Author's response to reviews

Title: Pre-arrest predictors of survival after resuscitation from out-of-hospital cardiac arrest in older people - a systematic review

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Author's response to reviews: see over
To the Editor of the Journal of BMC geriatrics

Amsterdam, 27 May 2013

Dear Editor,

Thank you for allowing us the opportunity to resubmit our manuscript entitled: “Pre-arrest predictors of survival after resuscitation from out-of-hospital cardiac arrest in the elderly – a systematic review” for consideration for publication in BMC geriatrics.

We would also like to thank the referees for their helpful comments and for their attention to detail with respect to our submitted study. We have taken considerable steps to address the concerns that they have raised. Especially the discussion section changed substantially, because of the suggestions of the reviewers. We believe that the discussion now more supports the result section and that the conclusion is more relevant to clinicians.

Enclosed you will find the revised manuscript and a point-by-point response to all the comments.

We hope you find the efforts we have taken, as detailed below, have improved the manuscript sufficiently for publication in BMC geriatrics.

Also on behalf of the other authors,

Yours sincerely,

Esther M.M. van de Glind.

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Response to reviewer 1:

Reviewer: Wilco Achterberg

Major Compulsory Revisions

Aim of the study
The authors aim was (I cite from the abstract) to provide elderly with information to enable them to make decisions about the appropriateness of CPR (clearly stated in the abstract). I therefore find it disappointing that in the conclusion (page 14), they are not willing to give any information that can be used to inform physicians and elderly about the realistic, poor chances of survival. As I understood the evidence they have gathered, the chances of survival for the elderly are 4% maximum (this is probably overestimating the chances) and it is decreasing with comorbidity and nursing home residency.

We agree with the reviewer that our conclusion does not match the aim. Therefore, we have rephrased the conclusion so that the findings of our study are more clearly stated and can be used to inform older people about their poor chances of surviving an out-of-hospital cardiac arrest.

The last sentence of the introduction however is much more obscure, and hard to follow: “This could inform decision-making when a do-not-attempt-resuscitation order is discussed.” Decision making can not be informed. This sentence touches the aim of the manuscript, and should be in line with aim as is discussed in the abstract. May be better: ‘To provide the decision making process with information on the actual chances of (good) survival in patients with advanced age, comorbidity and/or nursing home residency’.

We have changed this section towards the suggestion of the reviewer and added some more information into this sentence to make the aim more clear.

I find it very important that a review like this (very much driven by a clinical critical question) tries not only to summarize previous findings, but synthesize these into conclusions and recommendations. Bridging the gap between theory and practice was the major reason for conducting this review; in the discussion and conclusions therefore I feel that more efforts should be done to close this bridge a little bit more.

We have restyled the discussion and added some paragraphs to state more clearly the conclusions of this review and the evidence-based recommendations for future practice and research, although the available evidence is limited. In line with this, we changed the conclusion of the abstract.

In depth discussion of studies
I find the length and depth of the results section disappointing, and to make things worse, some parts of the result section are more eligible for the discussion section.

The subheading survival for instance is one of the most crucial section, but presentation of the effects of comorbidity and nursing home residency for instance is not discussed in the results section. The authors should try to do a better job in reviewing the results on age, comorbidity and nursing home residency.
We agree with the reviewer that the focus of the result section was too much on the quality assessment in stead of on the clinical implications of the results. Because we consider the quality assessment an important part of a systematic review, we did not change much on this paragraph. We did however put more emphasis on the subheading survival (page 10), because this section indeed is an important part of our review. We rephrased this section to put more on stage the available evidence considering comorbidity and nursing home residency. Because we had many data, we chose to put most of our results in table 2 and 3. Furthermore, we expanded table 3 with a section about nursing home residency. We believe by doing this we have made clearer what is known from the literature about these predictive factors. It is true that it is disappointing that only general results could be presented from which drawing conclusions is difficult, because relatively little evidence that investigated predictive factors was available.

We critically reviewed the results section to see which parts were more eligible for the discussion; there is one paragraph in the Characteristics of included studies that we moved to the discussion (“In the literature (…) samples from a cohort study”).

**Discretionary Revisions**

Page 2, second line: predictive value OF pre-arrest factors
We changed this.

