) and averaged (α_{T1} = .85, α_{T2} = .85, α_{T3} = .91).

Implicit Emotional Clarity

Implicit emotional clarity was operationalized as the speed of responding to the negative emotion items described above (response time in ms; RT). Using RT as an implicit measure of clarity follows a long line of research on attitude and judgment, which has used RTs to indirectly assess attitude strength, reflecting how much cognitive work one must do to respond to items and thus attitude accessibility (Bassili, 1996; Greenwald et al., 1998). In the context of emotional clarity, shorter RTs reflect higher implicit clarity because clear affective states should take less time to rate than unclear ones. Following Lischetzke et al. (2005), each participant's first RT (i.e., to *tired*, a non-emotion word) was discarded, outliers for remaining RTs were winsorized to 3 *SD*s about each person's mean, and median RT for negative emotion items was calculated at T1, T2, and T3 for each participant. Because tests of implicit emotional clarity were within-person, we did not correct for individual differences in baseline speed of responding (Lischetzke et al., 2011).

Subjective emotional clarity

We used five state-level items to assess how well participants felt they understood their emotions (see McLaughlin et al., 2007, e.g., "*I am clear about my feelings*"). Items were rated on a Likert-type scale (1 = *not at all,* 5 = *completely*) and summed, with higher scores reflect greater emotional clarity (α_{T1} = .91, α_{T2} = .93, α_{T3} = .92).

Perceived self-insight

Participants rated their beliefs about being an insightful person using 8 items from the self-reported insight measure developed by Lyubomirsky & Nolen-Hoeksema (1993), with the stem "*I am the kind of person who…*" added to some items to underscore self-perception. Items were rated on a Likert scale from 1 (*strongly disagree*) to 10 (*strongly agree*) and averaged, with higher scores reflecting more perceived self-insight (α_{T1} = .92 α_{T3} = .94).

Data Analysis Plan

Preliminary analyses were conducted to examine the effects of the mood induction and experimental manipulations and to examine randomization success. Main analyses consisted of repeated measures ANOVAs (RM ANOVA) to examine the effects of response manipulation on negative emotion, subjective and implicit emotional clarity, and perceived self-insight. Time was the within-subjects factor (baseline [T1], post-mood-induction [T2], post-response-manipulation [T3]), and response manipulation condition (distraction, rumination, mindfulness) was the between-subjects factor. However, perceived self-insight was assessed at only two timepoints (T1 and T3). When the assumption of sphericity was violated, *F* values were adjusted using the Greenhouse-Geisser epsilon. Significant time-by-condition interactions were probed using within-subjects contrasts to understand relative differences between pairs of conditions across time. When no interactions emerged, significant effects of time were explored using pairwise comparisons in the full sample and within each condition separately to examine effects of the negative mood induction (T1 vs. T2) and each response manipulation (T2 vs. T3). Effect sizes were expressed as partial eta-squared (η_p^2). Data were deleted listwise in the subjective emotional clarity model for two participants who did not rate emotional clarity at T3.

Results

Preliminary Analyses

Inspection of participants' open-ended descriptions prior to the mood induction revealed common themes, including loss (e.g., "My cousin was shot and killed"), interpersonal conflict (e.g., "My boss and I got into a huge argument, and I ended up getting fired"), and betrayal (e.g., "My boyfriend cheated on me with my roommate"). Most participants (n = 58; 67.5%) reported an event occurring over six months ago. Most rated the memories as unresolved, M = 2.55, SD=1.33 (mode = 1 [28.0%], *not at all*), and reported feeling bothered by them sometimes, M = 2.72, SD = 1.18 (mode = 2 [54.7%], *bothered every now and then*).

The global response fidelity check showed that the sample was attentive to response manipulations (M = 5.62, SD = 1.24), with no significant differences between conditions, $F(2, 71) = .589$, $p = .558$. One-way ANOVAS predicting debriefing indices indicated that each response manipulation produced the intended effects. There were significant effects of condition on rumination, $F(2, 71) = 39.82$, $p < .001$, distraction, $F(2, 71) = 17.71$, $p < .001$, and mindfulness scores, $F(2, 71) = 13.12$, $p < .001$, all in the expected directions (see Table 1).

