RC2: 'Comment on egusphere-2026-2225', Anonymous Referee #2, 07 Jul 2026 reply

This study evaluates two methods to quantitatively retrieve mid-visible aerosol optical depth (AOD) at night from the VIIRS Day-Night Band (DNB), a highly sensitive pan-visible band this sensor carries which is mostly used for nighttime imaging (e.g. city lights). The study area is off the west coast of northern Africa during 2017 and 2018, which is motivated by the availability of both solar and lunar photometer AOD data from AERONET at a site near Dakar, Senegal. The nighttime retrievals are also compared with daytime retrievals from MODIS. The first retrieval method is an analogue of the daytime reflectance-based over-ocean technique often applied to satellites (albeit restricted to the one DNB and using lunar rather than solar irradiande to calculate reflectance). This provides fairly broad spatial coverage. The second is based on darkening of artificial light sources (from cities) through aerosol scattering/absorption, and appears more of a point estimate (maps based on this are not presented).

The article is in scope for AMT and has sufficient novelty (it's an application and evaluation of two techniques which the authors have previously worked on to a new area, and nighttime AOD remains a data gap so there is a scientific need for this type of retrieval). It is fairly well written (with a few exceptions I try to cover below) and referenced, and the quality of language and figures is also quite good (a standard journal copy-editing would suffice for language). There are some points where I think clarification would be beneficial and I do think the authors could have pushed the analysis a bit further. I recommend minor revisions and would be happy to review the next version. My comments supporting this recommendation are as follows:

We thank the reviewer for detailed and constructive comments. We have revised the paper based on suggestions.

Question: General comment: The analysis gets a bit limited by the small volume of matchups with AERONET. I understand that this paper is led by a student (and it is good work) but scientifically it would be stronger if the data volume could be expanded. AERONET's lunar photometer data records are fairly extensive (https://aeronet.gsfc.nasa.gov/new_web/webtool_aod_v3_lunar.html) so to focus on one site for two years is quite limiting. There is a valid argument to be made about different optical models and cloud screening thresholds being needed for different parts of the world, but even within the region mapped within the article (for which this would likely not be a concern) there are multiple unused AERONET sites. The authors use the site Dakar from 2017-2018 but I believe it has data through 2020. There's also Dakar_Belair (about 60 km away) from 2019 onwards, and a little further out Capo Verde (2020-2025) and Mindelo OCSM (2022 onwards). And several more in the Canary Islands further North. These are all coastal sites so the LUT-based method could be applied, even if the city light one can't (and I'm not sure on that second front). I encourage the authors to consider extending the space and/or time period (a little – I acknowledge adding all of the above would be too much) when revising the manuscript, to see if the data volume can be increased, and make some of the quantitative conclusions more robust (retrievals don't necessarily need to be presented site-by-site in the paper so this should not cause much bloat).

Response: Thank you very much for your comments. We took the suggestion seriously and have done a few tests. Firstly, we tried to include two new sites, Cape Verde and Mindelo OCSM that have nighttime AERONET data. However, both sites do not pass the target selection for the city light method due to either very limited city size (requires a city to be at least 40 VIIRS pixels in size), or violation of the

stable light source assumption as mentioned in Zhang et al., (2025). Thus, we could not include the two sites in the study. We took the suggestion fand expanded the study area for the city light method and included both Dakar and Dakar Belair sites. In addition, we added one new year (2019) of data into the analysis as the reviewer suggested. While there are still limited data points, this is likely related to the nature of the nighttime retrieval itself as both nighttime AERONET and the reflectance-based retrieval methods can only be applied to nights with moon-light. Also, stringent cloud screening methods (Zhang et al., 2025) limited the number of retrievals from the city light-based method.

Question: A related point is that we now have multiple VIIRS sensors in orbit (SNPP returning data since 2012, NOAA20 since 2017, NOAA21 since 2022). I'm not sure if the DNB data from them are radiometrically equivalent, but adding a second VIIRS (NOAA20 seems the simpler choice given the time period) would also be an option. Given the platforms are not in identical orbits (same nominal time but staggered), observing on the same nights from different sensors would help give a look at how the difference in viewing geometry (the platforms will see the same areas at different view zenith/azimuth angles) affects the retrieval for the same scenes.

