

Interactive comment on “Iodine monoxide at a clean marine coastal site: observations of high frequency variations and inhomogeneous distributions” by R. Commane et al.

Anonymous Referee #2

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Review of “Iodine monoxide at a clean marine coastal site: observations of high frequency variations and inhomogeneous distributions”

By Commane et al.

This manuscript presents a set of iodine oxide measurements at Mace Head, which contributes to the rich dataset of iodine oxides available from this location. It further supports the presence of a large inhomogeneity in iodine near the intertidal region, which has been hypothesized and observed before, although not using an in situ measurement technique for IO.

The paper presents the results from point LIF observations, which are then compared
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to path averaged LP-DOAS observations, to arrive at conclusions about spatial inhomogeneity in the IO mixing ratios, which support previous modeling studies.

I feel that the paper is suitable for publication after some changes, which have been detailed below.

Major changes:

1) The paper includes a highly detailed discussion on the LIF instrument, which is not necessary. The measurement technique, albeit another version of the particular instrument used, has been used in the past and hence perhaps need not be discussed in such detail in the paper.

2) On the other hand, the title of the paper suggests a detailed discussion on the observations and more information about the IO distribution, which has not been dealt in detail in the paper. I would suggest adding more information about the IO inhomogeneity, which is discussed in only one paragraph. There is work done in the past suggesting and even showing the inhomogeneity in iodine compounds around Mace Head (e.g. Saiz-Lopez, 2006, Seitz et al., 2010). These should be discussed in further detail in the paper. The present manuscript does not offer many improvements on the past work, with the only novel part being point observations of IO to confirm the inhomogeneity in the iodine spatial distribution, which has been observed by other instruments in the past (e.g. for I₂). Maybe rather than a simple schematic representation of the distribution of macroalgae in the intertidal zone, the authors can improve on the past estimates by using bathymetry data, which should be available or macro-algae distribution maps if available. This will also help shed more light on the inhomogeneity and source strengths of the different regions.

3) Why are the high-frequency variations not discussed? The only effort to discuss these is on page 4544, but can be further expanded upon.

4) The introduction and the whole paper in general may need some more work and

the right references should be cited. For example, the formation pathway of iodine oxides was not suggested by Kaltsoyannis and Plane, 2008, they only do quantum calculations on various iodine compounds; or, the emission of I₂, and its role in IO concentrations, was not first observed by McFiggans et al 2004, but by Saiz-Lopez and Plane, 2004. The latest work in iodine compounds observations in the field from other locations like Roscoff, California and Galicia should be properly discussed.

An inclusion of a table with past observations of iodine compounds at Mace Head is also recommended to put the new point measurements in context.

Minor comments:

- Page 4534 Line 26: Include more references for simultaneous observations of iodine compounds and particle formation. - Page 4535 Line 5: The exact pathway of iodine particle formation is still under discussion. Mention this and provide further references to other studies. - R1 and R2 are not necessary. If the authors want to show a simple iodine chemistry scheme, I suggest a figure or more reactions rather than leaving it incomplete. - Page 4535, line 17: Should be (MHARS) - Page 4535 lines 13-15: Bale et al measured only at Mace Head. If the authors want to generalize this to 'coastal areas', include other I atom observations, eg. Mahajan et al, 2001 (ACP) or modeling studies estimating I atoms from Roscoff. - Page 4535, line 228: Insert references for LP-DOAS observations. - Page 4536: The authors define Mace Head as a clean marine site. Can they provide more details to substantiate this claim? For e.g. Heard et al, 2006 (ACP) mention that there were periods of clean marine air and polluted continental air observed at Mace Head. Were there NO_x or CO observations to show that the air was 'clean' over the campaign? - Page 4537 line 8: What dataset was used to calculate the back trajectories? - Page 4544, line 11: Kaltsoyannis and Plane did not suggest that growth of particles proceeds through higher oxides and nor were Furneaux et al. the first to observe IO for the first time with IOPs. - Page 4544, line 16-20: Are the authors confident on the 1 s data variability? If the variability is real, it needs much more discussion. This would shed more light on the distribution of the

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sources. - If the authors suggest that the highest mixing ratio was 49 pptv and are confident about the 1 s data, why are they using the 60 s data for the figures? If there is good data available, please show it. It may be confusing for the reader with half the paper referring to 49 pptv as the highest, but the other half referring to 29 pptv. - Page 4546 lines 3-5: What do the authors mean that determining the relationship between IO and particles was difficult at Roscoff? It has been characterized in detail in the past (McFiggans et al., 2010, Leight et al., 2009). If they are suggesting that it is in fact easier at Mace Head, why is it not done here? - Page 4546, line 8. McFiggans et al did not measure I₂. The observations were made by Saiz-Lopez and Plane, 2004 - Page 4546, lines 10-18: Discuss in more detail. Dixneuf et al., 2009 (ACP) have carried out studies on emission of time dependence of I₂ from different species. - Page 4546, line 25: Please quantify. - Page 4547, line 8: Reference. - Page 4547, line 14: Observations were not made by Saiz-Lopez et al 2006, but by Saiz-Lopez and Plane, 2004. - Page 4548 line 15: The light is diffracted by the spectrometer as opposed to analyzed. - Page 4548: The IO cross-section is reported by Gomez-Martin et al., 2005, which was a companion paper and not Spietz et al., 2005. - Why was I₂ not measured with LP-DOAS during the campaign? - Page 4551 line 5: Roscoff is not a polluted site according to the EPA standards. - Page 4551, line 23: What about the O₃ loss rate at 50 ppt? Have the authors done any calculations to estimate the O₃ loss or do they just use the McFiggans estimates from 2004? - Page 4552 line4: Could this be a reason for the reduced LIF/LPDOAS ratio rather than an increased inhomogeneity? Bathymetry maps or macroalgae distribution will show if this was the case.

Figures:

Figure 1: The authors may would like to consider replacing this figure since it is a very rough approximation. Maybe a google map will show more details on the macroalgae distribution.?

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