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Interactive comment on “Aerosol direct radiative effects over the northwest Atlantic, northwest Pacific, and North Indian Oceans: estimates based on in-situ chemical and optical measurements and chemical transport modeling” *by* T. S. Bates et al.

T. S. Bates et al.

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We thank Reviewer 1 for persevering through the entire manuscript and the kind comments about the manuscript in general. We address the minor points below.

1. How would the authors recommend detailing with the hysteresis effect in the relative humidity on the size of the aerosols? Is it not important to know the recrystallization relative humidity so that the dry branch of the hysteresis curve can be followed if the ambient humidities become low enough? Also their water uptake is limited to RH $<$ 90% by the measurements. How did they extrapolate to RH = 100% in their model?

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This is a good point and we needed to make some simplifying assumptions. These assumptions are outlined in section 3.4.2. We assumed that f(RH) for dust was 1, f(RH) for sea salt was 2.33 (which assumes it never dries out over the ocean), and that sulfur/carbonaceous aerosol f(RH) could be defined as a function of the POM mass fraction. In the last two lines of section 3.4.2 we state "RHamb was capped at 95% because of the large uncertainty in measurements above this RH. This somewhat arbitrary cap may result in an underestimation of the scattering coefficient and thus DRE and DCF."

2. On page 220, Line 11-12. It is stated that the SSA is 1.0 but then for submicrometer aerosols the value would be expected to be even larger. How can it be larger than 1.0?

Corrected to read "would be expected to also be 1.0"

3. Can the authors tell us what errors are made if we assume that all the fine mode aerosols are anthropogenic? Of course the answer will depend on location but they can compute this for the 3 regions studied. This is of some interest since this is often a necessary assumption when using satellite remote sensing to deduce the anthropogenic component.

This is a difficult question to answer as it is very domain and time dependent and requires assumptions about the fraction of POM that is anthropogenic. Assuming all the POM and sulfate is anthropogenic our MBL measurements in these regions suggest that all but a few percent of the fine mode fraction is anthropogenic (Quinn and Bates, 2005). The exception is during dust storms in ACE-Asia when the dust comprised on average 22% of the fine mode fraction (Quinn and Bates, 2005, Table 2). Over the large CTM domains defined by the rectangles in Figure 2 of this paper, the data from MOZART suggest that the anthropogenic fraction of submicron aerosols is: 73% (INDOEX, NIO), 83% (TRACE-P, NWP), and 69% (ICARTT, NWA). Note however for this model "fine mode fraction" includes dust up to1.2um dry geometric diameter and sea salt up to 1.0 um dry geometric diameter. This unfortunately includes some of the coarse mode volume when converted to ambient humidity and aerodynamic diameter. Since there are so many ambiguities in defining the answer to this question with the data at hand, we have chosen not to include this estimate in the paper.

4. In Section 4.2.4 an e-folding time of 1.6 days for hydrophilic conversion is assumed. Where does this time scale come from? Is it justified?

The 1.6 days comes from Cooke and Wilson (1996). We have referenced it in the text. Cooke and Wilson (1996) explain that this is meant to represent the process of chemical aging by which other chemicals mix with BC aerosols and convert them from hydrophobic to hydrophilic. That paper also considered the effect of other transformation rates (and other initial mixtures of hydrophobic/hydrophilic). Other papers have also used fixed conversion timescales – Cooke et al. (1999, 2002) both used a 1.15 day lifetime for BC and OC.

5. Page 234, line 13. “Dust was treated as a soluble component”. Dust is not a soluble substance. This sentence should be clarified.

Correct, dust is not soluble. MOZART does however treat dust as a hydrophilic component. We have changed “soluble” to “hydrophilic” in the manuscript.

6. Page 241, line 2-3. Why, in NIO, was the STEM dust burden half of MOZART in spite being a factor of 64 smaller in emissions?

Dust exhibits the largest difference between the two models. In the case of NIO the differences between the emissions and column burden is ˜1/3 (see Table 16). This reflects differences in the size dependent emissions models, and differences in removal processes (gravitational settling and wet removal). The uncertainties associated with dust calculations are large and we need a better understanding on many of their processes.

7. Can you comment on the differences in numerical diffusion, both vertical and horizontal, between the CTMs? This has some bearing on comparing transport properties and results.

In STEM calculations we calculate vertical diffusion based on K-theory using the boundary layer and surface products from MM5. The first-order difference between the models is due to model resolution, with the biggest differences between STEM and MOZART being in the horizontal dimension. Some of these differences can be seen in Figure 14. These effects will be most important when calculating aerosol optical depths etc . over regions with spatially varying emissions (e.g., over urban regions) and will be minimized over homogeneous regions (such as oceans).

8. Page 243, Line 1. “session” should be “season”?

Corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 175, 2006.

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