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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Laboratory and modeling studies on the effects of water and soot emissions and ambient conditions on the formation of contrail ice particles in the jet regime" *by* H.-W. Wong et al.

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We thank the referee for providing insightful and constructive comments in improving our manuscript. We have listed our responses to the comments and how the manuscript is accordingly revised point by point below.

1) The measurements miss to report simultaneous temperature measurements. Hence the relative humidity with respect to liquid saturation is unknown. The model measurement agreement is not good. I see huge differences, so this part of the conclusions is

not justified. For example (there are also other ones), Fig 5 b shows lines for theory and data from measurements. The theory mostly predicts either zero or one, with a sudden reduction at high soot concentrations. The data are between 0.2 and 0.5. How can you justify, page 26803, line 8: "The figure clearly shows that the model was able to capture the fall-off of the super-micron fraction very accurately at an exhaust 10 water level of 2–3% in molar fraction."

Response: We have included a new figure (Figure 2) in the revised manuscript to describe mixing between the exhaust plumes and the background co-flowing fluid. This figure illustrates plume relative humidity with respect to liquid saturation as a function of plume temperature during plume dilution in the measurements. Temperature and relative humidity values for each sampling location in the measurements are also plotted in this figure. We have also performed new data analysis and model calculations and reworded the text in the revised manuscript to better describe comparisons between the modeling results and the experimental data.

2) The printing of the figures in this ACPD paper is awful. The editors and authors should not accept such a printing. The lettering is far too small. I had to enlarge the ïňAgures for decoding the small lettering.

Response: We apologize for the lack of quality of the figures we put out. We have enlarged the fonts of the figures to make them more readable in the revised manuscript.

3) The literature review is incomplete. Not only "recent modeling studies" suggest that soot and fuel sulfur may impact ice formation. There were several in-flight experiments, starting with Busen and Schumann (GRL, 1995) reporting observations and measurements. See Fahey et al. (1999, chapter on Aviation-Produced Aerosols and Cloudiness, in Penner et al., IPCC 1999), and the series of papers on the SULFUR experiments in several later papers (1996-2002) on this issue, e.g. in JGR. On the other hands, it is generally expected that the impact of soot and sulfur acid is unimportant for the threshold temperature of contrail formation.

Response: We have added literature review of the measurement results in the Introduction section of the revised manuscript. We agreed with the referee that the parameters investigated are not believed to be important for the threshold temperature of contrail formation. However, these parameters may be important for contrail ice particle properties (such as size, number concentration, and surface coverage), which may be used to better estimate contrail radiative forcing and climate impact in large scale models. We have revised the title of the paper and the Introduction section to make it clear that our focus is on ice particle properties.

4) The PAL facility is not sufficiently described. Fig. 1 does not give the required details. The paper Tacina and Heath (2010) is not in the open literature. Hence, I request that all experimental details as needed to understand the results of this paper are repeated here. This includes a clear drawing with well-defined scales (with units identified - cm?) of the pipes and nozzles and the flow in the chamber with identification of the flows from the nozzle into the ambient chamber air and the sampling positions inside the chamber.

Response: We apologize for the erroneous citation for the paper by Tacina and Heath (2010). It is actually in the open literature as part of the conference proceedings from American Society of Mechanical Engineers (ASME) that can be purchased from ASME. We have corrected the citation and included the ISBN in the citation. We have also added a schematic drawing of the PAL facility in the revised manuscript (Fig. 1c), outlining the dimensions of the exhaust nozzle, transition pipe, and sampling lines.

5) The paper reports OPC data but no temperature and no humidity measurements at the positions of observations. Without these additional data the measurements cannot be fully interpreted.

Response: We have added the temperature and water vapor partial pressure values at the sampling locations derived from our tracer measurements in Figure 2 of the revised manuscript. Description of how we derived these values is also added in Section 2.3

of the revised manuscript.

6) I miss an identification of the ratio of water emissions to heat emissions (or the respective ratio of water concentration to temperature at the jet outlet) controlling the mixing line steepness in the Schmidt-Appleman diagram, besides pressure. In fact, I would expect to see a plot of humidity and heat mixing as is usually drawn in Schmidt-Appleman theory (water vapor partial pressure versus temperature, with saturation curve p_saturation (temperature), and status at jet exit and status in ambient air identified). A similar request was formulated in the comment by D. J. Cziczo. However, I would not refer to Koop but to the older Schmidt-Appleman figures.

Response: We have added a new figure (Figure 2) in the revised manuscript describing the Schmidt-Appleman theory. Mixing lines for different exhaust water levels under different conditions are plotted to illustrate threshold conditions for ice particle formation.

