Reviewer's report

Title: Implementation of self-management support in cancer care and normalization into routine practice: a systematic scoping literature review protocol

Version: 1 Date: 08 Jan 2019

Reviewer: Caroline Farmer

Reviewer's report:

Summary

This is a protocol for conducting a scoping review of studies evaluating self-management strategies (SMS) in cancer patients. The topic area is undoubtedly important and of interest to a large audience. The manuscript is well written and the authors provide a concise and compelling rationale for the aims and methods of the review. The methodology is rigorous, and supported by recommended practice. I suggest that the protocol is accepted with 'minor' revisions; although this involves recommendations for a few changes to the aims and methodology of the review, which could be easily made but may be considered 'major'. There are some very minor corrections needed to the text only. Overall this appears to be a very well planned review and I send the authors my best wishes for what will undoubtedly be a very large - but rewarding - piece of work!

Major Issues

I am concerned if and how the authors aim to achieve aim 5 (line 166) with the methodology set out in this protocol. As a scoping review, the aims of this approach should be to gather an overview of the evidence base, and to categorise the content and context of evidence identified (as also stated by the authors in line 310-311). Meta-analysis of data, which is the gold standard for evaluating effectiveness, is not planned; rather a qualitative analysis will be conducted to "tell the story" (320) of the evidence. Given the complexity of the evidence, including variation in intervention content, setting, timeframe, population, and measured outcomes, I'm unsure whether a narrative synthesis alone will be useful in addressing an aim to provide guidance on the effectiveness of SMS in cancer patients. I would suggest limiting the aims of the review to aims 1 - 4; which hold validity from the planned methodology, and I still think will be of significant interest.

The authors state that quality assessment of the studies will not be conducted. However, conducting quality assessment would augment the utility of the evidence identified for the readership. It would provide an improved insight into the available evidence in this area, and may provide guidance on further avenues of research. For example, if it was identified that the evidence for a well-known, widely used intervention was actually based on old, low quality evidence (hypothetical example!), then this may encourage further examination of this
intervention. Standardised checklists for evaluating quality exist for all study types to be included in the review. If concerned about resource, I think it would be more useful to limit the scope of the evidence (e.g. to more recent studies) than to exclude quality assessment. Particularly if the authors wish to retain aim 5, then quality assessment should certainly be performed, as it would not be recommended to provide guidance on the effectiveness of an intervention without ascertaining that the evidence is of adequate quality.

It would be useful to provide further information about how the identification and inclusion of evidence may vary from the approach stated if the search is found to be "unwieldy" (362-364). The criteria for 'unwieldy' should be stated; the search in MEDLINE alone is 5600+, so do the authors have a higher yield size that would be trigger an alteration in the approach? Furthermore, the authors should state if the PICOH criteria may alter in these circumstances, or only the search strategy. As an option, the authors may also consider specifying some criteria at the protocol stage that they will consider using to limit the yield. While some criteria for alteration may only become evident following the start of the screening process, it may be possible to provide some criteria of interest; for example, if the authors intend to alter PICOH criteria, it may be useful to specify if the authors will prioritise certain research in specific countries, interventions, populations, study designs, or settings. The findings of the review may vary widely depending on whether the authors decide to use criteria that emphasise breadth (limit number of studies on same interventions) or depth (identify all studies in limited number of interventions).

Minor Issues

Some terms in the MEDLINE search strategy (sup 1) appear to be overly broad (e.g. policy, quality control, innovation, diffusion, dissemination, adoption). I accept the expertise of the IS input and rigorous peer review of the search strategy, and therefore I only make a suggestion that (given the anticipated size of the yield) such terms may be overly sensitive and could be removed to much benefit?

Statements on the use of handsearching are unclear (260 - 263). What is being handsearched? When will these be conducted, and how will it inform the review?

While a rationale is provided for why a date cut-off of 1984 (stated to the intro of the 1st nurse-led SMS) was chosen, I wonder if studies this old will still be of relevance to the aims of this review, given the rapid changing context of cancer care and prognosis? Perhaps the authors could state whether this has been considered?

It is unclear from the manuscript whether head to head comparisons (i.e. different SMS interventions compared to each other) are relevant for inclusion. Based on text in line 150-151 only comparison to usual care is relevant, although this is not clearly stated in Table 3. Head to
head comparisons should be included in the review for completeness and to address the stated aims of the review.

The PRISMA-P checklist shows that the authors have not pre-considered prognostic factors, likely because the analysis will be narrative and not quantitative. However, it may also be useful to summarise the evidence according to known prognostic factors of the effectiveness of SMS interventions; for example disease type/severity, country, setting, age, mode of delivery etc. Is this something the authors have considered and wish to specify at this stage? Or will this not be considered in this review?

Will all literature databases be searched? Or will this depend on the outcome of the earlier searches? Stated that qualitative search terms will be included, but not currently in the MEDLINE strategy, so are these only to be added in only limited databases? If so, this should be clarified.

Can the authors please state more clearly that SMS interventions specifically for the management of comorbidities to cancer or side effects of treatment are not relevant for inclusion (this is implied, but i’m unclear and may be useful to state clearly)?

Minor editing/text issues

There is some confusion in the labelling of tables in the text: suggest that these be reviewed and altered where needed.

Suggest a rephrase of line 110 ("Moreover the unique demands of cancer as an illness and SMS needs…")

Move comma in line 256 to after "2018".

Charting is not sufficiently explained until line 309, and therefore line 285 is unclear ("Charting describes…sifting, charting and sorting")

Author 'AC' stated in the text but unclear which author this is?

Line 288, unclear if 1 or 2 reviewers will be extracting data

Suggest that Table 4 (taxonomy of SMS interventions) may be rather included in Supp material

Sentence beginning line 318 ("Narrative synthesis is defined as…) is too long and needs revision

Line 331 change "study" to "studies"

Line 366 suggest end of sentence after "used both terms".
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No