Reviewer's report

Title: Sodium bicarbonate-based hydration prevents contrast-induced nephropathy: A meta-analysis

Version: 1 Date: 15 February 2009

Reviewer: Richard Solomon

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Meta-analyses of bicarbonate therapy for prevention of CIN

The manuscript by Meier et al is a superb example of meta-analyses and its ability to explore reasons for divergent results among clinical trials. The authors have used an appropriate search and selection strategy and include unpublished data from abstracts and major national specialty meetings. Their initial analysis found bicarbonate therapy to be efficacious for prevention of contrast-induced nephropathy (CIN) compared to sodium chloride. These results were not confounded by use of additional prophylaxis with N-acetylcysteine. The authors then explored in a variety of additional analyses the consistency of the odds ratio by deleting each study and repeating the analyses, looking at the impact of type of contrast agent and type of procedure (elective versus urgent), and the average volume of contrast media used in the studies.

1. Is the question posed by the authors new and well defined? Other meta-analysis have been published but none have explored potential confounders with the diligence of this paper.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work? The methods are appropriate. Some further explanations (see below) would be welcome.

3. Are the data sound and well controlled? Not relevant

4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Not relevant.

5. Are the discussion and conclusions well balanced and adequately supported by the data? Yes

6. Do the title and abstract accurately convey what has been found? Yes

7. Is the writing acceptable? Yes

Discretionary revisions:

A table listing each study with the number of patients, baseline creatinine and eGFR, specific contrast agent used, definition of CIN used and the crude CIN incidence for each arm would enhance the value of the paper as an excellent resource on this issue (your Table 1).

The importance of study size is not clear from the analysis. See comments below. As a reference, the authors might review the N-acetylcysteine
meta-analyses by Gonzales et al in BMC Med 2008 and consider a figure to demonstrate the impact of study size.

The weakness of study level data is that there is a loss of power to explore confounders and explain heterogeneity. Patient level data is available for most of these trials by contacting the investigators. I would encourage the authors to follow-up this analysis with one done using patient level data.

Minor Revisions:
Page 8, 2nd paragraph: delete ‘benefit’ after the word NAHCO3.
Page 9, 2nd paragraph: Please provide an explanation for how the specific tests suggesting heterogeneity based upon study size should be interpreted (for the non-statistician). A figure, similar to that in the Gonzales meta-analysis, may be helpful.
Figures: Throughout the figures, the REMEDIAL study is referred to as REMEDIA.
Page 23, Figure 8 legend: “This Forest plot of odds ratios . .” (not “rations”).
Page 22, Figure 2 legend and Figure 2: For consistency, I would indicate that the studies are stratified by year of presentation/publication.
Figure 4: In the iso-osmolar section, the last study and the summary symbols have been reversed.
Figure 5: It looks like the slope is highly influenced by the one study at the far right with the most contrast volume. What is the impact of deleting this study on the slope?

Which journal?: Not appropriate for BMC Medicine: an article whose findings are important to those with closely related interests and more suited to BMC Cardiovascular Disorders

What next?: Offer publication in BMC Cardiovascular Disorders after discretionary revisions

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests.