Response to reviewers’ comments on manuscript *Differential effects on pain intensity and unpleasantness of two meditation practices* by Perlman, Salomons, Davidson, and Lutz.

Below, the reviewers’ comments are indicated in italic type with our response in roman type. We thank the editor and the reviewers for their helpful comments and suggestions. The table of re-calculated statistics can be found at: http://brainimaging.waisman.wisc.edu/~perlman/0903-EmoPaper/review/revisedStatsTable.pdf

**Reviewer 1:**
The report feels premature because a) of the small sample that is said to be in the process of being expanded, and b) there are imaging data that accompany every behavioral trial. Why not report all the relevant data?

**Reviewer 2:**
...the report is quite thin without physiological or performance data corroborating the changes in cognitive processes and in self-reports of pain in the different conditions and without further information documenting group differences in pain catastrophizing or pain-related anxiety.

**Reviewer 3:**
Self-report measures are notoriously problematic without correlated physiological or outcomes data.

1. In response to the reviewers’ requests for corroborating data:
Unfortunately the analysis of the fMRI data is delayed due to technical problems with some of the data set, which have not been resolved yet. In response to the reviewers' requests, we also attempted to analyze the recordings of heart rate monitored by the scanner. However, we encountered similar difficulty with this data.

We have unfortunately no additional data to add to this brief manuscript at this time. We still believe that this manuscript is relevant for this current issue on mindfulness, as it reviews the current literature on mindfulness and pain, and illuminates the possible regulatory mechanism on pain of meditation-based clinical interventions such as Mindfulness-Based Stress Reduction (MBSR).

**Reviewer 2:**
The expressions ‘Open Presence’ and ‘Open Monitoring’ seem to refer essentially to the same construct. If so, the same term should be used throughout. Otherwise, this distinction needs to be clarified.
Reviewer 3:
3) The authors introduce the terms of focused awareness and open monitoring in the introduction, but then appear to test Open Presence, which "shares common qualities with Open Monitoring-type practices." It is unclear why much emphasis is put on the discussion of OM (or why it is discussed at all) when it is not part of the experimental paradigm. It may help to guide the reader if the authors instead spent more time describing the "complex" practice of Open Presence, as this is what was tested. Further, the authors discuss focused awareness and open monitoring, with reference to their own theoretical work as a basis for their definitions and instructions (Lutz 2008), as well as to Gunaratana (2002). Are the authors describing Shamatha or possibly concentration meditation? Gunaratana describes concentration meditation, but nowhere in his book refers to this as FA. Again, given the "newness" of this field of study, which is now studying a long tradition of specific methods of teaching these practices, it is unclear how the introduction of the new terms of FA and OM adds benefit over confusion. It is important to unify definitions rather than introduce new ones. Bishop (2004) reported on a scientific consensus definition of mindfulness, and multiple authors have reported definitions used in the practical acquisition of these techniques (e.g. in the vipassana tradition: Gunaratana 2002, Goldstein 1993, Flickstein 2001, Goenka 1987. This also holds for both Zen and Shambala/Tibetan schools). Importantly, Bishop's definition fits with those that are taught as part of MBSR and other mindfulness-based treatments, whereas the acceptance/non-judgmental component does not appear to be part of the FA meditation. If the authors wish to forward their definitions, they will need to justify why their definitions, over those that have been taught for many years warrant study, and will then need to train people without prior mindfulness practice in these techniques such that these can be adopted without the influence of this practice.

2. In response to Reviewers 2 and 3’s comments about the meditation practices:
After some discussion, including consultation with experts in the tradition, we have come to the conclusion that the subtle differences between the specific traditional practice of rigpa chok shak, which we were translating as Open Presence, and the descriptive category of practice which we have termed Open Monitoring do not warrant the introduction of additional confusion here. Accordingly we will re-write to refer to Open Monitoring, with a reference for those who are interested in the traditional account of rigpa chok shak/Open Presence.