As shown in Table 1, there were no differences between conditions in demographic variables or baseline ratings of key variables ($ps > .226$). Baseline negative emotion was correlated with lower subjective emotional clarity, $r = -.41$, $p < .001$, lower implicit emotional clarity (i.e., longer RTs), $r = .46$, $p < .001$, and lower perceived self-insight, $r = -.40$, $p < .001$. Higher subjective emotional clarity was correlated with higher implicit emotional clarity (i.e., shorter RTs), $r = -.32$, $p = .005$, and higher perceived self-insight, $r = .35$, $p = .002$, but implicit emotional clarity was not significantly related to perceived self-insight, $r = -.21$, $p = .079$.

Table 1. Demographics and Baseline Descriptive Statistics in the Total Sample and By Condition

Demographics	Full Sample $N = 74$	Distraction $n = 22$	Rumination $n = 28$	Mindfulness $n = 24$
Age (M, SD)	25.27 (6.58)	24.93 (6.68)	25.55 (6.46)	25.42 (6.85)
Sex (n, %)				
Female	52 (70.3)	17	17	18
Male	21 (28.4)	10	5	6
Other	1 (1.4)	1	0	0
Race/Ethnicity (n, %)				
White	52 (70.3)	20	16	16
Black	13 (17.6)	6	2	5
Hispanic/Latino	9 (12.2)	4	3	2
Asian/Asian-American	7 (9.5)	1	2	4
Native American	2 (2.7)	1	0	1
Other	4 (5.4)	1	3	0
Education (n, %)				
High School	5 (6.8)	1	1	3
Some College	33 (44.6)	12	9	12
4-year College Degree	20 (27.0)	8	6	6
Some Graduate School	5 (6.8)	4	1	0
Graduate Degree	9 (12.2)	3	3	3
Doctorate	2 (2.7)	0	2	0

Baseline Variables (M, *SD)*	Full Sample	Distraction	Rumination	Mindfulness
Negative Emotion	1.89 (.85)	1.81 (1.05)	1.80 (.98)	1.58 (.66)
Subjective Clarity	20.36 (4.64)	21.14 (3.62)	19.79 (5.24)	20.33 (4.82)
Implicit Clarity	1561.51 (769.34)	1632.89 (760.67)	1600.48 (811.84)	1450.63 (746.36)
Perceived Self-Insight	5.77 (1.46)	5.77 (1.35)	5.60 (1.74)	5.98 (1.22)
Debriefing Items (M, SD)				
Distraction	3.72 (1.55)	5.09 (1.47)[a]	3.13 (1.26)[b]	3.17 (1.13)[b]
Rumination	3.95 (1.49)	2.97 (1.14)[a]	5.32 (.82)[b]	3.25 (1.16)[a]
Mindfulness	4.01 (1.30)	3.33 (.97)[a]	3.75 (1.28)[a]	4.95 (1.05)[b]

Note. Participants were allowed to self-identify as more than one race and ethnicity. Implicit Clarity = response time (RT) to negative emotion ratings, where higher RTs indicate lower implicit clarity. Conditions did not significantly differ on any demographic or baseline variables, *p*s>.05. For the manipulation check, values in a row with different superscripts significantly differed from each at $p < .05$.

Main Outcomes

Negative Emotion

The RM ANOVA showed no between-subjects effect of condition on negative emotion, $F(2,71) = 1.79$, $p = .175$, $\eta_p^2 = .05$, but there was a within-subjects effect of time, $F(1.68,142) = 52.74$, $p < .001$, $\eta_p^2 = .43$, sphericity not assumed (Greenhouse-Geisser $\varepsilon = .84$, $p = .001$). The time-by-condition interaction was not significant, $F(3.36,142) = 2.49$, $p = .057$, $\eta_p^2 = .07$. Given the possibility that low power led to this marginal result, we followed up this effect to examine whether the direction of effects was consistent with robust prior findings that rumination prolongs negative mood (Nolen-Hoeksema et al., 2008). Replicating this effect, a within-subjects contrasts between rumination and distraction showed a significant time-by-condition interaction such that groups did not differ from baseline to post-mood-induction, $F_{T1\text{-}to\text{-}T2}(1,48) = .31$, $p = .581$, but the rumination condition showed smaller declines in negative emotion from post-mood-induction to post-response-manipulation, compared to the distraction condition, $F_{T2\text{-}to\text{-}T3}(1,48) = 4.32$, $p = .043$. Within-subjects contrasts between other conditions showed no time-by-condition interactions (rumination vs. mindfulness: $F_{T1\text{-}to\text{-}T2}(1,50) = .80$, $p = .375$, $F_{T2\text{-}to\text{-}T3}(1,50) = 1.20$, $p = .279$; distraction vs. mindfulness: $F_{T1\text{-}to\text{-}T2}(1,44) = .13$, $p = .725$, $F_{T1\text{-}to\text{-}T3}(1,44) = 1.59$, $p = .446$). Finally, within-condition pairwise comparisons showed that participants in all conditions experienced increased negative emotion from baseline to post-mood-induction (T1 vs. T2), all *p*s < .001. All participants then reported decreased negative emotion from post-mood-induction to post-response-manipulation (T2 vs. T3), *p*s < .001 for distraction and mindfulness and $p = .045$ for rumination (see Figure 1A[4] and Supplemental Table S2 for marginal means and standard errors for all pairwise comparisons).

Subjective Emotional Clarity

The RM ANOVA predicting subjective emotional clarity showed no between-subjects effect of condition on clarity, $F(2,69) = 1.92$, $p = .154$, $\eta_p^2 = .05$. We observed a significant effect of time, $F(2,138) = 12.53$, $p<.001$, $\eta_p^2 = .15$, sphericity assumed ($\varepsilon=.93$, $p=.08$), with no time-by-condition interaction, $F(4,138) = 1.40$, $p=.238$, $\eta_p^2 = .04$. Pairwise comparisons across time showed a significant decrease in subjective emotional clarity from baseline to post-mood-induction (T1 vs. T2), $p < .001$, but no significant increase was observed from post-mood-induction to post-response-manipulation (T2 vs. T3), $p = .073$. Within-condition pairwise comparisons revealed that the decline in emotional clarity from baseline to post-mood-induction was statistically significant in the rumination condition, $p < .001$, but not in the distraction condition, $p = .055$, or mindfulness condition, $p = .511$. There were no significant changes in clarity from post-mood-induction to post-response-manipulation for any of the three conditions, *ps* > .20 (see Figure 1B and Table S2).

Implicit Emotional Clarity

The RM ANOVA predicting implicit emotional clarity RTs showed no between-subjects effect of condition, $F(2,71) = 2.17$, $p = .122$, $\eta_p^2 = .06$. There was a main effect of time, $F(1.72, 142) = 16.85$, $p < .001$, $\eta_p^2 = .19$, sphericity not assumed ($\varepsilon=.86$, $p=.002$), with no time-by-condition interaction, $F(3.44,142) = 1.27$, $p = .286$, $\eta_p^2 = .04$. Pairwise comparisons across time showed no significant change in implicit clarity from baseline to post-mood-induction (T1 vs. T2), $p = .258$, but clarity significantly increased (reflected by decreased RTs) from post-mood-induction to post-response-manipulation (T2 vs. T3), $p < .001$. Within-condition pairwise comparisons showed the same pattern in each condition: no change in implicit emotional clarity from baseline to post-mood-induction, *p*s > .372, but increased

[4] All RM ANOVAs reported in the results were re-estimated controlling for the effects of gender. Findings were robust against gender throughout, and we therefore report unadjusted models for parsimony. Analyses adjusting for gender are available from the first author upon request.