Answer: This is a fantastic idea. Still, different sensors have different sensor characteristics (e.g. filter function), observing conditions (e.g. cloud status, glint and viewing geometries) and calibrations. Also, the lunar model needs to be revised for VIIRS for different satellite platforms due to the difference in sensor filter function and possibly other correction factors included. The whole topic is a non-trivial undertaking. Thus to do it correctly, it shall be a study of itself. We leave this topic for a future study.

General comment: the paper jumps back and forth a lot between describing the radiative transfer, the AERONET data, the ocean approach, and the city lights approach. Some of the information is repeated between parts, and there was some stuff I found myself asking when reading a section about one of the data sets/approaches which is only answered pages later when the manuscript jumped back to the topic in question. This made it difficult to read in places. I wonder if the authors can find another way to organize the paper. Maybe pull all the algorithm stuff up front and put the AERONET stuff in a section just before the analysis of AERONET comparisons.

Responses: We agreed that some of the information is repeated and we appreciate the comments. We have done a few things to reorganize the paper: 1) we removed discussions from places that are redundant as suggested. 2) we took the suggestion and moved the discussion section (old Section 5) under the result section (now Section 4.6). We kept some algorithm related discussions unchanged as we think it is rather a writing style issue.

Question: General comment: I would appreciate a bit more information about the Miller/Turner lunar model since it is key to the approach. A paragraph or two about: where is it from (I'm assuming semi-empirical), what is the valid range of wavelengths/geometries, what is its uncertainty (understanding that may be a function of the previous), is this something which we expect can be improved, what is the relation between this and ROLO and what are their differences?

Response: We provide additional info for the Miller-Turner (MT) model in Section 2.4. We added/revised: "For this study, we used the empirical-based MT lunar model (Miller and Turner, 2009),

that estimates TOA downwelling fluxes based on spectral moon albedo, earth-moon-sun geometries, moon phase and the earth and sun distance at a 1 nm resolution for the spectral range of 0.2-2.8 μm (Miller and Turner 2009). The reported uncertainty of the MT lunar model is 7-17% (Miller and Turner, 2009) due to factors including sensitivity to high lunar phase angles (opposition effect) and libration effects (Zeng et al., 2018)."

The MT and ROLO models are not related and the comparison of the two models have already been conducted by a few other studies (e.g. Wen et al., 2025). Although it is interesting to intercomparing retrievals from the two models, but adding ROLO model to the study is not a trivial undertaking and it is a research topic of itself, and thus we leave this a future research topic.

Wen E, Li L, Hu X, Xu H, Gu H, Zhu J, Li Z. On-orbit radiometric calibration of FY-3E MERSI-LL low light band (LLB) using the Moon. Opt Express. 2025 Mar 10;33(5):9674-9694. doi: 10.1364/OE.551275. PMID: 40798634.

Question: Line 33: Somewhere in the manuscript I would write about why you chose to only investigate two of these three methods.

Answer: There are three methods explained in the study 1) city-light based with the measure of attenuated city lights; 2) city-light based with the measure of changes in variance in the city lights; 3) the reflectance based method. Since the method 1 requires a priori knowledge of reflectance from nearby non-artificial light regions, it is regarded as a less desired method and is excluded.

We added/revised discussions in Section 1 to highlight this issue: "The advantage of the variance method (versus that of the direct light emission method) is that only the VIIRS DNB radiance distribution over artificial light sources is needed. This excludes the need for observations from artificial light free regions as required by the direct light emission method (e.g. Johnson et al., 2013). Thus, only the variance method is discussed hereafter. "

Question: Line 73: At several places (e.g. also line 121) the text says something like "Miller and Turner model (Miller and Turner, 2009)" where it gives author names and a paper citation close by. This feels redundant, just cite the paper, or define the acronym "MT" for the model after first citation and call it by that. This will make the text more readable.

Response: We reference the paper and used MT in the study as suggested.

