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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A laboratory characterisation of inorganic iodine emissions from the sea surface: dependence on oceanic variables and parameterisation for global modelling" *by* S. M. MacDonald et al.

Anonymous Referee #2

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This paper presents an interesting set of experiments on the mechanism of iodine release from the ocean. It is a problem that has been quite debated in the past few years and is not resolved yet, but has important potential implications for ozone photochemistry. The experiments presented here shed new light on this process and therefore I think the paper should be published in ACP, following some minor corrections and clarifications.

General Comments

A key issue is the reproducibility of the experiments and the reliability of the observations. The authors acknowledge poor reproducibility (page 31451) and ∼50% error in their determinations of the iodine fluxes. It is noted that iodine is detected using an indirect method (the accuracy of which should be discussed in section 2). As a consequence, the parametrizations derived from these experiments likely suffer from rather large uncertainties, which should be addressed in section 4.5 together with the sensitivity analysis. I think that a longer discussion of the uncertainties involved in the experiments and the derived parametrizations is warranted. It might in fact help to explain the model-measurement discrepancies.

If the ozone+iodide mechanism proposed in this paper does indeed occur everywhere on the ocean surface, it seems to me it should generate significant levels of I2 during the night. These should be detectable especially in unpolluted and remote marine environments where concentrations of NO3 are likely too small. However, I2 was not reported in the Mahajan et al., 2012 or in the Grossman et al., 2013 papers. Can you comment on how to reconcile the mechanism with the apparent lack of I2 observations?

Various previous studies in different oceanic regions (eg, Jones et al. 2010, Mahajan et al. 2010, Grossman et al. 2013) have estimated that an I2 flux may be required to match the IO observations in addition to measured iodocarbons fluxes. How do the fluxes reported in those papers compare to those calculated with the parametrization presented here? And can the salinity, organics and temperature dependences explain the differences in the I2 fluxes estimated for different regions - as highlighted in the previous studies? A brief comment on these points could be an interesting addition to the paper.

Specific & Technical Comments

In section 3.2 is not clear whether you tried to reproduce the Hayase et al. experiments with fulvic acid and/or the Reeser/Donaldson experiments with octanol.

I don't understand whether ozone was measured or not during the cruise. It would

appear it was not from section 4.4, but figure 6 seems to suggest it was. Can you please clarify.

On page 31457 (line 10) change "ws" to "wind speed (ws)"

On page 31461 explain the acronym SSS

Please add the parameters of the linear fits to figures 3 and 7

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 31445, 2013.