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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Dust altitude and infrared optical depth from AIRS" *by* C. Pierangelo et al.

Anonymous Referee #1

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Referee's comment on Dust Altitude and infrared optical depth from AIRS by Pierangelo et al.

I find this to be in general an interesting and good overall quality paper, describing a new technique to extract potentially valuable information on dust burden in the atmosphere. In partcular the authors retrieve dust optical depth in the infrared and dust altitude. Both of these variables are of interest to those studying dust burden in it's own right, and to those studying the terrestrial radiation balance. I recommend therefore that this paper be published, but have some suggestions for improvements and clarification that I would wish to see considered before final publication.

Specific comments

My main concern pertains to the retrieval of dust layer altitude and the application of the method to the real atmosphere. As the authors state, their retrieval makes many assumptions, such as a single homogeneous layer of dust, and they cite plenty of

evidence that this is not the case. This must mean that the method is only applicable in a relatively small number of cases? It would be interesting to see a more detailed discussion of available observations or models of dust transport to be able to assess how often the method is likely to be in error. The method implies that it is sensitive to the middle of the dust layer (see discussion on p3340) but that there can be ambiguities when there is a very thick layer of dust. I am not convinced that it is appropriate to draw conclusions about variations in height of dust layer through the year from this method alone, when the variations in question (500m) are smaller than either the differences introduced by a thick layer of dust, and indeed the day to day variability of the dust altitude.

p3337, line 7. Would it be possible to use observations of aerosol size distribution instead of the OPAC model? p3337, line 13,14. Please would you identify the wavenumbers of the regions you are calling short wave and long wave in figure 1. I am not entirely convinced from figure 1 that there is that much difference in sensitivity of the lower wavenumer region to altitude or AOD. Could you include refractive index? There are studies that have shown this is important (Sokolik et al, Highwood et al) that you do cite in the introduction but they would seem to offer some information here too.

p3337, line 17. Is your conclusion that IR extinction coefficient is insensitive to size distribution consistent with your conclusion about the change in SW to LW ratio with distance from source?

p3338, line5. I find this discussion rather vague. Would it be useful to include a figure showing the sensitivity (or lack of) of each channel to dust, ozone and water vapour?

p3340, line 4. Please clarify what you mean by the distance. p3340, line 19. In reality, what is the likely uncertainty in extinction co-efficient at these wavelengths? Is it comparable to 20%?

p3341, line 11-12 (and p3343 line 15). I dispute the fact that background aerosol such as sea-salt is not temporally variable. It depends on wind speed for example.

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This assumption is needed for your retrieval but I would like to see some discussion or justification of it's suitability. Hence on p3343, AIRS is seeing the total temporally varying aerosol, and not necessarily just dust.

p3342, line 15. The comparison with TIROS is made over different time and space scales so it is hard to judge how good a validation it is. Consider making this comparison tighter.

p3342, line 27. Shouldn't the night vs day argument only be a problem if there is a robust diurnal cycle in dust loading? I can conceive of how this might be true from a meteorological point of view, but is there any evidence for it?

p3344, line 10. It should be possible to alter your aerosol model to remove particles larger than the cut-off of 7.3 microns, and recalculate to see if it really is the size distribution that matters (particularly as you say the IR extinction co-efficient isn't sensitive to size distribution). presumably this implies it is the SW part that changes?

p3346, line 6. Could you use real dust measurements from the field or lab? I would also say that the issue of multiple dust layers is a limitation of the usefulness of the altitude retrieval.

Technical comments

p3341, line 6 change "analyzes" to "analyses" p3341, line 19, change "but" to "except" p3342, line 20, "downwind of..."

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 3333, 2004.

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