Question: Line 74-76: Unless I am misunderstanding, this sentence seems out of place or not needed. Lines 68-70 already say that the two methods are applied so why the "Finally, we explored" here – this implies some third thing besides the other two?

Answer: To avoid confusion, we changed "Finally" to "In addition". As an additional task, we also exanimated the feasibility of applying daytime aerosol retrieval scheme (with some modifications) to nighttime aerosol retrieval in this study.

Question: Lines 93-95: More information is necessary explaining the different spatial resolutions of these data products.

Answer: We added further information as suggested to Section 2.1 regarding the VIIRS DNB data. We revised the text to state: "Both VIIRS Environmental Data Records (EDR) and VIIRS Sensor Data Records (SDR) datasets were used in this study. The VIIRS SDR dataset contains VIIRS DNB Geographical Sensor data record (GDNBO) and VIIRS DNB radiance sensor data (SVDNB) providing DNB radiance and geolocation data at a spatial resolution of ~750 meters. Furthermore, the VIIRS EDR dataset includes VIIRS DNB cloud cover (VCCLO) and VIIRS cloud-aggregated environmental ellipsoid geolocation (GCLDO) layers data, provided at a gridded spatial resolution of 6 km, were used for cloud screening. Since the VIIRS SVDNB radiance data and cloud products (VCCLO and GCLDO) are available at different spatial resolutions, VIIRS cloud products were resampled to the spatial resolution of the VIIRS SVDNB data before being used to cloud screen the VIIRS DNB radiance data (e.g., Zhang et al., 2019; 2025). To further assist cloud screening, a cloud mask from the VIIRS Black Marble dataset were also used. The spatial resolution of VIIRS Black Marble data is at a resolution of 15 arc seconds (~500 m resolution; Román et al., 2018). The nearest VIIRS Black Marble cloud mask data for a given pixel is also used for assisting detection of the presence of clouds. The NASA Black Marble data were downloaded from the NASA Level-1 and Atmosphere Archive & Distribution System (LAADS) Distributed Active Archive Center (https://ladsweb.modaps.eosdis.nasa.gov/; last accessed July 29, 2026).

For illustration purposes, VIIRS Moderate Resolution Band 16 (SVM16) and VIIRS Moderate Bands SDR Geolocation (GMODO) data were used to show InfraRed brightness temperature of the study scenes for selected case studies. The SVM16 data is a longwave thermal infrared band spanning the wavelength range of 11.54 – 12.49 μm at a spatial resolution of 750 meters. The dataset was downloaded from NOAA CLASS (last accessed July 28, 2026)."

Question: Line 110: the start of this line says "daytime" but I think this is an error, as later text and the analysis describe use of both daytime and nighttime AERONET data.

Answer: We revised the sentence to avoid confusion.

"The cloud screened, quality assured Version 3, Level 2.0 nighttime and daytime AERONET data from the Dakar, Senegal station (14.4° N, 17.0° W) were used to evaluate the VIIRS retrieved AOD data.

Question: Line 112: Somewhere around here I would add a citation to the AERONET paper on lunar photometry (which is currently a preprint): https://egusphere.copernicus.org/preprints/2026/egusphere-2026-1506/

Response: We added a citation to/from Schafer et al. (2026) to the text and reference section.

Question: Line 126: Somewhere around here, more information is needed about the radiative transfer simulations. The DNB is a very wide band. Were simulations run at some fine spectral resolution (if so, what) and then aggregated according to the spectral response? Or was representative central wavelength calculated (and if so, for which quantity) and effective monochromatic calculations done for that?

Response: Great question. We added/revised the following discussion. "For consistency in dust aerosol optical properties, we applied MODIS Collection 6 (C6) Dark-Target (DT) over-ocean algorithm values of aerosol physical properties including refractive index, effective radius, and radii standard deviation values obtained for 7 wavelengths individually that range from 0.47-2.11 μm. (e.g., Figure 1). The five wavelengths of the DT aerosol parameters used are from the 0.47, 0.55, 0.65, 0.86, & 1.24 wavelengths.

