Author’s response to reviews

Title: Prediction of life-story narrative for end-of-life surrogate's decision-making is inadequate: a Q-methodology study

Authors:
Muhammad Hammami (Muhammad@kfshrc.edu.sa)
Kafa Abuhdeeb (kabuhdeeb@kfshrc.edu.sa)
Muhammad Hammami (mhammami@health.fau.edu)
Sophia De Padua (jehans@kfshrc.edu.sa)
Areej Al-Balkhi (abalkhi1992@yahoo.com)

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Author’s response to reviews:

Dear Editor,

Thank you very much for the opportunity to revise and resubmit our manuscript. Attached please find a revised manuscript. Changes to the manuscript are highlighted. Our point-to-point responses to the Reviewer’s comments are below. We are grateful to you and to our Reviewer.

Best regards,
Muhammad M Hammami, MD, PhD

Tenzin Wangmo (Reviewer 1): As stated previously, this is a very interesting research on the topic. The methodology is now clearer and the readability has improved. I look forward to seeing it published in this journal. Below are a few suggestions/clarifications that the authors may choose to consider. Authors’ response: Thank you for your comments and for taking the time to review our reply.

I thank the authors for providing very good explanations in response to my point 1. Based on the clarification that the authors provided, I suggest to delete the sentence that names the three life story narrative approaches (authentic, substituted, and endorsed) on Page 5, lines 4 - 6, since it
does not add anything more to the paper. These three are mentioned again on Page 16, lines lines 48- 49, which could be deleted as well. Authors’ response: Thank you. We agree, these two sentences are now deleted.

I agree with the authors that it is logical to expect accuracy of decision prediction based on relationship. This is why I hoped that they would add a few sentences explaining this rationale of selecting relationship-pairs, even if there no study has done the same. This comparison is an added value of the study, although it is a secondary aim. Emphasizing on it in the conclusion would be useful. Authors’ response: Thank you. We agree. We have now added the following sentence: “Since prediction accuracy may depend on type of relationship, it was specifically examined in three categories of family pairs, husband-wife, parent-child, and sibling-sibling”. (page 5, lines 10-12)

The authors did not understand my comment regarding the country of study. Naturally, it is clear that they are from Saudi Arabia and thus it makes sense to do the study there. This is not what I meant. Studies in the field of medical ethics tend to be dominated by research carried out in Europe or North America. Thus, it is a welcome contribution to see work from other context. If the author names were blinded and the country was not specified, there is no reason to believe that this findings could be from any other country. Hence, my suggestion to explain why this context is relevant or irrelevant was to highlight why the context matters or not, in their opinions. As a reader I can imagine that its irrelevant because of the topic (end-of-life), its methodology and/or relationships that we all share irrespective of which society we live it. Also, an explanation to this would have intuitively made it evident why comparing this study with those from other context is valid.

Stating that no such studies are carried out in Saudi Arabia would also to good for the readers. Authors’ response: Thank you for the clarification. We would like to note the following: 1) end-of-life-studies have been done in various countries, including non-Western countries and Saudi Arabia (references 30-38, 9, 21-22) (page 17, lines 3-4). 2) However, the current study is the first to systematically evaluate surrogate’s accuracy in predicting life-story narrative, not only in Saudi Arabia, but worldwide (page 16, lines 25-27). 3) Nevertheless, studies on prediction accuracy of decision-control preference and healthcare-outcomes acceptability have not been carried out in Saudi Arabia. 4) Therefore, in agreement with the reviewer, we have now modified the related sentence on page 17, lines 13-15 to read: “In agreement with the results of previous studies conducted mainly in Europe and North America, [4-8] prediction accuracy of decision-control preference and healthcare-outcomes acceptability were similarly inadequate, suggesting that the observation is not culture-related.
Redundancies:

Page 16, paragraph 2: Part of the paragraph re-states what is said in the introduction about life story narrative model. This paragraph should be shortened to reduce redundancy, which will make the point of the paragraph clearer. Authors’ response: Thank you. We have now shortened the mentioned paragraph (page 16, lines 20-25).

The first paragraph of the conclusion summarizes the main results and discussion. Although useful, is it necessary? Authors’ response: Thank you. We think that summarizing our main conclusions here is necessary, especially for readers who would not read the entire manuscript. We elected not omit it.