Reviewer 2 Feedback:

Major Comments

1. "These approaches offer strong-validation and unparalleled control but might not accurately reflect the multidimensionality of emotional experience." — I don't quite follow this conclusion. The approaches do not offer "strong validation" or "unparalleled control" - they are just people rating images."

    Our response: We agree with this point and have updated our language to be more precise about what these approaches offer the field: "*These approaches offer an efficient, reliable, and standardized means of assessing self-regulation but might not …*"

2. "In the methods section it states, "Of the 182 self-reported events… 30.7% used reappraisal and 61.5% used distraction…" and later, a small proportion used suppression, and a negligible proportion used situation modification and situation selection. This information seems like it would be more useful to the reader if placed in the results section. In general, the results section could use more organization to make sure the key points sink in and have their space."

    Our response: We believe this was a misunderstanding stemming from unclear language. The 182 events we refer to here were not observed during Study 1, but rather during our pilot. We cite this statistic here as a justification to focus primarily on reappraisal and distraction in Study 1. However, we did update the language to hopefully make this more clear. We also do take the point about rearranging information in the results more generally and have made some modifications to further emphasize the key points, including breaking larger sections into smaller ones with more specific headings, rearranging sections, and adding sentences to further emphasize the takeaways for each section.

3. "In the "covariates" subsection, it is stated, "to assess each variable's potential covariation with affective intensity in predicting regulation usage" — do you mean each variable was tested as being a moderator? And later, it is stated that each variable was tested for "collinearity with affective intensity". But the only result reported is that cognitive load failed to predict the type of emotion regulation strategy. Where is everything else? It is also seems unusual, given the rest of the subheadings in the results section, to use "covariates" as a subsection when the inferential statistic is testing a specific hypothesis. In "Experiment 2 Methods", the purpose becomes clearer - "…even when adjusting for noted moderators like cognitive load." Perhaps that goal can be made more explicit above. "

    Our response: Thank you for highlighting this. The intention was to highlight that we did not find evidence to suggest nuisance variables that commonly might confound the relationship between intensity and usage, such as time of time or cognitive load, had any statistically significant relationships to our predictor or outcome variables. Earlier versions of the manuscript did include a detailed description of the analyses conducted and results for each nuisance variable, but these were cut both for word limit concerns and to direct audience attention to the details and analyses more central to the primary purpose of the study. However, these analyses, justification for these analyses, and the results of these analyses remained present in the markdown script at the time of submission.

    Our preregistration for Study 1 specifically noted that Cognitive Load may moderate the relationship between intensity and usage and, thus, was specifically mentioned, though we did not find the hypothesized association. This statement felt out of place and we agree that this section demands general revisions. As a result, we have altered the title, framing, and briefly summarized the results of each analysis. We also separated cognitive load from this section to its own section to more closely reflect our preregistration. If the reviewers feel further specific corrections are required or that this should be moved to supplemental materials, we would be happy to do so in a future revision at their request.

4. "In study 1, the descriptions were used to code for regulation strategies. In study 2, were the same descriptions given to a new set of subjects, who then indicated which regulation strategy they would use?

If so, is there a concern that subjects from study 2 could determine the regulation strategy that subjects in study 1 were using, and relying on that information to make their judgments? ”

Our response: Thanks for highlighting this, too, as it is a point that we should better clarify. Participants in Study 1 were asked to first describe the emotionally salient event in as much detail as they could, and to then separately describe in their own words how they attempted to regulate that event. Participants in Study 2 were presented with the event descriptions, but not the regulation descriptions. The event descriptions were screened for any indications of distraction or reappraisal (e.g., “… so I looked at my shoes”, “… so I imagined that this was a movie”, “… I closed my eyes and ran”) prior to being used in Study 2 and none were identified. As such, we do not believe that the event descriptions in and of themselves unduly directed Study 2 participants to choosing one of the available options, but I have revised the description of Study 2 to make that more apparent.

5. “The authors refer to environmental affordances as a potential factor that explains which regulation strategies end up being selected in a given situation. I think this is a nice way of interpreting the results. I think the section focusses too much on a passive viewing perspective (i.e. on how the environment grabs attention). The situation may also dictate the action affordances one has available. From Figure 4 examples, there are clearly actions available like running to the next room. It's unclear how reappraisal and distraction interact when these "escape" opportunities are there, too, in naturalistic contexts. Action affordances in general seem like an important aspect of what separates emotion regulation in everyday life v. in decontexualized experimental settings, which could be developed a bit further in the manuscript.”

