Reviewer's report

**Title:** Mindfulness-Based Cognitive Therapy in Obsessive-Compulsive Disorder - A qualitative study on patients’ experiences

**Version:** 2 **Date:** 2 July 2012

**Reviewer:** Joseph McGuire

**Reviewer's report:**

Summary: This is an interesting article that describes the patients’ subjective experiences after participating in a Mindfulness-Based Cognitive Therapy (MBCT) group. The sample includes 12 adults with Obsessive-Compulsive Disorder (OCD), and qualitatively explores facets of MBCT deemed beneficial and/or problematic for participants. The manuscript is quite novel in its application of MBCT to reduce obsessive-compulsive symptoms severity. Myself and many other researchers would be interested to see the results from the main efficacy paper. Although novel and interesting, this qualitative paper has several issues that warrant further consideration.

Major Compulsory Revisions

1. In the discussion section, the manuscript attempts to extrapolate the qualitative analyses to delineate potential mechanisms of action underlying therapeutic effects. It is unclear whether or not the participants reported benefits of therapy is the same as the mechanism of actions. Perhaps if the authors could provide homework compliance data to show that participants commonly practiced skills learned in group, the authors could more accurately comment on specific mechanisms of action. In the absence of such information or information relevant to mechanism of action, the manuscript would be strengthened if discussion on mechanism of action were curtailed, and instead the emphasis was placed on components of MBCT participants found useful and/or difficult, and how these comments can be used to inform the future MBCT protocols in OCD.

2. As many readers may not have in-depth knowledge on the principals of MBCT, the manuscript would benefit if the authors commented on how the current MBCT protocol is distinct from existing cognitive protocols for OCD beyond simple group versus individual format (see Wilhelm, Steketee, Fama, Buhlmann, Teachman and Golan, 2009). Furthermore, it may benefit the manuscript if the authors clarify how this MBCT protocol differs from other mindfulness based interventions (e.g., Acceptance and Commitment Therapy) that have demonstrated some efficacy in OCD (see Twohig et al., 2010).

3. It is unclear what the authors mean in the sentence on page 5 which states “in defined cases assisted by medication with selective serotonin re-uptake inhibitors (SSRIs),”. The authors should clarify for the reader if the authors suggest that CBT with E/RP often occurs in the presence of medication management (i.e.,
SSRIs) or if the authors suggest that CBT with E/RP only provides symptomatic reduction in the presence of SSRIs. If the authors are suggesting the latter, it should be acknowledged that there have been several studies of CBT with E/RP in the absence of SSRI medications that have provided therapeutic benefit to participants (see Rosa-Alcazar, Sanchez-Meca, Gomez-Conesa & Marin-Martinez, 2008 for a review).

4. On page 5, the authors note that “CBT with ERP is a highly challenging form of treatment…resulting in dropout rates of approximately 25%”. It would be beneficial if the authors highlighted the hurdles that CBT with ERP may have for patients and therapists, and comment on whether patients receiving (or therapists administering) MBCT face similar challenges. Alternatively, it is worth noting that this preliminary study of MBCT exhibited similar attrition rates and comparable self-reported improvement rates to efficacy studies of CBT with E/RP that the authors highlight in the introduction. For example in the present study, attrition was reported to be 25% and approximately 33% of participants failed to report improvement in obsessive compulsive symptoms. In general, the authors’ description of CBT with E/RP as “perceived as unacceptable by a substantial number of patients” raise some concerns and may indeed be a strawman position for several reasons. First, many studies of CBT with E/RP show very low drop-out rates (e.g., 13%), so the selection of approximately 25% is inaccurate and based on selecting studies that achieve this while ignoring many that do not (e.g., Simpson et al., 2008). Second, it remains unclear whether individuals who would decline to participate in CBT with E/RP would consider engaging in MBCT. Instead of describing CBT with E/RP as “unacceptable”, a more realistic presentation may be that there are several challenges that confront the implementation and dissemination of CBT w/ ERP. The authors should briefly discuss these challenges instead, and describe how MBCT could pragmatically provide an alternative or complementary treatment to CBT w/ ERP.

5. On page 5, the authors imply that the “clinical picture of OCD” is “a ‘state of mindlessness’, characterized by attention deficits as well as non-accepting attitude.” Although I appreciate the authors’ intent to highlight the appropriateness of a mindfulness-based intervention for OCD, this description of OCD does not appropriately capture the complexity of obsessive-compulsive symptoms or accurately describe the nature of the disorder. Instead, the authors may wish to note that the individuals with OCD often have difficulty with sustained attention due to intrusive thoughts and have been frequently reported to have oppositionality, and provide accompanying citations.

6. As the stated intent of the manuscript is to qualitatively assess the subjective experiences of participants in the MBCT therapy program, the authors spend an excessive amount of detail describing participants’ specific obsessive-compulsive symptoms on page 7. As the manuscript and participant interviews do not describe which obsessive-compulsive symptoms diminished/remitted after MBCT, this level of detail is not necessary. Please curtail this discussion on page 7, or if appropriate, discuss which obsessive-compulsive symptoms that participants reported responded well to MBCT.
Minor Essential Revisions:
None.

Discretionary Revisions:
7. In the conclusion section of the abstract, the authors may be more accurate in saying that the paper “provides preliminary evidence that OCD patients find aspects of the current MBCT protocol beneficial and tolerable”.

8. Although the authors suggest that MBCT may serve as a complementary therapy, there is minimal discussion in the manuscript on how the authors believe MBCT could fit into current management practices of OCD. A brief discussion of this would be welcome.

9. In the discussion section, the manuscript may be strengthened if the authors draw parallels from their findings to other qualitative examinations of cognitive therapies (e.g., Bevan, Oldfield, & Salkovskis, 2010).

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

Quality of written English: Acceptable

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.