Reviewer's report

Title: A Systematic Review Protocol of Stigma among Children and Adolescents with Epilepsy

Version: 0 Date: 15 Dec 2017

Reviewer: Ivan D. Florez

Reviewer's report:

Interesting idea. However, I found many weaknesses in the protocol, in terms of reporting but also in the analyses approaches. The following are my comments:

P4. Line 1. Please correct "at least"

P5, L15: Please consider adding more context about why epilepsy has the Stigma problem. There are historical and cultural factors that explain the reasons for this stigma (I suggest using some of the ideas from: DeBoer HM. Epilepsy 2010; 19(10):630-636)

P1, L39-40: Authors mention that the results from previous studies among patients in HIC and LMIC have been conflicting. How have they been conflicting? Were they reviews? Were they just cross sectional studies? Is there any systematic reviews combining them or is this the first one? Are you only interested in SSA A brief description of what they found and why we should not trust them, or why this study pretend to resolve that conflict, should be in the background.

P1;L52-58: Also, if SSA is you only interest, as it's described in the Objectives, your title and the Aims (P2, L7-10), should reflect that

PROSPERO registration states that this review started in June 2017, and it's expected to finish in February (in less than 2 months), and the PROSPERO stages are checked as if they have not started yet. There should be consistency among the stages and the current dates and the status of this protocol

P2, L7-11: Aims should not be included within Methods. It should a subheading before Methods

P2, L14-15: What is the meaning of the 1-Superscript along with the (SSA)1? Does not seem to be a citation, which used brackets
All P2: Trying to make your question fit into a PICO or PECO format is unnecessary and also inappropriate. SR of prevalence questions should have a question that reflects its real aim, not a PICO which is related mostly to interventions/exposures (and sometimes Dx tests). The following paragraph from the Joanna Briggs Institute Manual for SR of Prevalence and Incidence data might be useful:

-Reviews that aim to describe the distribution of existing variables or seek to answer the question "how common is a particular disease or condition in a specific group of individuals?" are often classified as descriptive and will utilize measures of prevalence and incidence to answer such lines of enquiry. The objective of these reviews is to describe the health issue (what), those affected by it (who) as well as the location (where) and the time period (when) in which the health issue occurred.

All P2: Also trying to make the Factors associated, fit into secondary outcomes is even more inappropriate. I think that determining factors associated is itself a different question. In addition to that I perceive a third question related to the available interventions. Authors may want to clearly describe this review a 3 questions review

P2, L42-44: Searches through CINAHL should be included. This database include info from some Nursing Journals that may include relevant information for your question. Also, there is no search strategy provided (even though authors stated in the PRISMA checklist it was in lines 224-239, which I could not find)

P2, L48-57: The eligible studies should be linked to each one of the questions this review has.

P3, L5: What will authors do if there are multicenter trials including SSA and non-SSA countries? How will they handle that scenario?

P3, L14-33: There is no clear mention of how the Title and abstract screening will be done. There isn't neither info about the full text screening. There is no info if these steps will be done or note independently and in duplicate. Additionally, authors plan to do the data extraction will be performed by one author and checked by another author. This approach has some limitations and is not the actual recommendation by the Cochrane Collaboration. This approach (one extractor + checking) has shown more errors than duplicate extraction (Buscemi; J Clin Epidemiol 2006). I suggest to reevaluate this approach and plan a data extraction in duplicate, since this might be an important limitation of this review.
P3, L36-48: Not clear to me why if they are using a NIH tool, there is need to use RevMan? Risk of Bias should be detailed in tables. No need to use a software designed for effectiveness meta-analysis.

ALL P3: I found a little bit annoying that many analyses and approaches are not referenced appropriately: No citation for the RoB Tool, No citation for the GRADE approach, no citation of the Egger test. I suggest authors to be cautious with this, since no adequate citations might be considered form of plagiarism.

P3, L53-55. I understand the final measure: Mean prevalence and CI. Not clear the analysis of the stigma associated factors with univariate and logistic regression? To start, this question has not been clearly described from the beginning, and now there is mention of logistic regression: What are the considered, or suspected independent variables? How will authors choose what variables to include in the regression? What will be the software for this analysis?

P4, L4-7. What will authors do with high heterogeneity?

P4, L10-12: WhEffect modifiers should not only be mentioned before the analysis, buy also an analysis of what potential impact they might have on the effect. Namely, what should be the change in the direction of the effect when these modifiers are present?

**Level of interest**
Please indicate how interesting you found the manuscript:

An article of limited interest

**Quality of written English**
Please indicate the quality of language in the manuscript:

Needs some language corrections before being published
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