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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling the direct effect of aerosols in the solar near-infrared on a planetary scale" *by* N. Hatzianastassiou et al.

Anonymous Referee #2

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General Comments

This study is an effort to quantify the global-scale radiative impacts of aerosols in near-infrared (IR) using a spectral radiative transfer code. The optical properties of aerosols are prescribed from the Global Aerosol Dataset (GADS), and the states of the atmosphere and Earth's surface are specified using reanalysis datasets and satellite measurements. Although numerous studies on aerosol direct effect exist in the literature, they often treat the whole solar spectrum as a piece. The novelty of this study lies in separating near-IR from ultraviolet (UV) and visible. So, it is relevant to better understanding the role of aerosols as climate forcing agents. The methodology as described in the paper is sound, and the results appear to be reasonable. However, I feel that, in some instances, the results have not been explained thoroughly or accurately (see

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Specific Comments). Based on the above considerations, I recommend acceptance after major revisions.

Specific Comments

1. When describing the results quantitatively, the authors often refer to the maximum values, and use the expression “up to” throughout the paper. Although in principle there is nothing wrong about this, it is more appropriate (and useful) to discuss the global-mean numbers for a global study like this one, especially in light of the paper’s emphasis on possible climate implications. For example, the abstract states that the TOA cooling could be up to 6 W m-2 regionally, while the global mean is only 0.48 W m-2. Omitting the former helps avoid leaving a reader the impression that the effect is larger than really is.

2. The signs of the DRE values are confusing. If aerosols cool at TOA, warm the atmosphere and cools the surface, and, as explained in the paper, radiative cooling at TOA is defined as positive, one would expect that the atmospheric warming is negative and the surface cooling is positive. In fact, the opposite is true. Most studies on aerosol direct effects define downward (incoming) radiation as positive, thus a radiative cooling is negative, whether at TOA or at the surface. This is also consistent with the IPCC assessment reports.

3. The second paragraph on p.2: “the models for radiation transfer, which usually include a few spectral bands in the whole SW range”. Do you mean that the bands are usually too few to allow for a precise calculation of radiation? If so, please state clearly.

4. In Sections 2 and 3, some interesting points, which could be important for understanding the results, are simply referred to Hatzianastassiou et al. (2004a). Those include the treatment of the solar radiation reflection, GADS aerosol properties and re-computation of aerosol properties depending on actual RH. For readers who may not have the cited paper handy, a brief description would be very helpful. It does not have to include all the detail, but the essentials.

5. The third paragraph on p. 7: Is there any way to quantify possible errors caused by interpolating and extrapolating aerosol properties? I am particularly concerned about extrapolating.

6. On p. 8: “Water soluble”, “water insoluble” and “soot” are different types of aerosols as defined in GADS. Please explain how they differ from sulfate and sea-salt in terms of chemical and optical properties. The last sentence (“In spite of Ě”) is not clear. How can a higher contribution of water insoluble and soluble components lead to a breakdown of the “general decrease” of g with increasing wavelength? Does this mean that the general trend is not applicable to these components? If so, it may not be called “general”.

7. How is the GADS-derived optical depth compared to satellite retrievals?

8. On p. 10: Enhanced aerosol absorption over highly reflective surface has been found in previous studies (e.g. Ming et al., JGR, 110, D20208, doi:10.1029/2004JD005573). References should be added.

9. On p. 10: Is AOT the only optical property that determines DRE at TOA?

10. The first paragraph on p. 11: The sentence beginning with “large values appear in regions with small cloudiness, Ě” seems to over-emphasized the effect of low cloud cover on DRE at TOA. Is not aerosol loading more important? To answer this question definitively, the authors may want to plot clear-sky DRE and compare it with all-sky DRE.

11. Figure 4: What is causing the blank? Why does DRE appear so “noisy” (i.e., the warming and cooling are mixed up) over the Saharan desert in July, while it is not the case in January? The author’s explanation from the viewpoint of surface albedo is not clear enough. The sentence beginning with “For example, note that Ě” is not well-written. For example, it is stated that the areas with lower surface albedo are “adjacent to areas with DRE at TOA < 0”. Is DRE at TOA < 0 being equated to higher surface

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albedo? However, as stated in the previous sentences, higher surface albedo is only one of the factors contributing to negative DRE at TOA. A sensitivity run with globally uniform surface albedo would be required to add more clarity to this issue.

12. The first paragraph on p. 11: The sentence beginning with "In contrast, DRE at TOA is very small in July Ě" is somewhat self-contradicting. First, it is stated DRE at TOA is small. Then, it is said that aerosol radiative forcing is important under cloud-free conditions. What is the reader supposed to make from it?

13. On p.13: The sentence beginning with "Overall, the presence of aerosols Ě" seems to imply that surface cooling and atmospheric warming results in TOA cooling. I do not think that this is a cause-effect relationship.

14. On p. 13: The sentence beginning with "Although smaller in magnitudes Ě" compares the DRE as defined in the paper (w/ aerosols minus w/o aerosols) with the forcing of greenhouse gases (PD minus PI). This is not an apple-to-apple comparison. This is also true for the abstract.

15. On p. 15: The sentence beginning with "Note that over specific regions Ě", in my opinion, does not explain the negative near-IR to total SW ratio. It explains why aerosol effect in near-IR is cooling (positive), but does not touch total SW.

16. On p. 16: The statement in italic is not clear. Do you mean that aerosols affect two spectra to different extents?

Technical Corrections

1. A parenthesis is missing before "between 0.85" in the abstract.

2. There should be a space between W and m-2.

3. Different panels in the same figure should use the same color scheme. Otherwise, it is hard to compare them.

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