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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.

Anonymous Referee #1

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The paper investigates contrail ice particle formation in a simulated aircraft engine jet, which is a topic often discussed in the literature with respect to aviation climate impact and contrail visibility. This paper describes measurements performed in a test chamber (PAL) simulating jet engine exhaust mixing with ambient air under cruise conditions. It reports optical particle counter (OPC) measurements of ice particles about 0.6 m downstream the jet nozzle. Measurements are reported for variable water vapor mol fractions and soot particle number concentrations in the simulated exhaust. The results are compared with a model.

The idea to develop a laboratory text facility for contrails is a good one because it allows to measure at positions which are hard to reach behind cruising aircraft. However, the paper is not yet acceptable.

The measurements and the descriptions and discussions are not complete enough to sufficiently support the conclusions drawn in this paper.

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."

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 figures for decoding the small lettering.

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.

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.

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.

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.

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.

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?

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.

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.

Page 26799, line 2: what are "stable ice particle concentrations"?

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

Page 26801, last line: what is a "trained" camera?

Fig. 2: What is [Ice]?

Fig. 4: What is "ice submicron fraction"?

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

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).

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.

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.

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