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Interactive Comment

Interactive comment on "Observations of I_2 at a remote marine site" *by* M. J. Lawler et al.

Anonymous Referee #1

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This paper ("Observations of I2 at a remote marine site" by Lawler et al.) presents the first observations of I2 in clean marine air not affected by coastal algal emissions. It is a subject fit for publication in ACP. The questions related to the presence of iodine in the open ocean (and in marine air in general) have received a lot of attention by atmospheric scientists in recent years; this paper adds a very significant contribution and new data to the debate. Therefore, I recommend publication, after the authors have addressed a few issues.

GENERAL COMMENTS

The authors state, correctly in my view, that the open ocean plankton may emit I2, although the emission rate is unknown. Could this be an explanation for some of the model-measurement discrepancies, without invoking the O3 + I- chemistry? Can the contribution of plankton emissions in the open ocean be ruled out? The authors should

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Interactive Discussion

Discussion Paper

comment on this, as they draw the conclusions of their study.

I think a more detailed "review" of the various mechanisms proposed over the past few years to explain the release of iodine from the sea-surface should be added on p. 25913. Some these mechanism propose the release of organoiodide compounds, as well as I2, and the implications of this for the conclusions of this study should be discussed.

Regarding the macroalgal emissions, the authors state that they collected some samples near the site, but no I2 emissions were observed from them. Were the authors able to identify the algae species? Were they different from those collected at other sites, such as Mace Head or Roscoff? This could explain why they did not emit I2, maybe.

How do the fluxes assumed in the FLAT and PHOTO model simulations to match the observations compare to those assumed by other studies (eg, Jones et al., 2010, Mahajan et al., 2010, Carpenter et al., 2013, Grossman et al., 2013)? Maybe the fluxes from other studies could be added to figure 7 for easy comparison?

My major concern with the paper is about the daytime I2 concentrations. The authors conclude in section 4.3 that these are likely artifacts. The reasoning is well explained and convincing, but I think that the way in which the data are presented in the abstract, on p. 25918-25919 and in figures 2, 3, 4 gives the reader the false impression that these concentrations might be real. I suggest the authors rephrase the abstract and the text on those pages and modify the figures to make it clearer that the daytime data are likely spurious. It is also confusing that the discussion of the model results (section 5 and figure 6) apparently attempts to explain the I2 daytime observations. If they are indeed artifacts I don't think the lack of agreement with the model is an issue and therefore both the FLAT and the PHOTO models can be said to explain the measurements rather well (considering the variability in IO showed in the Mahajan et al, 2010 paper). If, instead, the authors believe they may be real then they should be more explicit about it and revise the text in section 4.3.

The authors make an assumption that there are no atmospheric losses during the night (p. 25920): this assumption requires a justification. The authors should also explain why they think I2 emissions are high during the first part of the night and negligible during the second part of the night (as stated on p. 25920).

SPECIFIC COMMENTS

p. 25916: have the data been filtered for possible influences from the diesel generator?

p. 25916: did you notice any significant difference wrt design, position and length of the inlet in the two studies?

p. 25919, l. 11: it seems to me that in 2009 the concentrations were higher.

p. 25920, l. 11: is 1000 m a reasonable assumption at Cape Verde?

fig. 4: can you show also the 2009 campaign?

fig. 6: a comment on the different shapes of calculated IO seems warranted.

TECHNICAL CORRECTIONS

p. 25913, l. 14: check the syntax of this sentence. l. 15: add a reference p. 25914: subscript Ix and IOx p. 25915: I think Jones et al., used a 1D model p. 25924, l. 8: correct "modification"

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25911, 2013.

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13, C9293–C9295, 2013

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