Using this information, for coarse mode aerosols, T-Matrix (Mishchenko et al., 1996) calculations were performed by the research team to estimate dust optical properties including phase function, single scattering albedo, extinction cross section, and scattering cross section, assuming spherical dust. Note that the shape assumption implemented in this study is different from what is done by the MODIS team (assuming spheroids instead). This means the retrieval is assuming purely coarse mode spherical dust aerosols. This assumption may introduce larger error when AOD is low (minimal dust) and may have a better performance for high AOD cases. Next, SBDART is designed and run for simulations using optical properties such as single scattering albedo, extinction cross section, and Legrendre polynomial coefficients as provided by the T-matrix calculations at each of the seven wavelengths respectively. SBDART radiative transfer simulations were performed using discrete wavelengths from 0.49 – 0.90 μm in 0.005 μm intervals covering the spectral response range of the VIIRS DNB using the wavelength dependent optical properties from the T-matrix as explained. After which, the simulated radiances from SBDART were weighted (or spectrally averaged) by the VIIRS DNB spectral response function to produce a single-band integrated reflectance."

Question: Line 134: Note that the MODIS algorithm this model is taken from assumes this coarse mode is spherical. Spheroids are a more realistic assumption in terms of phase function (although the aspect ratio distribution assumed should be mentioned in the text) but the paper should state that this shape assumption is different from what is done by the MODIS team. It should also be noted in the paper that this therefore means the retrieval is assuming purely coarse mode dust-like aerosols with these properties, e.g. no fine mode aerosols, no sea spray aerosols. The fidelity of this assumption is probably worst when AOD is low (minimal dust) and best when AOD is high (minimal relative contribution from non-dust). I suggest that the text is expanded to go into detail about these assumptions a bit more.

Answer: We have added the following discussions in the Section 2.4:

"Note that the shape assumption implemented in this study is different from what is done by the MODIS team (assuming spheroids instead). This means the retrieval is assuming purely coarse mode spherical dust aerosols. The assumption may introduce larger error when AOD is low (minimal dust) and may have a better performance for high AOD cases."

Question: Line 158: Somewhere here I would add a sentence about why the ocean case is easier than the land case. My assumption is that this is because ocean is comparatively dark and spectrally and spatially uniform, while land has more spectral and spatial variation, so becomes a larger potential error source from mischaracterizing it.

Answer: We added that "Open ocean surfaces offer a favourable target for the reflectance-based approach as it is relatively dark with comparatively homogeneous surface properties in comparison with land, thus, over ocean aerosol retrievals are considered more reliable."

Question: Line 163: I think the comment and citation about water vapor being a marginal absorber needs a clarification here that this is only true for a broad band such as the DNB (and the single visible band of the earlier GOES sensors). For narrower bands used in multispectral retrievals (e.g. VIIRS/MODIS), we absolutely need to account for variations in water vapor absorption otherwise it induces a spectral bias.

Answer: We added the following to address the concern: “Additionally, while water vapor impacts the growth of hygroscopic aerosols, the influence of water vapor on AOD retrievals is considered marginal for wide spectral bands such as the DNB at the visible spectrum (e.g. Knapp et al., 2002). However, for narrower bands used in multispectral retrievals (e.g. VIIRS/MODIS), one needs to account for variations in water vapor absorption.”

Question: Line 187: It’s good to remind the reader that lunar phase is the same everywhere for a given night, but this also means that the bit about selecting the prime meridian is not relevant and can be deleted (I am guessing this means you used a calculator and plugged in those coordinates because it required you plug in some coordinates?).

Answer: We updated this sentence to remove the Prime Meridian info that was not needed. We used a Python library called pylunar which is a lunar phase calculator that requires coordinates as an input and is quite user-friendly.

Question: Line 198: I am assuming that lunar glint is the direct analogue to Sun glint, but I would add a note to state this and say exactly how this is calculated in the present study (e.g. do you use a Cox-Munk model to estimate strength or is it purely geometric)? How is the threshold for exclusion defined in the approach?