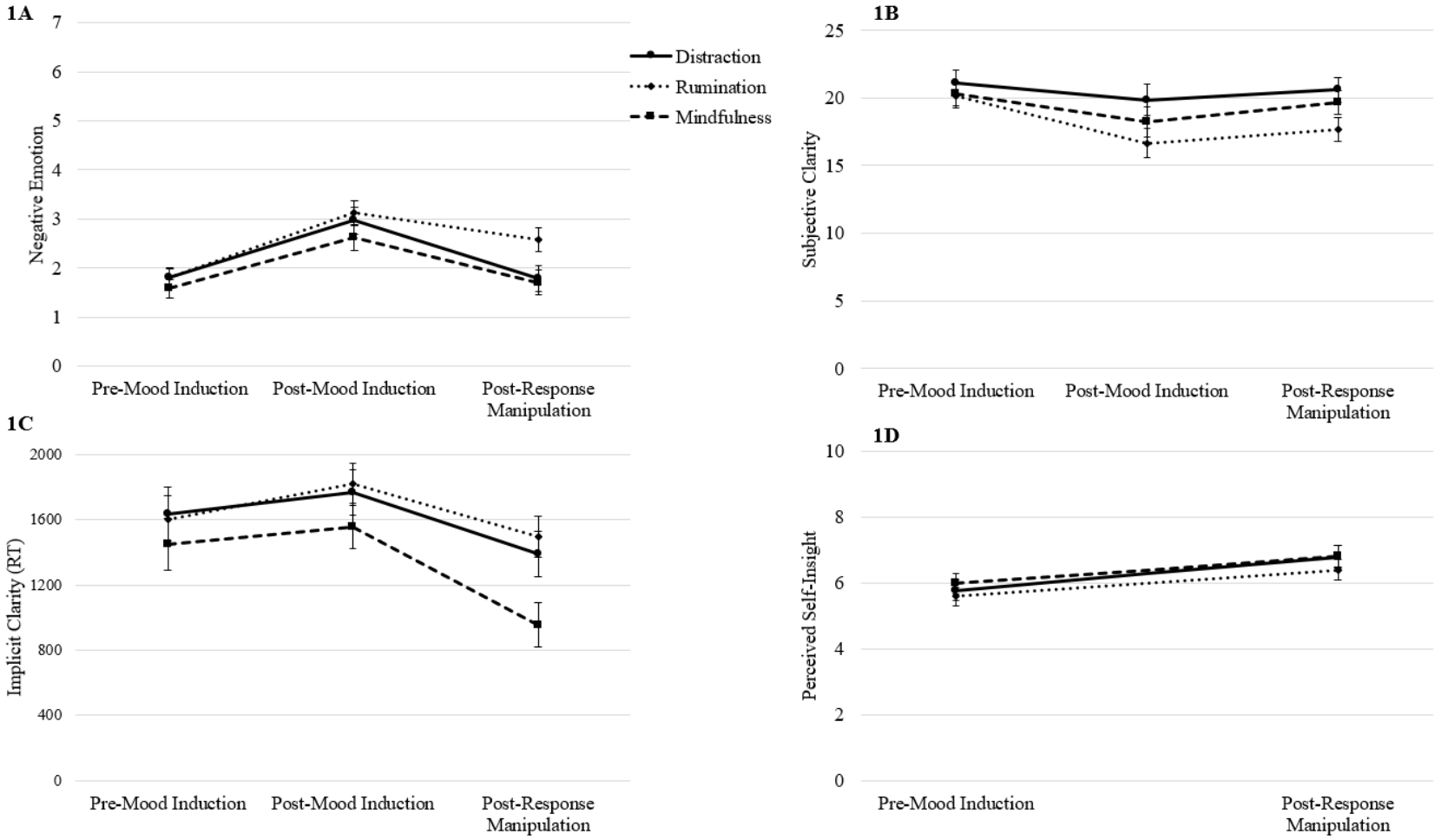
implicit emotional clarity from post-mood-induction to post-response-manipulation, *p*s < .005; (see Figure 1C and Table S2).

Perceived Self-Insight

Results of the RM ANOVA predicting perceived self-insight showed no main effect of condition, $F(2,71) = 4.43$, $p = .606$, $\eta_p^2 = .01$. There was a main effect of time, $F(1,71) = 86.11$, $p<.001$, $\eta_p^2 = .55$, with no time-by-condition interaction, $F(2,71) = .51$, $p = .605$, $\eta_p^2 = .01$. The main effect of time showed that all participants reported increased self-insight from baseline to post-response-manipulation (T1 vs. T3), $p < .001$. The results were the same when examined separately by condition: participants in each condition reported increased self-insight from baseline to post-response-manipulation, *p*s < .001; (see Figure 1D and Table S2).[5]

[5]We reanalyzed all RM ANOVAs adjusting for negative emotion, because all outcomes were correlated with negative emotion at baseline and because those in the rumination group reported more negative emotion following the response manipulation. Results remain largely unchanged from the unadjusted models reported above and can be found in the Supplement.

Figure 1. Repeated Measures ANOVAs Predicting Effects of Time and Condition on Negative Emotion (1A), Subjective Clarity (1B), Implicit Clarity (1C), and Perceived Self-Insight (ID).



Note. Marginal means and standard errors for pairwise comparisons are reported in Supplemental Table S2. Subjective clarity (1B) assessed via self-report, where higher scores indicate higher clarity; implicit clarity (1C) assessed via response time (RT), where higher numbers indicate lower clarity; perceived self-insight was not measured at T2. Error Bars Reflect Standard Error.

