<u>Reply to review of "The contrasting roles of water and dust in controlling daily variations in radiative heating of the summertime Saharan Heat Low" by Marsham et al. by Amato Evan</u>

This manuscript uses observations from the Fennec campaign during two summers to investigate the relative roles of total column water vapour (TCWV) and dust in controlling radiative fluxes over the SHL. While I think the data set is an interesting one, I find the paper to be unpublishable in its current form. Most importantly, I think the analysis has one important error that may be leading the authors to make somewhat erroneous conclusions. Furthermore, I find the organization of the paper to be burdensome, with an excess of plots and even improper (or at least odd) use of terminology. Along those lines, the main aim of the paper is not consistent throughout; it seems to vacillate between being a heat budget analysis, an analysis of the influence of dust and TCWV on observations of radiative fluxes, and a comparison between observations and ERAI, but none is truly carried out fully. I recommend major revisions.

We believe that the reviewer has misunderstood our aims. We apologise that our approach was not clear and have now clearly described our aims and methodology in the paper, as described below, to avoid such misunderstandings.

We do not attempt to isolate effects of either TCWV or AOD. 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. We do not aim to determine the sensitivity of fluxes to TCWV alone, or AOD alone, rather to evaluate their contrasting roles in determining day-to-day variability. Although we cannot isolate the effects of TCWV and AOD, the results still provide unique insights. The roles of TCWV and AOD are sufficiently distinct that they can be distinguished despite the correlations between them: the correlation between TCWV and TOA net flux is much stronger than for AOD and TOA net flux, but the reverse is true at the surface. This would not be the case if effects of TWCWV were simply due to associated AOD, or visa versa. We do not attempt a heat-budget analysis, but there is discussion of the implications of our results for the heat budget in the discussion section, clearly separated from the results (as note din the paper "The results give some insight into the Saharan BL energy budget"). Although we cannot analyse all causes of errors in ERA-I, the observational data provide an important and unique check on the analyses in this important region, showing how well they capture the observed relationships, which has important implications.

To avoid such misunderstandings of our aims and conclusions, we now clarify these at the end of the introduction,

"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.", 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”,

and in the conclusions,

“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.”

We have made further changes to clarify our aims, our methodology and its limitations as noted under major comment 2 below.

We have reduced the number of plots and no longer use the word “trend” as although we do not think its use was improper, it can clearly mislead some readers.

Major comments

1. In Figure 1a the authors show that TCWV and AOD are correlated. In fact, I think the correlation between the two variables will be much higher if they remove the data points containing the “interpolated” flux measurements; these interpolated data points are largely outliers in the scatter plot.

We wish to use as much data as we can to capture of much of the variability of the natural system we are observing as possible. TCWV and AOD are measured from radiosondes and the Cimel sun-photometer, and so are measured independently of surface flux data and therefore unaffected by any interpolation of the surface flux data. Omitting TCWVs and AODs from days when surface-flux data required interpolation would be misleading and is therefore not justified. Furthermore, the paper notes how the behaviour of the interpolated surface fluxes relationship with TCWV and AODs are physically consistent with the other un-interpolated data.

In the subsequent analysis (Figs 2–5) the authors attempt to quantify the effects of TCWV and AOD on LW & SW radiative fluxes via linear regression. However, since TCWV and AOD are correlated, the linear regressions do not isolate the effect of, for example, TCWV on SW fluxes at the TOA. Rather, they give us the sensitivity of TOA SW fluxes to TCWV + the component of dust (AOD) that is correlated with TCWV. This error is basically carried throughout the entire paper, and may be one of the main reasons why the sensitivity of fluxes to TCWV is much smaller in the ERAI data than in the observations.

If the authors want to determine the sensitivity of fluxes to TCWV alone, or AOD alone, then they must modify their statistical approach, or perhaps use a radiative transfer model (*e.g.*, STREAMER in Evan et al. 2015, J. Clim.).

As noted above the reviewer has misunderstood our aims, we do not aim to isolate the effects of TCWV or AOD, and we have clarified these in the paper (see above). Although we cannot determine the sensitivity of fluxes to TCWV or AOD alone the results reveal their contrasting roles and the conclusions are novel and well supported. Radiative transfer modelling would be needed to fully disentangle effects (and for clouds this is complex and there is a shortage of data) and this is out of scope as noted by other reviewers. This observationally-based study will provide motivation for future model studies to test the hypotheses raised.

To avoid such misunderstandings of our aims and conclusions, in addition to the changes noted above in reply to the reviewer's first comments, we now also clarify these 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 8 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.”