Our response: Again, we agreed completely that this was a valuable-but-missing point in our original manuscript and have expanded our discussion to incorporate it. We specifically added an additional paragraph under the environmental affordances section which we believe captures the spirit of this request.

6. “I'm wondering if the introduction might also benefit by more explicitly framing the argument that the study is testing the external validity and generalizability of conclusions from laboratory paradigms to situations that can occur in everyday life more generally. The current framing emphasizes more narrowly focusses in on certain dimensions (multimodality of experience), but I think it may help to nest this notion as a subset of the broader theoretical question at hand, which concerns external validity. The authors might also consider the following articles to help situate the theoretical space:

Miller, L. C., Shaikh, S. J., Jeong, D. C., Wang, L., Gillig, T. K., Godoy, C. G., ... & Read, S. J. (2019). Causal inference in generalizable environments: systematic representative design. Psychological inquiry, 30(4), 173-202. — which argues for the importance of representative design in psychology.

Lee, K. M., Ferreira-Santos, F., & Satpute, A. B. (2021). Predictive processing models and affective neuroscience. Neuroscience & Biobehavioral Reviews, 131, 211-228. — which argues in section 5 the importance of external validity and representative design based on predictive processing models of the brain (and integrates with constructionist theory insofar as emotions are constructed by volleys of an integration of predictions and prediction errors) ”

Our response: This is a framing that we were trying to incorporate, especially in earlier versions of the manuscript, so we are more than happy to lean further in that direction. We have added the noted citations as well as some additional ones in the third section of our introduction: *Generalizability of Extant Emotion Regulation Paradigms.*

7. “I'm wondering if the discussion section could also broaden to highlight other studies that challenge external validity - might there be other studies in emotion or in memory wherein findings in the lab do not converge with those conducted in everyday life settings? If so, it would bolster support for this study in contributing to a broader movement that underscores the importance of external validity and generalization to everyday life. ”

Our response: We did attempt to expand upon this in the section on Cold-Hot Empathy Gaps by citing literature emphasizing differences in outcomes when measuring regulatory phenomena in typical vs. optimal usage. We also try to emphasize the previously cited FeldmanHall et al. study slightly more strongly.

Minor Comments

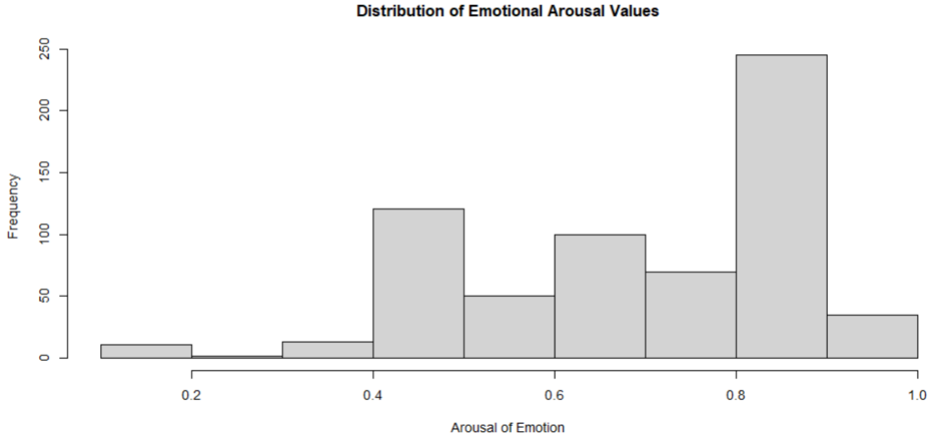
1. "I'm wondering why the plots show Emotion Intensity as z-scores (Fig 2, 3, e..g) - might the actual scale participants completed be informative here? Also, the endpoints of the scale for emotion intensity are unclear (even in the methods section; page 14). Is Emotion Intensity calculated by averaging over each emotion indicated per person and haunted house situation? I'm guessing not… but in general, this measure could be clarified in the writing. Relatedly, is it in principle possible that someone could have indicated "calm" or "sleepy"? In that case, would emotional intensity mean intensely calm or sleepy? How about intensely sad? While intensely afraid or excited seems to make sense when thinking about emotional intensity as arousal, in some of these other cases I'm a bit confused. Ultimately, it may just help to clarify what, exactly, emotional intensity is referring to. "

Our response: We apologize for the confusion. The range of possible intensity scores (0-4), as well as the average score (2.44) is listed on page 18. However, I have updated the description on page 14 to include the actual labels participants saw to hopefully better represent the experience.