Answer: The reviewer is correct in the assumption that lunar glint is the direct analog to sun glint. We used the equation as included in Remer et al. (2005) that allows for a geometric calculation of lunar glint. We added this reference to the paper and provided a proper in-text citation. We added/revised: “Equivalent to daytime aerosol retrievals, where regions with sun glint are excluded due to strong glint reflectance, regions with lunar (moon) glint are excluded for nighttime aerosol retrievals. Lunar glint regions are computed using a geometric calculation from Remer et al., (2005).”

Question: Line 200: I would say high “errors” and not high “biases”. Presumably the SBDART model is able to model the glint, so it is there albeit modeled with large uncertainty (leading to large errors) and it is not that SBDART has no glint (I would say “bias” only if it’s a physical feature which is missing from the system)?

Answer: Most if not all operational over ocean aerosol retrievals exclude glints due to high reflectance and strong spatial inhomogeneity in reflectance that create uncertainties in retrievals. As suggested, we changed “biases” to “uncertainties” in the paper.

Question: Figure 2: I think panel (a) should be reflectance not reflectivity (it’s what is observed not what is inherent). I suggest panel (b) is retitled “Cloud Fraction”. Panel (c) also needs a better title and an indication of units. Is this glint angle?

Answer: We used the reviewer feedback to update the Figure 2a title to “VIIRS DNB Reflectance”. We made this change to all Figures that contain VIIRS DNB Reflectance. We also updated Figure 2b title to “Cloud Fraction”, Figure 2c title to “Lunar Glint Angle (°)”. Yes, Figure 2c is displaying lunar glint angle.

Question: Section 3.2: I think the text should be clearer on whether this is the "direct estimation" or "variance" method mentioned in the introduction. Papers cited here are associated with both methods in the Introduction, and both rely on artificial (e.g. city) lights.

Answer: We note that the method used is a variance-based method. We added discussions to mentioned that direct estimation method requires a priori knowledge of nearby artificial light free regions which is a less desired method.

"The advantage of the variance method (versus that of the direct light emission method) is that only the VIIRS DNB radiance distribution over artificial light sources is needed. This excludes the need for observations from artificial light free regions as required by the direct light emission method (e.g. Johnson et al., 2013). Thus, only the variance method is discussed hereafter."

Question: Line 299: Why 1 degree (about 100 km)? This is much coarser than the 25 km distance often used for daytime data validation, and so will introduce additional uncertainty related to spatial heterogeneity. Does it really increase data volume that much (and if it does, doesn't the fact that you don't have retrievals close by but do have them far away inherently mean there is heterogeneity in the atmosphere)? Smaller areas could potentially also lead to less cloud contamination as there is a smaller area for a missed cloud to creep in. I would also write how the space/time aggregation is actually done, I am guessing simple averaging?

Answer: The 1-degree requirement is to ensure that both needed AERONET sites (Dakar and Dakar Belair, Senegal) are included. Also, for the city light-based method, it is the similar pixels used in the comparison process and thus colocation distance is less relevant. Also as suggested, the space/time aggregation discussion is added to the paper. We added: "All AERONET data points from an individual AERONET station within the temporal constraints are averaged to represent the co-located AERONET AOD for a single co-located VIIRS data point. For the lunar reflectance method, VIIRS AOD pixels within 1° (Latitude/Longitude; Euclidean distance) of the Dakar or Dakar Belair AERONET stations respectively are spatially averaged to represent a single VIIRS data point."

Question: Lines 311, 314: I understand aggregating the MODIS data set and your VIIRS retrievals from level 2 to 0.5 degrees for a common comparison. But why then compare MODIS with AERONET at 0.5 degrees instead of either 1 degree (as was done for VIIRS) or 25 km (as is normal, as stated above)? I suggest 25 km for both level 2 comparisons. Also, why choose 0.5 degrees for the MODIS/VIIRS comparison over 1 degree (I think the choice is fine just wondering why a third spatial scale is introduced for this part of the analysis)?

Answer: There is not a particular reason. We updated the collocation window to 1 degree as suggested and repeated the level 2 comparisons using this new distance threshold. By doing this, all comparisons/colocations with AERONET are completed at a distance of 1-degree.

