Reviewer's report

Title: UK quality statements on end of life care in dementia: a systematic review of research evidence

Version: 3 Date: 1 August 2015

Reviewer: Ladislav Volicer

Reviewer's report:

Major Compulsory Revisions

Authors of this manuscript took a very pessimistic attitude towards available data regarding end-of-life care in dementia. The situation can be seen as "a glass half full or glass half empty". To start with, the authors state that they could not find any paper supporting benefits of a care coordinator (S4). However, they found evidence of care coordinator benefits in general, just not in end-of-life care. It is hard to imagine, that the benefits of care coordinator would cease just because the person was at the end of life. It could be argued that they would actually increase.

Regarding the second statement (S5), definite benefits of care planning were reported in three out of four papers and for the rest of the measures there was not difference (no reason to mention harms or increased costs in the summary). At least one paper reported that care planning decreased use of ICU and of life sustaining treatment, increased use of limited or comfort care, decreased prevalence of hospital deaths, reduced emotional distress, and decreased cost of care. The differences between studies could be due to different scales or patient population but the trend is clear.

Similar situation was present with the third statement (S9). Although in one paper there was no positive effect of staff education, other two papers showed improvement in prevalence of hospice care, assessment of pain, non-drug treatment for pain, DNR orders, death in care homes, documented discussion of treatment preferences, and interventions in line with wishes. Again, one negative paper that could be due to insufficient education does not negate positive results of the other studies.

Minor Essential Revisions

In addition to problems with interpretation of data, the manuscript has several other problems:

1. Results section is very difficult to read because it repeats information presented in Tables 2 and 3 with detailed statistics. Streamlined discussion of the results would improve the paper.

2. It is not clear why the manuscript lists as the main outcome of interest EOLD scales when only one of the reviewed paper was using one of these scales.

3. The manuscript requires careful editing:
a. Results mentioned in the text on p. 16, line 5 are not included in the Table 2
b. Significance of results is not always indicated (e.g., p. 16, line 11-12 and corresponding information in Table 2).
c. Reference numbers are wrong or missing on several places (e.g., p. 13 line 19 and 22, p. 14 line 18).
d. Some of the abbreviations are not explained in the list of abbreviations (e.g., KI in Table 2).

In summary, I agree that there is a need for additional research but to question usefulness of UK recommendations in the meantime does not help patients or their caregivers.

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests