

Interactive comment on “Reevaluation of mineral aerosol radiative forcings suggests a better agreement with satellite and AERONET data” by Y. Balkanski et al.

Anonymous Referee #2

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General Remarks:

This paper contains a very important analysis suggesting that several previous model estimates of dust absorption are likely to be overestimates due to the use of erroneous imaginary indices for Mie calculations from the literature. While the scientific basis of the paper is excellent, I think the readability could be improved by presenting the paper in a somewhat more organized manner. I have some reservations about the true independency of the various numbers used in this paper (see detailed comments). Although I am not a native speaker myself, I have the impression that at a number of places the English could also be improved. I recommend accepting the paper after

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taking into account these revisions.

Detailed comments: 1. p. 8384 l.6. This discrepancy of models and satellite forcing should already be discussed in the introduction, since it is the main motivation for this paper. I could see the first reference to this discrepancy only at p. 8394 bottom. It should be clear which model studies did or did not rely on the old Patterson/Volz refractive indices

2. p. 8384 l. 17 It is difficult to understand why a uniform hematite percentage of 1.5 % should everywhere give the best fit. Or do you mean mainly Africa? Please justify the use of a uniform number

3. p. 83884 l. 28 I could not find in the paper a reason for why the Patterson/Volz estimates are so different. Was it a problem with the technique, was it a representativeness problem?

4. p8385 l. 3 what do you mean with 'state of the atmosphere'?

5. p.8385 l. 13 I think here you could present a table reporting on previous model studies and what the underlying assumptions on dust were.

6. p. 8385 l. 24 See also Perez et al. JGR, 2006

7. p. 8385 l. 23 I think also a main reason is the complications of including size resolved aerosol in a GCM/NWP model

8. p. 8386 l. 24 are these RF based on measurements, or another model?

9. p. 8385 l. 26 more likely to warm or cool the atmosphere. : somewhere it should be explained that this is one of the main uncertainties regarding the direct effect of aerosol in general (IPCC, 2001)

10. p. 8387 l. 16 I don't understand very well what is the motivation for the definition of a base case with hematite 1.5 %. Why not use the exact values in your database?

11. p. 8387 l. 23 are these sensitivity studies reported?
12. p. 8387 I assume that S2 is supposed to be most close to previous reported global studies? If so mention.
13. p. 8388 Did you assume internally mixed minerals for your base case?
14. p. 8388 l. 17 Did all models really use Volz/Patterson?
15. p. 8389 l. 2 Why the Patterson/Volz so different?
16. Consider to have section 2 after section 3 17. p. 8389 After reading this section it is not very clear what indices were actually the basis for the calculations of S1/S1b and S2. Table 1 contains several references for each mineral; which one is actually used? Do you follow the recommendations of Sokolik and Toon, or do you deviate from that. IN line 23 you mention sensitivity for 'other minerals': what do you tests and where do you describe that?
18. Is it somewhere mentioned that use of Mie calculations ignores non-sphericity of particles?
19. p. 8389 Regarding d'Almeida and Shettle and Fenn literature values: how do they enter the story? Which large scale model estimates are based on their values? How do these values relate the Patterson values?
20. p.8389 Figure 4; does that correspond to a certain case?
21. In summary section 2 needs a serious clean up to make clear what is assumed for what reason.
22. p. 8392 This comparison of AOD with measurements a) appears at a strange place should be rather a result b) it is not clear why stations choosen c) it is not clear whether this comparison refers to reference simulation. Tell the reader what we can learn from this comparison

23. p. 8393 l. 15: are you sure your estimate is independent from the AERONET estimates, or is somehow the choice of the 1.5 % made to fit the AERONET.

24. p. 8394 l. 4 Important conclusion- regarding the Weather Forecast papers: what did they assume as optical properties and how would those works be corroborated by the present work? Conclusion section!

25. p. 8394 l. 26 Seems to be a separate section at the end of section of 4 (comparison with other model results

26. p. 8396 These case studies 4.4 and 4.5 have nowhere been introduced (nor that this is an important issue. Maybe a proper introduction in the methods section of the simulations would help.

27. 8397 section 4.5. It is not entirely clear what this study is about. Do you assume the SAME dust emissions in the base case distributed over a single mode, in the sensitivity case over two modes. Or are also the overall emissions different? And how do you distribute the mass/number?

28. Section 5: Introduce to the reader why this alternative comparison with CERES/MODIS/airborne measurements is 'real' new evidence. What are the assumptions on refractive indices mentioned in p. 8397 l. 19? Is the comparison with this paper truly independent? P. 8389 l. 3?

29. p. 8389 Did you test if some reasonable biomass burning field from LMDZ-INCA added as an external mixture to the dust fields would give reasonable forcings?

30. p. 8398 l. 15 I am confused about the statement that 2.7 % hematite gives the best fit: doesn't this contrast earlier statements that 1.5 % would be best?

Figures 5 and 6 are hard to read

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8383, 2006.

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