As the reviewer rightly points out associations between TCWV and AOD may explain why the sensitivity of fluxes to TCWV is much smaller in ERA-I than in the observations, but this is noted in the paper, “The differences in the effects of TCWV in ERA-I and in observations are likely because of both errors in clouds in ERA-I and its lack of variability in dust” (We also note that there are numerous other places in the original paper where the importance of correlations between TCWV And ANOD are noted, e.g. “Impacts of TCWV on surface net heating are therefore a subtle balance of water vapour, clouds and associated dust”, “The underestimate of the longwave effect of TCWV at TOA in ERA-I is consistent with this suspected underestimation of cloud cover in ERA-I and also the lack of dust associated with TCWV”, “ the decrease in net shortwave with increased water vapour (-0.98 W kg^{-1}, Figure 2g), due to water vapour and associated clouds and dust.”, “much of the shortwave effects of TCWV are indirect, occurring via associated clouds and dust.”, “some of the observed trends with AOD are due to associated water vapour and cloud”, “*i.e*. dust, together with the water vapour and cloud associated with the dust, warms the surface in the longwave”, “ERA is of course lacking the variability in dust that correlates with TCWV”).

2.The purpose of the PCA is not clear (this is not explicitly indicated in the manuscript), and it’s difficult to determine exactly how the PCA was applied (also not explicit in the manuscript). If the PCA is important, why not dedicate a figure showing the PC time series and a table indicating the PC loadings for the various time series (it would be nicer for the reader to have these #s in a table rather than having to search through the paragraph to find relevant sign changes). Also, was the interpolated data included in the PCA? If so, are the PCA results changed if the interpolated data is not included?

The PCA results are revisited on page 19458, where it is stated that the results from the linear regressions are consistent with the PCA analysis. But here the authors are only reiterating that in the scatterplots the net surface flux is negatively correlated with dust and weakly correlated with TCWV, and that at TOA, TCWV is positively correlated with TCWV and weakly correlated with dust? Why do we need a PCA if we are only summarizing a subset of the scatterplots? I just don't see any scientific understanding added by the PCA, as it stands.

The authors found PCA a useful way to summarise the key modes of variability and their importance. They have however been removed as they are not essential to our conclusions and this simplifies the manuscript as the reviewer suggests.

3.Some of the text in the results sections is a bit confusing. For example, the authors write (P 19455, L 27), "*Daily variations in SW are anti---correlated with variations in LW such that as daily net TOA SW decreases, the net LW increases.*" The authors are simply stating that LW cooling balances SW heating. But is this surprising? Did the authors not expect this to be the case? It just feels like stating the obvious for no clear reason.

Shortwave heating does not have to balance longwave cooling on the time-scale of one day and the observations show that although, as expected, it does to a great extent, it does not completely. This is important and explored in the next sentences (discussed below).

On the next line, "*...decreased SW tends to lead to an increase in net heating due to the corresponding greater increase in LW*". I have spent some time trying to wrap my head around this statement, and I just can't make sense of what the authors are arguing here. As the downwelling solar insolation gets smaller, the radiative imbalance gets larger, and the upward LW radiation at the TOA gets smaller. Are the authors arguing that the net heating of the atmosphere is only a function of SW down? Surely other processes (thermodynamic and dynamic) are limiting the net heating? Are the authors assuming that net heating and net radiative heating of the atmosphere is the same thing?

We are sorry that our wording was not clear and we believe our argument has been misunderstood. We are not arguing as proposed above; the words "lead to" have probably caused this misunderstanding.

A multitude of factors affect daily-mean TOA net SW and LW over the BBM site in summer: the temperature and humidity profile, the dust profile, the cloud profile, and how these vary through the day. These factors are, as the reviewer notes, correlated and the net result of these competing effects is not obvious and has not previously been measured in the remote central Sahara; it might, for example, be hypothesised that days with extensive cloud cover and so reduced TOA net SW would have reduced TOA net, but we show that in our dataset the reverse is true, as on days with reduced TOA net SW there is a more-than-compensating increase in net TOA LW. Interestingly ERA captures this relationship at TOA but not at the surface.

To clarify this we now state,

“The observed gradient is -1.4, *i.e.* days with net shortwave reduced by combinations of dust and cloud are associated with increased longwave heating (i.e. reduced longwave cooling) from the water vapour, dust and cloud that more than compensates for the decreased shortwave heating, resulting in greater net heating on these days.” (new with-bold-font page 7 line 19)

Afterwards the authors write, “*As such, TOA daily variability at BBM is influenced more by variability in the LW than the SW.*” I don’t understand the justification for this statement. LW cooling is a response to SW heating. The two are coupled, and I don’t see how one can so cleanly disentangle them via the analysis presented here.

Again we believe the reviewer has misunderstood our reasoning. The two are coupled, but, on the time-scale of a day, for example: a large increase in water vapour will, without clouds, have a greater effect on net longwave than net shortwave, warming the system; brightening the land-surface would reduce net shortwave and not affect the surface emissivity, cooling the system. We have rephrased to avoid confusion we now state,

“Figure 1b shows how there is greater variance in daily longwave cooling than shortwave warming and therefore, although they are coupled, variations in longwave cooling make the larger contribution to variations in TOA net radiation.”

