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Atmospheric Chemistry and Physics Discussions

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

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Answers to the comment of Referee #2

First of all we would like to thank this referee for his in depth, constructive comments. They brought important improvements to the paper.

Here are the general remarks from the referee:

'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 taking into account these revisions.'

We made substantial changes to the organization of the paper, among which:

- An in-depth re-writing of Part 2 concerning the refractive indices - Part 2 and 3 have been exchanged as recommended by the reviewer - A new Table (Table 1) has been added to clarify the refractive indices used in previous studies - Table 2 has been augmented to indicate the spectral ranges over which mineral refractive were reported in the references used in this work - The introduction has been reorganized to introduce earlier that satellite measurements indicate a less absorbing dust than previously believed. - We tried to improve the English

Concerning the detailed comments of the referee:

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

The motivation of the paper now appears in the first sentences of the introduction.

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

The following paragraph was added:

The intent of this paper is to link the radiative effect of dust to its hematite content. Hence, the sensitivity studies that are presented refer to constant hematite content.

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Estimates of iron oxides in dust samples by Lafon et al. [2004] and Linke et al. [2006] give credence to these amounts since the ranges for iron oxides from the samples they analyzed are respectively: 1.4 to 2.8% and 1.0 to 2.5% in volume. In the simulations discussed in this paper, the amount of hematite of a given model run is kept constant. We will note S1 the reference run which refers to the hematite content 1.5% in volume (equivalent to 3.0% by weight). Two additional simulations were conducted to study the sensitivity to the hematite fraction; they are noted S1b and S1c for respectively 0.9% and 2.7%. To adjust the volume of the other minerals we only vary the illite content (Table 2). We checked that varying other mineral contents, (i.e., of quartz, calcite, illite and montlorillonite keeping hematite constant), led to insignificant variations in refractive indices compared to hematite (not shown).

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 representativiness problem?

It is not easy to answer this question, here is how we tried to address it in the text:

The refractive indices from Patterson et al. [1977] and from Volz [1973] overestimate by a factor of 2 the energy absorbed in the column during summer over the same region. This discrepancy is due to too large absorption in the visible but we could not determine if this is linked to the sample studied by Patterson et al. [1997] or to the method used in determining the refractive index.

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

This part has been changed.

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.

As mentioned above we added a new Table (Table 1) following the recommendation of the referee.

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

We read this paper and added this reference to the reference list.

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

We agree. This is now mentioned.

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

We clarified this part as follows:

In part 5, model-derived DRE is compared with sattelite-based measurements of clear sky shortwave radiative effect of mineral dust off-coast western Africa.

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)

We agree. This is now brought up in the introduction.

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?

We made the choice to be able to discuss the radiative effect of dust as a function of its hematite with three simple simulations with a constant hematite content. Had we taken the full 2-D map of hematite from the database, we would have had to make assumptions on the size of the particles containing hematite which is an added add complexity. The simulation proposed by the referee would require to follow 6 tracers with log-normal size distribution (as many tracers as minerals to describe the optical properties of dust). It is feasible but we doubt we would have learned much more.

11. p. 8387 l. 23 are these sensitivity studies reported?

We now mention that:

We checked that varying other mineral contents, (i.e., of quartz, calcite, illite and montlorillonite keeping hematite constant), led to insignificant variations in refractive indices compared to hematite (not shown).

12. p. 8387 I assume that S2 is supposed to be most close to previous reported global studies? If so mention.

We now mention it.

13. p. 8388 Did you assume internally mixed minerals for your base case?

The justification for the internal mixture has been also added to the text, and the reference to Reid et al., 2006 has been added:

Atmospheric dust sample show that dust particles are mostly found in the form of aggregates (Reid et al., 2003). The iron-rich fraction of the dust which includes hematite is generally embedded in a matrix made of clay (Reid et al., 2003). Therefore, an optical model is needed to derive the refractive index of these minerals as an internal mixture.

Another sentence helps remind the reader that we assume internally mixed minerals:

We will note S1 the reference run which refers to an internally mixed dust with a hematite content of 1.5% in volume (equivalent to 3.0% by weight).

14. p. 8388 l. 17 Did all models really use Volz/Patterson?

This point is now clarified in the text and with the added table (Table 1).

15. p. 8389 l. 2 Why the Patterson/Volz so different?

We answer this question in the same , here is how we address it in the text:

The refractive indices from Patterson et al. [1977] and from Volz [1973] overestimate by a factor of 2 the energy absorbed in the column during summer over the same region. This discrepancy is due to too large absorption in the visible but we could not determine

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if this is linked to the sample studied by Patterson et al. [1997] or to the method used in determining the refractive index.

16. Consider to have section 2 after section 3

We liked this suggestion and we exchanged the 2 sections

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?

