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

This study aims at assessing the impacts of dust aerosols and water vapor on the radiation budget during June 2011. After an introduction, the authors describe the radiative transfer model used and the observations for the validation. Except few minor points, these sections are clear and well written. I have more difficulties with the section 3, which is for technical and by construction not clear at all. Section 4 describes the main results of the simulations. Most of the time, these results are very descriptive and

it is hard to follow the authors. Finally the last section sums up and concludes this study. Here, some conclusions are too speculative considering the period of study and some approximations. After a careful reading, I consider this study relevant for the journal with innovative results that bring new insights. Nevertheless, I would recommend major revisions before to accept this paper. Please find the major and minor comments below.

Major comments: a- Section 3 is not clear. It is quite complicated to understand all the configurations and the conclusions drawn from these results on the choice of certain parameters. Finally the choices are not really justified and I am not sure it is necessary to provide all the information. I would recommend to simplify this section and to put some results in supplementary material. In this section I also found some parts not clear: p5 l5-12; it is quite weird to compare observations assimilated with model datasets. The authors do not explain the remaining errors. Is it due to the assimilation procedure?

b- 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. 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?

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

d- Some figures are not readable.

Minor comments

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

P6 l4; the two phases mentioned are not so clear.

P6 l19: title of subsection 3.2 not clear, please rephrase

P6 l24: optimal to what?

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.

P6 l39-40: the authors provide some results without explanations, what are these results (mean = . . .) and please clarify the conclusions/interest of this point?

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.

P8 l1: Section 4.1 is correct?

P8 l11: Is it necessary to use this acronym?

P8 l27: Section 3.1 is correct?

P11 l7-8: longwave and shortwave are equal

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?

Figures : For all the figures, please add the caption under the figures

Fig 1: Not relevant since the study is only for June 2011. Please provide the same map during the campaign and some information relevant to characterize 2011 vs the climatology.

Fig 6: This figure is not readable. Please change to a scatter plot, more adapted.

Fig.9: I recommend to transform some panels in scatter plots

Fig. 11: I recommend to remove this figure.

Fig 17: Please modify this figure. It is not readable.

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