1. The authors discuss the role clouds play in discrepancies in the regression coefficients between obs and ERAI (P 19456), “*The underestimate of the longwave effect of TCWV at TOA in ERA---I is consistent with this suspected underestimation of cloud cover in ERA---I...*” I’m not entirely clear what the “longwave effect” is referring to. Is this the sensitivity of OLR to solar insolation? If so, then I find this argument troubling precisely because the authors had previously stated that the time series of observed and ERAI cloud were highly correlated. I would think that the regression coefficient would not be sensitive to the cloudiness mean state; the offset would be sensitive to the mean state, but not the slope of the best---fit line. Furthermore, the last line in this paragraph, about the “magnitude of the trends” in OLR, etc... seems to have very little to do with the discussion of the clouds (and dust).

This has clearly been misunderstood, so we have now clarified,

“The underestimate of the regression coefficient of TOA net longwave with TCWV in ERA-I compared with observations(1.8 compared with 3.2 W kg^{-1}) is consistent with this suspected underestimation of cloud cover in ERA-I and also the lack of dust associated with TCWV reducing outgoing longwave (Haywood et al., 2005).”. This means that the last line,

“However, in ERA-I the underestimation of the magnitude of the regression coefficient of TOA net longwave with TCWV (1.8 compared with 3.2 W kg^{-1}) and shortwave with TCWV (-0.48 compared with -0.98 Wm^{-2}) compensate to some extent give a trend in TOA net radiation with TCWV of 1.3 W kg-1 in ERA-I, close to the 2.2 W kg-1 observed.”

is in a logical place and directly follows on from the preceding statements.

Lastly, there are way too many plots in this paper. Between figures 2–4 there are 29 scatterplots!!! Does the reader really need to go through 29 scatter plots when the only real message coming from them is that surface flux variability is strongly dependent on dust concentrations, and TOA flux variability is strongly dependent on TCWV variability (and that these two features are weaker in ERAI). I think I could show that in... 2 scatter plots. This multitude of plots is particularly unnecessary given the very nice summary schematic in Figure 5. Reducing the number of plots will help to clarify the message and make the paper more readable. If you want to showcase the Fennec observations, just put the excess plots online somewhere or in a supplement.

Since TCWV and AOD are correlated it is important to examine the changes in both shortwave and longwave fluxes with each, as well as in net fluxes, in order to reach robust conclusions, and we therefore included all plots in the submitted paper. The correlations and regression coefficients from all plots are, however in Table 1, and although other reviewers did not comment on this, we have moved many of the plots to 'Supplementary Material'.

Minor Comments

1. **With regards to the effect of TCWV on surface radiative fluxes, it would be nice to compare your numbers with those presented for Tamanrasset in Evan et al. (2015, J. Clim.).**

   This has been added,

   "The observed increase in surface net longwave with TCWV of 2.0 W kg^{-1} is within the range of 1.0 to 3.0 W kg^{-1} obtained by Evan et al. (2015) for Tamanrasset from observations, analyses and radiative transfer modelling. In summer at Tamanrasset TCWV might be expected to correlate with AOD as it does at BBM, and dust and clouds associated with TCWV in reality, but missing or under-estimated in analyses and radiative transfer modelling, may account for the greater sensitivity of surface net longwave to TCWV in observations compared with radiative transfer modelling and analyses, noted by Evan et al. (2015). The BBM value of 2.0 W kg^{-1} is slightly lower than the diurnal-mean observational value of 3.0 Wkg^{-1} for Tamanrasset obtained by Evan et al. (2015), which may reflect the greater prevalence of clouds at the high-altitude Tamanrasset site, where mountains trigger moist convection (Birch et al., 2012). The BBM results also suggest that although the increases in net surface longwave with TCWV shown by Evan et al. (2015) could largely be compensated by coincident decreases in net surface shortwave (as at BBM), this is not expected at TOA, supporting Evan et al. (2015)'s proposed role of water vapour in warming the SHL."

   Thank you for suggesting this, it helps put our results in a wider context.

2. **The word "trend" is improperly used throughout the manuscript. A "trend" implies some linear change in a time series (at the very least this is common usage in our field), but here the word "trend" is confusingly used to describe a "regression coefficient". More appropriate terms would be *regression coefficient*, *sensitivity*, or *slope of the linear regression*.**

A trend does not have to imply a change with time in physical science and is widely used for any linear relationship. In climate science it is often used for changes with time, so we have now avoided using “trend” in this context and use “regression coefficient” .

3. **The text in the scatterplots is too small to read (and it’s nearly impossible to differentiate the asterisks from the crosses). Also, it would be appropriate to include mention of statistical significance of those regression lines. This will allow the authors to objectively evaluate which fluxes have a dependency on dust or TCWV.**

The symbols have been changed and some plots made larger, so that all plots are clear. As noted in caption to Table 1 and stated in the first paragraph of the results, “bold values are significant at 90 % level”.