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Letter to the Editor

Should we focus on quality or quantity in meta-analyses?

In an invited commentary (Moritz *et al.* 2016), the developers of metacognitive training (MCT) have challenged the results of the meta-analysis on the efficacy of MCT on positive symptoms, delusions and data gathering bias (van Oosterhout *et al.* 2015). The recommendation of this meta-analysis was not to implement MCT in routine care at this moment, because of a lack of evidence.

In their introduction (Moritz *et al.* 2011) refer to another meta-analysis by Jiang *et al.* (2015) quoting significant effects for positive symptoms (as well as for delusions without the study by van Oosterhout *et al.* 2014). The van Oosterhout *et al.* (2014) study is a negative study, but it is the largest randomized controlled trial (RCT) and indeed was rated as the study with the lowest risk of bias by Jiang *et al.*; it is not clear why one would discard the best study to date? The developers could usefully be referred to the conclusion in the abstract of Jiang *et al.*: ‘The limited number of RCT trials, the variability of the method and time of the outcome evaluation, and methodological problems in the trials make it impossible to come to a conclusion about the effectiveness of MCT for schizophrenia. More randomized trials that use standardized outcome measures, that use intention-to-treat (ITT) analyses, and that follow-up participants at regular intervals after the intervention are needed to determine whether or not MCT should become a recommended adjunctive treatment for schizophrenia.’ In our opinion this conclusion is the same as ours.

In their rebuttal the developers apply four headings with different arguments; these are addressed below.

Studies omitted

Moritz *et al.* note that some studies were omitted; we reran our analyses including the appropriate studies. We added the data of Aghotor *et al.* (2010); Moritz *et al.* (2011) and Gaweda *et al.* (2015). We found the data in the meta-analysis of Eichner (2015), a PhD student of Moritz, who was kind enough to provide the data via Research Gate. We discarded two non-randomized studies: an extremely negative study by

Rocha & Queirós (2013) and an extremely positive study by Erawati *et al.* (2014).

It is not accurate to say that we omitted the So *et al.* (2015) study from our paper because it had not been published – or accepted for publication – at the point our meta-analysis was accepted for publication.

However, we have added this study in our reanalysis.

The results of the reanalysis demonstrate that there are some significant effects for positive symptoms ($g=0.32$) and delusions ($g=0.31$), but not for data gathering ($g=0.11$) if all the studies are considered. However, the findings from the high-quality and intention-to-treat data are similar to our original results with no significant effects on delusions, positive symptoms or data gathering.

Heterogeneity and moderator analysis

We chose to conduct some additional moderator analyses with the high-quality studies and not the low-quality studies as Moritz *et al.* suggest. Van Oosterhout *et al.* (2014) is the largest study with the lowest risk of bias, but is a negative study. It does not make sense to remove robust studies from an analysis only to remove heterogeneity.

MCT+ is not CBT

The authors (Moritz *et al.* 2011b) wrote in the paper on MCT+ that ‘Individualized metacognitive therapy (MCT+) followed group sessions according to the general guidelines for CBT (e.g. Fowler *et al.* 1995).’ As Fowler is one of the founding fathers of CBT for psychosis, this led us to conclude that MCT+ is based on CBT. In the same paper it is stated: ‘MCT ... is grounded on the principles of CBT (Fowler *et al.* 1995) and basic research on cognitive biases in schizophrenia (for reviews, see Garety & Freeman, 1999; Freeman *et al.* 2007), as well as deficits in social cognition/theory of mind (Frith, 1994; Frith & Corcoran, 1996).’ CBT in psychosis has included work on cognitive biases for over a decade. The main difference seems to be that MCT is set up like a course targeting transfer of ‘cold cognitions’, while CBT focuses on personalizing and experiencing the effects of biases in real life. In doing so, CBT is rather aimed at modifying ‘hot cognitions’.

Evolution of MCT

Moritz and colleagues suggested changing the inclusion criteria to patients without severe delusions. We

have checked the baseline data of the studies on delusions. The three studies with the highest baseline delusion scores were So *et al.* (2015), Favrod (2014) and van Oosterhout *et al.* (2014). Taking these three studies, the effect size is 0.49, while studies with the lowest delusion scores at baseline (Moritz *et al.* 2011a,b, 2013; Briki *et al.* 2014; Gaweda *et al.* 2015) have a pooled effect size of 0.25 on delusions. So the empirical evidence rather suggests that patients with highest baseline scores of paranoia in general benefit most from therapy.

In summary, we appreciate the opportunity to air these differences in opinion; but have to conclude that the best available evidence suggests that MCT is not yet at a stage to advocate its routine use. That is to say, we acknowledge Moritz *et al.*'s position that MCT is work in progress and this progress needs to be data driven. However, including less rigorous evidence into a meta-analysis may offer a different opinion, but this is probably not the most robust scientific way forward.

Declaration of Interest

None.

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