

Dear Dr Hatzianastassiou,

15 July 2022

We appreciate this opportunity to address your concerns and those of the reviewer and submit a final version of our paper. Much of the discussion in this last review (as well as throughout the review process) has revolved around the treatment of large dust particles and possible resulting errors in downward surface irradiance. The treatment of dust in general has been an important theme of the reviewer and we have previously added, thanks to this reviewer's suggestions, Table 6 and Figures 1, 14, and 15 which discuss the impact of dust on the radiative transfer results. We also, previously, added a new LW section (4.2) and Figure 16 detailing differences between fine and coarse mode observations by AERONET and values supplied from. In the most recent comments, the reviewer has rightly pointed out that our treatment of large dust particles can lead to the errors in downward shortwave and longwave irradiances over deserts as found in our results. As this appears to be the final remaining significant concern, to clarify this point we have added comments in Section 2 (lines 174-178) and in the Conclusion (lines 598-605). We have also clarified that all dust particle sizes are in term of effective radius.

The reviewer's remaining comments (in italics), are, we feel, adequately addressed in the paper to date. We have collected them below for clarity and address each in turn.

*“Aside from the dust-issue though, there are still larger differences between MATCH and MERRA which at least should receive some attention – as both models assimilated MODIS AOD data, which would imply a better agreement than demonstrated. So, make sure that critical deficiencies and differences are well explained.”*

And

*“It is a bit puzzling to me that you say that the ‘undesired’ land-sea contrast is likely associated with the MATCH model – as models are not expected to show these inconsistencies. It would be nice to get some more insights as to the reasons .. why?. “*

We disagree with the reviewer that the differences between the MATCH and MERRA2 AODs do not ‘receive some attention.’ Section 2 is dedicated to the differences found between MERRA2 and MATCH and reasons for such differences are discussed therein (for example see lines 218-220 and lines 277-283) and in various places throughout the paper. Other places discussing reasons for differences include the Abstract (lines 37-40), Section 2.3 (lines 336-337), lines (353-356) and in the Conclusion (lines 585-587). One topic he reiterates in this final review is the large negative values found in the clear sky comparisons of MERRA2 to MODIS over tropical oceans (Figure 3). While this bias does stand out and we mention it to the readers (lines 246-248) we do not have a good explanation so avoid making suppositions regarding another group's product.

*“Are there arguments why MATCH or why MERRA assimilations should be trusted more ... as the resulting AOD maps are so different? Does it simply mean that the influence of the baseline model (e.g. CAMS vs GOCART) is much stronger than the impact of the AOD data assimilations? That would be an important ‘help/issue’ to any manuscript reader.”*

This is a good question but one that reaches beyond the scope of this paper. The goal of this paper is twofold, to analyze the output aerosol optical depth from the MATCH model against observations and MERRA2, and to evaluate the resulting surface irradiances given the knowledge found in those AOD comparisons. None the less, we suspect the answer is basically yes, the influence of the underlying model is greater than the influence of the assimilation of MODIS AODs. However, determining that rigorously is left to a separate study.

*“The biases vary with AOD ranges (usually satellite remote sensing suggest larger (and likely overestimates) AOD at low AOD values, while AOD maxima are often missed) so possibly a separate linear plot for just the 0-0.3 AOD range could be an useful extra.”*

Again, we feel that information gleaned from such a plot is insufficient to warrant another plot and subsequent increase in the length of the paper.

In conclusion we believe we have addressed the reviewer’s major concern regarding large dust particles and include new text in the paper and its conclusion discussing this concern. The reviewer’s remaining comments, we believe, are either already adequately addressed within the text or are essentially a conversation on the strengths and weaknesses of aerosol assimilation and the models presented. This the reviewer points out with the comment *“I do want to not to hold up a publication any further.”*

Sincerely,

David Fillmore and co-authors.