Reviewer’s report

Title: Postdialysis Blood Pressure Rise Predicts Long-term Outcomes in Chronic Hemodialysis Patients: A Four-Year Prospective Observational Cohort Study

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Reviewer: Nicholas Selby

Reviewer’s report:

Thank you for asking me to review the manuscript ‘Postdialysis Blood Pressure Rise Predicts Long-term Outcomes in Chronic Hemodialysis Patients: A Four-Year Prospective Observational Cohort Study’. In a group of prevalent HD patients, the authors describe post dialysis BP rise and link this to outcomes. This is a relatively understudied area and for this reason the study has relevance. There remains a lack of a uniform way of defining ‘post dialysis hypertension’ and this is further compounded due the marked intra-person variability between dialysis sessions. The authors attempt to deal with this by taking a mean of 25 dialysis treatments per patient. Although understandable, this reduces the clinical applicability of their approach. Furthermore, the individual variation that is masked by taking an average may itself be a clinical sign (e.g. do those with greater variability with fluctuations between BP fall and BP rise post HD fare worse?). I have the following additional comments:

Major comments

1. There is no attempt to ensure that patients are at their dry weight at study entry, either by clinical examination or methods such as bioimpedance/ICV diameter/LA volume. Clearly, if a proportion of patients are chronically above their ideal dry weight this has the potential to introduce significant bias. Following on from this, relying solely on cardiothoracic ratio as determined from CXR is a relatively crude way of assessing volume overload.

2. The authors select a cut-off in change of BP of only 5mmHg from pre to post dialysis. I would question whether a single cut off at such a small threshold is clinically relevant. This may be one explanation as to why the authors report no association between fall in BP post dialysis with worse outcomes, an observation at odds to other published studies, in that patients with large post dialysis BP falls are grouped with those with relatively stable BP. Equally this argument applies to those with post dialysis rise in BP, in that there is no differentiation between those with small and large changes and whether this makes any difference to outcomes.

3. Following on from this point, it would help interpret the results if there were more data regarding BP patterns i.e. what proportion of patients had a BP rise in the majority of treatments versus those with fewer but larger rises, and the proportion of those in the ‘no change’ group that had stable BP versus those with
both BP rises and falls that cancelled each other out.

4. There are some issues around imbalance between the patients in the different groups; there is an excess of age and malignancy in the group with a BP rise that could be confounding factors that explain the higher rates of mortality in these patients. In addition, the group with fall in BP had significantly less cardiovascular disease.

5. Can the authors explain the apparent discrepancy in that the group with BP rise had smallest UF volumes and an association with mortality; elsewhere the authors report that high UF volumes (as would be expected) have an independent association with mortality.

Minor comments

1. In figure 1 (histogram) BP increments are at 10mmHg intervals whereas the study methodology has a 5mmHg cut-off.

2. Page 4, para 2; the second half of this paragraph is methodology that should not be in the introduction.

3. There is a lack of clarity in the description of statistics section; please state clearly the primary endpoint used for Cox proportional hazards model.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests