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Interactive comment on “Global impact of nitrate photolysis in sea-salt aerosol on NO_x, OH, and O_3 in the marine boundary layer” *by* Prasad Kasibhatla et al.

Anonymous Referee #2

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This is a very nicely written paper that addresses some open questions regarding the role of particle nitrate photolysis in ‘renoxification’ using the GEOS-Chem model and several observational datasets. The measurements from CVAO, in particular, have been used in previous recent studies to explore this topic, but only using a box model, and the 3D model used here has the advantage of not needing to prescribe the impacts of transport. The portion of the analysis that explores the impact of removing an alkalinity limit of the conversion of HNO3 to particle phase nitrate on sea spray aerosol is important and likely to be of general interest. The model is also able to provide valuable information about the spatial extent of the impact of this sea salt-associated nitrate photolysis.

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I recommend publication after the authors address the following comments: Page 3 Lines 25-30 The authors indicate that sea spray aerosol is generated/transported in two size fractions, and then go on to say that only the nitrate and sulfate associated with ‘coarse mode’ particles is designated as sea spray SO4S and NITS. Why not also explicitly track this sulfate and nitrate in the smaller particles? Is there some particular reason of is it related the history of the model? What is the consequence of excluding it for this analysis? This comes up again a couple of times and it might be useful to provide a bit more information here.

Page 4 Lines 29-32 What is the rationale for using JHNO3 to scale JNIT? Given that the rate of photolysis is so different, isn’t it also likely that the wavelength dependence is also different? In that case, the scaling factor is likely to change with solar zenith angle. Was this considered?

Page 6, Lines 21-29 Was any particle filter used upstream of the LOPAP? If not, is it possible that some of the signal attributed to gas phase HONO might actually be related to particle phase nitrite associated with SSA? An artefact like this could lead to a strong positive bias in the inferred gas phase HONO measurements, with important implications for the radical budgets. In particular, it could explain why a scaling factor of 25-50 is sufficient to reproduce the NOx measurements at CVAO, but not the ‘HONO’ measurements.

Section 3.3 and Figure 8 Does the absolute rate of NOx production from PAN decomposition also change when nitrate photolysis is included? How much of the change in the NOx production rate is via this indirect impact as opposed to the nitrate photolysis itself? This may be a minor point, but would be interesting to know.

Figure 10 and last paragraph of section 3.3 – would this conclusion of the limited spatial extent of the impacts still be true if the nitrate in the accumulation mode SSA had been considered? It might be worth a sentence or two addressing this here. This is a slightly different question than what is being addressed in Section 3.4.

Section 3.4 The caption for Figure 11 and the associated text are very confusing. What do the asterisks denote – the inclusion of the photolysis scaling factor for all nitrate? That's not clear at all. I can understand why the authors are keen to extend the modelling exercise to probe the impact on nitrate photolysis more generally, but given the strongly non-linear results and the coarse model resolution, to me this section detracts from the rest of the manuscript rather than adding to it.

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