Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-975-RC1, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A Global Analysis of Dust Diurnal Variability Using CATS Observations" *by* Yan Yu et al.

Anonymous Referee #2

Received and published: 5 December 2019

General comments:

Characterizing any possible diurnal variation of different aerosol species in the atmosphere on a global basis is clearly an important project and can only be achieved by satellite measurements. However because of orbital restrictions and coverage issues most current satellite instruments lack this capability. CATS lidar that flew on the International Space Station between February 2015 and October 2017 was the only one which could provide diurnal information after suitable aggregation of the data and this has been one of the main planks of the CATS mission. This paper attempts to characterize the diurnal variability of dust globally using CATS data. The introduction is well written and lays out the scope and objectives of the paper in a structured way. However the analysis that follows does not provide evidence of diurnal variability of

dust from these measurements in a clear manner. In fact some of the aerosol diurnal variability using CATS data were already presented in Lee et al. (2019) which included North Africa and Middle East with dust being the dominant species. However, the very recent paper by Pauly et al. (2019) point out the high daytime lidar calibration uncertainty at 1064 nm (16-18%) with a corresponding uncertainty of ∼21% in daytime total attenuated backscatter, which is significantly larger than the uncertainty in the nighttime total attenuated backscatter at the same wavelength (∼7%). The day and night extinction profiles and optical depths will be impacted by these differences. Any meaningful discussion of diurnal variation in CATS aerosol data should clearly address this issue. The authors discussed the day/night data issue by comparing with AERONET data, which is not convincing (see below) and do not even mention the daytime calibration issues as discussed in Pauly et al. (2019). The presentation of the diurnal variability using time vectors is interesting but does not clearly establish the full diurnal variability. The vertical information content from the lidar measurements in characterizing the diurnal variability has also been ignored. I was also frustrated by the frequent references to "hypothetical" meteorological drivers despite the claim of "underlying meteorological processes are discussed in detail". Overall, I am not quite convinced by the analysis and regret that I am unable to recommend publication of this manuscript in its present form in ACP.

Specific comments:

1. More details should be given about the CATS data for the sake of completeness and for the reader who may not be initiated into the lidar terminology. For instance define the depolarization ratio and meaning of the various QA terms. How is "dust" classified and what does "dust mixture" refer toâĂŤis the latter like the "polluted dust" in CALIPSO terminology? It might even be nice to present a browse image of a dust plume as captured by CATS and/or an extinction profile to set the context.

2. I am a bit surprised that the authors do not attempt to use the CALIPSO lidar data in their analysis. Dust retrieval is probably one of the best products from CALIPSO

measurements. Even if CALIPSO reports only at 1:30 and 13:30 hrs local time, it should be useful to compare the CATS dust profiles at those local times. This was done, for instance, by Noel et al. (2019) in their study of cloud diurnal variation using CATS data.

3. More details of the figures should be given in the text. For instance, it is not clear if the supplemental Figure 1 (also Figure 4) is for a specific year or climatology. Similarly the presentation of maximum/minimum DAOD in Figures 5-10 could be clarified in the text and not just in the caption to the figures.

4. The comparison between CATS and AERONET data in Figure 1 is intriguing. As the authors themselves point out, the nighttime AERONET data are not quality assured. The sample size in the nighttime is quite low compared to the daytime. In particular, the nighttime sampling is very sparse over the dust belt and this paper is eventually concerned with the dust diurnal variability. As mentioned above, the significantly lower SNR in the daytime data and the high uncertainty in the daytime calibration (Pauly et al., 2019) are issues that should be clearly addressed in the context of day/night data quality and how they impact the diurnal variability of DAOD.

5. I do not understand the point of presenting Figure 3. The authors simply show 550 nm DAOD from MISR and 1064 nm DAOD from CATS without any attempt to convert the two datasets to the same wavelength, even if the purpose is to compare only the general spatial pattern. In a similar study using CATS aerosol data, Lee et al. (2019) had converted the MODIS data to 1064 nm using an Angstrom exponent. Once again this analysis could be done using the CALIPSO data as well. Besides, the correlation coefficients (less than 0.4 in all seasons) hardly bolster the authors' argument. This figure is essentially a comparison of daytime data and once again the data quality issues come to mind. I am also curious as to why CATS and MISR both show significant dust plumes over the South Atlantic region in the biomass burning season in southern Africa (JJA/SON)? If this dust was generated over the land, I would have expected to see dust also over the source regions in the land and a corresponding gradient from

land to ocean. Is there a scope for misclassification of smoke by any chance? Why do we see so much dust at the highest southern latitudes in the MISR data in SON (less so in the CATS data)?

6. The color bar used for Figure 4 does not show any dust loading over the north western US during the majority of the time windows, presumably because DAOD is less than 0.1 – this can confuse things as the authors later discuss diurnal variability over this region. Similarly I do not see much dust loading over Australia in this Figure. A different color bar should be used.

7. Figures 5-10 are presented as evidence of the diurnal variability in various regions of the globe with the vectors giving the times of maximum and minimum dust loading. This seems like an interesting way to present this, but not particularly convincingâĂŤoften they show multiple times of maximum within the same box and do not provide a sense of the full diurnal variability in a quantitative way. The boxes should be labelled within the plots for easy readability and given for both maximum and minimum plots. In fact a regular plot showing the DAOD as a function of the local time and a discussion vis-a-vis the day/night difference in data quality would be more convincing. In all cases, some supporting evidence from primary meteorological drivers should be presented in a quantitative way, rather than simply hypothesizing. In Figure 6 the box over Thar desert seems to cover much of central India including parts of the Indo Gangetic basin, which is misleading. What about the Gobi desert?

8. I believe this paper can be improved by delineating the diurnal variation in different seasons. For instance, this might reveal any diurnal variation over the Thar desert area during the boreal summer monsoon season when local dust sources dominate rather than transported dust from West Asian sources, as stated in section 3.3.2. As well, the vertical information available from CATS lidar should be exploited to discuss the altitude of maximum diurnal variation (e.g. Lee et al., 2019).

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-975,

2019.

C5