Question: Line 320: It would be interesting to present the standard deviation of these five domains. This would have contributions from aerosol heterogeneity and from retrieval uncertainty. One could examine whether high variability between domains is associated with worse agreement with AERONET or not. I

would also like some justification for the choice of 25 km x 25 km domains. Since the comparison is already restricted to times close to a full moon, I would not have thought that too much averaging for noise reduction is needed? Ok, I see this topic comes up again lines 420-425 (one of my points earlier was about information jumps around the paper) but this still doesn't say why this decision was made, discuss variability between regions, and introduces a new threshold (40 light sources) without justification. These later lines also switch from calling them "domains" to calling them "grids" (please be self-consistent with terminology).

Answer: We inherited the processing code from Zhang et al., (2025). In that study 25x25 km^2 is used to ensure enough light sources in a given 25x25km^2 area. The 40-pixel threshold is also used in Zhang et al. (2025), as the change in spatial variance in artificial light sources is used for detecting aerosol optical depth. The spatial variance is less representative/stable with less pixels involved in the calculations. Those topics are discussed in details in Zhang et al. (2025), thus we referenced Zhang et al. (2025) for readers who are interested in details. In the new approach (we increase the study area to add both AERONET stations), although 12-13 25x25 km^2 domains are found to have artificial lights, only two domains passed QA checks as mentioned. We completed an analysis of the two domains that passed QA checks and found a total of 29 nights of which AOD retrievals are available from both domains. A mean difference of -0.02 and a correlation of 0.78 are found between the AODS retrieved from the two domains. We included the discussions in the text.

Question: Line 326: Do you mean average difference or average ratio here?

Response: We updated the wordage to average ratio. The difference between the AODs at 0.55 and 0.675 μm was calculated as a ratio and then averaged.

Question: Lines 402-403: I don't think that the data volume is high enough to make statements about what optimal thresholds are. Sure, report on what was seen for the sample of data you have, but I don't think this can be generalized.

Answer: Point is taken from reviewer 2. We added that "The optimal cloud screening threshold is found to be a standard deviation threshold of 0.01 and cloud fraction threshold of 0.2. However, the data volume is quite small to generalize this conclusion*."*

Question: Lines 410-414: I am not sure I understand what is being said here. Is this saying that even though the threshold for minimum lunar illumination was 0.5 (line 191), in practice the minimum value found for the AERONET matchups was 0.73 and all except for that one had 0.85 or above? I would rephrase this text.

Answer: We hope to reduce confusion by improving this section of the paper to state:

"Five co-located nighttime AOD retrievals were found with a lunar fractional illumination value of less than 0.85. Seventy-two co-located retrievals using a standard deviation of 0.01 and cloud fraction threshold of 0.2 were completed with lunar fractional illumination values greater than 0.85."

Question: Table 1: I am not sure "error" is the right term here. Maybe for the lunar AERONET data if we can treat this as a reference. But the comparison to daytime AERONET and to MODIS is really a "difference" not an "error" since the daytime data are quite different in time, and the MODIS retrieval has non-negligible uncertainties itself and is also from daytime not nighttime.

Answer: We note the description provided by reviewer #2 and appreciate the clarification. However, a suggestion made by Reviewer #1 included a revision of Table 1 to simplify Table 1 to include the cloud screening thresholds, Data Count, RMSE and R^2.

Question: Section 4.4: I found this section quite weak. From my understanding the authors have the two years of data available, why is the comparison restricted to two scenes? Were they cherry picked because they look good? How about making e.g. seasonal composites from the two sensors across the study region and showing that as well? (These could also be restricted to common grid cells from the same day/night to reduce some of the sampling related artefacts.) How about a time series analysis over the domain or something? That would also help look at differences in less vs. more dusty periods.

Answer: Figure 6 is an example of the retrieval process for a dusty and low-dust case. We rewrote the text to highlight that this figure is only used for *qualitative comparisons only* to illustrate the retrievals process and the comparison with MODIS. We did use all collocated MODIS and VIIRS data pairs that are available (47,021 - 84,847 pairs of 0.5x0.5 degree grids), yet not in form of figures but in form of statistical numbers as shown in Table 1.

