Author's response to reviews

Title: A Metacognitive Perspective on Mindfulness: An Empirical Investigation

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Editor
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Dear editor

We are very thankful for the thorough review of our study and the opportunity to revise our article. Our response to the reviewers’ comments follow on the following pages.

Sincerely,

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Reviewer #1

1. Firstly, the authors use the term of “metacognition” inaccurately what leads to some misunderstandings. In the article there is no reference to metacognition as such, in its original and broad meaning, i.e. cognition about cognitive phenomena and ability to control and monitor cognitive processes (Flavell, 1979). Instead, the term metacognition is described as an equivalent of the phenomenon called Cognitive Attentional System. The “Metacognitions Questionnaire 30”, which was used in the present study, measures not metacognition as such but several metacognitive beliefs typical of some disorders. Therefore the title of the article, the ways in which the authors contrast metacognition and mindfulness in the introduction as well as some conclusions in the discussion can mislead potential readers. The problem is important because mindfulness can be easily understood as a metacognitive in its nature, and metacognition is rather a more general phenomenon that can manifest both as mindfulness and CAS. This misunderstanding has consequences throughout the article.

Reply: The revised manuscript provides more background to the concept of metacognition and we have included additional references. With regard to the MCQ-30; it measures two dimensions of metacognition considered important in the S-REF model; metacognitive knowledge (beliefs) and metacognitive awareness (i.e. cognitive self-consciousness). In accordance with the reviewer’s point we now clearly specify that the MCQ-30 measures metacognition as implicated in the metacognitive model of emotional disorders. It is also clearly stated already in the abstract that: “The primary aim of this study was to explore how metacognition, as implicated in metacognitive theory of emotional disorder (Wells and Matthews 1994)”.

The following references are included in the reference list:


2. Definition of mindfulness in unclear. In one place the authors cite the Kabat-Zinn`s well-known definition often used in clinical context (65-67), while in the next sentence they implicitly refer to the Brown and Ryan`s (2003) understanding of mindfulness conceptualized as a opposite of acting on “autopilot” (68-69). These two ways of understandings significantly differ when we want to refer mindfulness to metacognition. While the former seems to be metacognitive in its essence, the latter is not necessarily directly related to metacognition. So, the way the authors definite mindfulness throughout the article should be clearly chosen.

Reply: We have included the following in the introduction: “As noted by Jankowski and Holas (2014) the abundance of meanings related to the term mindfulness makes it difficult to define. In the current study mindfulness is understood and operationalized according to the FFMQ.”

3. Further, the authors suggest that in mindfulness-based approaches (MBA) disorders result from a state of mindlessness or “autopilot” and contrast them with the metacognitive therapy (MT) in which improvement is facilitated by “detached mindfulness (Wells and Matthews, 1994, Wells 2005), which is a reaction to thoughts that is the opposite of the CAS, involving standing back and not reacting or trying to deal with them but remaining flexible with low levels of ideation” (77-80). The problem is that in MBA non-reactivity, decentering or meta-cognitive insight (Teasdale,1999) are considered as main therapeutic factors, often more important than activity with awareness. So there is no such contrast between mindfulness used in MBA and “detached mindfulness” as the authors suggest. Moreover, the FFMQ which is used in the present study as operationalization of mindfulness, consists of such scales as “non-reactivity”, “non-judging”, “observing” which obviously overlap with the concept of “detached mindfulness”.

Reply: We briefly touch upon detached mindfulness in the introduction. However, detached mindfulness is not measured in the current study. Thus we would prefer not to put further emphasis on detached mindfulness in the manuscript as it detracts from the use of the MCQ which measures metacognition (not detached mindfulness). We do include the following statement in an attempt to meet the reviewer’s comment: “See Wells (2005) for a closer description of detached mindfulness and its similarities and differences from mindfulness.”

Also, as the reviewer denotes, the FFMQ consists of elements which could overlap with detached mindfulness. In our study we don’t have a measure of detached mindfulness. Instead we measure metacognitive beliefs. In order to accommodate the reviewers’ comment we have included the following in the discussion: “The study did not measure detached mindfulness as described in Wells’ metacognitive theory. As such the study cannot address the relationship between mindfulness and detached mindfulness.”

