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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Laboratory studies of the homogeneous nucleation of iodine oxides" *by* J. B. Burkholder et al.

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The paper describes a series of laboratory studies on new particle formation from photolysis of volatile organic iodine species. Based on the observations in their experiments with CH_2I_2 and CF_3I in the presence of ozone and the assumption of a single species homogeneous nucleation (OIO), the authors develop a coupled chemical aerosol model. Several experimental parameters are varied, such as aerosol precursor concentration, the ozone concentration and the temperature. In order to match the observations, certain nucleation model parameters, such as enthalpy of OIO condensation or bond enthalpy of OIO dimers, are optimized. Finally, the authors use the aerosol model in an atmospheric box model to evaluate the potential importance of iodine oxides in new particle formation processes in the marine boundary layer.

Certainly, the topic of the paper is highly interesting for the readers of ACPD. New particle formation processes in the troposphere are still poorly understood, especially from natural sources. The paper is well written and organised, the conclusions drawn are sound. Literature is adequately cited. However, I do have some specific remarks to the paper.

Table 1 gives the calculated photolysis rate coefficients for I_2, IO and OIO. Are theses values also calculated for the lamp used in the experiments ?

Figure 8 shows the results from photolysis pulse experiments. I dont understand where the peaks in particle number concentration are coming from. Any speculations ?

In the abstract and conclusion part the authors summarize their results based on the measured IO and OIO concentrations, stating ".... that IO and OIO concentrations reported in the field measurements .... are not sufficient to account for significant aerosol production...". Although this statement is correct, in my opinion it is the wrong way of argumentation, since it is based on the intrinsic feature of the underlying measurement technique (DOAS) to average concentrations. Therefore, the line of argumentation is - in principle - based on a "measurement artefact". However, I do agree with the main conclusion that locally high concentrations are a likely source for new particle formation in Mace Head.

A final remark. Certainly, one of the most important aspects of iodine oxide nucleation is the question if it might also happen in the open ocean marine boundary layer. As discussed by the authors, this would be strongly dependent on the emission rate and photolysis lifetimes of the precursor compounds as well as the photolysis of the iodine oxides. In my opinion there are still too many open questions (including uncertainties about emission of iodine compounds from oceans) to conclude that the particle formation mechanism is restricted to the hot spot areas discussed in the paper. Therefore, I would conclude that several hypothesises "... await confirmation by future field and laboratory studies."

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3, S2042–S2044, 2003

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3, S2042–S2044, 2003

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