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.

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 un-publishable 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.

"Signed,"
Amato Evan

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. 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.).

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.

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.

   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?

   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.

4. 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).

5. 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.

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.).

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*.

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.