7) Instead of referring to flight level altitudes under standard day conditions, which is an ill-defined term for the ACP reader community, I ask for a table listing ambient pressure, and ambient temperature. In addition, it would be good to know ambient relative humidity with respect to liquid saturation and ambient aerosol concentrations.

Response: A table is added in the revised manuscript for each of the conditions we studied. Threshold contrail formation temperature and exhaust water molar fraction calculated from the Schmidt-Appleman criterion are also included in the table.

8) Page 26796, line 7: I suggest a careful explanation of the sampling used for OPC measurements. Is the sample taken at constant temperature or could it be that part of the ice particles are evaporated before reaching the measurement plane in the OPC? Can you quantify any losses during sampling?

Response: We have provided additional details about the sampling lines in Section 2.2 of the revised manuscript. We also state in the revised manuscript that evaporation of the ice particles is believed to minimum, but particle scatter and loss were present and is the main reason contributing to the discrepancies between the modeling and the OPC results.

9) I ask for a short section giving some background on how an OPC works and what kind of limitations such an instrument implies for this investigation. You mention particle loss and scatter problems, but I am not an expert in OPC aspects and cannot assess these hints therefore.

Response: We have added a brief description about the principles and limitations of the OPCs in Section 2.2 of revised manuscript.

10) Page 26801: lines 15 etc.: Before concluding on homogeneous nucleation (I do not believe that this occurs for your conditions), please discuss the potential that there were at least a few aerosol particles in the ambient air that could served as ice nuclei. This concern was also formulated in the comment by D. J. Cziczo. Perhaps the number concentration of ice particle is low but not zero. The resultant ice concentration might be low and too low to be detectable for your OPC.

Response: By saying “homogeneous ice particle formation”, we referred to “ice particle formation from homogeneous nucleation followed by homogeneous freezing of liquid water”. We have revised the manuscript accordingly. During our experiments, a background particle concentration between 10 and 400 cm-3 was measured. We have stated in the revised manuscript that although these particles may serve as ice nuclei, no ice particles were measured from the OPCs and observed from the video snapshots in our measurements.

11) Page 26799, line 2: what are “stable ice particle concentrations”?

Response: We have reworded the sentence from “To measurement stable ice particle concentrations...” into “To reduce fluctuations in ice particle concentration measurements...”.

12) Page 26792, line 18: What are standard day conditions? Do you mean standard ICAO atmosphere?

Response: The standard day conditions are now referred to the temperature and pressure values listed in Table 1 of the revised manuscript.

13) Page 26801, last line: what is a “trained” camera?

Response: We have changed the word from “trained” to “focused”, meaning that the camera was taking snap shots of the intersection of the light beam and the exhaust plumes.

14) Fig. 2: What is [Ice]?

Response: The label [Ice] is replaced by “Ice Particle Concentration” in the figure in the revised manuscript.

15) Fig. 4: What is “ice submicron fraction”?

Response: The term “ice submicron fraction” means the fraction of ice particles that is smaller than 1 micron in diameter. Since we use “ice super-micron fraction” everywhere else in the manuscript, we have changed the figure to show “ice super-micron fraction” instead for consistency.

16) Fig. 5a: how can the two (left and right) vertical axes get interpreted as being equivalent?

Response: We have redone our data analysis and now two axes (OPC results and extinction data) are decoupled and compared with relevant modeling data separately.

17) P. 26804, line 25: The conclusions discuss ice particle size distributions. But I did not find a measurement of the size distribution in the paper (except fractions of ice particles being larger or smaller than one micrometer).

Response: The referee is right about the experimental particle size data. We meant to refer particle size distributions calculated from the model compared with the two-channel particle size data from the experiments. We have revised the manuscript to make this clear.

18) P. 26804 lines 11 etc: In the conclusion part you list results which appear to be consistent with published results (except the part on homogeneous nucleation). Hence, the results are not surprising and not really new.

Response: We have reworded the Conclusion section to reflect the referee's comment. We have emphasized in the revised manuscript that our work is to demonstrate that contrail ice particle formation can be reproduced in a laboratory setting like the PAL.

19) I would accept this. It would be important enough to demonstrate that the expected conditions can be reproduced in this kind of experiment. This kind of experiments can be extended and used to investigate aspects not yet understood in future studies. For example, I would like to learn from future studies with this PAL facility which fraction of the soot particles contributes to ice particle formation, and how does this fraction depend on ambient conditions, on jet mixing properties, and soot properties. The results likely depend on the time scales of mixing with ambient air relative to time scales of ice particle formation and sublimation. These time scales will differ in PAL from those in real exhaust jets of big aircraft engines.

Response: We have some brief discussions about how the combined PAL/modeling approach can be utilized in the future in the Conclusion section of the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 26791, 2011.