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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Atmospheric nitrogen deposition to the northwestern Pacific: seasonal variation and source attribution" *by* Y. H. Zhao et al.

Anonymous Referee #3

Received and published: 7 August 2015

This paper presents a calculation of the total nitrogen flux to the Yellow and South China Seas, and also determines contributions from various emission source categories to that flux. It appears to be a very thorough paper, and contributes to the further understanding of nitrogen deposition to oceans. It is well organized and well written and I recommend it be published in ACP, after addressing a few details below.

P 13659, L 1: I think you should be a bit more precise here, especially for the last line of your abstract, and which could have significant policy implications. Maybe change to "limiting the effectiveness of NH3 emission controls on reducing nitrogen deposition to the Yellow and South China Seas". At first glance it reads like reducing NH3 isn't

useful at all.

P 13660, L 10: If 40% enters ocean, does other 60% end up on land generally? (Ie, is this global?)

P 13663: I believe nighttime GEOS mixed layer depth in this version of GEOS-Chem had some problems. What do you do for mixed layer depth? Does it influence the results at all?

You mention NH3 from the oceans here and then later in text/figures, but for the meantime, it would be nice to get an idea of the magnitude of NH3 oceanic emissions when you are discussing N emissions from Asia. What fraction of the total natural NH3 is from the oceans? (Maybe I missed this.)

I don't believe GEOS-Chem has bidirectional exchange in the model, which may cause uncertainties in net flux for certain nitrogen species. Will this influence ocean estimates at all?

Satellite data: Can you be a bit more specific about what exactly you are trying to achieve with this satellite validation? A spatial validation of GEOS-Chem NOx emissions? Since NO2 has such a small deposition velocity, why care about NO2? There have been lots of OMI NO2 comparisons with models. Maybe list if there are DOMINO NO2 and GEOS-Chem papers already published, or at least DOMINO NO2 validation to show OMI is useful.

Have you used the OMI scattering weights (column averaging kernels) to compare with the model? Huijnen et al 2010 showed the kind of differences that ignoring these can cause in model comparisons (Huijnen et al. "Comparison of OMI NO2 tropospheric columns with an ensemble of global and European regional air quality models." Atmospheric Chemistry and Physics 10.7 (2010): 3273-3296.)

Why is OMI data not treated as TES data and matched to GEOS-Chem coincidence along track? Do you expect any kind of bias might result? What is the cloud filtering criterion for these data?

There's not much discussion of uncertainties in the paper. I'm mostly wondering about the adjoint. Is there a way to estimate uncertainties in these contribution estimates?

Minor comments:

P 13658, L 15: Change "downwind the Asian" to "downwind of the Asian"

P 13659, L 8: Remove word "But" (never start a formal sentence with but).

P 13659, L 24: You are not really addressing the issue (that's for policy makers). Change "address" to "study"

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 13657, 2015.