Reviewer’s report

Title: Analysis of Factors Predicting Mortality of New Patients Commencing Dialysis Therapy in a Single Year after 10 Years Follow-Up

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Reviewer: Jochen Raimann

Reviewer’s report:

The current manuscript is an analysis of 10 years’ survival in a cohort of patients commencing treatment in the United Kingdom. The manuscript is a second step analysis (as phrased by the authors an update) of a previously published analysis of 5 years’ survival in the very same cohort. The authors confirm some aspect of the initial analysis but also find differences in terms of bone and mineral metabolism as a predictor of death. The paper is generally well written but draws conclusion which I am afraid can not to be made from the current analysis. This being said I can not recommend the analysis for publication in its current form. Major revisions and change of analysis aims are necessary to make this worth to be considered for publication.

1) Firstly, I’d like to emphasize that I do not feel the current analysis is an update to the previous data. An update would be, in my eyes, an inclusion of more patients, newly acquired parameters or more sophisticated statistical approaches (along these lines it may be mentioned that in the previous publications the authors stated that multivariable analysis was not possible due to the low sample size – however in the current analysis it appeared to be possible (in regard of the fact that it is the same cohort and thus sample size).

2) Along these lines a very small sample size needs to be emphasized – there is literature available on observational data which shows factors relating to survival in larger datasets (even providing insight in race and ethnicity as predicting factors).

3) The longer observation period is used as an argument of the current analysis being an addition to previous data. However, the longer observation period does the opposite – it renders baseline period as even more meaningless for the survival at the end of the observation period. It would be of much greater impacts if the authors would include longitudinal data and trends during the analysis (in time dependent statistical approaches) in the analysis. Alternatively, it would be of interest to look at trends in parameters before and after HD initiation as a predictor of death in the following observation period.

4) I repeatedly searched for information on cardiac function and or blood pressure in this analysis and couldn’t find any information. It has been shown that BP change during the first 120 days and also trends during the first year is a strong predictor of death. This is undoubtedly a stronger predictor that Ca*P product and needs to be considered.
5) Patients not surviving the first six years should be excluded (or more interestingly analyzed separated from the rest of the cohort in a subset analysis)—death during this period is not conclusive about general dynamics and also adds a survival bias.

6) The analysis of KTX patients is not uninteresting but appears in the context of the current analysis a little half-heartedly conducted. It can be looked at in a different context but not in this analysis – I’d suggest to censor these patients since KTX patients are more or less considered to be CKD3 patients and thus not really comparable to the cohort analyzed.

7) Residual Renal Function: Abundant literature shows how RRF affects survival and this information needs to be included. Not only at initiation but also trends before and after initiation. Although comorbidities will affect the dynamics of RRF, RRF can also be influenced by the treatment per se (e.g. volume overload, intradialytic symptoms, etc.).

8) HbA1c at low levels do not necessarily only reflect diabetic treatment but can also be a reflection of malnourishment. Along these lines – no association (an additional point – the authors write about correlations with mortality – this is not really a commonly used term – one may consider speaking about relationships, associations or more accurately termed “statistical predictors” in the context of the results of the analysis) between body composition (which only surfaces briefly and very indirectly as creatinine concentration at HD initiation) and survival. Changes in body composition (e.g. muscle loss) are associated with outcomes – this needs to be addressed in this analysis.

**Level of interest:** An article of insufficient interest to warrant publication in a scientific/medical journal

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

No relevant interests to declare.