Reviewer's report

Title: Healthcare decision-making in end stage renal disease-patient preferences and clinical correlates

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Reviewer: Sabine van der Veer

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Jayanti and colleagues report data on ESRD patients' preferences for receiving information and for being involved in decision making. The topic is relevant, and they collected rich data in the context of the BASIC-HHD study to better understand patients' preferences in this area. My main concerns are related to the authors' approach to the statistical analyses of the data.

Major Compulsory Revisions

1. To build their multivariate model, the authors selected covariates based on univariate analyses, only including those with a p-value <0.15 (pg.6, lines 161-2). Looking at Table 3, I was expecting 14 covariates to be included in the model presented in Table 4. However, this model only contains 7 factors, suggesting that additional criteria were used for variable selection. The authors should make these criteria explicit.

2. An additional concern regarding the multivariate models is the lack of detail on if and how the authors addressed potential confounding (see www.ncbi.nlm.nih.gov/pubmed/17978811). It seems that all covariates reported in Table 4 have been adjusted for all other covariates in the table. However, the causal pathway between some factors and the outcome variable may be confounded by other factors. For example, patients with higher educational attainment are more likely to be in the home HD group, as well as having higher API IS scores. Therefore, group and API IS score are on the causal pathway between education and API DM scores, potentially mediating the association. These should therefore not be included in a multivariate model exploring the association between education and API DM scores. I strongly recommend the authors to revisit their multivariate models, and to develop a separate model for each factor to explicitly address the potentially confounding effects.

3. In the Methods section on Statistical analysis (pg. 6, lines 154-65), the authors report the use of cut-off values that are all arbitrary. These include the values used for dichotomisation of the outcome variable for the information seeking analysis, including variables from the univariate analyses in the multivariate analyses, and cut-off values for creating subgroups based on API decision-making scores. The authors should perform sensitivity analyses to enable assessment of the effect of these arbitrary decisions on the reported results.
4. The authors perform a multivariate regression analysis to identify factors that are associated with decision-making preferences as measured by API DM scores. One would expect the factors with the strongest associations to be the ones related to participants being classified as 'delegators' or 'autonomists'. Therefore, I don't understand why the authors performed an additional univariate analysis with API DM scores as categorical outcome variable to 'understand factors associated with these scores in the highest and lowest tertiles' (pg 6, lines 164-5). It is also unclear why the covariates included in the multivariate model differ from the factors reported in Table 5. Both points warrant further clarification.

5. The authors should clarify how they accounted for potential centre-level clustering of the data in their analyses.

6. To better understand to what extent the study population was a selected sample, the authors should provide information on the eligibility criteria for the BASIC-HHD study and how many patients were invited to participate, reasons for declining participation (incl. numbers), and report some information on the 77 patients who were enrolled, but did not contribute to the study findings. The potential influence of selection bias on the study's conclusions should be addressed in the Discussion.

Minor Essential Revisions

1. Even though the impact of starting dialysis on decision making preferences was one of the three study objectives, none of the reported results in the abstract refer to it. The authors should consider dedicating a sentence to this part of the findings.

2. The sentence in the abstract stating that 'By understanding factors ....to individual patient's preferences' (pg.2, lines 41-3) is not warranted by the results, and should be removed or reformulated into a weaker statement.

3. References 2-5 in the Introduction are all at least 25 years old. Please replace them with more recent studies to support the statement on the effect of increased patient involvement in decision making.

4. The study by Flynn et al. (www.ncbi.nlm.nih.gov/pubmed/16697096) also investigated patients' preferences for receiving information and for being involved in treatment decisions, and came to very similar conclusions as Jayanti et al. I would expect the authors to refer to the work by Flynn and how the present study relates to it.

5. Please state in the Methods on (1) which analyses have been adjusted for multiple testing and how (pg. 6, line 157), (2) the analysis used to calculate the Cronbach’s alpha values (pg 6, line 163), and (3) the cut-off value used to dichotomise API IS scores as outcome variable.

6. In the Results (pg.7, lines 176-177) the authors state that the 'group comparisons are important to adjust later analyses for potential confounders'.
However, it is unclear how the findings in Table 1 have informed further analyses. This needs clarifying.

7. The authors have been quite generous with providing information in tables and figures. Although this is generally a good way to summarise information, it didn’t help me to grasp what were the important bits related to the study’s objectives. As an overall comment, I would recommend the authors to revisit their tables and figures, and present only those that are essential to answering their research questions. Those that are not should either be removed or be included in a supplement. For example: (1) Why present the results in Table 1 per study group? (see also previous point); (2) Why report item-level results for all participants in Table 2? If the authors consider this information essential, they should include the item descriptions; (3) Why does Table 3 only contain results from the univariate analyses for decision making, and not for information seeking?: (4) Figure 2 could be replaced by one sentence in the results stating that IS and DM scores were significantly different for all study groups.

8. The footnotes in Table 5 are missing. Also, the way the results are presented in the p-value column are confusing. The

9. Figure 3 is difficult to interpret without an explanation of the scores on the x-axis in the figure caption.

10. It is unclear what the dots in Figure 4 represent. For example, the Delegators group consists of 57 participants, but there are only 31 Delegator dots displayed in the figure. This should be clarified.

Discretionary Revisions

1. The Introduction is rather long and could benefit from more focus. The authors could consider shortening it by 25-30%, while making sure that readers are guided more straightforwardly towards the study objectives.

2. The statement in the Introduction on pg 4, lines 81-82 ("The dominating concerns ... context of ESRD") requires a supporting reference.

3. Figure 4 is difficult to understand when using a hard-copy printed in black-and-white. It would help if different groups would be represented with different symbols.

4. I suggest the 'Study limitations' section in the Discussion (pg. 13, lines 357-65) to be placed before the Conclusion section.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests:

I declare that I have no competing interests