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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A negative feedback between anthropogenic ozone pollution and enhanced ocean emissions of iodine" *by* C. Prados-Roman et al.

Anonymous Referee #1

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Prados-Roman et al. pesent an interesting progression of the topic on inorganic iodine emissions driven by the reaction between iodide and ozone at the sea surface. They attractively illustrate that sea-surface reactions between ozone and iodide may have resulted in substantial changes in the destruction of tropospheric ozone since pre-industrial times and that inorganic iodine flux to the atmosphere has also altered considerably. This modeling exercise is concisely and clearly presented and the important points are generally well argued. My main concern with the paper is the limited acknowledgement of the levels of uncertainty inherent in the model and interpretation of it's output.

Specific points:

1. The manuscript would benefit from a more comprehensive assessment of the uncertainty in their interpretation in several areas, moreover, this uncertainty should be expressed in the values presented for increased ISG emissions, increased rates of ozone loss and the overall impact on radiative forcing. MacDonald et al. 2014 carried out some sensitivity analyses of their model parameterizations and a similar assessment of the robustness of the results of this model is needed. Amongst this uncertainty are: 1.1 The relationship between iodide and sea-surface temperature used in Equation 4. While the recent papers by Chance et al. 2014 and Macdonald et al. 2014 found temperature to provide the best predictions of iodide concentration on a global scale, temperature explained at best, only 50 % of the variability in observed iodide. The authors acknowledge this to some extent (P21921, L 17+). It would be useful to understand how variability of this magnitude would alter model predictions of ISG emission, etc and the authors' conclusions. 1.2. The level of understanding of the reaction kinetics between O3 and I- and emission of I2 and HOI is based on a limited number of laboratory experiments, very few of which have been carried out at anything like in situ concentrations or in 'real' seawater. It is very ambitious to extrapolate these findings to the global scale and at the very least some acknowledgement of this uncertainty should be made in the conclusions. 1.3. MacDonald et al. 2014, acknowledged the high sensitivity of their model output to windspeed. How does uncertainty in the wind speed fields used in the present study impact the results? For instance, at low wind speeds (<0.3 m s-1) ISG emissions may be substantially overestimated.

2. In presenting the % change of the ISG fluxes since pre-industrial times (Figure 4), in the Iy budget (Figure 5) or percentage acceleration of the ozone chemical loss (Figure 7) the equation 100 x (PD-PI)/PD is used. It would be more logical in my view to use 100 x (PD-PI)/PI to express this percentage change when referring to change since the pre-industrial situation.

3. L8, I would suggest altering 'laboratory studies have established the oceanic

gaseous emission of. . ..' to 'laboratory studies have demonstrated the potential for. . .' ; or something along those lines.

4. P21922, L17. This point is not entirely fair. This single factor might lower the estimates of ISG fluxes but the overall results comprise many other levels of uncertainty that could shift the balance between under or overestimation.

5. P21925, L3.3 I suggest altering the title to 'Iodine-mediated change in ozone radiative forcing. . ..'

6. P21926, L25+. As stated above, a comprehensive explanation of the model uncertainties is needed in order to demonstrate how robust these conclusions are.

7. P21926, L25+. The sentence beginning 'Note that. . ..' could usefully be rephrased.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 21917, 2014.