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Comment

## ***Interactive comment on “In situ measurements of molecular iodine in the marine boundary layer: the link to macroalgae and the implications for O<sub>3</sub>, IO, OIO and NO<sub>x</sub>” by R.-J. Huang et al.***

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This paper presents unique in situ measurements of I<sub>2</sub> over coastal algal beds. This is an important addition to the study of coastal iodine emissions, which has heretofore largely been dominated by long path spectroscopic measurements. The paper presents two very interesting observations – 1) the positive correlation between O<sub>3</sub> and I<sub>2</sub> near the sea surface in the presence of macroalgae, which is presumably reflecting the source mechanism for I<sub>2</sub>, and 2) the negative correlation of O<sub>3</sub> and I<sub>2</sub> away from the surface, which presumably reflects catalytic ozone destruction related to iodine chemistry. I think this new data is definitely worth publishing, and I think the

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interpretations offered are generally reasonable. However, there are several issues which should be addressed prior to publication.

Some specific comments:

General comment - I am a bit at a loss to understand the meteorological situation here or what the authors envision as the air flow field. Is the studied being carried out in a two-dimensional flow field? Is there evidence to support this? Is the air flow onshore all the time (no seabreeze)? Is the ozone loss reasonable in view of the short transit time between the kelp and the ozone sensor? How much time is there? I'm pretty confused here as to whether the same air is being measured by the various techniques and what the temporal relationship is between them. Defining the meteorological framework is essential to the conclusions of the study.

Section 2.1 beginning - Huang and Hoffmann, 2009 needs to be cited here as a place for the reader to find out how/why this technique works, and to see how it has been validated. Detection limits need to be explicitly stated and justified, if only with reference to earlier paper. Some actual field evidence of this would greatly strengthen the paper.

Section 3.1 P6 line 2 "expanding air mass" is unclear. Does this mean that emissions are wind speed dependent? I did not understand the point of this statement.

p6 sentence beginning "Ozone destruction is of utmost concern..." An artificial argument is posited here, between stratospheric and tropospheric halogen chemistry. There really is no contradiction, and no need for "Nevertheless...". This paragraph could just start with "Recent models..."

p6 14 lines from bottom: I think I would say that halogen-mediated ozone destruction was "suggested" not "observed" by Read et al.

p7 line 13 - I believe there have been other field measurements supporting the existence of this reaction. My recollection is that Saez-Lopez and co-workers observed IO and NO<sub>3</sub> at night and derived a rate constant for this reaction. Perhaps in their

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p7 3 lines from bottom: “Exemplary” is not the right word here. I am not sure exactly what you mean.

p9 line 3-4. This sentence does not make sense and should be rewritten. Measurements were made during daytime, but so what? What are potential implications and how do they differ from regular implications? Also, “concentration levels” is redundant. Just levels is fine.

Figure 4. axis label and caption. "ppt" is a unitless measure of mixing ratio, not a concentration.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 361, 2010.

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