**Reviewer 1 Feedback:**

This reviewer provided high-level feedback regarding our manuscript, which makes it difficult to respond to point-by-point as I had with other reviewers. As such, we will go through the review line-by-line to address each critique but will note if the point had already been addressed in a previous response. All following references to page numbers will refer to the tracked changes version of the manuscript:

“*In Study 1, the authors discuss their hypotheses related to 'intensity' as if intensity was an independent variable that's being manipulated (as it is in Gal Sheppes foundational work) but this may not be appropriate in Study 1.”*

**Our response:** While we make comparisons to Sheppes’s work as the preeminent standard to which almost all emotion regulation research is currently being contrasted to, we do explicitly note that our approach is observational and that emotion intensity is not experimentally manipulated in Study 1 (e.g., “*It must be noted that we did not directly manipulate emotional intensity within this design*…”, pg. 53). Even when discussing model composition, we are careful to use terms such as ‘predictor’ rather than ‘independent variable’ when describing our terms to avoid fueling misinterpretation that our variable was experimentally controlled (e.g., “…*models using either z-scored emotional intensity or person-centered emotional intensity as the primary predictor were constructed iteratively…*”, pg. 23). As such, we make this comparison in awareness of this difference. As noted in recent edits (See pg. 20), both approaches demand assumptions and we chose to make design decisions in line with the broader ecological-validity goals of this project.

We agree that intensity captured as a standardized rating (as occurs in the Sheppes paradigms) and intensity captured as an idiographic self-report metric (as occurred here) is an apples-to-oranges comparison, but one that has to occur if psychology is to pursue pragmatism. Both represent some facet of experienced affective intensity, or at least that’s how I believe an audience interprets them.

which we believe constitute a valuable contribution to the field, given the lack of extant literature attempting to translate this already well-trodden effect beyond   
  
While having access to standardized ratings would have been an excellent supplementary analysis (and we discuss reasons why we weren’t able to achieve in that in edits to page 53),

However, we added additional statements to emphasize the differences between this study and previous studies on pages 6, 9, 20, 27, 31, and 53.

*“In this study, intensity is a measured variable (not a manipulated one) and in this particular study context, it's very likely an outcome of regulation just as much as it's a predictor of regulation.*”

**Our response:**

*“For example, another way to interpret figure 4 is that - rather than this being an unexpected finding - it might be exactly what one would predict if emotional intensity was the \*outcome\* of regulation success, which it very well could be given that these variables were measured at approximately the same time: as regulation success decreased, emotional intensity increased.”*

**Our response:**

*“For this reason, I wouldn't necessarily feel comfortable with this take-home message, which makes it sound like the present results are the opposite of what prior work has demonstrated: "Though the extant literature from comparable lab studies should motivate us to expect the efficacy of distraction to increase and reappraisal to decrease as affective intensity increases, our data seems to document a deviation from this pattern in a high-intensity, quasi-naturalistic setting: distraction appeared to be less - not more - successful as affective intensity increased.”*

**Our response:**

*“Aside from this moderation effect, the main effect between intensity and strategy choice was the primary analysis in this study, and there was no reliable/significant association found. That could be informative but, given the nature of the study, I'm not sure how informative this null association is. If emotional intensity drives use of distraction vs. reappraisal, we'd expect a positive association between intensity and distraction use. But if [lower] emotional intensity is also the \*outcome\* of successful regulation - especially distraction, which was the modal strategy used - we'd expect a negative association between intensity and distraction use. These two patterns operating at once could yield a null result, which could explain Study 1 findings.”*

**Our response:**

*“This alternative interpretation also tracks with what the authors found in Study 2: when new participants are told that a given event is higher vs. lower intensity (i.e., intensity is manipulated here, rather than measured like in Study 1), they choose distraction (vs. reappraisal) more often.”*

**Our response:**

*“This is essentially a conceptual replication of the Sheppes work because intensity is manipulated (i.e., given to participants) and isn't really comparable to the intensity variable in study 1, which is a complex experience that is likely being affected by regulation as much as it's affecting regulation.”*

**Our response:**

*“For this reason, Study 2 can't effectively be used to help explain the pattern of results from Study 1.”*

**Our response:** We completely agree, which is why we stated.

*“The authors then conducted Study 3 to learn whether the link between intensity and distraction choice (vs. reappraisal choice) would be present in forecasted regulation contexts (like Study 2) but not in executed regulation contexts (like Study 1).”*

**Our response:** You misunderstand.

*“But by my read, this isn't the core difference in the findings between Study 1 and 2 and so when I saw that the experimenters were again manipulating intensity in Study 3 (this time with pre-piloted lower vs. higher intensity film clips), it seemed fully reasonable for them to replicate the 'canonical relationship' between intensity and distraction, which they did. This pattern makes good sense if Study 2 and 3 are interpreted as solid conceptual replications of the original Sheppes work, where intensity is carefully manipulated for participants.”*

**Our response:**

Based upon the highlighted passages, we interpreted this feedback as containing three primary but related points, which we will response to here:

1. The authors discussed intensity in Study 1 as if it were a manipulated variable rather than an observed variable. The use of observed emotion intensity is a substantive deviation from the Sheppes paradigm, limiting the extent to which results from the two can be compared.

**Our response**: If intensity were a product. We don’t observe a negative association. We observe a positive one but it does not pass standards of statistical significance. We aren’t attempting to explain study 1 with study 2. I state in the fucking article they are incomparable. Study 3 is the closest we come to a direct replication. Study 2 is not a conceptual replication.

