Atmos. Chem. Phys. Discuss., 9, C10124–C10126, 2010
www.atmos-chem-phys-discuss.net/9/C10124/2010/

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 #3

Received and published: 26 January 2010

The manuscript by Furneaux et al. describes in-situ measurements of IO by LIF at Roscoff, France, in 2006. The manuscript has several objectives. Measurements by the LIF IO instrument are compared to LP-DOAS and MAX-DOAS observations. In addition, the data is interpreted with respect to dependence of iodine chemistry on tidal height, NO_x, and meteorological parameters. The correlation of new particle formation and the presence of IO is also investigated. Finally, model calculations on the impact of iodine on HOx chemistry are presented.

This is an interesting manuscript that contains unique IO observations. The data interpretation follows, in large, strategies performed in previous publications. Generally I recommend this manuscript for publication in ACP. However, a number of aspects of the manuscript need to be refined/clarified before its final acceptance.

Detailed comments and questions:

Section 3.2: The argument that IO is more dependent on the macroalgae source than on solar irradiation is misleading, after all IO mixing ratios are very low at night. The reason why the authors do not see a dependence is that most I_2 is photolyzed between time of emission and time of detection. I recommend rephrasing this section by explaining why one would not expect a dependence on solar irradiance during the day if the I_2 source is more than 30 - 60 seconds upwind of the IO measurements.

Figure 5: I am not sure that I understand Figure 5. In Figure 6 there are clear times of high J(I_2) with low IO. These times do not show up in Figure 5. Was some kind of filtering performed? If the authors mean "averaging", by "binning" then should there not be an error bar, as in Figure 4?

Section 3.3, and Figures 8 and 9: The authors state that IO was not observed at high NO_x. However, Figure 9 shows at least one event of 14ppb NO_2 in the presence of 3ppt of IO. While the IO-NO_2 dependence in Figure 5 looks convincing one has to wonder if this is in part an artifact of the 60min averaging. Why was this interval chosen and how would the curve look if it were not averaged? Also, why are there not times of low IO AND low NO_x in figure 8?

Page 25752 A reference to a paper describing the LP-DOAS system should be added.

Section 3.3 The definition of the detection limit used in this manuscript should be added. I am puzzled that so many negative mixing ratios of the LIF and the LP-DOAS fall outside of the detection limit range (see also Table 2). The argument for the presence of IO at night is not very convincing. The statistical analysis of the LIF data in Figure 11 is very qualitative. The nocturnal LP-DOAS measurements shown in Figure 12 are more often negative than positive. I also do not understand how the LP-DOAS can measure with a 0.9ppt error if the negative values are -2 to -4ppt. One would expect the error to be more in the range of 2ppt during this night. The text suggests also that I_2, IO and NO_3 were never measured during the same night. Considering how

variable all of these species are, the argument that one should see IO formed from I_2 + NO_3 during the night of Sept. 8-9 is not convincing.

Section 3.4 The authors argue convincingly that the LIF and the LP-DOAS do not measure the same airmass. This begs the question of why the observations of the two instruments are compared in a more quantitative way in Figure 13? Interestingly, the fact that LIF and LP-DOAS measured similar amounts of IO on Sept 9 was interpreted by assuming that the LP-DOAS intercepted air from more extensive macroalgae fields. Does this mean that LP-DOAS data can sometimes be interpreted as true mixing ratios? This would contradict the general belief that, due to the inhomogeneous release of I_2, LP-DOAS data are lower than the true IO levels.

Figure 14: The CMAX-DOAS observed IO near high tide. This seems to contradict earlier statements on the dependence of IO on tidal height.

Page 25758, line 19. Remove the word “September” and replace with “th”.

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