Reviewer's report

Title: Comparative Effectiveness of Psychological Treatments for Depressive Disorders in Primary Care: Network Meta-Analysis

Version: 2 Date: 20 February 2015

Reviewer: Jane Gunn

Reviewer's report:

This manuscript is one of a series of articles published by the authorship team investigating the effectiveness of treatments for depression in primary care. They report on a network meta-analysis designed to assess the comparative effectiveness of psychological interventions. The work is important and will be of particular interest to the primary care depression field.

The questions posed by the authors are well defined. The work is supported by a previously published protocol and is well presented.

The methods appear appropriate and they are well described. For the non-expert in the area of network analysis an explanation of the terms 'frequentist method', consistency model, inconsistency model would be helpful.

The data appear sound and the authors have provided a large amount of very useful information in the tables and supplementary files.

The authors acknowledge that this work builds upon a recently published meta-analysis in the Annals of Family Medicine (ref#11)– but this could be stated even more clearly and earlier in the paper and the authors should point out how this analysis differs from that already published work and why it is justified.

The information about the three assumptions that need to be met for a network analysis is very helpful. I think this information would fit better being introduced in the methods section and clearly stating at that point the concerns that the authors had around the transitivity assumption and stating clearly their argument for why they continued with the analysis despite their concern.

After line 323 I think it would be helpful to include a paragraph which discusses the findings in this paper in the light of the findings from their conventional metaanalysis published recently in Annals of Family Medicine (ref #11). From my reading of both papers – the findings are complementary – so what does this new analysis add? The Tables in ref #11 are very similar to those in the Suppl. files – it will be important that the difference between these two papers is clearly explained.

The authors note that for some of the comparisons they had little data. Can they make a comment about the IPT data- as they state this intervention has conflicting evidence – do they think that they have had sufficient data to generate a reliable finding?

The finding of reduced effectiveness for the minor severity disorders is
interesting. Do the authors consider “reducing depression severity” as a good outcome measure for judging effectiveness in this group of disorders? Do they think that alternative outcome measures might be more appropriate? Depending upon their view, they may wish to comment on this in the discussion.

Line 342 – I suggest they delete the comment regarding CBT and SSRIs’ – as they state they did not have enough data to draw a reliable conclusion and their comment as it stands could lead the reader to draw a conclusion that is not supported by the data presented.

Line 353 onwards. This section raises the issue of whether usual care was consistent across the studies. What can they say about this? What limitations does it bring?

It seems to me that the major finding of this review is captured at line 332-333 – that CBT is effective and that it appears to be just as effective when delivered remotely and using minimal contact with health professionals. This finding could be given more prominence and appear in the Abstract? The authors could discuss this finding in more depth- how strong do they consider their evidence is for this finding. Is it strong enough to support health care reform or do we require further studies? Discussing the health care policy implications of this finding would enhance the paper.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

'I declare that I have no competing interests'