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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimation of Reactive Inorganic Iodine Fluxes in the Indian and Southern Ocean Marine Boundary Layer" *by* Swaleha Inamdar et al.

Anonymous Referee #3

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The paper by S. Inamdar is using a large data set of seawater iodide, atmospheric ozone and atmospheric IO concentrations to test the reactive inorganic iodine fluxes calculated from different parameterisations of seawater iodide,. The authors propose new parameterisations of seawater iodide that are specific for given regions of the global ocean, and compared to already established parameterisation for the global ocean. They find that the parameterisation used has little impact on the computed atmospheric IO concentrations. Observed IO concentrations cannot be adequately computed using inorganic iodine fluxes and chemistry. As IO is correlated to Chl-a, the authors suggest a biogenic impact on iodine in the region investigated. The paper is well and clearly written and organized. Iodine fluxes, chemistry and impacts on the

atmospheric composition are poorly understood and this study brings a nice input into our understanding. I suggest the paper is published after only minor comments (below) are taken into account.

Minor comments

Section 2.1 iodide parameterisations

Lines 201 to 218 : the argumentation on the need to have regional parameterizations should go in the introduction ?

Line 226 : would be nice to recall why sea surface nitrate concentrations were chosen as a parameter influencing iodide concentrations

Section 2.2 ozone measurements

Contaminations on a ship may occur from other sources than the ship's smokestack (such as cooking exhausts, or air conditioning exhausts). Were there any indicator of anthropogenic compounds concentrations available to exclude contaminations?

3.Results

3.2 Iodide line 432-433: the end of the sentence is not clear, please reformulate 3.3 Iodine fluxes line 491: premature to mention discrepancies between modelled and measured IO in this section? Would better fit in the discussion section

4. Discussion

line 712: concerning the lack of correlation with satellite base Chl-a while in situ Chl-a concentrations are correlated to observed IO concentrations. May this be due to geographical differences in what biological species Chl-a represent in these different regions, or may be due to uncertainties in the Chl-a retrieval from satellite, or even also scaling problems. Did the authors try to extract satellite Chl-a where the actual Chl-a in situ measurements were performed to compare one with the other?

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