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: Laura Plantinga

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I tend to agree with the first reviewer that the paper is not ready and could use better statistical techniques (or at least better explanations of what was done).

Some thoughts:

1. In the abstract the data about Ca-P should be presented if the authors are going to make conclusions about it.

2. Why were MANOVA and Cox regression used? Was the proportional hazards assumption not met over this long period of time?

3. Related to the above, even if the proportional hazards assumption is met, a segmented analysis (or equivalently, the addition of an interaction term with time) might be interesting and relevant to the study question. For example, an indicator of <5 vs. >=5 years could be created, and this term could be tested in interaction with variables of interest to determine whether there is evidence of effect modification by time (e.g., if Ca-P is associated in early years and not in later years, as the authors suggest). This gets at the question more directly than simply comparing the same analysis with more years.

4. Why are there no adjusted analyses (only Kaplan-Meier)? Is this due to small numbers of events? It seems at least half the cohort died (~50) so there is probably some wiggle room for adjustment for 4-5 important confounders.

5. The median follow-up is 8.7 years. This seems extraordinary if those who died at <5 years were included (which seems to be the case from Table 1). Further, it appears from Table 2 that 1/2 of the patients who started dialysis 10 years prior were still alive, which again seems high. Do the authors have any thoughts on whether this is a highly selected cohort? Or is this typical in the UK? Either way this should be discussed.

6. Those who dialyzed <90 days were excluded. Did this include early deaths?

7. While the exclusion above could have resulted in selection bias if it were differential by variables of interest, I am not sure why the results that show kidney graft is protective are indicative of selection bias (see first paragraph of Discussion). Could the authors clarify?

8. Table 3 needs the beta estimates, not just the CIs. Or, perhaps, exp(beta) since this is a risk ratio (from Cox regression). The Wald statistic is not of
interest.

9. Limitations should include: confounding by both known and unknown factors (since there was no adjustment); misclassification of the outcome (cause of death only) and of potential confounders; and lack of longitudinal data on all the risk factors, as biomarkers (like Ca-P), vascular access, and comorbid conditions could change dramatically in 10 years, making them less relevant closer to death.

10. The conclusion is not supported by the data. The authors don't present longitudinal data on CVD/diabetes treatment nor do they adjust for it, so it is speculative that changes in treatment account for the changes in deaths. (Further, it would require a comparison of cohorts who started at different times to examine this issue, since there are competing risks within the same cohort.) It seems more likely that the distribution of causes of death changes because those who die of CV or infectious causes die earlier.

11. Minor: spelling throughout should be checked (Mantle and Kaplin instead of Mantel and Kaplan, for example). There are also a few sentence fragments.

**Quality of written English:** Needs some language corrections before being published