Page 3, last sentence abstract: “…these factors should be evaluated too.” Do the authors wish to express that physicians evaluate these factors in practice, or that these factors should be evaluated in future studies?
*We indeed meant in future studies, therefore we changed this.*

Page 4: what could be the background of the lack of improvement of survival in the last 30 years (line 4)?
*We added some clarification from the referred review by Sasson et al.*

Page 4, line 5: is the risk of morbidity and disability increasing with increasing age, or the prevalence?
*We changed “risk” into “prevalence”.*

Page 4, last sentence: “…between prearrest variables and the probability…”
maybe characteristics is better than variables, because we are talking about a clinical situation, not a research setting?
*We agree that the term ‘variables’ is confusing. For the matter of consequence, we decided to use the term “pre-arrest factors”; in the paragraph (page 5) we added a phrase to explicate this term: “pre-arrest factors such as cognitive impairment and comorbidity”.*

Page 6; why only search the medline database? Especially because the number of studies included are relatively limited, extension to EMBASE, CINAHL, PSYCHINFO etc could have provided more information. This also holds for the inclusion criteria ONLY ENGLISH
*In addition to our search in MEDLINE, we used the Dutch multidisciplinary guideline of Verenso, the Dutch Association of Elderly Care Physicians and Social Geriatricians as an additional source of studies. For this report, an extensive search was performed from 1950 to 2008 in MEDLINE, Embase, Web of Science, CINAHL, Cochrane DSR, DARE and Cochrane Controlled Trial Register. We refer to the flowchart (figure 1) that was added on request of reviewer 2; from this flow chart it becomes clear that searching in the other databases did not add much to searching in MEDLINE only (n=1). We limited ourselves to publications in English because adding studies in other languages would probably increase the heterogeneity in used patient populations, EMR services and protocols.*

PAGE 7: line 4: the protocols have been changed in the last years- is there no effect on survival of the new resuscitation guidelines?
This is one of the many reasons that accounts for heterogeneity between the studies. We did not investigate the effect of changing protocols over the years, but we discuss this issue and other issues with regard to heterogeneity in the discussion section. We only included studies that were published after 1980.

Page 10, line 7: “....adjustment for by response time....” This is not clear to me.
We rephrased the sentence to make it grammatically correct and more clear.

Page 13, line 17 “.....should be reported too.” I would like to add: in future studies, or is that not what the authors try to say?
We added: “in future studies”. 
Response to reviewer 2

Reviewer: Sandra Pereira

Minor Essential Revisions:
1. P.6 vs P.7 – In page 6, the author explicit who were the researcher in charge of selecting the studies, who were the researchers who screened the full texts more thoroughly, and with whom the disagreements were discussed. Although the authors did this explicitly in this section of the article, they did not do in the section about “quality assessment” (page 7). So, in order to maintain the coherency of the article, if the authors decide to mention “who did what” in one of the sections, it would be more coherent if they mention it in the other section as well (or the other way around i.e. not mentioning “who did what” in none of the sections, since this information can be added in the end of the article and not into it).

We added the initials of the authors who performed the quality assessment.

2. Page 9 – Identification of studies – A flow diagram of the selection process of the articles would be useful.

We added the flow chart (figure 1).

3. Page 10 – “Many studies reported survival to discharge...” – What is the meaning of “many”? This term introduces subjectivity; it would be relevant to provide the number of studies that reported this aspect.

We agree with the reviewer that it is more relevant to add the numbers of the studies that reported these outcomes as well. Therefore, we changed this and added references to this section.

4. Page 11 – “Of the included studies, few assessed the quality of life...” – As mentioned previously, it would be relevant to know in how many studies quality of life was assessed, moreover if we consider that this was one of the aims of the article.

We agree with the reviewer that it is more relevant to add the numbers of the studies that reported these outcomes as well. Therefore, we changed this and added references to this section.

5. Page 13 – “The retrieved studies indicated that the quality of life of survivors was acceptable; (...)” – What is an “acceptable” quality of life?

This part was removed to the discussion because of the suggestion of reviewer 1 and more explicated.

6. Although the authors refer briefly to some limitations (page13), they could have mentioned to the limitations explicitly. Also, based on these limitations and on the results, the authors could have pointed out some keys for further studies.

We thank the reviewer for this useful suggestion; both reviewers suggested that the discussion should be more detailed and thorough. We added to the discussion paragraph a section about limitations and future studies.

7. Page 14 – Do these conclusions really refer to the systematic review that has been conducted and answer to the research question and aims of the article? Some of the conclusions are beyond the results and discussion presented by the authors, appearing to be a statement that the authors would
like to provide, more than to what they can really conclude from the research that they have conducted.

*This remark is in line with the comments of reviewer one. We changed the conclusion into a more usable one for clinical practice. Indeed, the original text was more a statement; therefore we removed this part to the discussion paragraph.*