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Atmospheric Chemistry and Physics Discussions

Interactive comment on "*Fucus* and *Ascophyllum* seaweeds are significant contributors to coastal iodine emissions" *by* R.-J. Huang et al.

Anonymous Referee #1

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General comments:

Huang et al. present evidence in their study, that Fucus und Ascophyllum seaweeds are much stronger emitters of iodine than previously thought. This is an important finding since so far Laminaria has been considered the only significant contributor to coastal iodine emissions. Laminaria might still be the most important contributor, but the authors show that the other species contribute similar high amounts of I2 when exposed to ambient air for longer periods of time. In future studies it will be important to investigate the biology and the release processes of these species in more detail, e.g. like Kuepper et al, 2008 did for Laminaria.

This paper provides important new results and I therefore recommend its publication with some modifications/revisions.

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Interactive Discussion
Discussion Paper

I am not sure if ACP is the best platform for such a biological paper, although it has implications for atmospheric chemistry and papers of this type have been published before in ACP. BG might be the better option, but that is something the editor has to decide.

How are your values of I2 to be judged in the context of other observations, e.g. of IO, and how do they, or could they contribute to model simulations of the iodine chemistry? Accordingly you should also discuss the recent publication of Commane et al., 2011 in the introduction. Have simultaneous measurements of IO been taken?

A thorough error analysis is missing in this paper – see also comments below.

Specific comments:

Page 25916, line 6: What are these values? Means? See comment below on Table 1.

Page 25918, line 2-4: What are you indicating with the 3 arrows?

Page 25918, line 24: Please explain FW – for a non-biologist this is not immediately clear.

Page 25919, line 19: Please explain spp. - for a non-biologist this is not immediately clear.

Page 25920, line 3 et seqq.: Why did you pick these sites? The reasoning/logic in this paragraph is not clear.

Page 25920, line 17 et seqq. and throughout section 2. Please list and explain your sources of error (precision and accuracy). All values given in the text and in the figures should have an error/show error bars (Fig. 2, Fig 3, Fig 5) – see also comments on Table 1.

Page 25921, line 20 et seqq.: Are there any other stress factors for the plants? Can they be ruled out?

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Page 25924, line 26: apple, bump, mixed? I guess these distributions are not standard terms and can not be understood without knowing the Vana et al. publication.

Page 25925, line 17/18: Why not naming the number of the site according to Fig.1?

Page 25925, line 28: Is one of these days the same day as for the measurements at this site? It is not clear from the text.

Page 25925, line 18 et seqq.: You write that: A lower mixing ratio was observed at the beginning of ebbing tide when the macroalgae were just exposed to the ambient air.

Assuming a photolytical lifetime of I2 of 5s and taking your wind speed measurements of up to 11m/s, then one can speculate that the area from which you are gathering data might also change with ebbing tide, which on the other hand might partially be the reason for the increase of I2 over time. None of the locations looks like it is getting air from the open ocean. It's not clear from the information given in the paper. A more thorough description is necessary. Somehow partial information is scattered around the paper, but not presented concisely at the places where needed. A plot showing, Time(UT), water level (and maybe resulting area of algae exposed to air), solar radiation and mixing ratios for each observation would be helpful.

Page 25934, Table 1: Remove the details given in the Location column, e.g., 150m away from MRI-Carna etc. I guess that is useful information only for people actually familiar with that site, but not for the general audience. In column 2, I2 (ppt), of that table it is not clear what you mean with the plus/minus values. Are these numbers the standard deviation? This would only be a useful quantity, if your samples were taken simultaneous under the exactly same conditions. Or a range?, which would not make sense, since it would be pure coincidence if the highest and lowest value would spread symmetrically around the mean. Or are these values the error of your measurement technique (precision and accuracy)? It's not clear from the text and the table – see also comment on errors.

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Are all these measurements taken during daytime, or also during nighttime? I guess that during daytime I2 is photolysed fast enough, so that the area from which you are gathering data is localized and rather small, but during nighttime that would be different and transport processes could play a much more important role.

Page 25935, Figure 1: Use higher resolution and different colours for the numbering and stations (red). Add a scale, so one can estimate distances. Acknowledge the source of the graph.

Page 25937, Figure 3: Why are the fit equation and the fit curve shown in Fig3 and not discussed anywhere? What does it tell us? Is it important? If yes, then please discuss in the text. If no, then remove.

Page 25939, Figure 4: Why does the time series stop after 1 hour?

Technical comments:

Page 25915: Unify the details given in the affiliations, e.g. some have postcode, others don't.

Page 25917, line 25: The Carpenter, 2003 reference is not listed in the back – only Carpenter, 2001.

Page 25918, line 7: Saiz-Lopez et al, 2006 – 2006a or 2006b ?

Page 25918, line 19: Kundel et al., 2012 – 2012a, or 2012b ?

Page 25926, line 18: Variable emissions between plants HAVE been found . . .

Page 25931, line 22: Kundel et al., 2012b is not used in the text. In this case remove the 'a' in the preceding reference.

References:

R.Commane, K.Seitz, C.S.E.Bale, W.J.Bloss, J.Buxmann, T.Ingham, U.Platt, D.Pöhler, and D.E.Heard, Iodinemonoxide at a clean marine coastal site: observations of

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high frequency variations and inhomogeneous distributions: Atmos.Chem.Phys.,11, 6721-6733, 2011, www.atmos-chem-phys.net/11/6721/2011/doi:10.5194/acp-11-6721-2011.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25915, 2012.