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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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Response to interactive discussion Short Comment (SC) from C. Lavaysse 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

The comments and suggestions made by C. Lavaysse are useful. We have addressed all the comments raised by the reviewer. Our responses and changes (if any) are indicated in the corrected version of the paper. We put original comment of the reviewer (typed in italic font) followed by our responses to make it easy to follow.

Major Comment a.

1. Section 3 is not clear. Quite complicated to understand all the configurations and the conclusions drawn from these results on the choice of certain parameters. Finally choices are not really justified and I am not sure if it is necessary to provide all the information. I would recommend to simplify this section and to put some results in the supplementary material.

Response

Part of section 3 has been moved to the supplementary material (Section S2). This includes all the model configuration analysis. Accordingly, Section 3 now describes the data and the design of the hypothesis testing experiments and Section 4 focuses only on the results of those experiments.

Changes Made

We have reorganized section 2 and 3 into a more clear structure. The new structure of the whole paper is as follows.

Section 1. Introduction Section 2. Description of RT model Section 3. Data and method 3.1. Observed top of atmosphere and surface radiation measurements 3.2. Atmospheric profile and surface characteristics 3.3. Dust properties and extinction profile 3.4. RT model Experiments Section 4. Results and discussions. 4.1. RT model validation 4.2. The radiative flux and heating effects of dust and water vapour 4.2.1. Dust 4.2.2. Water vapour 4.2.3. The relative effects of dust versus water vapour Section 5. Summary and Conclusions Original draft Page 6:L19-40, Page 7:L1-3, Page 7:L28-37 moved to supplementary material (section S2). See also minor comment #3.

2. In this section I also found some parts not clear: p5 l5-12; it is quite weird to compare observations assimilated with model dataset? The authors do not explain the remaining errors. Is it due to the assimilation procedure? Response We are pointing the fact that despite assimilation of the radiosonde data there remain biases in the

reanalysis. Fennec was a short-term experiment and since then there remains only one radiosonde station for the whole Sahara. As such, the reanalysis errors we derive are almost certainly much lower than those typical of the rest of the Sahara. We also now cite the errors estimated from Garcia-Carreras who compared radiosonde data to a forecast model first guess (independent of assimilation) The magnitude of errors are different among the different reanalysis products. The possible reasons for the remaining error between observation and reanalysis products could be due to differences in models core dynamics and in assimilation procedures.

Changes Made

Corrected draft Page 4. L36-38. A statement added suggesting the possible reasons for differences in error among reanalyses.

Major Comment b.

1. Section 4 is too descriptive with too much information that are not necessarily significant or important to the conclusions of this study. This is particularly true p9 and 10. I strongly recommend to reduce this part to the most important results and to put the others results into an annex.

Response

Part of section 4 has been moved to the supplementary Material (section S3), specifically sections describing the sensitivity experiments towards the model optimum configuration, as we agree these are not the key significant results. We choose to retain some of the results originally presented in pages 9-10 because we feel it is important to demonstrate that the simulated quantities of top of atmosphere radiation budgets are within the observational uncertainties. To give sense of results in subsequent sections, it is necessary to have a feeling of the surface and TOA radiative budget under the mean state.

Changes Made

Original draft page 8:L30-33, page 9:L3-8 moved to supplementary material (section S3)

2. The summary of the subsection 4.1 is too speculative. How the authors can conclude the simulated flux errors of the optimal configuration are comparable to the observational uncertainties? What does ‘acceptable’ mean?

Response

Given that we do not have accurate data for all the input required to run the RT model, it is not unexpected to get some uncertainty in our results. However we have chosen the inputs in such a way that the calculated flux are as close as possible to observation. This is what we mean by an ‘optimum’ model configuration. The optimum configuration is deemed to be ‘acceptable’ because the model error in top of atmosphere fluxes (perhaps the single most important quantity) with respect to observations is within the uncertainty in the observational estimates of those quantities. Model estimates lying within observation range is a commonly used indicator of acceptable model performance. Thus we suggested the RT model is configured to produce acceptable results and thus can be used for further experiments.

Major Comment c.

1. Some conclusions are too speculative. The authors conclude about the impacts of the dust aerosols and water vapor on the SHL but, in that study, only June 2011 is used. The SHL is the most important from end of June to mid of September (when it is installed in its Saharan location). Even if the authors used only one month (June), they have to characterize this specific year to the climatology (in term of dust, humidity, large scale forcings). This point concerns the title (‘summertime’ is not appropriate), the conclusions (p15 l8-10), and the abstract.

