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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurements of iodine monoxide at a semi polluted coastal location" *by* K. L. Furneaux et al.

Anonymous Referee #2

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The manuscript describes LIF measurements of IO made during the RHaMBLe campaign performed near Roscoff at the NW coast of France in the fall of 2006 (other RHaMBLe data from the Roscoff campaign were published by e.g. McFiggans et al. 2009, Leigh et al. 2009, and Mahajan et al. 2009, Whitehead et al. 2009) and thus presents an important data set. LIF is a relatively new, very sensitive technique for IO measurement and is shown to be extremely useful for the purpose. Overall the manuscript presents no fundamentally new conclusions from the data, however there are many interesting findings supporting earlier observations to warrant publication in ACP. In detail I have reservations with several of the conclusions and should like to recommend changes as detailled below (A-D). In addition there are nummerous minor errors or unclear points, which are summarised in a separate list at the end of this report (1-15).

A) Nighttime IO: From the stated measurement errors of the LIF instrument, and even more so from the histograms shown in figure 11, it is unlikely that the nighttime IO observations are actually significantly different from zero. For instance the histograms in fig. 11, though showing mean online IO levels around +1 ppt, exhibit several ppt wide probability distributions, thus the deviation of the mean from zero is not significant. Accordingly the discussion of nighttime IO sources should be removed from the manuscript (it is a repetition of arguments given by Kaltsoyannis and Plane 2008).

B) The discussion of the differences between LIF and long-path DOAS measurements (section 3.4.1) is not convincing as given. Apparently only slight differences in the wind directions on Sept. 8 and 9 (this is in fact said twice in lines 12, p 25755 and 1+2, p 25756) are causing differences in the LIF/DOAS ratio of a factor of 2, why? Perhaps a figure showing the algae fields crossed by the trajectories at the two days could help. Why are the differences on Sept. 17 so large?

C) In section 3.5 a linear relationship between IO levels and particle formation is suggested. What is the basis for this and if Fig. 17 actually show a linear relationship what is the explanation? From R4-R9 one would expect a quadratic or higher dependence of the particle formation rate on the IO concentration since the IO self reaction and halogen oxide cross reactions are involved.

D) The model calculations on the impact of IO on HOx partitioning are not new and not backed by HOx measurements so no new results or insight are presented here and the reference to the several previous studies is sufficient. Thus section 4 could be deleted.

Minor points: 1) It would be informative to the reader if RHaMBLe and Roscoff were mentioned in the abstract.

2) Section 2.2: The Ti:Sapphire laser probably was frequency doubled?

3) P. 25745, line 9: the 184.9 radiation was provided by a Hg lamp?

4) Section 2.3, page 25746, the statements in lines 17 and 19 on permanent hetero-

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9, C10059–C10061, 2010

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geneous loss of IO and recycling of IO appear to be in contradiction, clarify.

5) page 25746, Equations (1): In the centre Eq. “A:y” should probably read “Ay”, in the right Eq. w should probably be omega (as used in the centre equation).

6) Page 25748, relationship between IO and TH: Why should there be an exponential relationship? Is not rather the contour of the coast the determining factor, i.e. the relationship of additionally exposed kelp area per meter of tidal height change. In other words the orography of the intertidal area is important. The conclusion about similar IO production pathways in lines 13/14 is therefore not correct.

7) Page 25748, line 20: ... flatter diurnal profile ...

8) Sentence at the bottom of page 25748, top of page 25749: This is clouding the issue, apparently solar radiation is essential to photolyse I2. To me the conclusion rather appears to be that there are no alternative sinks of I2, thus less solar radiation leads to higher I2.

9) page 25750, 1st line: There is another, even higher NO2 spike at about 9:50 (Fig. 9), corresponding to a another dip in NO2.

10) Page 25751 (and 25741) lines 27/28: what is a "point source technique“?

11) Page 25752, delete R17 and replace by ref. to R2.

12) Pages 25752 and 25752: Refs. to Mahajan et al. 2009, there are two publications by Mahajan et al. 2009, ref. needs a, b.

13) Page 25757, the statement in lines 11+12 is obvious and should be deleted.

14) Section 3.5: The instrument description belongs into section 2.

15) Page 25762, paragraph in lines 10-16: These are no new results, delete para.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 25737, 2009.