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Interactive comment on "Tropospheric distribution of sulphate aerosol mass and number concentration during INDOEX-IFP and its transport over the Indian Ocean: a GCM study" *by* S. Verma et al.

Anonymous Referee #1

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This manuscript describes a new model which has been developed within the LMDZ model to simulate sulfate aerosols, its evaluation using INDOEX data, and its application to INDOEX interpretation. It represents very interesting initial steps towards a thorough study, and has the potential to make a nice ACP paper, but it needs significant extension of the analysis, as well as clarification of several points. The english usage is excellent and I will refrain from pointing out the few typos at this stage, since it will almost certainly require a re-review. My recommendation is publication after major

revisions, outlined below.

Major points:

1) It appears to me that the new sulfur model is generally well-designed, though I have several concerns outlined below. One general point that is not clear is the motivation is behind developing a new sulfur chemistry model. What advantages and disadvantages does it have compared to the independent models developed by Boucher and Hauglustaine? The abstract says "the originality of this module is its ability to predict particle mass and number..."; the M7 model already does this for not only Aitken and accumuluation with sulfate, but also for nucleation and coarse modes for all the main chemical components of the aerosol, so it's not clear what is particular about the new model described here. M7 and other models need to be cited here, and contrasted briefly with the new model.

2) Why is the reaction of DMS with NO3 and other species left out when Boucher et al (2003, ACP) showed it could be 1/4-1/2 of the total DMS oxidation? I would suggest that the runs really should be redone including this reaction to make them quantitatively valid.

3) Assuming Henry's Law equilibrium for SO2, H2O2 and O3 can be rather inaccurate when the aqueous phase components are quickly reacting away with each other; can the authors justify the use of this approximation here?

4) Why is a prescribed liquid water content used (section 2.3)? Doesn't the LMDZ microphysics provide more accurate and appropriate values?

5) Not distinguishing between liquid and ice scavenging (i.e., treating ice as if it were simply liquid) is likely to be an important deficiency; a sensitivity run would help to elucidate this (should be relatively easy given the simple representation of scavenging)

6) The way in which the release of gases from evaporating precipitation is treated will err strongly towards too much release of highly soluble gases (like HNO3), which

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should remain in the droplets until nearly the entire population evaporates; this should be commented on, and again a sensitivity run to elucidate whether this effect is as important as one might expect would be valuable. Likewise, the choice of 0.5 retention for aerosols in evaporating droplets is extremely arbitrary; does it have any physical basis, and how sensitive are the runs to the uncertainty in this parameter?

7) The split operator treatment of doing scavenging before deep convective transport is certainly better than the opposite order of operations, but it still may not be sufficient, especially if the scavenging scheme is (properly) limited to only scavenging the fraction of the grid cell where precipitation is falling, and the deep convective mass fluxes are (as typical) applied to the entire grid cell. It is, however, quite an effort to do better; the authors should simply make this clear and expect it to possibly lead to discrepancies in their results.

8) The second really major problem I see with the manuscript is that the evaluation is taken much too lightly. Saying that modelled levels agree with observations within a factor of 1-2 does not say much, and in this case is not even correct (in March the discrepancy goes up to 2.5-3x). The agreement for aerosol quantities shown here is much poorer than that shown for gases like O3 and CO by de Laat, Lawrence, and others. What can we learn from this? The term "reasonable" agreement is used a lot, which again is not very informative (admittedly, the term is used very often by modellers, and I've also been guilty of this in the past, so this paper should not be singled out, but it is important to try to take a step beyond this). The evaluation needs to be made more quantitative, and the authors should not see a discrepancy as a bad feature of their model, but rather as an opportunity to learn some about what is missing in our current theoretical understanding (to the extent that it is represented in the model). Where specific sources of uncertainty are suspected, this needs to be discussed, rather than simply citing previous studies (like Wilson et al. and Ackermann et al.).

9) My final major point is that the analysis needs to dig in deeper; most of the points

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are reconfirmations of earlier findings, such as the north-south gradient, and the contrast between Aitken and accumulation modes. The only major new point I found was the difference between the results using the two inventories, though this was also only treated very roughly - it was not clear what in the underlying development of the inventories may have been responsible, and also whether one is really better (the RV run did come out better often, but generally only marginally better). Since INDOEX has been chewed on a lot, and since this paper also takes on the task of describing a new model, I don't think the authors need to overdo it in terms of trying to find new points (it is understandable that one can't do everything in each paper), but I do think that the paper does need a bit more depth to the analysis in order to reach the standards of final publication in ACP.

Minor points:

1) There is some redundancy in the abstract (regarding the model reproducing the observations)

2) Andronache's work with a cloud resolving model (in the JGR INDOEX special issues) should be cited along with the other earlier studies (introduction).

3) The setup of the model runs is not clear; in 3.1 it says it was run over the IFP, spinning up from 20 December 1998, but then later it appears that a run was also done for the FFB (Jan-Mar 1998), while on the other hand in section 3.2.3 it is stated that "the model meteorology is different from the actual one" (this could be interpreted as meaning for a different period, or simply meaning that there are errors in the ECMWF analysis, in which case it should be stated more precisely).

4) section 3.3.1: "captured by the model" - since this is not compared to observations, it is difficult to see what feature the model is "capturing"

5) section 3.3.2: "highest AOD is observed" - should read "is simulated"

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 395, 2005.