Response

We agree that the period of study does not coincide with the peak of the summer season when the SHL is established in its northernmost position. However, we are limited by the period of the Fennec field campaign whose data underpin our analysis. Accordingly we have changed all references to 'summertime' to 'early summer'. In addition, in Section 3.2 we note that during our study period of June 2011 the SHL underwent a rapid transition from a 'maritime phase' to a 'heat low' phase. As such our analysis actually covers the transition period and SH states characteristic of both early and high summer. We have now amended this section to include an analysis of the conditions during June 2011 with respect to the mean conditions during June.

Changes Made

References to summer changed to summertime. Figure 1 changed to show position of SHL in June, 2011. Corrected draft Page 16:L14-20. A paragraph added

2. Also the discussion on the impacts on the SHL pulsations should be carefully discussed since the authors do not analyze the contribution of the large scale temperature advections and they never show the real position of the SHL in June 2011 (in June, the SHL is migrating to the north with a large spatial variability).

Response

Real position of SHL in June is shown in Fig 1.

The comments on our reference to variability in SHL specifically the 'pulsating' of SHL intensity and the potential role of dust and water vapour feedbacks in this process is also raised by anonymous referee #1. We do feel it is important in this paper to relate the radiative heating rates derived from our RT simulations to the behaviour of the SHL, but of course recognise that the full dynamical response requires an analysis of advective heating. As such in the original paper p16 para 1 we note that radiative heating is of 'comparable magnitude' to published estimates of advective cooling from comparable monsoon surge type events. In this way we make only a broad inference about the net effects of advective and radiative terms on the SHL. We have now changed the text

slightly to emphasise the speculative nature of this inference.

Changes Made

Corrected draft Page 15:L26-28. Additional statement included. '

3. Finally at climatological scale, the authors should pay attention to the climatological evolution of the dust that tends to reduce (p15 l16)

Response

Our comment in the original draft page 15:L16 concerns other analysis which implicate long term trends in SHL temperature to that in WV, but do not include dust in their analyses. We simply aimed to point out that this should not be neglected. Our paper is not concerned with resolving long term trends in dust over the SHL so we do not include plots of long term satellite derived AOD over the SHL.

Major Comment d.

1. Some figures are not readable

Response

Unreadable figures corrected.

Minor Comments 1. P2 l11 the authors should mention this reference: Lavaysse, C., Flamant, C., Evan, A. et al. Clim Dyn (2016) 47: 3479. doi:10.1007/s00382-015-2847-z Response: Reference included, P2:L11 and reference section page 18: L32. 2. P6 l4; the two phases mentioned are not so clear. Response: These two phases are previously stated on original draft page 4:L40 and page 5:L1 3. P6 l19: title of subsection 3.2 not clear, please rephrase Response: changed to 'RT sensitivity experiments to choice of inputs', now moved to supplementary material. 4. P6 l24: optimal to what? Response: Optimal configuration means model configured to produce results closest to observations. 5. P6 l37-38; how do the authors conclude the Ceres measurements are uncertain and that explain the large RMSE? The term RMSE refers to a reference (usually observations) that are considered as the correct value. Here, I do not understand what is the reference and how they can conclude that. Please clarify. Also the term RMSD (difference) should be more appropriate. Response: We do agree with the reviewer's comment that RMSD is comparison of modeled versus observation. From the data we have CERES is considered correct, despite its limitations as with any observation, can be used to measure the error modelled variables. Changes Made: RMSE changed to RMSD in all occasions. 6. P6 l39-40: the authors provide some results without explanations, what are these results (mean =...) and please clarify the conclusions/interest of this point? Response: Rephrased, point of interest described in section 5 7. P7 subsection 3.2.2 I recommend to put the first part of the paragraph in the introduction section and the result in supplementary material. Response: Some of the information and results on optical properties of dust is now moved to section S1 of supplementary material. 8. P8 l1: Section 4.1 is correct? Response: Corrected 9. P8 l11: Is it necessary to use this acronym? Response: Acronym definitions summarized in table 2. To be consistent throughout the paper, we found it necessary to use acronym. 10. P8 l27: Section 3.1 is correct? Response: corrected, for the details look at response to Major comment a. 11. P11 l7-8: longwave and shortwave are equal Response: TOA SW DRE of dust is small, whereas LW has a net warming effect at TOA(less LW escaping out of atmosphere due to dust.) 12. P12 l36-37: The SHL is measured in between 925 and 700hPa, not at the surface. Do the authors conclude there is a cooling of the SHL intensity due to the water vapor? Response: Here we are discussing the immediate radiative effect of dust and water vapour. But the net effect may not be cooling as the feedback resulting from surface warming in the LW and thus more sensible heat flux could result in net warming of the atmosphere which needs further investigation using regional climate models that include the feedback processes. 13. Figures : For all the figures, please add the caption under the figures Response: All changes are made to the figures according to the given recommendation.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-397/acp-2017-397-AC3-supplement.pdf

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