Author's response to reviews

Title: Influence network linkages across treatment conditions in a randomized controlled trial of two implementation strategies for scaling up evidence-based practices in public youth-serving systems

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Author's response to reviews: see over
RE: MS #1708727357516378 “Influence network linkages across treatment conditions in randomized controlled trials”

Dear Implementation Science Editorial Team:

Attached please find a revised version of the above referenced manuscript. In accordance with the comments and suggestions provided by the reviewers, we have made the following revisions:

Reviewer #1:

1. **Networks are constructed based on semi-structured interviews. It needs to be clarified which criteria were used to construct a linkage between nodes/representatives (e.g. all persons that are mentioned by the participant, or only specific collaborations).**

We clarify on p.7 that the social network data was obtained primarily by means of a web-based survey and supplemented with information obtained during the semi-structured interviews. To emphasize this point, we have re-ordered the two paragraphs in the data collection section for that information on the social network survey appears first. We also include specific mention of the questions/information used to construct network linkages, which were based on nominations of individuals to whom one obtained information or advice relating to any new or innovative program or practice.

2. **A motivation or objective for investigating networks without representatives outside the RCT should be included.**

We explain on p. 8 that we included non-RCT network members to determine whether linkages across the two treatment conditions may have been facilitated by third parties.

3. **Furthermore, the results of these networks (figure 3 and 4) could be elaborated on more extensively in the results and discussion section; the added value of figures 3 and 4 is now unclear.**

We elaborate on the findings reported in Figures 3 and 4 on p. 14 of the revised manuscript.

4. **The conclusion of the paper is that the integrity of the RCT was not compromised, since there are very few direct ties across conditions. The current networks are likely conservative estimations, since they are based on information of only 38 of the 176 nodes in the network. Most individuals have indirect ties with individuals of the opposite treatment condition (75.9% within 4 steps), and authors mention that it would not be surprising if system leaders of child public service systems would go to each other for advice. With this in mind, the current conclusion seems to be too much said.**

We agree with the reviewer and include this point as a further limitation in our analyses on page 19. “Our findings thus pertain to the first cohort whose relations with colleagues from counties scheduled to implement MTFC later cannot be affected by their personal experience with either implementation as
they are naive to this condition. In contrast, later cohorts could be affected by the experiences that their colleagues from previously implemented counties had with their respective implementation.”

5. **Authors mention that it is important to take the role of influence networks into consideration in both design and analysis of RCTs. It should be explained more extensively how other researchers can implement this method in their own study and what to do with or how to interpret possible outcomes of such a network study.**

We now offer some suggestions as to how other researchers might take into consideration the potential influence of influence networks in designing and analyzing the results of randomized controlled trials.

6. **Setting and study population should be mentioned in the title of the paper.**

We include mention of the setting and study population in the title of the paper.

7. **A typo in the abstract (‘46.7.0%’ in results paragraph).**

We have corrected the typo as noted by the reviewer.

8. **% missing for bay-area participants (n=18; 47.4) on page 11.**

We have corrected the % missing for Bay area participants on p. 11.

9. **It is mentioned that over two-thirds of participants were directors, though this is only 31.6% (page 11).**

We have corrected the reference to two-third of the participants being agency directors. It now reads “A little over one-third…”

10. **Some information mentioned in the interviews such as nature of the collaboration/reasons for communication, remains unmentioned in the results. Though it might be beyond the scope of this study, it would be interesting to use this information for linkage characteristics (e.g. apply strength of linkages). Authors could mention how nature/content of relations between individuals could be processed and what they mean for interpreting influence networks.**

We now discuss the possibility of supplementing the social network data with information from the qualitative interviews in future directions on p. 20 of the revised manuscript.

11. **Results are mentioned in the methods paragraph (data analysis). These include ‘This matrix consisted of all 176 nodes, including the 39 nodes that were not randomized to either treatment or standard conditions…’, and ‘Indeed among the 38 organizations, 1 had a direct tie to an organization in a different treatment condition…’. I would only include results in the methods paragraph if they are essential in explaining the used methods.**

We have removed the aforementioned results from the methods section and placed them in the results section.

12. **Significance of differences between CDT and non-treatment may be added in table 2.**

We have now provided an explanation on p. 12 of the results section as to why we report no p. values in Table 2.

Reviewer #2:

1. **Could the authors discuss whether the 60/40 split among the administrator sample across intervention and comparison counties is a problem or not and why?**

We have revised Table 1 to show comparisons of participant characteristics by experimental condition and show that there were no significant differences in percent of directors, associate directors and
managers between the two conditions. We note in the discussion, however, that these individuals may have different network sizes and structures that should be examined in future studies.

2. It appears that the number of interviews obtained by country ranged from 2 to 6. Are there any potentially problematic implications with the range of responses?
We have revised Table 1 to show comparison of number of people interviewed in a county by experimental condition. We note in the discussion section, however, that the number of participants may potentially confound findings that should be examined in future studies.

3. A third of the interviews were completed by phone, and two-thirds face-to-face. Was this by design? Could the authors explain this difference?
We explain on p. 7 in the methods section that some interviews were conducted by telephone due to scheduling conflicts that prevented face-to-face interviews when site visits to each county were conducted.

4. I am wondering if it would be possible for the authors to include the interview guide they used for the interviews as a supplemental file. It would be helpful to see the probes used and get a fuller sense of the interaction.
We have provided a copy of the interview guide as an Appendix in the event that such material can be included on the journal website as supplemental material.

We have provided two copies of the revised manuscript, one with markups as requested and one clean copy. In the event the manuscript is accepted for publication, we recommend use of the clean copy.

We wish to thank the reviewers for their thoughtful and helpful comments. We believe our efforts to respond to these comments have resulted in an improved manuscript. We look forward to hearing from you at your earliest convenience.

Sincerely yours,

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