Reviewer’s report

Title: A randomised controlled trial of performance review and facilitated feedback to increase implementation of healthy eating and physical activity-promoting policies and practices in centre-based childcare

Version: 0 Date: 29 Oct 2018

Reviewer: Wynne Norton

Reviewer's report:

Finch and colleagues report on a large, multi-site parallel group randomized trial to test strategies for increasing health eating and physical activity policies and practices in childcare settings. Specifically, practices randomized to the implementation strategies group were tested against practices randomized to the information-only control group; the control group received electronic newsletters but no other assistance with respect to implementation of 6 healthy eating and physical activity policies and programs. Results of the trial did not demonstrate any statistically significant differences between the intervention and control groups at 12-month follow-up on any of the 6 practices and policies. The authors discuss possible explanations for the lack of differences between the two groups.

Comments and questions for the authors are below. Although none of these should require major revisions, it would be helpful for the authors to address and revise their manuscript accordingly.

1. The authors describe the study as a pragmatic trial, both in the title and briefly in text. However, it is not clear that this study is truly a pragmatic study—it appears the term pragmatic is used more as a descriptor of the type of intervention rather than the actual trial design. The authors do not include any mention of PRECIS-2, the premiere tool available for designing trials with a more explanatory or more pragmatic attitude. To avoid misconceptions in the field, the authors are strongly encouraged to remove the term pragmatic throughout the manuscript (including the title), as it is relatively inaccurate and inconsistent with the more substantitive conceptualization of the term pragmatic. In other words, using the term pragmatic to describe a trial done in the ‘real-world’ with less stringent inclusion/exclusion criteria and a use of a relatively low-intensity implementation strategy is a misnomer. Note that this is an extremely common use of the term, but it would be beneficial to the field overall if this misconception is not promulgated. For these reasons, the authors should remove this term from their manuscript unless they indeed planned the trial a priori to be more pragmatic across most—if not all—of the PRECIS-2 domains.
2. To avoid confusion and to be consistent with the field, the authors should remove the term 'implementation intervention' and replace or describe as implementation strategies. See Curran et al. (2012) for examples of interventions vs. implementation strategies, as well as articles by Powell and colleagues for more detailed descriptions of implementation strategies. The term 'intervention' should be reserved for approaches that target individual or patient-level health behaviors or outcomes; the audit and feedback approaches described in the manuscript should be labeled implementation strategies.

3. The authors should describe—in more detail than in the current version—exactly how CFIR informed the content of the implementation strategies.

4. Provide a citation or brief explanation for of a 20% difference between the two groups was used in the power analysis. The CFIR model suggests that multi-component strategies are necessary, so why only use audit and feedback?

5. Why did the authors decide to only use audit and feedback strategies if previous research has found that multi-component strategies are important (essential?) for increasing adoption of evidence-based practices?

6. Why was the study conducted if they didn't achieve the target sample size? Why conduct a study if you are under-powered from the beginning?

7. If practices were excluded from the study because they were implementing many of the policies already (as stated on p. 19), why did both groups show relatively high rates of implementation at baseline? Shouldn't they have made the inclusion/exclusion criteria a bit more stringent to avoid ceiling effects at baseline, and thus limiting potential for improvement at follow-up? If the authors are only referring to two policies, as mentioned in the results section, perhaps the statement on ceiling effects at baseline could be softened.

8. The selection of a 12-month follow-up time point should be justified. Why not 6-months and 12-months, which would allow for more sophisticated analyses (GEE) and potential assessment of sustainability? Data collection with CATI seems relatively easy; an explanation for why additional data points were collected would be beneficial.
9. Were there any policy changes outside the context of the trial that may have led to increases in the control group? Is there evidence that adoption of these or similar policies increase over time without any implementation strategy? A potential explanation for why the control group increased adoption of policies over time would be helpful.

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