We believed that using z-scored scales rather than the original raw scales would improve interpretation, but we are open to the feedback and criticism regarding this supposition. Our rationale was that the original scale may have limited generalizability beyond the scope of this study (e.g., what does "I feel a 2 out of 4" really mean?), but a standardized scale has a readily-accessible and universally-accepted interpretation, which could facilitate cross-model comparisons. To perhaps achieve a 'happy medium', we've modified our plots to show the raw scale values but report the statistics for the standardized relationships where relevant in the text of the manuscript. These instances either explicitly state that the variables were standardized or use a symbol associated with a standardized effect (i.e., β) when reporting statistics. Again, though, we are open to feedback if the reviewers feel an alternative style of presentation might be a better fit.

Regarding how emotional intensity was calculated, we have clarified the association between usage and intensity on page 15: "*Thus, rather than exploring this phenomena at the event-level, which might require regressing the probability of using a strategy upon the average intensity of all emotions experienced in that event – an assumption we would not make in confidence - we draw associations between regulation strategy usage and the emotions that participants identify as directly motivating them.*"

In principle, this design would allow for participants to select intensity-emotion pairings which might be unexpected, such as 'very intensely sleepy'. In practice, though, such an indication may be slightly more interpretable, as although I keep referring to this variable as 'intensity' for linguistic convenience, the labels participants actually selected to describe each emotion are less arousal-specific. For example, a participant who indicated feeling sleepy would have the options to select being "not at all", "a little", "a moderate amount", "a lot", or "a great deal" sleepy. The emotion responses that participants wrote were piped into the subsequent questions about that emotion so that participants could assess that emotion on this scale. Again, while perhaps not without flaws, I do think these labels provide a better description of what we're capturing when we talk about intensity. Additionally, as you suggested, we rarely saw low arousal words used by participants in practice. The following plot is the distribution of arousal values associated with each emotional response participants provided, as determined by Mohammad (2018)'s NRC lexicon. The distribution appears positively skewed ($\bar{x}$ = 0.698, *median* = 0.773, *sd* = 0.175), indicating many of our responses were much closer to 'afraid' or 'excited' than calm. To perhaps better illustrate the types of responses that this design elicited, we added a new figure containing a word cloud of all responses participants gave, as well as those responses that met eligibility for our primary analysis. Hopefully this helps alleviate confusion and adds clarity to this aspect of our study.



2. “Several models were run to address contra-hedonic regulation activity (page 20). The exploratory findings are presented in Table 1. One model surpassed a "traditional" threshold, which I think means an uncorrected alpha level. As a side note, perhaps the term "nominal" might be better here. This model seems like it would involve a lot of parameters. Does the modeling approach penalize for model complexity? Separately, I think the motivation to address contra-hedonic regulation and the broad conclusion that it is not the case is useful to state in the main manuscript. However, the details of the method and results (null findings), including Table 1, might be better suited to the supplementary materials (unless there is a bigger point to this table that I'm missing). ”

    Our response: Thanks for noting that; I’m frankly embarrassed that I forgot to include the adjusted significant values. We have updated both the manuscript and table with that information and reframing. The significance of this approach was in response to criticisms we had received in the past that our approach might be obfuscating an otherwise well-established effect that should exist in the data. We wanted to minimize researcher bias and demonstrate that we could not find this effect no matter how we filtered the data. Yes, our modeling approach penalizes model complexity and identifies which of the two compared models are most parsimonious. I am fine with moving those to the supplemental materials and have done so.

3. “For study 2 methods, "reviewed examples of how both strategies might be employed" - it may be informative to include concrete descriptions of the examples that were used to train participants since these may influence the priors for the test items. ”

    Our response: We have added these to the manuscript.

4. “In the discussion, it states, "Affective intensity predicted regulation extent…" I couldn't connect "regulation extent" to anything in the results section. ”

    Our response: This was an oversight on my part. This was measured in the pilot study and was originally included in the primary materials, rather than in the supplementary materials where we currently have it placed. I have corrected this language, added an explanation, and noted that the finer details are contained with the supplemental materials.

5. “Some primary results are presented in the discussion, which seems a bit unusual. To focus the discussion, I suggest developing these alternative account in the results section. In particular, "Although

participants were instructed to not discuss their experiences, the group context in which the experience occurred may have influenced behavior choices and cognitive perceptions. However, post-hoc analyses failed to find any association between group membership and strategy usage (F(30,45) = 0.93, p = 0.57). The presence and strength of friendship among group members was also assessed and was not predictive of regulation (t(60) = -0.4, p = 0.70)"

> **Our response:** This was another situation in which I worried noting so many additional auxiliary statistical tests that we ran for a specific purpose (e.g., assessing sample or study limitations) some distance away from the context in which they become relevant (e.g., the limitations section) might become confusing or overwhelming, but it seems that it had the opposite effect. We have tried to centralize all of these types of analyses within the results section as requested.