We agree that Bishop's (2004) definition of mindfulness accurately covers the purpose of most, if not all, Buddhist and Buddhist-like practices in a wide variety of traditions, and so admirably serves a unifying purpose. However, many accounts, including Bishop (2004), divide this process into two interrelated parts: one relating to the control of attention, and the other relating to an open, accepting stance on experience. There is no suggestion that these parts are entirely separable, and indeed it seems unlikely that any practice tradition would have a reason to want to separate them entirely. However, the
two components of the model do have different qualities which are compatible with a
cognitive conceptualization, and this calls out for an experimental approach that could at
least begin to analyze possible differing contributions of the two components. Our goal
in setting out the FA/OM distinction in Lutz et al. (2008) was to set the stage for this
investigation, in particular taking advantage of the fact that some traditions, at least, have
a selection of practices with more or less emphasis on one or the other of these
components, even if they do not attempt or wish to separate them entirely. In addition,
we hoped to establish standard terminology for the research community to use when
studying this distinction, grounded in a detailed analysis of several different accounts of
the practices.

In the current manuscript, since we are interested in the differential effects of the two
components, we chose to use the separate labels for those two components set forth in
Lutz (2008). We do not mean to imply any criticism of Bishop’s formulation of the
definition, we simply found it convenient to use differentiating terms that were already
familiar to us from our previous publications.

To further clarify the problem with OP and OM: The term "Open Monitoring" was
coined in Lutz et al. 2008 to categorize a class of meditation practice-related cognitive
processes which occur in various guises in various places. OM could be used to refer
specifically to a cognitively delineated component of a meditation practice, such as the
second component of Bishop’s (2004) two-component model. Alternatively, it could be
used as a categorical label to classify a particular practice which emphasizes that
component. In this study, the long-term practitioners were practicing a specific
traditional practice called rigpa chok shak, which has been translated in previous
literature (e.g. Cambridge Handbook of Consciousness chapter Lutz 2008) as "Open
Presence". The instructions for this practice emphasize OM-type processes of open
acceptance, so we feel it is appropriate to refer to it with the categorical label OM. In this
study our primary interest is in the differential effects of the two components described
by Bishop (2004) and also by Lutz (2008), rather than in any characteristics unique to OP
which separate it from other OM-type practices, thus our decision (prompted by
reviewer's comments) to simplify our terminology and refer only to OM in this context.
We are currently considering other research projects which would be intended to study
specific, unique properties of OP which would not be the same as OM. Unfortunately,
our thinking about these other future projects spilled over into our thinking for the current
manuscript. Thanks to the reviewers it is now very clear that we made a mistake with
that.

Finally, although it was not specifically raised here, we would like to address one further
point that may not have been entirely clear in the manuscript. One might ask, if
mindfulness itself has two components, which we claim map onto our two component
labels of FA and OM, then how can we classify mindfulness as OM? Although
mindfulness practices can be described in terms of the two components, as in Bishop (2004), in most cases it is agreed that the goal of the practice is to develop the open, accepting awareness, and that such attentional control as is involved is, in some sense, a means to this end. This contrasts mindfulness from a practice such as shiné / shamatha, which is inherently oriented towards the development of attentional control.

We accept that, as usual, there is considerable overlap between different categories, as well as between different systems of categorization, but we believe that the distinction between the cognitive process categories we have named FA and OM is relevant for this type of investigation.

Some of the confusion has arisen because we felt that this short manuscript was not an appropriate place to get into an extremely detailed exploration of different definitions and terminology. We hope that, with the help of the reviewers' insights, we have been able to revise the manuscript to be more clear in its own context. However, the more detailed discussion of some of these issues must be left to the references. Also, it may be appropriate to publish a more detailed unification of Bishop's terminology and ours from Lutz 2008 in another context. We will look into the possibility of doing this in cooperation with other interested researchers.

Reviewer 2:
Seriously, there is no way one could come close to a valid match of subjects or fully control response biases in such a study so let's just admit there is a variety of confounds to be acknowledged, the most important of which should be explained, including group differences at baseline, group differences in the interpretation of instructions and implementation of cognitive strategies (partly mentioned in the discussion), suggestions/expectations, and demand characteristics.

Reviewer 3:
4) Related to point #3, the authors note that they incentivized novice practitioners to practice meditation techniques based on a bonus payment for BOLD signal decrements during pain. As practices such as concentration meditation are "goal-less", with specific emphasis on letting go of being attached to a certain outcome, this may introduce a confound in the comparison of novices to experts: striving (novices) versus non-striving (experts). Though interesting, this question may be different than measuring FA and OP, especially as it is likely that the experts have been taught non-striving as an integral part of their practice. This should be discussed as a potential confound in the discussion section.

3. In response to Reviewers 2 and 3’s comments about incentivization of the control
We agree wholeheartedly that the idea of monetary incentivization as a control for the lifetime devotion of the long-term meditators is insufficient. This incentive was included because it was requested by reviewers of previous studies we have done with this population. We will clarify and amplify our acknowledgement of the various unavoidable confounds raised here. In addition we will add an acknowledgement of the side effect of the incentives raised by Reviewer 3.

Reviewer 1:
Did the one expert with fibromyalgia influence the results in any way? - maybe this person has greater experience with pain regulation?

Reviewer 2:
Chronic pain patients should be excluded from the study. The authors argue that the inclusion of a fibromyalgic (FM) patient would work against their hypothesis but it seems equally or even more plausible that it would work in favor of the hypothesis as chronic pain patients may also develop an expertise in coping with pain! This hyperalgesic subject (the primary clinical complaint in FM) may also skew the distribution of pain sensitivity in experienced meditators and may reduce baseline differences between groups. Chronic pain may also affect the brain morphology as well as the brain response to acute pain so I would advise excluding those participants from the brain imaging study as well. Any other medical condition potentially involving pain or cognitive/emotional impairment should be part of the basic exclusion criteria.

4. In response to Reviewers 1 and 2’s questions about the participant with a previous diagnosis of FM:
We thank the reviewers for calling attention to this. The wording of the original manuscript was inaccurate. The participant was diagnosed with FM about 20 years ago, but considers him/herself 70% improved and would no longer meet the diagnostic criteria at this time. Exclusion of this participant and re-analysis did not appreciably change the significance of any tests. See table.

Reviewer 1:
Did the one expert of Tibetan background differ noticeably? Such quirks carry even more weight in a small sample.

5. In response to Reviewer 1’s question about the Tibetan participant:
Exclusion of this participant and re-analysis did not appreciably change the significance of any tests. See table.
Reviewer 1:
I did not find it intuitively obvious that: "Our results ... support the interpretation that reducing the cognitive elaboration of a sensory experience can reduce the perceived unpleasantness, which is consistent with the theoretical framework of mindfulness meditation." -- Can you elaborate why OP is less cognitively elaborate than FA? (or what was meant, if that's not it?)

6. In response to Reviewer 1’s question about cognitive elaboration and unpleasantness of sensory experience:
Please see above in our reply number 2. We hope this will be more clear thanks to the simplified terminology. The introduction refers to the background literature on how elaboration can exacerbate an emotional response to, particularly, pain, primarily with reference to Kabat-Zinn’s and (now included, although this reference was accidentally omitted originally) Bishop’s foundational descriptions of the mechanism of benefits of mindfulness. Our description of the practices, including the instructions for the OM practice we used, hopefully makes a case that the OM-type practice in this manuscript is characterized by a reduction in elaboration. We agree that the deleted discussion of OP made it seem rather more elaborate.

Reviewer 2:
There is a potential advantage in looking at highly experienced practitioners as these may reveal more robust effects. However, one major limitation of studies comparing experts to novices is the difficulty to control for confounding factors. Differences between groups may be explained by baseline factors independent from meditation practice itself but which may be associated with the likelihood that one may engage in meditation practice. One way to better assess this possibility is by testing whether the practice itself (frequency, duration, years of training, nb and frequency of retreats??) predicts group differences in pain regulation. Although this retrospective assessment is not sufficient to establish a solid causal link, it may provide some evidence that group differences are indeed related to the actual practice.

7. In response to Reviewer 2’s question about hours of practice:
Determination of hours of practice and its effect on particular tests is complicated by various factors. It has only been possible to obtain an estimate of total hours of practice, not separated for the two practices. Since both practices might be expected to be affected by hours of practice, and the effect in question in this manuscript is a difference between practices, we did not believe that correlations of this effect with hours of practice would be informative.

Other major confounding factors include ethno-cultural differences between groups. Simply comparing meditating and non-meditating Tibetans or meditating
and non-meditating Americans would be more appropriate.

