Author's response to reviews

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

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Author's response to reviews:

Dear editor,

Thank you for your letter of February 13 re the MS: 3704510372797973

De-tabooing dying control. A grounded theory study.

We want to once again thank the reviewers for their thorough readings of our ms and valuable critique, and hope that the current version is improved enough to be published!

We have read and acted on reviewer professor Cohen’s very valuable comments and our responses are seen below.

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 (…)

Response: Point taken. We have altered the abstract accordingly.

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
Response: Prof Cohen is right that our empirical data is weak to back up that claim, and the sentence containing the phrase - “This is illustrated in public debate, where criticism against euthanasia and PAS is seen as almost intolerant or antidemocratic (...)” - is therefore omitted.

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.

Response: Prof Cohen is of course right that more data from other cultures and countries would be desirable. However, when data from the Swedish surveys were compared with data from non-Swedish interviews, the four American internet discussion forums and the literature (i.e. references 51 and 53) the same attitude patterns emerged over and over. This leads us to conclude that the Swedish survey data was rich enough to allow conceptualizations that are relevant to other cultures as well.

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.

Response: Prof Cohen is right. The sentence is difficult to understand and has been expanded for better clarity.

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.

Response: Point taken, paragraph removed.

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.

Response: Point taken. Paragraphs are now merged into one that has been shortened.

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.

Response: Prof Cohen is right - these limitations are not important from a GT point of view since the concepts do not change. But, this also goes for the fact that that the survey data was only Swedish. As already said, the concepts found in the Swedish data were also evident in the interview and discussion forum data as well as in the literature. This is now mentioned in two new sentences.

We found it interesting to read the paper comparing metropolitan Bruxelles to non-metropolitan surrounding Flanders. Our reflection of the difference in euthanasia practices makes us think more of a language-cultural difference than a rural-urban difference. Bruxelles being French speaking but the surroundings Flemish. In your recent interesting article (Cohen J, Van Wesemael Y, Smets T, Bilsen J, Deliens L. Cultural differences affecting euthanasia practice in Belgium: one law but different attitudes and practices in Flanders and Wallonia. Soc Sci Med. 2012 Sep;75(5):845-53) - you show important differences between Flanders and Wallonia with regard to euthanasia – practices more common and attitudes more positive towards euthanasia in Flanders than in Wallonia. So could not the differences seen in your study from Bruxelles and surroundings be explained by language-cultural differences instead of rural/urban?

Our argument for a rural-urban difference in attitudes is supported by a large Dutch study (ref 52) that probably better represents the situation in a mono-cultural society as the Swedish.