Atmos. Chem. Phys. Discuss., 10, C14444–C14448, 2011
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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Concurrent observations of atomic iodine, molecular iodine and ultrafine particles in a coastal environment" *by* A. S. Mahajan et al.

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Received and published: 8 March 2011

Concurrent observations of atomic iodine, molecular iodine and ultrafine particles in a coastal environment

Mahajan et al.

Response to review by anonymous referee 2.

We thank the referee for the helpful comments on the manuscript. Here we detail the response to the questions raised and have made the corresponding in the manuscript.

The paper presents measurements from a week-long campaign on the coast of north



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Interactive Discussion
Discussion Paper

west Spain aimed at probing iodine chemistry and associated new particle formation. The results are interesting and logically interpreted, using supporting chemical observations (O3, NO, NO2) as well as meteorological data, and a 1-D numerical model. The paper is highly suitable for publication in ACP.

R2.1) I have only one more substantive comment, relating to the modeling calculations. I am content that the conclusions reached by the authors are robust, but I would have preferred it if they had either used specific cases, or overall averages for all their parameters. The ratio of I2/I of 32 is a daytime average across the campaign, i.e. low and high tide. However, the modelling work to reproduce the average uses input parameters pertaining to low tide, specifically for April 30th (10 s injection, equivalent to a 4 m/s wind speed across the kelp bed). It might therefore be more consistent to compare modelled I and I2 to the observations of April 30th, or at least investigate the average scenario also for wind speed. It would also be interesting for the reader to know how sensitive the calculations were to the assumed HO2 and OH, taken from a different (albeit mid-latitude MBL) study. As the HOx/NOx/halogen chemistry seems to be highly interlinked, and their concentrations are spatially highly heterogenous, this would seem to be a relevant sensitivity to consider. It would also help the reader to know if there have been any other estimates of I2 fluxes from macro-algae, to put into context the fluxes assumed in this study.

RESPONSE: The average ratio for I2/I is computed for all data that have positive detection for both species. Hence it is an average over low tide periods only, when both the species were observed above the instrument detection limit. This is now made clear in the manuscript (Line 220). We chose April 30 because it best represents the average conditions and displays the highest observed I2 and I mixing ratios with the largest tidal variation. The modelled ratio is not highly sensitive to OH and HO2. This is mainly due to the fact that the measurement site was close to the emissions that control the I2/I ratio. For example a 2 fold increase in OH and HO2 mixing ratios results in a $\sim$1% drop in the I2 mixing ratio by the time the air mass reaches the measurement point, while



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and 2 fold decrease results in a ∼1% increase (Line 258). A comparison of fluxes used in previous modelling studies is now briefly discussed in the manuscript (Line 295).

Minor comments/concerns:

R2.2) Overall: There is a tendency to overstate things in the paper. The results are in agreement with other studies that iodine compounds and new particles have a source in the intertidal zone, but they really don't show a "strong" tidal signature (p 27233 line 19) when out of 7 days of measurements only 4 are above the detection limit. Please tone this down a bit.

RESPONSE: Changed (Line 190).

R2.3) I would emphasize more the heterogeneous nature of the region where the measurements were made which Fig 1 doesn't really do justice to. Google maps shows the extent and range of the local coastline, with suggestion of patches of laminaria in many places.

RESPONSE: Figure 1 has now been changed to incorporate a google map image that shows the extent and range of the local coastline. The text has also been changed to emphasize this (Line 202, 206).

R2.4) P 27230 line 6: include hyphen in "newly-developed" P 27230 line 21: Cavity-Enhanced needs capital E

RESPONSE: Changed.

R2.5) P 27231 line 24: please give the model number of the 2B technologies instrument (model 205?). Please also explain why the O3 record shown in Fig 2 is incomplete.

RESPONSE: Added. Line 139 and line 175.

R2.6) P 27232 line 7 to 12: I would suggest moving information about instrument performance to the earlier "Experimental" section 2.1 which deals with the ROFLEX technique.



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RESPONSE: Changed (Line 105).

R2.7) P 27232 line 24: please give a brief description of the Birmili et al method P

RESPONSE: Added (Line 160)

R2.8) 27233 line 12: “replace “while” with “with”

RESPONSE: Changed.

R2.9) P 27235 line 18: I just wanted to check that the vertical resolution really is 10 cm with a boundary layer height of 1 km.

RESPONSE: The vertical resolution was 10 cm for the first 20 m and 5 m above it to 1 km. This has been corrected in the manuscript (Line 253).

R2.10) P 27236 line 3: particles would not have “had” enough...

RESPONSE: Changed.

R2.11) P 27239 line 12: Figure 1 should be abbreviated to Fig.

RESPONSE: Changed.

R2.12) P 27242 line 26: replace “where” with “were”

RESPONSE: Changed.

R2.13) Figures: It would be very helpful if figures 2 and 3 were larger! Caption for Fig. 1 states that Arosa is 3 km away, whereas throughout the rest of the paper it is 3.5... you might want to correct this for the sake of consistency.

RESPONSE: Changed.

R2.14) Fig 3. Is the fact that classic “banana-shaped” particle growth curves are not observed further evidence that the particles formed elsewhere and were transported? Can you give an indication of the timescale for the formation of such new particles?



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RESPONSE: We do not model the particles with the use of THAMO mainly because their exact growth process is still not completely understood. Hence it is not easy to give an estimate as to the timescales of formation. Further work needs to be done in the laboratory before accurate estimates on the particle growth rates in such complex environments can be made.

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 27227, 2010.



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Interactive Comment