1. Because this study does not attempt to manipulate emotion, we cannot know whether self-reported emotion intensity captured post-exposure is a product of or precursor to self-regulation.
2. A null association is inherently ambiguous. The results of the supplementary analyses contained within could be explained by emotion as either a product or precursor. Therefore, our conclusions may be overstated.

**Our response:** If Figure 4 represents emotion intensity as a product rather than a precursor, then

We do not overstate the null effect, but we accurately report our supplementary results. They are not meant to be taken as nullifying the original results. I think we are very clear that we are extending that paradigm to places it hasn’t been before. WE make no claims that the original hypotjesis wouldn’t be observed with trained subjects or less intense circumstances. Just that we observed this pattern in this context.

**Our response:** Why is it very likely? It’s perhaps equally as likely, but I have no reason to believe that it is more likely. There are two dissociable components to this critique. We agree that the analysis that we used did not manipulate and did not seek to manipulate emotions. Why would manipulation of an emotion differ from a non-manipulated one? They make more assumptions than we do, as they often use the standardized measure of emotion intensity rather than the experienced emotion. Though not perfect, some degree of affective manipulation did occur in the study. Subjects traversed the haunted house through 4 sections: two of which were deemed high intensity by the designers and two of which were deemed to be of lower intensity. When we analyze regulation behavior using these metrics, we still find the same regulatory behaviors.

Acknowledge that one means of accomplishing this could have been to have independent raters quantify the intensity of each event and to use those. This was difficult in our case given the time scale we had to work on.

Make the point that by taking an observational approach and capturing personal experience, we're forced to assume that the personal experience that they report is pre-regulation. However, if we were to take an IAPs approach, we'd also be forced to assume that the standardized value as we have represent personal experience.  
  
It is true that intensity could be either a product of or precursor to self-regulation in this study. While we argue that the language that we used (and "did you attempt to reduce or intensify this emotion?") suggests that we intended to target the emotional precursor (i.e., you may be able to prepare regulatory responses, but you cannot regulate something that hasn’t happened).   
  
. We [will change language in the manuscript to reflect this].

Specker et al., 2023 found that pre- and post-regulation emotion ratings were highly correlated (r = 0.81, p < 0.001). As best as I can tell, no one has tried to assess whether post-regulation intensity is a significant predictor of affective intensity. I could not find any articles which have explored whether there are still statistically significant differences in the post-regulation intensity of high and low intensity images after regulation. Shafir et al. 2016 did find a main effect of emotion category upon attenuation with a considerable effect size (ηp2 = 0.83), but this was calculated as the difference between the self-reported post-regulation rating and the standardized rating, so the baseline may be somewhat unreliable for this purpose.   
  
I tried searching for an emotion regulation dataset in which to test this theory, but none were publicly available.

**Our response:** It’s not intended to be read as the opposite, but it is intended to document the same phenomena. If the only want to comment upon Sheppes’s reappraisal and distraction is by using standardized stimuli in controlled settings with trained individuals whose experience we can dissect then no naturalistic study will ever comment upon them. We will be doomed to forever entertain a pristine, bottled version of emotion regulation that psychologists fawn over and the kind of self-regulation that everyone else actually has to use.

**Our response:**

**Our response:** I think it’s a large assumption to assume that this study design manipulates intensity. We’re just telling them intensity was. We can’t say Study 1 doesn’t match Sheppes but Study 2 does just because Study 2’s conclusions better fit Sheppes if we squint. There are many aspects of Study 2 that still differ from Sheppes, such that I would never call it a straight forward replication. It’s simply more similar in the specific dimension of controllability.

**Our response:** Make the point that studies 2 and 3 were not straight forward Sheppes replications. we get some patterns that are consistent with sheppes, specifically in participants that are either extremely psychologically distant from the experience (study 2) or making predictions about what should be normatively done for others (study 3). Emphasize the ways in which StuThe none option, measuring intensity before and after, using independently assessed intensity to predict self-regulationdy 3 deviated pretty substantially from Sheppes in both design and outcome.

Examine whether the other study that used individual intensity ratings also deviated from Sheppes [[SPECKER 2023]]

Claimed to be the first to measure pre- and post- regulation intensity in order to assess the importance of regulation flexibility (Specker 2023). Had Sheppes as a middle author. Their language is very confusing, but I think what happened is they aggregated trials and found, across the sample, an association between intensity and choice, but when they disaggregated, they found no association between intensity and choice. They kind of dismiss the latter finding as not having enough experimental control.

**One possibility is that although individuals from clinical groups can use reappraisal successfully when cued, they fail to appropriately identify moments at which ER would be helpful in everyday life. Alternatively, members of clinical groups may in fact be able to identify moments at which ER would be helpful but for one reason or another choose not to use reappraisal very frequently in everyday life (as implied by Dryman & Heimberg, 2018). It is also possible that laboratory use measures capacity, which is an overestimate of actual success everyday life. Finally, it is possible that this disconnect between reappraisal use and success is an artifact of the way these constructs are measured (cumulative emotion ratings on a laboratory task vs. self-report responses). The source of the disconnect could have important implications for ER intervention science, which would respectively focus on using reminders and encouragement to select and initiate reappraisal in everyday life, improving conditions for implementing reappraisal in everyday life, or developing, refining, or combining measures of reappraisal that most closely correspond to documented emotional difficulties in clinical groups.**

**Reviewer 4 Feedback:**

No comments