Discussion

Despite perpetuating depressive mood (Nolen-Hoeksema, 1991), ruminating might help people better understand themselves and their problems (Parageorgiou & Wells, 2001; Watkins & Moulds, 2005). In trait-level research, however, rumination shares an inverse relationship with emotional clarity (Salugero et al., 2013; Vine et al., 2014). In our experimental study, we found no evidence that rumination impairs emotional clarity; rumination produced increases in implicit emotional clarity and perceived self-insight much like those following distraction and mindfulness. Our finding is consistent with the idea that ruminating might help people make meaning and seek answers about what they feel (Papageorgiou & Wells, 2001; Vine et al., 2014; Watkins & Moulds, 2005). As this is the first study on this question, replication studies are needed to confirm whether rumination enhances self-understanding. If rumination has perceived or real benefits for self-understanding, this could be a mechanism reinforcing this maladaptive process (Lyubomirsky & Nolen-Hoeksema, 1993). Future work could also examine the mechanisms of rumination and confirm whether, as speculated previously (Vine et al., 2014), ruminating is a spontaneous response to feeling unclear about one's emotions.

Interestingly, the effects of rumination on emotional clarity were statistically equivalent to effects of distraction and mindfulness. We expected that distraction and mindfulness would benefit emotional clarity at least in part because of concurrent alleviation of negative emotion (Huffziger & Kuehner, 2009; Singer & Dobson, 2007); less intense negative emotion states should be easier to understand than more intense ones. Rumination similarly elevated emotional clarity, but this occurred in the context of higher negative emotion. Compared to distraction, the rumination group exhibited prolonged negative emotion, consistent with well-established effects (Nolen-Hoeksema et al., 2008). This distinct affective context raises questions about whether rumination, distraction, and mindfulness benefit emotional clarity via shared versus unique mechanisms. Distinct cognitive processes could have increased emotional clarity by different means, according to principles of equifinality. Perhaps distraction increased clarity by reducing distress, whereas mindfulness did so by recruiting affective information from attention to the body (Roemer et al., 2015). Maybe the perseverative nature of rumination increased clarity by making affective information more cognitively accessible. These and other mechanistic hypotheses could be explored in future studies.

Similarly, all conditions produced statistically equivalent increases in perceived self-insight. This result is interesting because the measure used here assessed *beliefs* about one's perceived self-insight, which suggests that even dispositional characteristics about one's self-understanding are malleable, at least in the short-term. Our results are partially consistent with Lyubomirsky and Nolen-Hoeksema (1993), who found that dysphoric and non-dysphoric students induced to ruminate reported higher perceived self-insight compared to a distraction response manipulation. Our results build on this finding by showing that rumination, distraction, and mindfulness are all capable of *increasing* people's baseline sense of self-understanding, though the longevity of these gains remains a question for future work. Further, mirroring the emotional clarity results discussed above, these conditions could affect perceived self-insight via distinct pathways. For example, rumination and mindfulness may increase self-understanding by drawing greater attention to one's thoughts and bodily sensations. Distraction, by contrast, might improve one's emotional state and spur problem-solving and instrumental behaviors that are positively reinforcing and capable of building self-efficacy (Nolen-Hoeksema et al., 2008).

The similarity of effects across all conditions might at first glance suggest that response manipulations failed. However, experimental features mitigate concerns about the validity of our manipulations. We used response manipulation prompts from past research (Huffziger & Kuehner, 2009; Nolen-Hoeksema & Morrow, 1993) and administered debriefing items to assess their success. Participants also reported equivalent fidelity to the response manipulation instructions across all three conditions. Moreover, condition-specific debriefing questions showed each condition was experienced in the intended way. For instance, those assigned to ruminate reported more negative, past-focused, self-focused thinking, while those assigned to mindfulness reported more attention to their bodies, etc.

(Table S1). Together, these quality checks suggest the response manipulations were distinct and operated as intended.

