Reviewer's report

Title: Barriers to Successful Care for Chronic Kidney Disease

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Reviewer: Gordon Taylor

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General
My overall impression of the paper is that although it is generally an interesting paper I have a number of concerns over the statistics. I am concerned that the statistics used may lead to spurious and misleading results. I would therefore wish to see a competent analysis of the data undertaken prior to the publication of this paper.

In particular:

I would like to see a justification for the long and short treatment groups. Why have these groups been chosen as they are. I would be particularly interested in how long the median length of treatment for the LNC group is. I presume that a number of the SNC group patients will in time become LNC patients and are therefore SNC only because they have not been in the system long enough. It does appear me that one would not expect these two groups to be similar in the first place due to confounding factors and that artificially splitting them and then comparing them to say they are different is not a great surprise.

Table 1: In this table the authors appear to have tested every level of every variable (SNC vs LNC). This is an inappropriate way to analyse the data. The authors need to consider using an overall test for each characteristic and then considering whether it is appropriate to undertake further post-hoc analysis to determine where the difference is. eg for the age characteristic one needs to perform an overall test (possibly a t-test) and then if they are sig. different then identify where these are. The test given is meaningless and misleading as the P values calculated will depend on how the age variable is split into groups. The GFR levels are similarly misleading.

As a general point I would prefer to see the significance level of the non-significant variables.

Table 2: This table is unnecessarily complicated. It is curious that the authors have presented medians and quartiles and yet undertaken an ANOVA. ANOVA assumes that data is normally distributed whereas the use of medians (and some of the data itself) suggests that the data may be skewed. The Bonferroni adjustment is appropriate with the large number of tests being undertaken. However, I suspect that the adjustment is only within each parameter tested and not across parameters.

Although I would not necessarily wish to see the authors adjust for
everything using Bonferroni. The paper needs to be much clearer of its limitations and its primary objective. From reading the paper I am not convinced that the LNC, SNC split was not just a serendipitous post hoc split rather than part of the initial protocol.

Table 4: I found the whole analysis of number of goals achieved rather unconvincing. Why was 5/9 goals chosen for the logistic outcome. What difference would 4 or 6 have made to the outcomes of the analysis? As all of the goals equally difficult to achieve? Are the goals independent of one another? The analysis therefore requires further justification and / or a sensitivity analysis.

I am sorry that the above is rather critical but as the paper currently stands the statistics presented are not appropriate for publication.