We have clarified this part with a complete rewriting of the section, here is the part that answers the referee comment:

The intent of this paper is to link the radiative effect of dust to its hematite content. Hence, the sensitivity studies that are presented refer to constant hematite content. Estimates of iron oxides in dust samples by Lafon et al. [2004] and Linke et al. [2006] give credence to these amounts since the ranges for iron oxides from the samples they analyzed are respectively: 1.4 to 2.8% and 1.0 to 2.5% in volume. In the simulations discussed in this paper, the amount of hematite of a given model run is kept constant. We will note S1 the reference run which refers to an internally mixed dust with a hematite content of 1.5% in volume (equivalent to 3.0% by weight). Two additional simulations were conducted to study the sensitivity to the hematite fraction; they are noted S1b and S1c for respectively 0.9% and 2.7%. To adjust the volume of the other minerals we only vary the illite content (Table 2). We checked that varying other mineral contents, (i.e., of quartz, calcite, illite and montlorillonite keeping hematite constant), led to insignificant variations in refractive indices compared to hematite (not shown).

Do you follow the recommendations of Sokolik and Toon, or do you deviate from that.

We follow their recommendations, the references for refractive indices are not identical than that of this article but we come with the same conclusion on how to treat the mineralogy.

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IN line 23 you mention sensitivity for ‘other minerals’: what do you tests and where do you describe that?

We clarified this part as follows:

We checked that varying other mineral contents, (i.e., of quartz, calcite, illite and montlorillonite keeping hematite constant), led to insignificant variations in refractive indices compared to hematite (not shown).

18. Is it somewhere mentioned that use of Mie calculations ignores non-sphericity of particles?

Yes, it is indicated and we now insist more upon it in the following sentence:

Using Mie theory assumes a spherical shape for particles which can introduce uncertainties of up to 15% (Mishchenko et al., 1997).

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?

Table 1 summarizes which large scale model estimates are based on their values. D’Almeida et al. indices are partly based upon Shettle and Fenn numbers. So is the OPAC database.

20. p.8389 Figure 4; does that correspond to a certain case?

We have now clearly indicated in the figure caption and in the text that this Figure refers to cases S1, S1b and S1c.

21. In summary section 2 needs a serious clean up to make clear what is assumed for what reason.

We made extensive changes to this section which is now section 3 of the new text.

22. p. 8392 This comparison of AOD with measurements a) appears at a strange place

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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

We have moved this comparison to the part where result are discussed. It is now in subsection 4.1; the figures 5 and 6 have been redrawn and we tell the reader what this comparison means for the paper.

Figs. 5 & 6 indicate that the modelled dust captures well the seasonal variations and the loads of dust aerosols over these regions.

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.

No it is really independent and this agreement was not expected at the onset of the study.

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 corrobated by the present work? Conclusion section!

We thank the referee for pointing out this conclusion that we added in the Conclusion section:

Over the tropical Atlantic, the North Indian Ocean and the Sahara region, the atmospheric heating is 3 to 4 times smaller when hematite is considered internally mixed (S1 case) than in the simulation with the refractive indices from Patterson-Volz (S2 simulation). This finding has important implications regarding the weather forecast simulations.

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

This part is now a separate subsection

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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.

We clarified the text but preferred not to extend the methods section that we deem already too long

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?

We wrote a better description for this sensitivity studies:

To investigate the sensitivity of this forcing to the variation in size distribution we vary in case S4 the size spectrum of the dust in our 3-D INCA model. We added to the standard case with one mode (MMD =2.5 um equivalent to a modal diameter of 0.59 um with a width sigma = 2.0), a coarse mode with an MMD of 5.0 um (modal diameter of 1.18 um and sigma = 2.0) at the point of injection. The yearly amount of dust injected in this simulation is twice the amount of dust injected in simulation S1. The coarse aerosol mode treated alone is unable to capture the observed optical depth across the equatorial Atlantic whereas when both a coarse and a fine mode the optical depth is well represented (not shown).

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?

The CERES measurements used by Li et al. are direct flux measurements at the top of the atmosphere. In addition, the optical depth from MODIS is used to normalize this flux per unit optical depth. It is therefore 'real' new evidence. For the surface fluxes,

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an aerosol model from MODIS is used since the single scattering albedo is needed for the computation. The MODIS model are independent from the one used in this paper. We wrote the following sentence to describe what was done in section 5:

In part 5, model-derived DRE is compared with sattelite-based measurements of clear sky shortwave radiative effect of mineral dust off-coast western Africa.

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?

No we did not look into the influence of biomass burning for this paper

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?

Although the refractive indices from AERONET are in excellent agreement with the case 1.5% hematite, the atmospheric heating reported in Li et al. is intermediate between a hematite content with 1.5 and 2.7%. I changed that sentence to:

This value is intermediate between the two hematite cases with 1.5 and 2.7% which respectively produced an amount of energy absorbed in the atmospheric column of +22 and +32 Wm-2 (tau)-1

Figures 5 and 6 are hard to read

These figures have been redrawn

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