

## ***Interactive comment on “Observation- and Model-Based Estimates of Particulate Dry Nitrogen Deposition to the Oceans” by Alex R. Baker et al.***

**Anonymous Referee #1**

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This manuscript is relevant for publication in Atmospheric Chemistry and Physics. The authors compile extensive datasets of ocean based observations of nitrate and ammonium concentrations from aerosols. They then compare and contrast observed aerosol concentrations and calculated dry deposition fluxes to a global model (TM4) and the mean product from a global model intercomparison project (ACCMIP). There are limitations, both in how the data are treated and in the model–observation comparisons, making it difficult to draw new conclusions beyond what is essentially already known. Still this represents an important compilation of data, an important recognition of the state of the field and recommendations on needs for significant improvements to advance this science. The work is well cited and includes a table in the supplement of all

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data used in the compilation, which is also available on a public database via the SOLAS website. Below I outline areas of the manuscript that could be improved, however, overall I recommend publication of the manuscript with minor revisions. The primary weaknesses include: a lack of discussion of the influence of meteorology on results; a need for a more representative abstract; and a need for clearer recommendations to the community to improve and make advancements. Finally, this manuscript is an important review of the state of this field, and as such should address a few basics that will be important to the broad readership of AC&P.

**Abstract:** The abstract could better represent the findings in the work. The last sentence of paragraph 1 (line 37-38) should mention how/why mineral dust alters deposition of N (thinking of a broader audience). The second paragraph should include some type of quantitative summary of the results. It should be addressed here why the focus here is on comparison with TM4 (which is justifiable but why mention ACCMIP if not actually discussing the comparison here?). Indeed, the TM4 model over-estimates NO<sub>3</sub> and underestimates NH<sub>4</sub><sup>+</sup> - however, the comparison with NH<sub>4</sub><sup>+</sup> is much better in all of the ocean basins. Can this be quantified/summarized more concretely here? Also missing from the abstract is concrete suggestions or recommendations. The abstract makes it appear that little is concluded in this study beyond the clear limitations of our understanding of dry deposition velocities. What's needed to really address this or are there specific things the community should at least be worried about addressing in the near future? In other words, the abstract should address this a bit more to be more representative and garner community attention. At the very least, the clear recommendation that measured aerosol concentrations be reported for observation and models and that this be the key comparison that is made rather than dry deposition fluxes is an important point that belongs in the abstract.

Section 2.2: Include units in defining the variables for Equation (1).

Section 2.3 For each model description, it should be mentioned what is considered surface level in each model – i.e. what vertical resolution is the model output averaged

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over?

Lines 141-142: replace “sigma” with 1 sigma or 1 std dev or similar

Line 152+ : It is not discussed whether ACCMIP includes a flux of NH<sub>3</sub> from the ocean similar to TM4 and how much is emitted on an annual basis.

Is it possible to report what percentage of NO<sub>y</sub> is NO<sub>3</sub>- in the ACCMIP products? In the comparisons this is brought up as a reason for disagreement between model and obs, but it would be interesting to note whether NO<sub>3</sub>- is a majority of the NO<sub>y</sub> deposition or not.

Discussion:

It would be useful to include some discussion on the influence of meteorology. ACCMIP output is based on average met fields for 2000-2009. TM4 uses ECMWF. The calculated deposition for variable vd uses ECMWF wind speeds. But clearly, amongst all products and calculations, there is an important role of meteorology in determining deposition. Perhaps the variable vd could be calculated with another, different year of meteorology to give an estimate of how uncertain the influence is of a model-data met product on the calculations? While analyzed met fields are useful, their utility over the open ocean where direct measurements are limited may be an issue. This should be better addressed here.

Section 4.4: It is mentioned that the representation of mineral dust in models is limited. Can this be discussed specifically in the context of TM4 and ACCMIP? Are there direct model-obs comparison related to this from other studies? Is there some estimation of how poorly this might be represented in these models specifically?

Section 4.5: There is great emphasis placed here and in the abstract and conclusions on the uncertainty associated with dry deposition velocities. How can new progress be made on this issue? Some type of recommendation, from the clear range of experts who appear here as co-authors, should be made. Also, it would be helpful to list the vd

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in Table 2 for both model products and that used to calculate the observed dry deposition here. From the text, it appears that modeled vd and assumed/calculated vd are not so vastly different, yet the discussion here (and in the abstract and conclusions) makes this appear as a vital issue. Does this suggest that we need to constrain velocities to within +/- 25% or more (or less)? More can be spoken to/digested here from this model-calculated data comparison.

Conclusions: Suggest renaming this to “Summary and Conclusions”

Line 469: See also above – suggest making some type of recommendations on actions that can be taken here to improve the state of the field. More studies in different places? Specific types of studies – i.e. laboratory vs field measurements? More passive sampling?

Table 2: Suggest reporting dry deposition velocities (vd) for the models and the calculations for each ocean basin in this Table.

Typos/minor edits: Line 121: “to” should be “for” Line 123: “associated” should be “association” Line 124: missing ‘with’ prior to “final aerosol fractions”

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