8. In response to Reviewer 2’s question about ethno-cultural differences:
All but one of the long-term meditators in this study were Caucasian Americans or Western Europeans. See above for re-analysis excluding the one Tibetan. Results are not materially affected by eliminating this participant.

I understand that at least some of the experienced meditators were brought to the testing lab from another country, with some coming directly from a long-term meditation retreat (p. 10). An example is given of a participant leaving an 11 years meditation retreat to come directly to the lab for testing. Wouldn't this require some adjustment period? Has there been a reasonable time allowed for subjects to adapt to the new environment? There may be basic stress-related effects associated with travelling to a new environment that may affect pain perception and cognitive processes.

With the known interactions between sleep quality and both pain and cognition, I would worry that testing participants soon after their arrival from a long trip (jet lag effects) may affect their baseline pain sensitivity as well as their ability to self-regulate pain.

9. In response to Reviewer 2’s inquiry about an adjustment period for participants who traveled:
Most of the participants included in this data set traveled from within the continental US. The few that traveled farther had been in the US for at least three days before the beginning of the experiment. Participants all had at least one night of sleep in a hotel nearby before beginning testing.

The introduction appropriately refers to the concept of catastrophizing but the study did not include a questionnaire of pain catastrophizing (PCS) (or pain-related anxiety, hypervigilance, or fear of pain). It might be possible to get this data from the participants (the PCS is relatively stable over time).

10. In response to Reviewer 2’s request for data from the PCS:
At the time this project was originated it did not occur to us to collect the PCS, but now we agree that we should have. We are including it with all future pain-related data collection. Unfortunately it will not be logistically possible for us to obtain the PCS from most of the participants in this study before the publication deadline.

The report includes a discussion of this important limitation (i.e. lack of baseline condition) but the authors argue that experienced meditators would
likely "revert to some meditation practice when no task was otherwise demanded of them." I don't understand how a baseline condition using the target temperature would be problematic and the calibration phase could be valid (p. 19: "This procedure was performed while not practicing any kind of meditation"). The argument implies that effects of the cognitive intervention are intermingled with baseline group differences. In such case, the methodological indication would be precisely a group comparison of baseline differences in pain perception (no intervention), and then testing additional modulatory effect of a specific state relative to that baseline in each group. Baseline group differences could then be interpreted in light of additional individual measures of meditation practice, pain catastrophizing or anxiety, or reports of spontaneous engagement in FA or OP states at baseline.

11. In response to Reviewer 2’s question about why a non-meditating baseline would be invalid during the experiment, but valid during the calibration procedure: The calibration procedure requires continuous monitoring and evaluation of the sensation, and a readiness to respond when a threshold is reached. In contrast, during the experiment, the stimulus is presented during meditation with no performance demand until the end of the block. Thus, after discussion with the experts in the tradition whom we consulted with, we believe that compared to the meditation blocks in the experiment, the calibration procedure is a cognitively demanding task that will "set" the practitioner's mental state, rather than letting it revert to meditation. This is now clarified in the manuscript.

Of course we acknowledge that this still leaves a notable limitation. To circumvent the problems with "resting" baseline, in future studies with this population we will attempt to find active control conditions that can be considered comparable across groups.

The description of the pain protocol is incomplete. The rate of temperature changes and the ISI for both the calibration and experimental phases should be reported.

12. In response to Reviewer 2’s request for details of the thermal stimulus: These rates were added to the manuscript.

Were there indications of sensitization or habituation? If so, the order of conditions should be controlled for statistically.

13. In response to Reviewer 2’s question about response changes over time, and the order of conditions: We thank the reviewer for raising this possibility. Investigation of changes in ratings across trials indicates no significant sensitization or habituation in any conditions, except
one: the unpleasantness ratings for the novices showed a significant decreasing linear trend over time, \( p=0.046 \). This trend was the same for both practices, interaction \( p=0.974 \), so it is unlikely that it would effect the results of interest.

The order of meditation practices was not counterbalanced across participants. The reason for this is that the original design of the experiment took into consideration the traditional order in which the practices are engaged within a session, since this would be how the LTMs were accustomed to practice. We agree that counterbalancing would be desirable for statistical reasons. However, the order was the same between the two groups, so order effects are unlikely to explain the group differences that comprise our findings of interest reported here. To check this, we re-analyzed excluding the first trial, which was FA, and the last trial, which was OM. Excluding the first trial, the remaining data begins with OM and ends with FA. Re-analysis resulted in no changes in significance of any tests. See table.

