Reply to Anonymous Referee #1: Marsham et al., The contrasting roles of water and dust in controlling daily variations in radiative heating of the summertime Saharan Heat Low

The authors provide a comprehensive empirical observational study of relationships between water vapor, dust aerosol and radiation over the important Sahara Heat Low region. Recent research suggests that this area is of importance in determining feed-backs on climate and the regional water cycle (e.g. Evan et al. 2015 doi:10.1175/JCLI-D-14-00039.1; Dong and Sutton, 2015, doi: 10.1038/nclimate2664). The analysis, though quite simple, is very well composed and useful in assessing the key drivers of radiative energy balance in the region and use of new observations make the evaluation quite novel. I have a number of mostly minor points outlined below that I consider the authors should address before the paper is ready for publication.

We would like to thank the reviewer for their thorough and valuable review.

We now cite Dong & Sutton in the introduction,

"variations in the SHL modify the WAM on time scales from days to decades [Thorncroft and Blackburn 1999; Peyrillé and Lafore 2007; Biasutti et al., 2009, Lavaysse et al, 2009, 2010; Chauvin et al., 2010, Xue et al., 2010, Martin and Thorncroft 2014, Martin et al., 2014, Dong and Sutton, 2015]. " and

"Evan et al. (2015) suggest that the increasing temperatures within the SHL over the past 30 years, key to the recovery of the Sahel from drought, are driven by longwave impacts of increasing water vapour, in the "Saharan Water Temperature" feedback and Dong and Sutton 92015) propose a greenhouse-gas driven increase with a feedback through water vapour"

We address the reviewer's other points in turn.

GENERAL POINTS

1) Since this is an empirical study it cannot demonstrate cause and effect. Figures 2-4 show relationships between variables (not "trends" or cause/effect). Further detailed radiative transfer calculations and additional modeling is required to do so. Presuming this is beyond the scope of the study, there are a number of places where this should be stressed and the text modified accordingly (see specific points).

We no longer use the word "trend", as although it can refer to any linear relationship in physics, it is often used for changes in time in climate science.

Radiative transfer modelling is out-of-scope as the reviewer suggests, and it is challenging to account for the uncertain cloud fields using this approach. We have clarified the limitations of our method by noting its limitations in the abstract,

"Although the empirical analysis of observational data cannot completely disentangle the roles of water vapour, clouds and dust, the analysis demonstrates that TCWV provides a far stronger control on TOA net radiation, and so the net heating of the earth-atmosphere system, than AOD does. In contrast, variations in dust provide a much stronger control on surface heating, but the decreased surface heating associated with dust is largely compensated by increased atmospheric heating, and so dust control on net TOA radiation is weak.", at the end of the introduction,

"In this paper we use observations of surface radiative fluxes from Fennec and retrievals of TOA fluxes from satellite data to investigate how dust and water together control the day-to-day variations in energy balance over the Fennec supersite-1 in the summertime SHL region, and how this is represented in ERA-Interim (ERA-I) reanalysis. Results in Section 3 show that TCWV and AOD are correlated and we cannot completely isolate the effects of either TCWV or dust. However, TCWV and AOD have sufficiently independent variations, and sufficiently distinct impacts at solar and infrared wavelengths, which conform with physical principles, that the results give unique insights into their contrasting roles in the central Sahara.",

at the start of the results,

"In order to determine how the changing amounts of water and dust over BBM affect the changing radiative heating at the surface, TOA and within the atmosphere we analyse relationships …" and "There are correlations between dust and water (discussed below) which mean that effects of either cannot be completely isolated from the other, but nevertheless the approach allows identification of how variations in these variables affect radiative heating.",

in the discussion,

"Although modelling is needed to fully understand the observed effects of water vapour on the radiation"

and this is already discussed at the start of the conclusions,

"Although there are limits to the extent to which our empirical approach can disentangle the roles of dust, cloud and water vapour, largely due to correlations between these factors, the results provide new insight into their roles in controlling the radiative balance of the unique environment of the central Sahara (schematic in Figure 5)."

We also made other have changes to the text that clarify our approach and what we infer. In the results,

"At the surface there is a strong and significant decrease in net radiation with increasing AOD (Figure 3b) with a regression coefficient of -13.1 W m^{-2} per AOD".(new with-bold-font page 9 line 26)

"Decreases in surface heating associated with dust are largely compensated by direct radiative heating of the atmosphere"(new with-bold-font page 11 line 2)

And in the conclusions,

"However, variations in water vapour (and associated variables such as temperature and cloud) and not variations in dust dominates day-to-day variability of TOA net radiation"

“At the surface, dust (and associated water vapour and cloud) decreases net surface radiation in reality by around 13 W m^{-2} per AOD.”