4. In my opinion, the re-interpretation of obtained factors should be considered. The first factor can be interpreted just as disposition to intensively control one’s own thoughts perceived as dangerous. I suppose it is just the CAS. In this context the meaning of particular aspects of mindfulness – non-judging and acting with awareness – should be discussed. The label of the first factor – ‘metacognition’ – is too general and therefore misleading, because it suggests that the second factor – ‘mindfulness’ – has nothing to do with metacognition.
However the aspects of mindfulness not included in the first factor can be rooted in the other set of adaptive metacognitive beliefs which initiate mindful observing, non-reacting and describing. Therefore it is possible that both factors might be interpreted as metacognitive, and the first expresses non-mindful metacognition while the second expresses mindful metacognition.

Reply: The reviewer is correct in that the first factor corresponds with metacognitive beliefs as specified by Wells’ metacognitive model of emotional disorders. As previously mentioned the revised manuscript denotes that we use the term metacognition as specified by Wells and not metacognition in general.

5. When comparing mindfulness as it is present in MBA with detached mindfulness in MT the authors use single items from chosen measures. I think it is not a good approach – theoretical constructs should be compared on the theoretical level – their properties and essence should be derived from their definitions and theoretical base but not from the ways they are operationalized. Single items might be just misleading.

Reply: The presentation of items is used in order to give an indication of similarities and differences with regard to how the different constructs have been operationalized. We agree that constructs should be compared on a theoretical level, but that does not exclude also examining operationalizations, comparisons between items, and empirical investigations as to how these theoretical constructs relate to each other.

6. The following statement is problematic: “However, in the metacognitive model cognitive self-consciousness should be positively related to troublesome symptoms, while the mindfulness construct of observing should be negatively related to troublesome symptoms” (118-119). Original papers on FFMQ (Baer et al. 2006; Baer et al. 2008) show that relation between observing facet of mindfulness and psychological adjustment depends on the level of meditation experience. This fact should be taken into consideration when “cognitive self-consciousness” and “observing” scales are compared.

Reply: We thank the reviewer for the insightful and nuancing comment and we have adjusted our statement. We now state that: “However, there might be differences between experienced and inexperienced meditators as they have different cerebral activation in response to stressors (e.g. Froeliger et al. 2012) and meditation experience could also affect the relationship between observing and well-being (Baer et al. 2006; 2008).”

The following reference has been added to the reference list:


7. It is surprising why the authors have chosen explanatory approach instead of making clear hypotheses (126-128). Deeper theoretical analysis of the mindfulness construct might easily lead to some hypotheses about the metacognitive nature of some facets of mindfulness. For example, observing facet is conceptually near to monitoring aspect of metacognition, non-judgment should be related to specific metacognitive believes about nature of emotions, thoughts and other internal events while non-reacting could be referred to executive aspect of metacognition. If the authors investigated not a metacognition as such, but CAS as the particular set of metacognitive believes, specific hypotheses about its relation to mindfulness are still possible. In my opinion, an in-depth theoretical analysis and specific hypotheses could improve the article a lot.
**Reply:** The reviewer has good suggestions for hypotheses. However, as the MCQ measures metacognitive beliefs as specified by Wells’ model for emotional disorders, it does not measure metacognitive elements as specified by the reviewer. However, the revised manuscript refers to a recent and thorough theoretical review which addresses these relationships (Jankowski and Holas, 2014). Also, as previously noted, the study does not address detached mindfulness and in view of the paucity of theory behind mindfulness we defend our exploratory approach rather than taking a hypothesis driven perspective in relation to identifying latent structure and predictors of symptoms.

8. The authors are not consequent presenting aims of the study. If they decided to explore factor structure of the MCQ30 and FFMQ scales, they cannot predict how many factors they will obtain. Until it is done, it is probable that EFA will give one general factor. In such a case there would be no relation between factors to examine. Examining relations between factors cannot be set as an a priori aim without specifying a hypothesis about numbers of postulated factors. This aim seems to be set post hoc, and as such it should be add after presenting EFA results as post hoc analysis.

**Reply:** We now specify that exploration of the latent factors and psychopathology were post hoc analyses.

9. Describing FFMQ, the authors present results about relations of FFMQ with various indicators of psychological adjustment (163-170). In my opinion they should be better presented in the introduction section as a base for hypothesis about relation of mindfulness with CAS. In turn, in introduction there are descriptions of the particular scales of FFMQ (82-101) which might be better placed in section on measures used in the study. Instead, constructs of mindfulness and CAS should be more deeply described and compared in introduction, not measures (102-120).

**Reply:** We agree that the paragraph mentioned by the reviewer could be moved to the introduction. The revised manuscript has done so.