These matters are now acknowledged in the manuscript.

The maximum temperature used was 49 deg C but some subjects did not reach the target pain level of 8 on the 0-10 pain scale. This ceiling effect implies that the mean and SD temperatures reported in Table 1 do not adequately reflect the temperatures required to produce that level of pain across subjects. How much pain was produced to the maximum temperature of 49 in those subjects? This observation further reinforces the need for a baseline condition against which the experimental conditions should be contrasted within-subjects to control for baseline individual differences in pain sensitivity.

14. In response to Reviewer 2’s question about ceiling effects of the maximum 49C temperature:
We thank the reviewer for raising this issue. There is a moderate difference in overall ratings between the group of participants (3 novices and 3 experts) who used 49 degrees, and the rest. The 49-degree group had somewhat lower ratings, \( F(1,15) = 2.52, p=0.133 \). However, no interactions between the 49-group factor and any other factor were significant, \( ps > 0.4 \) (except practice x group49, \( p=0.21 \)). Re-analysis including this additional between-group factor resulted in no changes in significance of any tests. The Group x Rating type interaction during OM only, which was originally marginally significant at \( p=0.083 \), became even more marginal, \( p=0.164 \). Since this test did not quite reach significance in the original analysis, it seems likely that this was a power limitation in either case.

What was the unpleasantness of the pain rated at 8/10 in the calibration phase?
15. It was a limitation of our calibration procedure that unpleasantness ratings were not measured during the calibration. In future protocols we are using a more advanced psychophysical calibration procedure that provides more detailed information about each participant’s pain response characteristics.

The lower ratings obtained for unpleasantness than intensity is characteristic of heat pain and should not make the interpretations of other effects "ambiguous" (p. 14).

16. In response to Reviewer 2’s comment about lower ratings for unpleasantness than intensity:
We thank the reviewer for pointing this out. We agree that this statement is inappropriate and have changed it, with reference to background literature on intensity and unpleasantness ratings.

How close to p=0.05 was the interaction between Group and Rating Type during OP (p. 14)? Stats should be reported.

17. In response to Reviewer 2’s question about the Group x Rating type interaction during OM:
The Group x Rating type interaction during OM was at p=0.083, while Group x Rating type during FA was at p=0.347. These statistics were presented in the results section of the original manuscript.

Contrary to the statement made in the first paragraph of the discussion (p. 14), I believe that Grant and Rainville did report larger effects of mindfulness on unpleasantness than intensity although the significance threshold was not reached on the latter variable. This seems generally consistent with the present results in the OP condition. I expected that the discussion would raise methodological differences to explain the differences in the results between those quite similar studies rather than a general statement that the reason for those differences are "not clear". There seems to be several methodological differences that might explain those differences in results: confounding group differences between experienced and novice meditators (see above), type and duration of meditation training in the experienced meditators, training of novices, within-subject baseline, instructions to attain a meditative state before the experimental conditions, task instructions?

18. In response to Reviewer 2’s comments about Grant and Rainville’s results:
We thank the reviewer for raising this issue and clarifying previous results. We had based our statement on the significance for intensity versus unpleasantness, but re-examining that paper (Table 2) we now see that indeed the mean difference for the meditators was greater for unpleasantness than intensity, for mindfulness relative to baseline, although not by much (1.26 vs. 1.25). We agree that our previous statement was inaccurate. Also, it seems that discussion of the methodological differences may have been inadvertently deleted during editing. This is certainly important to be raised here. The discussion has been changed to more accurately represent these previous results, and to consider the methodological issues the reviewer points out.

The notion of expert meditators is based on the total number of hours the practitioners have accumulated. Ideally, expertise should be based on some measure of performance. Given that an operational performance criterion might be difficult to achieve, it would be more appropriate to describe the experts operationally as long-term, highly-trained, or experienced meditators.

19. In response to Reviewer 2’s comment about expertise of meditators: We thank the reviewer for this suggestion. This terminology has been changed in the manuscript. "Expert" has been replaced with "long-term meditator" or "LTM".

