Reviewer's report

Title: De-tabooing dying control. A grounded theory study.

Version: 3 Date: 7 January 2013

Reviewer: Joachim Cohen

Reviewer's report:

The authors have done a fairly good job revising their manuscript and have paid attention to most of my concerns. The manuscript has improved considerably. I have some skepticism about the GT approach, but let us concluded that these are mere personal paradigm differences.

There are a limited number of comments that I still have nevertheless:

1. I find the research question in the abstract “What is going on in the field of dying today?” too vague. I would suggest taking the research question that the authors also used in their introduction section: what is going on in the field of dying control, and to present a grounded theory (…)

2. Page 10: “This is illustrated in public debate, where criticism against euthanasia and PAS is seen as almost intolerant or antidemocratic (…) This is an example of what I meant in my previous review of a selective use of empirical findings in the authors’ GT approach. I’m not sure whether there are many empirical data about this, and if there are it still reads as a very subjective judgement. It does also not contribute to the grounded theory. My suggestion would be to leave out this section.

3. Why did the authors choose to include the 2 Swedish surveys. Why not including surveys from different countries given that GT (as the authors also claim themselves) is abstract of time, place and people.

4. RESULTS, page 11: “A reversible medical control of dying is therefore more inclined to support paternalism than to defend the taboo of questioning autonomy”

This needs a bit more explanation.

5. Discussion: the paragraph: “Both in the survey data (…)which might have influenced their willingness to express themselves in open comments [13]”. I do not understand why this paragraph appears in the discussion as it presents results from the 2 selected surveys and these results were not presented in the results section. More importantly though, I do not see what it contributes to the grounded theory on de-tabooing dying control. I would suggest removing this paragraph.

6. Page 12: De-tabooing goes on in other substantive areas as well. A literature
search reveals that (...)which was seen as a de-tabooing process explaining parts of the phenomenon of the growth of right wing voting in Europe these last decades [51].

These two paragraphs should be combined as one paragraph and shortened as it merely serves as an illustration of how de-tabooing also occurred in other societal domains.

7. page 13. “The attitudes of the public in the survey data from this study came from a sample of people living in Sweden’s largest urban area. While it is known that euthanasia is twice as common in Dutch urban parts as in rural areas [52], had our sample also involved rural areas, the views might have been less positive to PAS.”

This is very well possible but there are no really good reasons to assume these rural-urban differences. A study in Belgium specifically focusing on metropolitan vs non-metropolitan differences did not find such differences in terms of euthanasia, but did find an inclination to more paternalism in the metropolitan areas (Cohen et al. 2010, Health & Place).

I also wonder why this is so important in a GT approach. Does it matter that much if the survey results would have been a bit higher or lower, if the analysis is at the conceptual level?

A more important limitation in my mind is that only Swedish survey data were used.

**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:**

I declare that I have no competing interests