As our study is an empirical investigation which uses MCQ-30 and FFMQ as ways of measuring metacognition and mindfulness, we find it of interest to describe these measures in detail in the introduction as they represent the two constructs of interest in our study. The reason for describing the FFMQ in the introduction is for highlighting similarities and differences between the operationalization of mindfulness and metacognition (as defined by Wells). A closer description of metacognition and mindfulness is as previously mentioned now included in the introduction. With regard to the reviewer’s comment on describing the CAS in more detail; the study mentions CAS, but it does not include a measure of the CAS. As of such, we think that the descriptions of CAS included should prove sufficient for the current study.

10. It should be clearly explained in introduction why particular sorts of disorders (OCD and GAD) have been chosen. What arguments support such choice? What predictions are postulated?

**Reply:** We could have chosen different disorders. However, the ones chosen were so due to related ongoing research being conducted from our research group on these patient populations. We now describe the following: “Measures of anxiety and depression were chosen as symptom measures because they are the most frequent emotional disorders in the population. A measure of OCD was also included as mindfulness and metacognitions have been implicated as important in OCD, however, other measures of other emotional disorders could have been included as well and a measure of global functioning would be a reasonable addition to the study. To ease burden on participants we chose to include a few brief symptom
measures. Future studies should look into the relationship between mindfulness and metacognition on other symptom measures as well as measures of global functioning.”
Reviewer #2

1. the authors attempt to draw linkage of the present study to psychological theory of emotional disorders, and theory of psychological treatments. However, whether it can contribute to theory of treatment remains unclear, with the limitation of the existing study design.

Reply: The reviewer is correct in that this study is a modest first step towards contributing to the theory of treatment. However, we find it important to do this study first as an empirical test of the two models, and we are working on studies with similar research questions in patient populations. The additive effect of these studies should be able to address whether certain constructs are more important to address in treatment than others. Future studies should also include dismantling studies to address which components of metacognitive and mindfulness elements are important in therapy. Limitations concerning the sample are already mentioned in the manuscript. To accommodate the reviewer’s comment we now also state in the discussion that: “A single study does not implicate changes to theories of treatment. However, if further studies replicate the findings from our study using patient populations and more rigorous designs, there would be indications as to which components of mindfulness and metacognitions are most important to address in therapy.”

2. The theory for mindfulness-based therapies is not clearly illustrated. in fact, I do not think there is an existing psychological theory can offer good explanation to the mechanism of mindfulness. However, p.4 line 72, "emotional disorders are thus understood as a lack of mindfulness skills in the mindfulness-based approach" can be misleading. It would be much better to explain that mindfulness can be useful in managing cognitive vulnerability and reduce the chance of relapse in major depression (Segal, Williams, and Teasdale, 2013).

Reply: We agree with the reviewer as we have found no existing psychological theory that can offer a good explanation to the mechanism of mindfulness. We have revised line 72 and the wording is now more careful: “Emotional disorders could possibly be understood as a lack of mindfulness skills in the mindfulness-based approach. However, there is a lack of theory to offer a good explanation as to the mechanisms of mindfulness, but there are indications that mindfulness could be useful in managing cognitive vulnerability and reduce the chance of relapse in depression (Segal, Williams, and Teasdale 2013).”

The following reference has been added to the reference list:


3. although FFMQ has been identified as the mindfulness elf-reported scale with best psychometric properties there are many criticisms about the scale (eg. Van Dam, Hobkirk, Danoff-Burg and Earleywine, 2012). FFMQ’s relationship with psychological disorders and treatment outcomes are inconsistent so I doubt the study aim can be achieved, using FFMQ.

Reply: The reviewer makes an excellent point which we attempt to incorporate in the revised manuscript: “A systematic review of instruments to measure self-reported mindfulness found that the FFMQ has the best psychometric properties (Park et al. 2013). However, there have also been criticisms of the scale (e.g. Van Dam et al. 2012) such as problems with combining positively and negatively worded items. Future studies should thus continue developing and improving the assessment of mindfulness.”

The following reference has been added to the reference list:


4. For FFMQ, the inconsistency in the labels of subscales should be handled carefully.
Reply: Labels of subscales are described consistently throughout the revised manuscript. The five subscales are now consistently labeled as: observing, describing, acting with awareness, nonjudging of inner experience, and nonreacting to inner experience.

5. P.10 line 226, ten percent… line 231, eleven percent...use of exact figures are recommended.
Reply: Exact figures are now used in the text.