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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 #1

Received and published: 28 January 2014

This manuscript is concerned, in general terms, with trying to understand and model the observed iodo emissions from ocean surfaces. As such it is interesting, and certainly takes an ambitious stance. However, as written, it is two quite different papers, put together into a single MS. The two - a laboratory exploration of the influences exerted by various parameters on the emission, and a modelling study, using a new parameterization of HOI and I₂ emissions - are quite unconnected in almost every way. I suggest they be disentangled, and published separately.

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Some comments I can make on the MS follow.

Page 31448, lines 20-21: I think you must assume the I₂ is at its equilibrium vapour pressure here. This should be made explicit, as well as any checks done to ensure this was so.

Page 31449, lines 17-19: What absorption coefficient(s) andwavelengths were used to extract the [HA] from the absorbance?

Page 31450, lines 3-8: Please either show the trace impurity result, or state quantitatively how much I₂ was associated with the impurity in the NaCl.

line 21: the monolayer-forming conc for SDS seems very high.

Page 31451: How did the results of experiments done with the different photolysis sources compare? What was the advantage of having the 3 sources?

Page 31452, line 20-21: I think it was Tobias and Jungwirth who should be credited here.

Page 31454, lines 20-25: I do not follow how a lack of T-dependence gives rise to an activation energy, as proposed here.

Page 31456, line 1 AND Fig 5: Could there be 2 different dependences here?

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

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13, C11597–C11598,
2014

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