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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The summertime Saharan heat low: Sensitivity of the radiation budget and atmospheric heating to water vapor and dust aerosol" *by* Netsanet K. Alamirew et al.

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Major Comments

1. Error Analysis: The authors spend a good bit of time estimating uncertainty in their modeled fluxes via comparison to satellite retrieved fluxes. However, when it comes to the data analysis, these uncertainties are not taken into consideration. I think it's great that the authors have a handle on the RT model errors, but I think it would be far more useful to carry those uncertainties throughout the entirety of Section 4. Doing so would make the paper and results much stronger and would afford the community opportunity to make a more precise comparison between yours and future dust forcing estimates.

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Discussion paper

Response

We agree the importance of including error analysis despite we have reduced the uncertainty using sensitivity experiments. This is addressed qualitatively to some extent in section 4, i.e. error associated with the uncertainties in the input.

Changes made

Additional information quantitatively expressing the error in flux calculation associated with uncertainties in some of the input data is provided. Page 8 L25-28 and L37-39.

2. Radiative Transfer Model. To generate the mie coefficients the authors use two different size distributions (Dubovik and Ryder) but the same index of refraction. However, what's the source of the refractive index? The authors conclude that the Dubovik size distribution is more representative of the actual size distribution based on a comparison of the model and observed/retrieved fluxes. However, it is completely possible that the index of refraction used here also biased. For example, it's possible that the Ryder distribution is correct but doesn't produce enough SW dust forcing because the MEC is too low at the appropriate size parameter, thus the forcing in the SWE for Dubovik would better match observations because it's biased towards smaller particles. At any rate, my only point is that you have two degrees of freedom and you can't say conclusively that one size distribution is more representative than another one b/c the index of refraction isn't constrained.

Response

We agree that the refractive index may cause uncertainty in the flux calculations especially in the SW absorption. It is also interesting to test the sensitivity of radiative flux to refractive index. In general for a given size distribution of dust, when refractive index is increased net SW heating will increase and net LW cooling will increase to a lesser extent. This however is a complicated function depending on the surface albedo and cloud. (Liao et al., 1998). Here we used recent measurements for dust refractive

index over the Sahara (Ryder et al., 2013) which is function of the composition of dust particles, independent of the size distribution. It could be possible that if we reduce the refractive index, the SW heating will reduce in Ryder distribution, which is the biggest discrepancy compared with satellite measurement. But we haven't made sensitivity test as we have measured refractive index.

3. RT Model: The authors state that the vertical profile of the dust mass mixing ratio is adjusted so that for a given MEC the AOD matches observations. Is the profile linearly scaled by a single value to match the observations? Is a single coefficient derived for all cases or is this done independently for each RT simulation?

Response

To be clearer, first an average extinction profile is derived from CALLIOP and this profile is used to derive the extinction profile at each time step, i.e. the average profile is adjusted to match the measured AOD from AERONET. So to answer the question, for each RT calculations independent extinction profile is derived.

4. Flux comparisons: It the text it is not clear if the flux comparisons are performed in a robust manner. For example, why are monthly mean fluxes from CERES compared to the observations and output from the model? The proper way to conduct the comparison with CERES would be to access the daily nighttime and daytime data and then sub sample the observations/RT model output/GERB retrievals in order to conduct an apples-to-apples comparison. The authors acknowledge this (Page 9 line 35) so it's puzzling why a more thorough analysis wasn't performed. This approach includes the task of making comparisons to the reanalysis data (again, authors note that interpolating MERRA surface temperature may be biasing the flux comparisons). Furthermore, more insight would likely be gained by comparing the clear-sky fluxes only, since cloud forcing is not important to the study.

Response

An important aspect of this study that needs to be noted is it is intended to provide season (one month) study of the radiative budget and sensitivities to water vapour and dust variability over the Saharan heat low. In order to do so we have used the best available input dataset through sensitivity experiments. It is useful to carry out comparison of the radiative flux at the time steps of CERES data (which is twice per day) as the referee suggested. We have actually made comparison of RT model outputs with CERES data with the respective time step to derive RMSE. This is presented on page 9 line 21(corrected draft). This will give us a good picture of the uncertainties of model simulations. However further comparisons using average of two time steps per day will not enable us to achieve the target we put at the outset.

To compare simulated flux with observation, GERB data is used. Further reanalysis data is also used which is available daily and thus used the same days as the RT model simulation days. CERES data is not used to compare simulated flux except for sensitivity experiments and estimate cloud DRE. We understand that using month mean CERES clear sky and all sky flux will bring some error but it will give us first order estimate of cloud DRE over the region. This will help emphasize need to improve the error on the radiative budget due to underestimated cloud in reanalysis dataset despite the challenges in making these comparisons.

