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Psychosocial Interventions for Negative Symptoms: a note on meta-analyses

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We read with interest the paper by Lutgens et al., ¹ describing the results of their meta-analytic study on the effect of psychosocial interventions on negative symptoms for people with psychosis. Despite commending the aims of the study we have some methodological reservations on the results presented.

We believe the number of the studies included is only a partial representation of the research conducted in the therapeutic modalities considered. For example, we have recently completed a meta-analysis on the effect of cognitive remediation on negative symptoms ². Our study had a similar time frame to Lutgens et al., ¹ and the same participants' inclusion criteria. Our search retrieved 45 eligible studies compared to only 16 retrieved by Lutgens et al., ¹ in their neurocognitive therapies category. We believe this is due to the search strategy used, which included the term "negative symptom" and therefore retrieve only studies with this term in the abstracts. This has two effects. It is more likely to retrieve studies reporting positive findings, and when investigating interventions not specifically designed to target negative symptom will miss a large body of studies across all the therapies modalities considered.

The nature of the control condition is also important when considering effect sizes. In their study Lutgens et al., ¹ conflate passive with active control conditions. Active control conditions for one study (e.g. cognitive remediation) were then considered active treatment condition in subsequent analyses. We also noted some overlap in therapy groups considered. Both art and music and exercise therapy included dance-based interventions. The miscellaneous category adds to the limited clarity in categories definition by considering comprehensive "care packages" such as Garety et al., ³ which include medication management and allocation to a psychosocial intervention amongst a number recommended by clinical guidelines (i.e. family therapy or CBT). These limitations, in our view, make it difficult to reliably compare effect sizes from the intervention groups considered.

We also wish to point out some methodological considerations that may limit the accuracy of the results reported. Firstly, it appears the authors considered only end of therapy data to estimate effect sizes. This does not account for relative change. In other words, this method considers a symptoms reduction of a hypothetical 3 points of a negative symptoms scale to be the same in individuals entering the study with an initial score of 5 or 23. The importance of taking into account baseline levels in meta-analysis is clear and it is considered best practice ⁴. There is also evidence that the DerSimonian and Laird method has limitations when compared to other methods using restricted maximum likelihood estimator ⁵.

Lastly, it is unclear how the authors considered the treatment participants received as part of treatment as usual (TAU). The authors stated: "Compared with TAU, 59% (10/17) of studies reported CBT to be more effective at the end of treatment". From this statement one may assume that participants received either TAU (e.g. medication) or CBT. In all likelihood studies compared CBT + TAU to TAU only.

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