

## ***Interactive comment on “Characterization and application of artificial light sources for nighttime aerosol optical depth retrievals using the VIIRS Day/Night Band” by Jianglong Zhang et al.***

### **Anonymous Referee #1**

Received and published: 22 January 2019

This is the third paper from this group investigating the possibilities of deriving aerosol optical thickness at night from the VIIRS DNB. Interestingly I also reviewed the first paper in this series from 2013. I did not review or read the second paper in the series (McHardy et al., 2015) until I accepted this review. The group is making progress, but progress is slow. I would have hoped that they would have offered more than an incremental improvement to their 2015 paper, which is what we have here, and for awhile I was trying to determine whether this increment was sufficiently novel to warrant publication.

In this installment the authors implement a retrieval developed in the 2015 paper that

C1

uses the blurring of the spatial variability of light emitting city pixels at night to derive aerosol optical thickness (AOT). This is different from the paper that I reviewed in 2013 that used the contrast between the lit cities and the surrounding dark countryside. The idea of linking AOT to reduced sharpness of the image is old, and I am thinking of Tanré and Legrand (1991). The goal of a night time AOT from an imager is fundamentally ambitious. Everything from cloud clearing to the instability of artificial lights to the impossibility of collocating retrievals with current AERONET observations is working against you. Yet, the pay off is extremely rich, and as an aerosol product user, I am very happy that this group has not given up hope.

The manuscript as it stands now could use some scrubbing to make it easier to understand, and I will make some recommendations. In some places the authors have made things complicated when simplicity should suffice. In particular the derivation of the cloud/aerosol free spatial variability measure for each target is complicated. It would also be helpful to start to quantify AOT accuracy using this method. Previous papers made that attempt, but here there was nothing.

Overall the technique and implementation are sound, the presentation is sufficiently clean and the results sufficiently novel for publication. Therefore, I recommend publication after revision.

Tanré, D., & Legrand, M. (1991). On the satellite retrieval of Saharan dust optical thickness over land: Two different approaches. *Journal of Geophysical Research: Atmospheres*, 96(D3), 5221-5227.

Point by point comments and recommendations.

Introduction. There are a few other groups besides this one that have attempted similar aerosol retrievals using the DNB, or investigated the possibility theoretically. In particular I recommend referencing Wang et al. (2016), and possibly Choo and Jeong (2016), though the latter is very local in its scope. Wang et al. (2016) put to rest the importance of variable water vapor in the method. If they hadn't, I would be worried about

C2

that factor.

Wang, J., Aegerter, C., Xu, X., & Szykman, J. J. (2016). Potential application of VIIRS Day/Night Band for monitoring nighttime surface PM<sub>2.5</sub> air quality from space. *Atmospheric Environment*, 124, 55-63.

Choo, G. H., & Jeong, M. J. (2016). Estimation of nighttime aerosol optical thickness from Suomi-NPP DNB observations over small cities in Korea. *Korean Journal of Remote Sensing*, 32(2), 73-86.

Lines 88-95. I found the use of present tense “develops” (line 88) and “proposes” (line 90) a little odd. Those studies are done and published. I would have used “developed” and “proposed”, but I’m not sure this is required.

## 2.1 Data sets

What is the time span or dates of the study? Is it one year of data or multiple years? I don’t see this stated anywhere.

Likewise no where is it stated that this is specifically Suomi-NPP VIIRS, because there are now more than one VIIRS flying. There’s NOAA 20, and over the years there will be more.

The specific data sets look to me that they are IDPS data sets. IDPS processing and archiving of data sets has stopped or will stop very soon to be replaced by EPS data sets and processing. I think this should be clarified in the text.

Line 172 “attachment”. First should this be “attachment” or “supplements”? Note that I was unable to download or to see any of the supplementary files. This is likely a problem between me and the journal, not with the authors, but this should be tested.

## 2.2 Retrieval methods

Line 182. “optical depth”. Elsewhere the authors use the term “optical thickness”. It would be good to be consistent.

C3

Line 219. I believe the authors meant 0.6 to 1.0.

## 2.3 Data processing pre-checks

Beginning here the authors need greater clarity in their description of what was done.

Lines 256-260. What does background mean? It seems that an a priori assessment has to be made. Or are the light emitting city pixels included in the mean cloud-free background pixels?

Figure 2d. The green points are very hard to see. Would light blue be a better choice? Would it be better to use yellow for these pixels and green for signal saturation? Line 285 should be “retrievals”

Line 286, “attached” again. “Supplement”?

Figure 3. If the edges of the image correspond to the edges of the bounding box, please state so. If not, then draw the bounding box on the image.

Lines 295-304. In the discussion of cloud contamination and partly cloudiness there is no discussion of thin cirrus. There should be.

Line 309. Why the lopsided statistical cleaning? Dark Target aerosol algorithms do a symmetrical filtering over ocean in order to not impose a bias on the statistics. Over land the filtering is skewed because of bright surface pixels skew unfiltered biases towards too high AOD. Why the skewed filtering here?

Lines 321 – 325. I’m not clear on this. Are you finding the mean for each green pixel? For each city of many pixels, so that first there is a spatial mean of all the green pixels and then a temporal mean to get the average over the time span? Or is this a spatial and then a temporal mean for all the green pixels within the bounding box? Is there a difference between green pixels within a bounding box and the green pixels within a “city”. I have been assuming throughout the paper that standard deviation is a spatial standard deviation of all the green pixels either within a city or within a bounding box.

C4

This is supported at line 386. But here it could be a temporal stdev or an ensemble stdev from all cities on the particular Julian Day.

Line 329. Should that be  $\pm 0.1X N\_STD$ ? plus or minus?

Line 346-349. This could be worded better. “non-totally cloudy” is clumsy. “non-overcast” is better. Though, “minimum number of non-overcast observations” is also clumsy. How about “June and July had the fewest acceptable observations”.

Figures 4, 6,7,8,11 are not informative. How about density scatter plots? Or bin and average the data with standard deviation bars? Or calculate the regression statistics of those red lines in Figure 4? There will be additional comments on the scatter plot figures.

Lines 365-367. There is a lot of discussion in the paper and speculation about snow contamination with no evidence to support the speculation. Could snowy and non-snowy places (ie. California) be compared?

Lines 374 – 376. More on figures. That red line doesn't look like a good fit in Figures 4g and 4h, which makes the statements in these lines hard to accept. The red curves have a minimum around 30 deg, not nadir, so slant path length doesn't seem to be the best answer. This leaves anisotropic light sources. This is very interesting. Why?

Figure 5b. It would certainly be helpful to see some regression statistics.

Lines 399-408. The metric for DEL-Ia is complicated. The average stdev of the most variable 30% of the spatial stdevs, PLUS 2 stdevs of the stdevs of the most variable 30% of the spatial stdevs. I just don't see how this eliminates cloud contamination and lightning strikes. You have the top 1% of spatially variable light sources, but that top 1% will include clouds and lightning strikes. Also why can't you just use Rstd(1%) instead of finding mean and stdev of the top 30%.

Lines 460-461. Regression slope is mentioned here. I know that there is a line of thought in the community that wants to get away from regression statistics, and so

C5

previously, even though I wanted to see regression statistics in the plots, I've refrained from demanding them. Except here regression slope is mentioned. If it is mentioned, then please show slopes, intercepts and R on the plots. If the regression is not linear, then show RMSE and R.

Lines 471-472. These criteria will eliminate places with highly variable day-to-day changes in AOT.

Figure 7 is crying for a color density plot.

Line 586. Sure for the U.S., but in other places the winter season might be fine.

---

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-424, 2018.