Reviewer's report

Title: A Systematic Review of Empirical Bioethics Methodologies

Version: 2
Date: 3 January 2015
Reviewer: Bert Molewijk

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Review of resubmitted paper BMC Medical Ethics (Bert Molewijk, reviewer 4)

Paper: Davies, Dunn, Ives: A systematic review of empirical bioethics methodologies

I am impressed by the thorough answering process of the authors. The way they addressed all the comments from all the four (!) reviewers is impressive. It contributes to my appreciation of the work and the thoughtfulness of the authors. Some of my concerns, however, are not yet fully addressed.

I will use the structure of responses to the four reviewers that the authors used in their response to the first review round.

4.2 Thank you for the additional justifications on page 5. To me it seems that these justifications are rather pragmatic/practical (‘has received most methodological attention, is the most heterogeneous and is most in need of systematic review’). Also the fact that the IEEN said that these questions were important is in itself not a (principle) argument for choosing for integrative approaches. I feel that the fundamental or normative principle for the authors to focus the SR on ‘integrative’ approaches, which in my view is connected to their pre-understanding of Empirical Bioethics (or ethics in general), is not mentioned. I still think this is important for the reader to be transparent about their focus and to be explicit why other approaches of EB are NOT chosen. I wonder whether the authors think that any kind of (bio)ethics should at least concern ‘normative conclusions’ and ‘justifications for their normative conclusions’.

Furthermore, I do not see that they paid attention to the consequences of their focus (that is NOT including other approaches). The consequences of their choice for integrative approaches could be mentioned at page 5, within the Discussion section or within the Limitations section (e.g. in which way are the results of their SR relevant or not relevant for EB that does NOT use integrative approaches?).

4.3 Thank you and thanks for changing the incorrect reference :)

4.4 I do not think that the authors have addressed my point (sufficiently) in the Discussion section (and I think that the new reference to the Nuffield report is much too extensive/detailed and is distracting from the more general and more important point of the authors regarding heterogeneity within the field). The sentence ‘There is little common ground … and indeed whether such an identity
is needed' on page 18 is of fundamental relevance and needs still more attention. What are the consequences of these statements for the field of EB and how should we deal with such a conclusion? What is the normative position of the authors regarding this conclusion: Do the authors think that an identity of EB is needed or not? Is this conclusion problematic for the development of the field of EB? The reader has the right to know how the authors evaluate their description of the field of EB.

Furthermore, this way of ending the paper (i.e. the lack of consensus about what is required of a methodology is problematic) without responding to that problem or without elaborating about possible ways to deal with that problem does not contribute to the further development of (methodology within the field of) EB AND does not take into account the strengths and results of this paper for the field. For the sake of the field of EB and the relevance of this paper, I think it is wise to reflect more constructively on how to deal with this conclusion and give some clues how this paper at least may contribute to the further development of (methodologies in) EB. This is also a way of saying to the authors: you did some great work, please clarify more explicit in the Conclusion (and in the Abstract) how your paper contributed or may contribute to the notified problems.

4.5 With normative status of the normative conclusion I meant whether the EB researchers that draw and justify the normative conclusion think that this specific conclusion should be understood as ‘prescriptive’ for other practices and theories (or even be has universalistic epistemological claims), or whether this is just a local conclusion.

4.6 Clear now.

4.7 OK for me.

4.8 OK for me (while still an interesting point for discussion later on since it relates to our understanding of empirical research in the first place).

4.9 Clearly improved.

4.10 OK. To be continued elsewhere #.

4.11 Perfect.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

3rd January 2015

Last year the last two authors (Dunn and Ives) and me (together with
Schildmann) received some funding from the Borcher Foundation for the organisation of a workshop on methodologies Empirical Bioethics (May 2015). However, I tguink this has not influenced the first and second review of this paper in neither way.

Bert Molewijk