We rewrite the sentence to avoid confusion:

"As a qualitative comparison given the day and nighttime AOD differences (for illustration only), Figure 4 shows an example of gridded VIIRS and MODIS AOD data from 1 June 2018 and 3 May 2018 for a qualitative comparison."

Question: Lines 459-461: this is another example of information that was already given earlier being repeated (too much jumping around – I suggest confining the info about MODIS/VIIRS aggregation for comparison to one another being kept all together, maybe here).

Answer: We have removed the sentences as suggested. As mentioned previously and in regards to the reviewers general comment we tried to limit redundancies made consistently between sections that are not necessary to repeat. We were vigilant in improving the readability and flow of the paper as suggested by the reviewers.

Question: Figure 4: See earlier comment that "Cloud Screening" as a plot caption could be "Cloud Fraction". I don't know that these panels are needed for this figure anyway?

Answer: We agree with the reviewer that the "Cloud Fraction" panels shown in Figure 4 were not needed and subsequently removed. We replaced this with VIIRS Band M16 IR imagery as suggested from reviewer 1.

Question: Line 500: The word "agreement" here is not very meaningful. Correlation is a measure of linear association, so this means that the two move in the same direction with some commonality. It doesn't say anything about how similar the magnitudes are. Is the timing of changes more important than their magnitudes (which is what using correlation for agreement implies), and if so, for what application?

Are these correlations strong enough for some practical purpose or is it just nice that they are non-negligible? I suggest using some more precise wording.

Answer: We have updated with wording in these lines and elsewhere that use the language "agreement" with "consistency" as suggested by the reviewer.

Question: Section 5: This feels misplaced, and there's only one subsection so it doesn't need a number. I feel it would be better earlier on (when talking about lunar illumination fraction thresholds), or in an appendix, or else merged into the conclusions (also when talking about coverage relationship to lunar illumination).

Answer: Thanks for your suggestion. We moved this section under the result section. So, this section is the new Section 4.6 now. In addition, we added plots of retrieval performance as a function of illumination fraction as suggested by reviewer 1 in the section.

Question: Line 541: how much of this is really "random noise" vs. physical signals which are hard to model? Just saying "random noise" feels like hand waving away complexity. Sure, Figure 7 shows noise increasing when illumination drops but also a change in the baseline value (also seen in Figure 6) which implies some real signal. If it were really noise then this could be overcome by spatially aggregating the VIIRS data. I suggest expanding the discussion here and being more careful with word choice.

Answer: We do not know a method for separating "noise" with "physical signal" in observations. We agree with the point as mentioned and removed the use of the words random noise. We used high spatial variance to reference to the "noise" and rewrote the discussion as needed.

Question: Figure 7: why is the land white in all these panels? Is it that reflectance was just not computed for land (in which case "value of 1" and "missing data" should be distinguished somehow by the color bar), or that it is always above 1?

Answer: The reflectance data in Figure 7 was masked out over land thus appearing as white. We have fixed the issue in Figure 7 by including the VIIRS DNB reflectance over land such that the land does not appear exclusively white. Also, to improve consistency, we ensured that all Figures displaying VIIRS DNB Reflectance over land do not include a land mask.

Question: Lines 568-570: I suggest rewording this, it feels a bit redundant (starts and ends talking about reflectance definition) unless I am misunderstanding something (in which case it should be reworded because it leads to reader misunderstanding).

We removed the reflectance definition as suggested. The sentence has been updated to: "2) This study confirms that if reflectance values from VIIRS DNB data can be obtained, a LUT for reflectance-based daytime AOD retrieval using the VIIRS DNB can be applied to nighttime by normalizing by the lunar albedo integrated over the VIIRS DNB spectral response."

Question: Line 595: A link is missing for the T-Matrix software

Answer: We added a link to the Mishchenko T-matric model: https://doi.org/10.1016/S0022-4073(98)00008-9 in the Code and data availability section.