Reviewer's report

Title: A Metacognitive Perspective on Mindfulness: An Empirical Investigation

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Reviewer: Tomasz Jankowski

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The reviewed article concerns intuitively reasonable but seldom investigated relation between mindfulness and metacognition. In context of vast literature that documents beneficial effects of mindfulness, there is currently a need to understand mechanisms of mindfulness more deeply. I think that the concept of metacognition is very useful to explain how mindfulness occurs. Therefore the research presented by the authors is valuable and interesting. At the same time, I believe that some major improvements should be considered to enhance contribution that the paper can make to comprehension of mindfulness.

Major Compulsory Revisions

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.

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.

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”.

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.

**Minor Essential Revisions**

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.

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.

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.

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.

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).

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?

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests