

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

**Anonymous Referee #2**

Received and published: 4 January 2011

The paper presents measurements from a week-long campaign on the coast of north west Spain aimed at probing iodine chemistry and associated new particle formation. The results are interesting and logically interpreted, using supporting chemical observations (O<sub>3</sub>, NO, NO<sub>2</sub>) as well as meteorological data, and a 1-D numerical model. The paper is highly suitable for publication in ACP.

I have only one more substantive comment, relating to the modelling 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 I<sub>2</sub>/I of 32 is a daytime average across the campaign, i.e. low

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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.

Minor comments/concerns:

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.

I would emphasise more the heterogenous 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.

P 27230 line 6: include hyphen in "newly-developed"

P 27230 line 21: Cavity-Enhanced needs capital E

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.

P 27232 line 7 to 12: I would suggest moving information about instrument perfor-

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mance to the earlier “Experimental” section 2.1 which deals with the ROFLEX technique.

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

P 27233 line 12: “replace “while” with “with”

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.

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

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

P 27242 line 26: replace “where” with “were”

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.

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

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