We were surprised that neither rumination, distraction, nor mindfulness affected subjective emotional clarity. The discrepancy between subjective and implicit clarity findings points to the value of assessing clarity using multiple methods. Subjective emotional clarity is believed to tap global attributions about one's emotion abilities (Robinson & Clore, 2002). This could possibly explain why response manipulations yielded different effects on subjective and implicit clarity, but it would not explain divergent findings across subjective self-report measures (i.e., subjective emotional clarity vs. perceived self-insight). Moving forward, it will be important to assess the effects of rumination and other response styles on emotional clarity and related constructs using multiple indicators, to identify potential sources of inconsistency. As for implicit emotional clarity, we cannot rule out alternative variables that could influence RTs to negative emotions. Fatigue and low mood, for example, are two variables not measured here that might be associated with decreased RTs. Though the use of random assignment and the within-subjects nature of our analyses help mitigate concerns about individual differences in these variables affecting RTs to negative emotion, it is possible that intraindividual changes across the experiment contributed to changes in RTs. We recommend that future work identify and account for these variables and others to better understand the factors involved in using RTs to negative emotion items as a measure of implicit emotional clarity.

Our study has important limitations. First, our sample overrepresented White females and college-aged students, limiting generalizability to other populations with different emotional experiences. Second, we did not conduct an *a priori* power analysis and some findings emerged at trend-levels, indicating that we may have been underpowered to detect small effects. A sensitivity analysis showed that we were powered at .80 to detect small effects (i.e., f =.17 at an alpha of .05) for within-between interactions. Though our sample size is akin to past work on experimental manipulations of rumination, distraction, and mindfulness in non-clinical (Kuehner et al., 2009) and clinical samples (Huffziger & Kuehner, 2009), the true effects of these response manipulations might be smaller than we were powered to detect or larger in clinically depressed, dysphoric, or remitted samples used in past work (Nolen-Hoeksema et al., 2008). We therefore cannot rule out that similarities observed across conditions were due to low statistical power or use of a non-dysphoric sample. Third, we did not train participants in the response manipulations of interest nor did we assess individual differences in habitual strategy use (i.e., how often people use the strategy on average) or the efficacy of implementation (i.e., how "good" or "bad" people are at using the assigned strategy), which likely affect how people experience their emotions (e.g., Lask et al., 2021). Though the use of standardized prompts partially tempers these issues, future research might benefit from including formal training in each condition, measuring how participants use each strategy on average, and by assessing how effective participants are in implementing the provided strategies. Finally, because we did not measure self-insight directly after the mood induction, we cannot speak to whether the response manipulations first depleted and then restored it, or whether self-insight increased steadily across the protocol.

This study also has several strengths. We used an idiographic mood induction allowing participants to self-generate complex emotional reactions to stressful personal memories. The fact that participants rated memories as unresolved and bothersome, along with participants' open-ended description of these memories, suggests that we successfully achieved high external and internal validity with this induction. This design allowed participants to experience a diverse range of naturalistic emotions instead of inducing the same specific negative emotion for all participants (e.g., sadness or anger induction). Given that we were interested in emotional clarity, specifying a target emotion would have been problematic, as it would have suggested to participants how they were "supposed to" feel and thereby invalidated our measures of emotional clarity. Our unconstrained approach might have unintended consequences, however, as some participants may have generated more complex emotional experiences that makes achieving clarity more difficult. We also assessed emotional clarity using a variety of methods, including self-report and a well-established implicit measure using response time (Lischetzke et al.,

2005). We focused on state-level changes in emotional clarity to capture emotional processes that are typically addressed at the trait level, avoiding known response biases (Robinson & Clore, 2002). Finally, another strength previously discussed was our response manipulation checks.

Despite a growing literature examining the downstream effects of emotional clarity on rumination, few studies have examined the reciprocal impact of rumination on emotional clarity. Replicating prior work, we found that rumination was less effective than distraction in reducing negative emotion. We observed that rumination led to increases in implicit emotional clarity and perceived self-insight that were comparable to those of distraction and mindfulness. By contrast, no conditions exerted an effect on subjective emotional clarity. Together, we found preliminary support for the notion that rumination might benefit emotional clarity, despite keeping people entrenched in negative emotions. If borne out by replication, this might suggest a positive reinforcement cycle helping to perpetuate this otherwise impairing cognitive process.

Additional Information

Supplementary Materials

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Conflict of Interest

We have no conflicts of interest to disclose.

Ethical approval

All study procedures were approved by the Yale University Institutional Review Board and were completed in concordance with the ethical standards set forth by the American Psychological Association.

Data Availability

Requests for the data or materials from this manuscript can be sent via email to the senior author at vera.vine@queensu.ca.

Author CRediT Statement

Conceptualization – All authors
Methodology – Vine
Formal Analysis – All authors
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