6. "Is there a typo in Figure 4A, "jot" should be jolt?"

> **Our response:** Thanks for highlighting this; we have corrected it.

Reviewer 3 Feedback:

Major Comments:

1. "On p. 28, the authors distinguish between the effects of emotional intensity on the choice of emotion regulation strategies in "low-stimulation" and "high-stimulation" paradigms (concluding that intensity does inform choice in low, but not high stimulation settings). It took us a while to understand what was being referred to here as the authors previously framed the difference between Study 1 and Study 2 in terms of experiencing (Study 1) versus reviewing (Study 2) emotional experiences. This may be a more appropriate framing, partly from the perspective of consistency, but also because the authors do not present evidence that forecasters (in Study 2) found the reviewed events less emotional / stimulating than participants who experienced those events in Study 1. "

"The authors also need to be careful in this section not to slip back into asserting that Study 1 measured participants choice of regulation strategies. Indeed, it might be prudent to consider the limitations of comparing findings between two studies which differ in a number of other ways (as the authors' point out in their introduction), including the way that emotion regulation was measured. In short, it may not be differences in (overall level of) emotional intensity between Study 1 and Study 2 that drive the difference in the way that people say they would choose to regulate (Study 2) or report regulating (Study 1) their emotions between specific situations that differ in emotional intensity."

"The best way forward might be to conduct and include an additional study that directly manipulates whichever variable (or variables in a factorial design) the authors believe accounts for the difference in the effect of emotional intensity on emotion regulation (e.g., experiencing vs. reviewing emotional events, being trained in specific strategies vs. being untrained). An additional study would prevent the authors having to draw comparisons between studies (both within their manuscript and others' research) and provide confidence in conclusions."

> **Our response:** We do agree with this point, also feel that the framing was inconsistent in the original submission, and have since revised our Study 1 and Study 2 framing to focus on experiencing and forecasting. Additionally, we made efforts to minimize the differences between forecasting and experiencing by running a third study which manipulated both emotion as well as the conditions under which participants use or choose regulation strategies. We were not able to explore these variables in a field study design as we had in Study 1, but we believe that our approach addresses many of the concerns you had noted while still somewhat improving upon the ecological validity of comparable paradigms in many ways. We look forward to your feedback on it.

2. "Emotional intensity was not manipulated, but rather was measured (retrospectively) using self-report. This is a significant limitation because retrospective recall of emotional experience may differ significantly from in-the-moment experience. This methodological decision is also difficult to

understand, given that it might have been possible to manipulate intensity within a haunted house. If an additional study was conducted (as suggested above) then this limitation could be addressed, but it certainly needs to be considered in the section on limitations."

> **Our response:** I am unfortunately not sure what you have in mind to manipulate intensity within the haunted house. I apologize if we are missing something that may seem obvious to you, but we have added a footnote to perhaps add further clarity: "*The haunted house has a limited seasonal run time, and we cannot experimentally manipulate the intensity of the events in the haunted house as it is run by a private company*." Though designing and maintaining our own 'attraction' might be theoretically possible and offer some greater degree of control, the resources required to create our own version of this at the same quality as this would be unattainable. The set, events, and actors within the setting we used are chosen or designed by the organization that we had partnered with to run the experiment, but we did not have the ability to influence this.
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> Although in an ideal world we would capture descriptions of the events as they occurred, this would ruin the immersion, have undesired effects upon the emotional experience, and would have fundamentally altered the way that participants experienced subsequent events within the haunted house. If the primary objective was to document how untrained participants self-regulate when unprompted to do so, interrupting the experience to ask self-regulation-related questions may have prompted introspection and later self-regulation, as noted on page 15. Additionally, there were many events that participants could have self-identified as emotionally salient and significant variation in which events participants did identify as emotionally salient.
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> We do agree with both of your points about limitations, however, and have expanded our limitations discussion to include both.

3. "It was great to see that the authors considered the effectiveness of the strategies that participants chose. However, the authors conclusions may be too strong considering how effectiveness was measured (i.e., participants reports of how successful their regulatory efforts were). For example, on p. 21 the authors state that "our data seems to suggest the efficacy of using distraction within this high-intensity, quasi-naturalistic setting to be of a lesser magnitude than what had been found in lab studies wherein distraction was used". The authors need to provide references for the previous work that has looked at the effectiveness of distraction in response to high-intensity stimuli - both in the results section and in the introduction - and tone down the conclusions given the differences in the way that efficacy of regulation was measured."

