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 G Raimann

Reviewer's report:

From a bird's eye view – the paper aims to analyze survival over 10 years taking baseline parameters at HD initiation (not including key parameters such as blood pressure, residual renal function, amongst others) in account. The only significant predictor in this analysis remains presence of age and diabetes at baseline (as per the Cox regression model). Potentially modifiable predictors are not significant as compared to a previous analysis from the same group, which was conducted in a similar fashion investigating a 5 years observation period. In regard of all the parameters being affected by treatment regimen (dialysis, anti-diabetic treatment, vitamin D substitution, phosphorus binder, dietary restriction) and thus not necessarily associated to the outcome over a 10 years period, I think that only longitudinal analyses can answer the difficult and interesting research question investigated by the authors. I believe that the current analysis does not satisfactorily answer these questions.

I acknowledge the hard work the authors have invested in the current manuscript, however I am still not convinced that this paper adds to the current knowledge in the field and would not recommend it for publication. It is acknowledged that it has improved in terms of raised points, but I feel there is still room for improvement. In addition I would like to mention that I believe the dataset the authors have at their disposition could be the basis for the analysis of (more addressable) research questions of equal importance. However, if my fellow co-reviewers recommend the paper for publication I also don’t have objections to its publication if the editorial board deems it valuable and appropriate for publication in BMC Nephrology.

Previous points raised:

Ad 1) Removing the term update is certainly a good idea – re-analysis sounds like a better term for this analysis. Concerning the terminology: In the methods, the analysis is declared as being cross-sectional, which I am not sure is the appropriate term in regard of 10 years data of dialysis patients’ survival. It appears to me to be a somewhat discussable description of the study design.

Ad 2) The small sample size is now clearly stated as a limitation and the specific mentioning of the high probability of a type II error is important.

Ad 3) I acknowledge your disagreement with my suggestion of using longitudinal analyses and to also include trends of parameters in the analyses (particularly of survival). In regard of several of the parameters strongly being influenced by the
dialysis treatment this would have made the analysis much more informative. The analysis incorporating data prior and after HD initiation would be of great interest, but I agree with the authors that it probably should be conducted based on data from larger scale registries/databases.

Ad 4) Regardless of the well-established differences between ABPM measurements, home blood pressure measurements and pre HD in-center BP measurements, the pre HD in-center BP is a well-established predictor of mortality and should be included in the analysis. The analysis of LVH as a predictor of death would be worthwhile to be shown in regard of the high fraction of cardiovascular deaths.

Ad 5) I apologize for this horrible typo in this comment – meant was six months (instead of years). Including patients which died before formally being prevalent dialysis patients may show different effects of the baseline parameters. Particularly in the Kaplan Meier curves there appears to be a sudden (not unexpected) drop in the fraction of surviving patients which is not consistently found for all patients. I believe that exclusion of subjects not surviving the first 6 months would be of interest since mortality is high in the first months after HD initiation and potentially of different cause than for prevalent patients. An in depth analysis of mortality in incident patients only may also be of interest in this unique Caucasian (98% - along these lines – what race/ethnicity are the other 2% - didn’t find this mentioned at all in the manuscript) population.

Ad 6) The message you are trying to convey to the reader in this underpowered analysis prone to a type II error is that kidney transplantation does not result in a survival advantage to a patient suffering from renal failure – did I understand this right? I consider this a debatable result which should not be made with insufficient statistical power.

Ad 7) I didn’t find any mentioning of residual renal function in the limitation section – did I miss it? The only mentioning I found is eGFR (or GFR as commented on by Reviewer 1 – I agree with his comment that this should be uniform throughout the manuscript). Was this the only assessment of its kind prior to HD initiation? Speaking of HD initiation – I would not talk about “pre HD…” when talking about a value prior to HD initiation – pre HD is considered to be reserved for pre HD assessments prior to an individual treatment.

Level of interest: An article of limited interest

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.