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Discussions
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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.

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1. This paper used field experiment data at BBM in southern Algeria from June 2011 and a radiative transfer model to calculate the effects of dust and water vapor on radiation budget both at the surface and the TOA in order to understand the radiative processes within the SHL during summer. Generally, the manuscript is straightforward and well organized. However my main concern is that some of the input data for the RT model may cause large uncertainties that are helpless to fill the research gaps as the authors mentioned in the introduction.

Response

Printer-friendly version

Discussion paper

We fully recognise the challenge of adequately constraining the input data to the RT model in this region, where observations are sparse and as a results reanalyses models have limited assimilation of observations. This is indeed a challenge and one which the Fennec project set out to address. In using Fennec data we therefore utilise the best available data for our RT simulations. Moreover, we undertake a very comprehensive analysis of the sensitivity of radiative heating to uncertainties in those input field not directly measured during Fennec. Indeed reviewer 1 felt that this model configuration section was too comprehensive to be included in the main paper! So we believe we have addressed the issue of data input uncertainty as thorough and comprehensive manner as could be reasonably expected. This is now included in the supplementary material section so as not to distract from the core hypotheses the paper sets out the test.

2. For example, dust can absorb thermal infrared radiation, the night time AOD estimated from the nephelometer, which measures aerosol extinction coefficient near the surface, could induce a large error without an accurate aerosol extinction profile.

Response

Lack of complete input data is one of the challenges in the study of radiative effect of aerosols. Because of this, there is always assumptions or approximations to overcome the arising difficulties. Using surface nephelometer measurements to estimate night time AOD will not significantly affect our result. This is because there is only LW forcing at night which is in general smaller compared with SW forcing. Besides researchers practically use uniform dust extinction profile across the boundary layer as the difference in forcing results compared with the actual extinction profile is not small. [Liao and Seinfeld 1998, Osipov et al., 2015,] We have also confirmed this through a sensitivity experiment to test the difference in LW radiative flux and heating rate when we use different daytime and nighttime extinction profile. We find a small difference less than 3 W.m-2 both at the surface and TOA. The atmospheric heating rates do not change significantly when different extinction profiles are used for day and night except small difference in the lower levels by less than 0.20 K day-1. We conclude in general that this will not affect what we wanted to show and hence the overall result of the paper.

3. Reanalysis data generally has poor representations of clouds and their properties. However, the authors selected clouds properties from the reanalysis. These could directly affect the reliability of the model results.

Response

This was also our concern at the beginning of this research work as we understand the limitations of cloud representations in models. We could have undertaken the RT experiments only in clear sky mode as many other authors choose to do. We do include clear sky only experiments but we complement these with all sky experiments to provide a more thorough and comprehensive analysis, from which we compare observations of TOA fluxes in which cloud screening is problematic. Our all sky RT experiments use what we feel is the best available 3-D information on cloud, that comes from the reanalysis models. Alternative cloud profiles for RT models simulations is not available. It is totally expected that our results will bring error due to cloud under (or mis) representation. We discuss this on Page 9: L14-20 of corrected draft and page 3:L1-5, L14-16 of supplementary material. However, we stand by our analysis not least because comparison of the errors in the all sky vs clear sky RT results actually provide some first order indication of the error on radiative budget due to underestimated cloud in reanalysis dataset. We have included a clearer and more explicit caveat regarding the limitations of the cloud fields in our experiments and note the need for further work in this area.

Changes Made

Page 3:L14-16 of supplementary material.

4. Sections 2 and 3 are a bit long. I would recommend to combine and simplify this part.

Response

This part has been restructured in a more clear way (please refer to the comment of reviewer 1, reviewer #1 Major Comment a #1.)

Changes Made

Refer to the response of reviewer #1 Major Comment a #1 for the simplified layout of the paper.

5. What the authors concluded cannot be totally supported only from the radiative forcing and heating rate calculations.

Response

Reviewer #1 also raised this comment. Please refer the responses made to reviewer #1, Major Comment C #2

6. The manuscript also need a thorough editing. Some typos and confusing expression make the text difficult to follow at times.

Response

Manuscript thoroughly read and corrections made to typos.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-397/acp-2017-397-AC4-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-397, 2017.