   > **Our response:** We had not intended to make an intercontext or objective comparison between settings, but to rather highlight that the relationship between reappraisal and distraction that one might expect to see from lab studies has appeared to deviate in this context. As such, we have revised our wording to hopefully make that more clear.

4. "On p. 30, the authors state that "affective intensity predicted regulation extent but not usage"; however, it is not clear how "regulation extent" was measured. This should be clarified in the methods and/or results section of Experiment 1."

   > **Our response:** Thank you for highlighting this. Including this statement on page 30 was an oversight and it has since been corrected. Regulation Extent referred to the effort or extent to which participants tried to self-regulate. It was originally addressed in the primary manuscript, but was moved to the supplemental materials in this revision. We have updated our language and noted the specific wording of the to hopefully make more clear how this variable was assessed.

5. "It was disappointing to read that the physiological data collected from participants in the pilot study has been reported elsewhere; namely, in a paper in Neuropsychologia. The paper in Neuropsychologia does not focus on how participants regulated their emotions, but the heart rate data provides a useful validation

of the intensity of the emotion experiences and so would have been useful in this manuscript. We also note the suggestion that a third 'forthcoming manuscript' will report on a memory test given to participants (p. 13). It may be appropriate to split the data in this way, but the authors should do so having carefully considered issues around 'salami-slicing of publications' and the pros and cons of contributing to a proliferation of academic content."

> **Our response:** We had taken this criticism into consideration and attempted to conduct additional exploratory analyses where possible using the physiological and regulatory data from the pilot and Study 1. However, we did not design the capture of physiological data within the pilot study to answer specific hypotheses regarding self-regulation. While physiological data was captured continuously and time-locked, specific events participants experienced were not. We were able to estimate approximately when specific events may have occurred in the physiological time-course for analyses beyond the scope of this manuscript, given the consistency in the haunted house design. However, for the purposes of this manuscript, both the temporal estimation and the one week delay between exposure and strategy self-report would make assessing the reliability of any outcomes we find from this analysis challenging. Though we intended to use physiological data for exploratory analyses of self-regulation in Study 1, technical issues during collection left us with insufficient data to reasonably conduct the analyses that we intended.
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> While we do note that 'salami-slicing' is a valid concern in scientific publishing, we strongly believe the contributions that each of these manuscripts provide to the field are largely orthogonal to one another. We as collaborators pursued this rather complicated, resource-intensive project to build a shared resource – a rich, novel dataset - that we could all pull from to answer our disparate, respective questions. Salami-slicing often fragments narratively-related or similar results, but the overlap present in each manuscript produced from this dataset largely ends with having used the same dataset. We do reference certain methodological decisions within this manuscript relevant to memory or physiology, but that is primarily for the sake of transparency so that our audience understands why perhaps-otherwise confusing decisions were made (e.g., the week delay between exposure and self-report during the pilot). Attempting to adequately explain the complex literatures informing the disparate hypotheses explored by each subdomain (i.e., memory, physiology, self-regulation), explaining each of the distinct analytic approaches used, reviewing the results, and contextualizing the contributions each of the findings in every subdomain would likely yield an unwieldy and difficult-to-comprehend manuscript. As such, though multiple publications using this dataset may be produced, I and my collaborators feel strongly that these publications are not characterized by the redundancy or quality dilution that characterize instances of salami-slicing.
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> If there are other specific features of this manuscript that do lean more towards the "salami-slicing" side of things but which I could correct, whether those be concerns about citation practices, transparency concerns, etc., please let us know. We are open to feedback and hoping to pursue best practice.

6. "Finally, please simplify language where possible. For example, "had predictive utility towards" can just be "predicted". "…the affective intensity experiencers reported" (p. 26) could be "…the intensity of the emotions that experience reported" etc. The authors should also be consistent with the language used throughout the manuscript to ensure that the conclusions drawn accurately reflect what has been measured. For example, when discussing the findings of Study 1 throughout the manuscript, the authors should refer to strategy use and not choice (throughout)."

> **Our response:** We have reviewed the language and corrected where appropriate. Choice is referenced when referring to much of the cited literature but use or forecasting is used when referring to what we've measured in this study. Please let us know if you still feel that the language in this revision is not sufficiently economical or consistent.