Several scientific journals are now requiring that effects sizes be reported. This seems quite relevant here as there are power issues associated with some negative findings.

20. In response to Reviewer 2’s request for effect sizes: We thank the reviewer for bringing this omission to our attention. Effect sizes have been added to the manuscript.

Given that the ratings of intensity and unpleasantness are not fully explained by each other, and assuming that there is no ceiling or floor effect on either measure in any subject (to be verified), it may be useful to compute an index of relative unpleasantness based on the ratio between the two measures (or %). This may help reveal additional effects.

21. In response to Reviewer 2’s suggestion to use a ratio index of unpleasantness/intensity: We thank the reviewer for this very useful observation. We are planning on using indices such as this in future studies, using a much larger data set to compare the utility of various indices such as the ratio or the difference. In the current analysis we found it most convenient to do it this way. In the current results, the highest-order interaction of interest was rating type * practice * group. This is equivalent to using the difference
between the two measures, rather than the ratio, and testing practice * group. Re-analysis using the ratio of unpleasantness to intensity is, in fact, more sensitive in this data set. Using this ratio, the Group x Practice interaction is significant at p=0.0003 (improved from p=0.005 in the analysis reported in the manuscript). This is definitely a promising avenue for future exploration.

In the introduction, distraction from pain is argued to be more relevant than attention to pain in the FA condition. However, the reverse argument is made in the discussion where the authors indicate that the instructions given here in the FA condition are at odds with the usual attentional stance taken by experienced meditators who may focus on the pain or on the affective responses to pain in order to better self-regulate pain experiences.

22. In response to Reviewer 2’s comments about our discussion of distraction and FA: The introduction was merely drawing a parallel between the instructions used in the FA condition of this experiment, namely direction of attention away from pain, and the body of literature on distraction of attention away from pain. It was not meant to be an argument. The "reverse argument" in the discussion was merely an attempt to formulate a potential explanation why the usual effects of distraction from pain, well established in the literature, were not seen in this case. This necessarily consisted of pointing out flaws in the parallel drawn in the introduction. A note to this effect was added to the introduction to make this more clear.

The discussion of divided attention is quite interesting. Miron et al (1989), referred to in the introduction, have found that pain is not significantly modulated in the context of divided attention. This ref would be relevant here too.

23. In response to Reviewer 2’s comment about divided attention: We thank the reviewer for bringing this to our attention. This reference is very helpful in this context. It was helpful to support several different points and has been added to this portion of the discussion.

CBT should be spelled-out (p. 15).

24. In response to Reviewer 2’s note about CBT: We thank the reviewer for calling this to our attention. This was corrected in the manuscript.

Please add pagination to the manuscript.

25. In response to Reviewer 2’s request for pagination of the manuscript:
We thank the reviewer for calling this to our attention. Pagination has been added to the manuscript. Sorry about that oversight.

**Reviewer 3:**

*Further, the authors discuss focused awareness and open monitoring, with reference to their own theoretical work as a basis for their definitions and instructions (Lutz 2008), as well as to Gunaratana (2002). Are the authors describing Shamatha or possibly concentration meditation? Gunaratana describes concentration meditation, but nowhere in his book refers to this as FA.*

26. In response to Reviewer 3’s question about FA and Shamatha: “shiné” in Tibetan is an approximate equivalent to “shamatha” in Sanskrit, and "focused attention" is nearly synonymous with "concentration" in English. These terms will be more clearly connected in the text. The reference to Gunaratana was replaced with a reference more specific to the practice terminology of this tradition.

*Bishop (2004) reported on a scientific consensus definition of mindfulness, and multiple authors have reported definitions used in the practical acquisition of these…*

27. In response to Reviewer 3’s note about Bishop’s consensus definition of mindfulness: We apologize for omission of reference to Bishop's 2004 definition and thank the reviewer for bringing this to our attention. We have improved the manuscript to include reference to Bishop's two-component model at the point where we are discussing the two components of mindfulness practice. Also see more discussion of terminology above.

*The authors may wish to do a more thorough literature search on pain and mindfulness paradigms. There are several published studies involving the cold pressor challenge (e.g. Kingston 2007).*

28. Thank you for calling this to our attention, we have included additional references including Kingston 2007.