5. Flux comparisons: Tables and Figures. There are too many tables and the main figure (9) for this section is not particularly useful. Firstly, the tables are cumbersome and don't communicate the main results well (for example, color could be used to indicate if RT model output or reanalysis output is biased high or low in comparison to surface obs or satellite retrievals. In addition, the flux comparison Fig 9 are tough to interpret because the annual cycle is included. A better way to do this is to have one plot comparing the mean annual cycles, and another comparing the anomalies.

Response

We agree to remove Table 5 since the information on this table is also found in Tables

2 and 3(corrected draft). An additional table is moved to the supplementary material.

Colours included on the wDwC results in tables 3 and 4(corrected draft Tables 2 and 3) red indicating model results overestimated and blue indicating model results underestimated compared with observation.

Some of the figures were corrected based on referee #1 and reviewer's comments. Figure 9(also Figure 6) is corrected and it is easier to read. We therefore keep it as it is. But have also made additional plot using anomalies but we put it in the supplementary document. See also page 9 L18-20.

Changes made

Table 5 removed Colours used on column 6 of table 2 and table 3 Additional figure included in supplementary material page 3, figure S2

6. Forcing efficiencies for dust and CIWV should also include the 95% confident interval from linear regressions.

Response

We agree that the regressions should be expressed to 95% confidence level. All the regression results are expressed within the 95% confidence interval.

Changes made

These are included in section 4.2.1(page 10-11) and section 4.2.2(page 12) on the corrected draft.

7. Figure 12 and 16 are not interesting. Consider including observations here as well (at least for TOA). BTW - CERES produces surface flux products. These could be folded into the analysis as well. Response

Here the plots are made using daily averaged variations in dust AOD or water vapour. That is dust AOD (and CIWV) is increased linearly in each RT run. This is a theoretical

work designed to investigate the sensitivity of dust and water vapour on the radiation budget. There is no such observational data, at least at one particular point which is the observational data we used here. This can be tested for a number of grid points of Satellite observation to see sensitivity of radiation to AOD variation (e.g Young et al., 2009). However this is not the objective of this study and thus it is not included.

8. Figure 17 is impossible to read/interpret, and I don't even wear glasses (yet)! Please consider a more simple and straightforward way to describe the vertical sensitivities. A good rule-of-thumb would be to only include in the plot information that you actually describe in the text.

Response

Figure made easier to read. Additional explanation regarding the figures provided

Changes made

Now we put the two panels of figure 17 as independent plots, Figure 16 and Figure 17 in the corrected draft. Statement added on page 13, L18-19.

Minor Comments

1. Individual panels of the figures should be labeled as a,b,c,. . .

Response

All figures prepared accordingly

2. Figure 5: This figure is not very useful in terms of understanding the relationship between the AODs and IWV. Can you please just replace with one or two scatter plots?

Response

We used SEVIRI AOD to show that there are cases where AOD is missed in AERONET which we suggest to be due to confusing dust with cloud. This we believe is important to show there are cases where dust might be missed in AERONET. We have comple-

mented this using nephelometer measurements.

3. Figure 6. If the authors removed the diurnal cycle from this plot we'd have an easier time interpreting the magnitude of the biases. As it is presented here, the magnitude of the differences are small relative to the magnitude of the diurnal temperature changes, making it difficult to interpret the results.

Response

Figure 6 is now made easier to read and thus we keep it as it is. In addition we put the anomalies in the supplementary material. Additional information included in the supplementary material SP2 L:21-22

4. Page 9, Line 2: You write "Dubovik Optical Properties" do you mean optical properties generated using the size distribution from Dubovik and the index of refraction that you've been using up to now (that hasn't been referenced)? It's just not clear.

Response

Restated. Now on page 7 line 10 and 14. Refractive index used comes from measurement. It is now made clear, Citation included, page 5 line 36

5. Page 12, Paragraph starting on line 28: The finding that IWV and AOD contribute approximately equally to variance in the radiative budget is by far the most interesting (and new) finding reported in the paper. Why not take a little more space to flesh this out a bit? And please include the uncertainty estimates.

Response

We agree this is an important point. Additional statement highlighting the significance of dust on controlling the radiative budget is included. Page 13 Line 4 of corrected draft.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-397/acp-2017-397-AC2-

supplement.pdf

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Discussion paper