

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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This paper reports some very interesting new measurements of iodine-containing gas-phase and aerosol species at a previously unexplored coastal site in Spain. The reported correlations between the measurements are complex but the different modelling scenarios do go some way to explain the observed trends.

The main comment/observation/question I would like to direct to the authors is the relatively low total ultrafine particle numbers ($1\text{--}2 \times 10^4 \text{ cm}^{-3}$) detected during ‘bursts’. Although clearly significantly higher than the background levels reported,

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such levels are 1-2 orders of magnitude smaller than those reported during similar daytime low-tide events at Mace Head, Ireland and at Roscoff, Brittany. Firstly, I think that this point should be clearly stated in the paper, particularly as a quantitative comparison is made for the I_2 and I atom mixing ratios. Are the lower numbers likely to be a reflection of the relative sizes/density/ages of the seaweed beds, ambient NO_x levels, meteorological conditions, geographical considerations, or a combination of these or other factors? Apologies if I have not fully understood the modelling/analysis section and the authors infer that the lower particle numbers are related to the reported anomalously high levels of I_2 and I_2/I (is this the case?), but I would suggest that a more detailed comparison of such factors from Mace Head (or Roscoff) and this ‘new’ site would make the picture clearer for the reader.

Reference is also made to the lack of a typical ‘banana’ particle growth plot as observed in other studies but no further discussion is made of this point. It would be fruitful (sorry, couldn’t resist the pun) if some discussion was included to provide insight into why non-banana type growth might occur at this coastal site and not others, which presumably accounts for the lower particle growth rates at this site?

My final comment relates to the authors suggestion of a ‘mystery’ organic species/group which could account for removal of iodine. Having made the suggestion, a fairly thorough job of ruling things out is done but there is only a vague, generalised comment on what it could be. One possible candidate group which I would flag up are the benzene-1,2 and -1,3 diols (catechol and resorcinol respectively) which have direct anthropogenic (industrial/combustion) sources and are also oxidation by-products of PAHs. These species are water soluble and react readily through aromatic substitution with iodine (Willard and Wooten, 1950). There is essentially no atmospheric abundance data available for these species but it’s unlikely that 1-2 ppbv mixing ratios suggested in the modelling analysis are realistic. However, the aromatic 1,2 diol group has been identified in humic-like substances (HULIS) which constitute

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an important fraction of secondary organic aerosol or SOA (Graber and Rudich, 2003; Ofner et al., 2010). It is this functional group which is thought to be responsible for the fixing of iodine in natural waters to form non-volatile iodine-containing organics (e.g. Francois, 1987; Reiller et al., 2006), and so could also play a role in the uptake of iodine by SOA with a HULIS component?

It might be stretching things a bit but I note that the measurement period was only a few weeks after the severe storm ('Xynthia') hit NW Spain causing reported damage to the local eucalyptus and pine forests which are used for timber and pulp production. The combustion or pulping of these tree types are known to result in the release of organics including HULIS (e.g. Gonçaves et al., 2010). If there was any increased clearance of storm-damaged trees by burning or pulping in the period between the storm and the measurements, this may have increased background levels of secondary organics in the gas and aerosol phases, and also that of background primary organic aerosol (POA) in fine and coarse modes i.e. at sizes beyond those measured. Both HULIS-containing POA and SOA could play a possible role as a sink for iodine? How does the ultrafine background level of 1700 cm^{-3} compare with those reported at Mace Head/Roscoff?

References

- Francois, R., *Geochim. Cosmochim. Acta*, 51, 2417-2427, 1987.
Gonçaves, C., et al., *Atmos. Env.*, 44, 4474-4480, 2010.
Graber, E.R., and Rudich, Y., *Atmos. Chem. Phys.*, 6, 729-753, 2006.
Ofner, J., et al., *Atmos. Chem. Phys. Discuss.*, 10, 17369-17405, 2010.
Reiller, P., et al., *Radiochim. Acta*, 94, 739-745, 2006.
Willard, H.H., and Wooten, A.L., *Anal. Chem.*, 22, 670-671, 1950.

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