“If effects from TCWV were simply due to correlated changes in AOD, or visa versa, these contrasting roles of TCWV and AOD at the TOA and surface would not be so distinct.”

Please also see responses to specific points below.

2) How representative is 2011 and 2012 of the regional climatology. Some further analysis or links to previous work would help in answering this.

For a 2 degree box centred on BBM for June 2011 and june2012 the standardised AOD anomalies from MISR, Deep blue (Terra and Aqua) and OMI are all within one standard-deviation of the long term mean. Water vapour mixing ratios at 850 and 925 hPa from analyses are also within one standard-deviation of the long-term mean, so conditions at BBM in both 2011 and 2012 are not ‘anomalous’

This is now noted in the first paragraph of the results,

“Similarly, for both June 2011 and 2012 analysed water vapour at 850 and 925 hPa and AODs from MISR, Deep blue (Terra and Aqua) and OMI are all within one standard deviation of their mean values (not shown) and there is no indication that the weather regimes affecting BBM in these periods were anomalous.”

3) It would be beneficial to consider or at least mention the CERES radiation data. The SYN product can provide daily averaged fluxes based upon satellite overpasses and geostationary diurnal cycle "shape". There are also estimates of surface and atmospheric fluxes that require the combination of reanalysis and additional satellite data with CERES measurements.

Given the large errors in reanalyses in the region (e.g. Marsham et al. 2011; Garcia-Carreras et al., 2013; Roberts et al., 2015) and the challenges of capturing Saharan cloud (Roehrig et al., 2013; Stein et al., 2015, both now cited in final lines of the paper) we think that for the aims of this paper it is preferable to use observed surface fluxes rather than estimates based on combinations of satellite data and analyses. We do not believe that uncertainty in TOA fluxes is the major limitation of this study (it is rather the empirical observation-based approach as noted by the reviewer) so we do not think that CERES TOA fluxes will significantly improve the paper.

4) There is some good evaluation of ERA Interim (e.g. p.19459-60). It would be useful to also consider work that has included model simulations in which the effects of dust are included (e.g. Allan et al. 2011, doi: 10.1002/qj.717).

We now put our results in the context of Allan et al., (2011) in the discussion,

“or the 20 to 40 W m^{-2} model bias that Allan et al. (2011) show can be removed by the inclusion of dust” (new with-bold-fontpage 11 line 29)

5) Given the strong influence of cloud on radiative fluxes and the co-variation between cloud, AOD and TCWV implied in the present work a more detailed analysis of these co-variations and influences of cloud would be beneficial.

The paper is an observationally-based evaluation of the roles of water and dust in the surface and TOA energy balance in the summertime Sahara, comparing unique new observations and ERA-I. Determining the role of clouds is challenging and there is a limit as to how far examining co-variations in the data will take us in this regard, especially as there is a shortage of relevant data for clouds (and as noted detecting small clouds over the bright desert is challenging). Radiative transfer modelling would be needed to further disentangle effects and as noted by the reviewer this is out of scope of this study. Rather, we see the role of this study is to demonstrate the contrasting roles of dust and water vapour, and motivate further study as the reviewer suggests, and as noted in the final lines of the paper.

6) In places the meaning of net fluxes or heating/cooling are potentially ambiguous (e.g. p.19458). It should be stated clearly if net fluxes are defined as downward and whether increased net downward fluxes correspond to an increased heating (SW) or reduced cooling (LW).

This has been clarified,

" Figure 3e (gradient -1.1) shows that at the surface in ERA-I, unlike in observations, decreased net shortwave is always compensated by increased net longwave (i.e. reduced longwave cooling)." (new with-bold-font page 10 line 3)

We now state in the first results paragraph that,

"Net fluxes are defined as downward, with increased net downward flux corresponding to increased shortwave heating or reduced longwave cooling."

SPECIFIC CHANGES

p.19448, L6 - please provide information on the site location (abstract and also in the Introduction)

We now state,

"observations from Fennec supersite-1 in the central Sahara during June 2011 and June 2012" in the abstract and

"observations of surface radiative fluxes from Fennec supersite-1 in the central Sahara" in the introduction.

