Atmos. Chem. Phys. Discuss., 3, S2240–S2242, 2003
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Volatile particles formation during PartEmis: a modelling study" *by* X. Vancassel et al.

Anonymous Referee #2

Received and published: 18 December 2003

The modeling results presented in this paper are a useful simulation of the condensation processes occurring in a probe and sampling system used in a combustor emission measurement program. By comparison with measured volatile particle data, the conversion of fuel sulfur to S(VI) (i.e., SO_3 and H_2SO_4) in the combustor has been determined. This conversion, as the authors point out, is an important parameter in determining the amount, and consequently potential impact, of sulfate particles emitted from aviation engines. This study is a useful contribution to the field, extending previous modeling work by these authors to analyze the data from this combustor test program.

The paper could be improved by clearing up some potentially confusing statements or discussion. The first is the discussion of existing data (page 5805, lines 7 on) and modeling results that have been used to determine ϵ. For the measured data, a few

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results are quoted from the broader set of experiments that have been performed, but the present paper does not refer to Kärcher et al., 2000. The Kärcher paper has collapsed much of the scatter in the data available at that time by accounting for the differences in measurement parameters of the particle measurement systems.

For the modeling studies, the quoted results are a subset of the available data and represent a wide variety of disparate studies. The numbers quoted put ϵ values for conversion occurring in the plume only directly in contrast to the total conversion measured in the developed wake of an airplane (obviously including combustor, turbine, and plume conversion contributions). This is very much an "apples to oranges" comparison. Further, one of the analyses was attempting to interpret the Concorde wake encounter data which, in hindsight, was likely affected by issues of instrument cut-off size and atypical engine technology. Not to diminish the importance of the present work, but the state of understanding is not as confused as the present paper portrays. This discussion needs to be more complete and accurately portray the data and modeling studies presented, or a more fair summary statement needs to be crafted that indicates that uncertainties still exist (but are not as extreme as presented here).

The analysis (page 5814 lines 2 on) of the impact of chemi ions is important and significant, in that it suggests results potentially contrary in appearance to previous papers as referenced here. Two points of confusion in the discussion of these results complicate their direct interpretation. First, the case is made on the previous page that CE = 50% and ϵ = 2.7% represent "very good agreement". Yet in the CI analysis CE=100% and ϵ =5% are used. Perhaps this switch will not affect the final conclusions, but there is no justification given for this switch in the selection of parameters.

Further, changing the CI emissions by an order of magnitude is determined to have little impact on the estimate of ϵ. Previous papers have emphasized the importance of CI in determining or matching the measured volatile aerosol quantities. The regime of the CI changes and the apparent minimal impact on ϵ needs to be discussed in light of these previous studies. Finally, the comment that the small particles are depleted by

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wall losses (line19) needs to be reconciled with the comments on page 5810, line 7-8.

A minor comment on the abstract: The sentence "ϵ ranges between roughly 2.5% and 6%, depending on the combustor settings and on the microphysical approach used." is confusing. The combustor settings appear to have a modest impact (2.5-2.8 or 5.2 to 5.7%) while the other changes of roughly a factor of 2 (2.5-5.2 and 2.8 to 5.7) are due to changes in the collision efficiency, which is just a parameter used in the modeling. It does not appear that there were any changes in the "microphysical approach" employed in obtaining those results. So I think that sentence should be reworded, separating the two effects and clarifying the cause of the bigger effect.

Also I am confused by figure 3. My reading of the text would suggest that the probe was in the same positions for the two different combustor conditions. Section 2 discusses eleven transverse locations that the probe was positioned, where later it is mentioned that the extreme positions are not analyzed due to their proximity to the combustor wall. However figure 3 indicates that the old cruise probe positions are 5-8, while the modern cruise positions are 12-17. Presumably the combustor conditions are all that has changed and the comparable data are for identical probe positions, because if the probe were sampling a different part of the combustor many differences might be expected. The discrepancies between the top and bottom horizontal axes need to be clarified. Further, the discussion indicates that both calculations and data are presented for the two cruise cases (for each of three CEs). I only observe calculations or data in figure 3 and it is not clear which is missing (and data should have error bars?).

Lastly, figure 5's captions says that the "size distribution changes by a few when ..". A few nanometers?

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5803, 2003.

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