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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.

Anonymous Referee #2

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Review of "The summertime Saharan heat low: Sensitivity of the radiation budget and atmospheric heating to water vapor and dust aerosol." by Alamirew et al.

This paper explores the sensitivity of the radiation budget within the Sharan Heat Low (SHL) to changes in water vapor and dust in order to understand the influences of each on synoptic (and potentially longer) time scales. This sensitivity analysis is carried out using observations during the intensive measurement period of the Fennec experiment during June 2011 at Bordj Badji Mokhtar in central Algeria. The main finding presented here is that dust and water vapor contribute approximately equally to variability of the SHL radiative budget.

I have some concerns with regards to the tuning of the dust in the RT model (although I just may be misunderstanding the description of the model setup). I think the authors should do more in terms of carrying their error analysis throughout the entirety of the dust and water vapor forcing analysis, and I think the paper is way too long (unnecessarily wordy and too many superfluous plots/tables). I am suggesting a major revision, but the work required to satisfy these comments is super "do-able".

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.

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.

3. RT Model: The authors state that the vertical profile of the dust mass mixing ratio is

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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?

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 subsample 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 temperature3 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.

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.

6. Forcing Efficiencies: The efficiencies reported for dust and IWV should also include the associated 95% confidence intervals from the linear regression.

7. Figures 12 and 16 aren't really all that 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.

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.

Minor Comments: 1. Figures: The individual panels of the figures should be labeled (a., b., c., . . .).

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?

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.

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.

5. Page 13, Paragraph starting on line 28: The finding that IWV and dad 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.

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