The latitude and longitude of the site are in the methods, which we believe any reader who wants a precise location will look for it, and here we also now describe the location,

"We use data from Fennec supersite-1 in the central Sahara, located at Bordj-Badji Mokhtar (BBM) at 21.4N 0.9E (in the very south of Algeria, close to the triple point of Algeria, Mali and Niger), close to the SHL's climatological centre…"

p.194448, L11 (abstract) - it is not necessarily TCWV which is driving these changes as it may be clouds associated with the TCWV variability.

Clouds are likely associated with TCWV as you say, but so is dust, but results show it is the TCWV that controls TOA net radiation far more than the dust AOD. It is difficult to explain the full detail succinctly in the abstract, but we have clarified by stating

“Although the empirical analysis of observational data cannot completely disentangle the roles of water vapour, clouds and dust, the analysis demonstrates that TCWV provides a far stronger control on TOA net radiation, and so the net heating of the earth-atmosphere system, than AOD does”

p.19460, line 3-6 (Section 3.3) - it is not correct to say that increased LW heating is expected with increased water vapor and clouds as this depends very much upon the altitude (low clouds or moisture will increase longwave radiative cooling to the surface)

Thank you for pointing this out – this statement has been removed.

Table 1 - please check units. Does AOD:TOA Net mean dAOD/dNet (Wm-2)-1?

No, gradients are from graphs in subsequent figures so are dRadiation/dAOD so units are correct

p.19450 - do inadequacies in model simulation of dust mean that responses of the hydrological cycle are questionable (e.g. Dong and Sutton, 2015, doi: 10.1038/nclimate2664)?

I do not think we can answer that question in this paper, but as in the final paragraph of the conclusions or work highlights the importance of models capturing water, clouds and moist convection in this region (as well as dust). We now cite Dong and Sutton (2015) in our introduction.

p.19451 - MPEF is a simple IR-based cloud product which may miss low cloud so some further justification or explanation is required to justify its use.

Clouds over the Sahara form at the top of the Saharan convective boundary layer at around 5-km (Cuesta et al., 2009) and confirmed by observations from Fennec aircraft, so we do not think there is a problem with low clouds. This also is the only flag that is available at the spatial/temporal scale of the GERB HR product that gives some measure of cloud presence throughout the diurnal cycle.

SECT 3.1 - "Figure 2a shows that water vapour warms the atmosphere, with a trend in TOA net radiation with TCWV of +2.2Wkg-1." This is not strictly incorrect. Figure 2a shows that net downward radiation at the top of the atmosphere increases with TCWV.

It is not a "trend" but a relationship and cause and effect is not demonstrated for which radiative transfer calculations or other modeling would be required.

Although “trend” can be used for any linear fit in physical science, we understand that in climate science it is often used for changes with time, so we now avoid using the word “trend” throughout the paper. To clarify this sentence we now state,

“Figure 2a shows that TOA net downward radiation increases with TCWV, with a regression coefficient of +2.2 W kg^{-1}.”,

which is consistent with the new sentence at the start of our methods that states, “Net fluxes are defined as downward, with increased net downward flux corresponding to increased shortwave heating or reduced longwave cooling.”

p.19453, L24 - remove 1st ","

Done

p.19455, L4 - remove "presumably"

Done

p.19455, L12: relationship not a trend (also p.19457, L8; p.19459, L21; p.19460, L16)

Corrected.

“The increase in net TOA radiation with AOD occurs because the increase in TOA longwave (+10.5 W m^{-2} per AOD) dominates the decrease TOA net shortwave (-5.2 W m^{-2} per AOD; Figures 2e and 2h).”

“However, in ERA-I the underestimation of the magnitude of the regression coefficient of TOA net longwave with TCWV”

“There are significant increases in net shortwave and net longwave radiative heating of the atmosphere with increasing TCWV (Figures 4d and 4g, Table 1).”

“ERA has a significant positive increase in shortwave atmospheric heating with TCWV (Figure 4i, 0.91 W kg^{-1}) from absorption by water”

p.19455, L17 - "shortwave cooling" is misleading as it is reduced shortwave heating

Corrected to “Therefore the observed reduced shortwave heating associated”

p.19457, L13 (Sect 3.2) - again a relationship (not a trend) is shown and so a "control" on net radiation by AOD changes has not been demonstrated

Updated to “At the surface there is a strong and significant decrease in net radiation with increasing AOD (Figure 3b) with a regression coefficient of -13.1 W m^{-2} per AOD”

p.19458, line 14-19 - I was slightly unsure about where the PCA analysis fits in and was confused about this discussion which seems to suggest AOD and TCWV both increase together in mode 1 but are anti-correlated in mode 2. What physically do these modes represent?

