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Interactive Comment

Interactive comment on "Modelling molecular iodine emissions in a coastal marine environment: the link to new particle formation" by A. Saiz-Lopez et al.

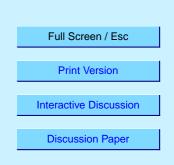
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Received and published: 14 September 2005

The paper contains many intersting and new results regarding the distribution of molecular iodine at Mace Head. The comparison with box model calculations provides a stong indication that I_2 is responsible for the observed particle "bursts". I would like to comment on some aspects concerning the box model calculations:

p. 5409, line 10: In my opinion, the description of the "aerosol-condensation algorithm (Jacobson, 1999)" is too superficial. It would be very helpful to provide the numbers of the equations used from Jacobson (1999) or a more precise reference to make clear



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which approaches and assumptions you used for coagulation and especially condensational growth. Which gases contribute to cluster/particle growth? Do you assume the condensing gases to be non-volatile? How do you treat the nucleation step, i.e., the formation of thermodynamically stable clusters?

p. 5411, lines 6-9: You write that "all iodine-containing species are allowed to vary". In contradiction to this, CH_2I_2 , CH_2BrI and CH_3I are obviously constrained. What about I_2 (see also next point)?

p. 5416, line 1: How do you prescribe I_2 emission rates: Do you prescribe emission fluxes (if yes, how large are the fluxes?) or mixing ratios? If you prescribe gaussian distributed fluxes, I would expect the I_2 mixing ratio peak to be a little delayed (i.e., asymmetric) due to some accumulation of I_2 .

p.5419, lines 10-14: Is the two-hour model simulation to produce Figure 6 identical with the model run to produce Figure 3b? From comparing the two figures it seems that the IO mixing ratio is slightly different especially towards the end of the respective peak (e.g., compare IO for delta(t) between 20 and 30).

I would be happy if the authors would consider the above comments for the final version of their paper.

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