PCA modes do not have to represent anything physically, but explain most of the variance. The PCA has, however, been removed to aid clarity.

p.19458, L25 - does "greater net surface longwave" mean that net downward surface longwave becomes less negative?

Yes. The convention we follow is now described at the start of the results,

“Net fluxes are defined as downward, with increased net downward flux corresponding to increased shortwave heating or reduced longwave cooling.” and this sentence is clarified to, “This occurs since in ERA-I greater water vapour leads to greater net surface longwave (i.e. reduced longwave cooling, Fig. 3f),”

p.19459 - the influence of dust aerosol on atmospheric net radiative cooling is also discussed by Slingo et al. (2006) doi:10.1029/2006GL027869 and Slingo et al. (2009), doi:10.1029/2008JD010497.

Added, “This is consistent with the results of Slingo et al. (2006) and Slingo et al., (2009) for dust over the Sahel.”

p.19460, L3 - "The increase in net longwave heating with TCWV is expected due to the warming from both water vapour and clouds." This is not precise since the longwave changes depend very much on the altitude of water vapor (e.g. Previdi 2010 doi:10.1088/1748-9326/5/2/025211) and cloud. Increased low level cloud or water vapor will increase atmospheric radiative cooling to the surface but influence the TOA only marginally.

As noted above, this has been removed.

p.19460, L5-6 - please check this sentence and also reference Fig. 1i on L9

We have corrected the reference to Section 3.3.1 and now reference figure 4i as suggested.

p.19461, L3: "errors"; L5-7 the altitude of water vapor is important (changes in mid and upper tropospheric humidity are rather important for TOA clear-sky longwave)

Corrected to “Small errors in TCWV, in the altitude of the water vapour, or in associated cloud, could cause errors in clear-sky longwave radiation…”

p.19462, L6-10 - this is an interesting discussion but it should be caveatted by the need for radiative transfer calculations or additional modeling to confirm cause and effect.

Added,

“Although modelling is needed to fully understand the observed effects of water vapour on the radiation, the observations show that monsoon surges at BBM are expected to have significant effects on radiative heating rates. In June 2011 BBM experienced ….”

p.19462, L21 - please define ITD

“Inter Tropical Discontinuity” added.

p.19463, L6-7 - I suggest "due to longwave radiative cooling that is partially offset by shortwave radiative heating"

We have kept the original text, as the longwave cooling is more than offset by the shortwave warming at TOA and the surface to give net heating.

p.19463, L14 - TCWV may be associated with daily fluctuations in TOA radiation but could this be through co-variability in temperature and cloud

Amended to,

“However, variations in water vapour (and associated variables such as temperature and cloud) and not variations in dust dominates day-to-day variability of TOA net radiation”

p.19463, L27 - is there a reference for the ERA-I underestimation in cloud (also next page L23)?

We have added a reference where cloud bias is discussed, “These comparisons with data both support the hypothesis that ERA-I underestimates cloud cover (consistent with Dolinar et al (2015) Figure 4).”

p.19464, L4 - although the effect of TCWV is weak overall there is a strong physically robust influence on surface net longwave which could be stressed here

Added, “Although increasing TCWV reduces the surface longwave cooling, the effect of TCWV on the net surface radiation is weak, variable and a subtle balance between the competing effects of water vapour, clouds and dust (-0.2 W kg^{-1}).”

p.19465, L3 "it is important that"

Added

p.19463-5 - can the energy advection be implied from these results?

The TOA net heating suggests a balancing advective cooling, but there can be significant heat gain/loss in the system on these time-scales, and we prefer not to discuss here as we cannot say where the advection is occurring.

Figure 1 is a bit small

This has been made larger, with many figure moved to supplementary material, at the suggestion of another reviewer.

Figure 5 is a nice idea - I think it could have more impact to simply show a moist dusty and dry clear profile in a 2-panel figure

We would prefer to keep the four panels, as we wish to separate the effects of TCWV and AOD (as much as we can from our approach) and although dust and TCWV are correlated, no all moist atmopsheres are dusty, or all dusty atmospheres moist.

Figure 4 - "convergence" in the y-axis title is potentially misleading and should be changed to radiative convergence/divergence or heating/cooling

If “convergence/divergence” (or “heating/cooling”) is used the reader does not know the sign convention. A convergence of radiative flux gives a heating, so as it is it is clear that the negative values are divergence. The axes are defined in the first line of the relevant section (Section 3.3), “The TOA and surface fluxes are differenced to give the radiative flux convergence within the atmosphere, *i.e*. the direct radiative heating of the atmosphere (Figure 4).”