Interactive comment on "Global clear-sky surface skin temperature from multiple satellites using a single-channel algorithm with viewing zenith angle correction" by Benjamin R. Scarino et al.

Anonymous Referee #1

Author Response

The article describes a methodology to derive Land Surface Temperature (LST) using observations from a single channel in the thermal infrared, which can be applicable to various sensors. On top of this, the authors propose a simple model to correct angular effects on satellite LST, allowing its correction to nadir view. The latter together with a large set of validation results form the main novelty of the current manuscript. The study is of interest for AMT and the manuscript is overall well presented. The results and in particular the LST angular correction require, however, further discussion. Following my comments below, this manuscript should be subject to major revisions before being considered for publication.

Thank you for your careful, thorough review. Please see our responses following each concern below, with details on where to find changes in the text (if applicable). Please note that any changes in the manuscript that are in response to your questions/suggestions are highlighted in blue or green. Other colors correspond to changes prompted by other reviewers.

1) My main concern refers to the angular correction suggested in this article and its interpretation. The characterization of LST angular effects is based on LST estimates from GOES and MODIS-Aqua, using the same algorithm and collocated in space and time. The authors then derive global regressions shown in Fig.7, where the correction depends only on the deviation of the view zenith angle to nadir. Results in Fig. 7 suggest that the coefficients of (daytime) regressions have a strong seasonality. I find those monthly fits to be strongly dependent on the seasonality observed over northern America, where most land pixels with high view angles are. The authors seem to somehow acknowledge this fact (lines 19-20 page 9), but discard its main implication, i.e., that those angular corrections may be applied everywhere. The validation over and (where angular effects are more relevant) is performed for ground sites over North America. I have strong doubts whether similar corrections would yield similar results in other sites (e.g., mid-latitudes in South America).

The purpose of this paper is to begin the process of identifying and correcting for VZA dependence. Although this was perhaps unclear, we did not try to claim that this model can absolutely represent variations around the globe, but it is at least a start (e.g., the aforementioned acknowledgement on page 9). Developing true global correction methods with other GEOs requires significant additional analysis and is more appropriate for a follow-on paper. To make our intent more explicit, we've added sentences on page 9 at the end of the second full paragraph (lines 29-34), giving caution to the application of this model everywhere, even South America owing to its comparatively muted seasonality to that of North America. Also, we have reduced the emphasis on global application of, specifically, the model by adding a sentence in the last paragraph before Section 5.1, which begins by discussing 3-D radiance anisotropy for global retrievals (page 8, lines 7-8). The paragraph

now stresses that initial development must start regionally, and we highlight that this is a first step. It should be understood that other pertinent uses of "global" in the text fit their context of being retrieval-algorithm-related, which is certainly global (or quasi-global if based only on GEO satellites).

2) The angular effects on LST depend on surface types (vegetation density, orography), illumination angles, which in turn strongly influence temperature contrasts among surface elements within each pixel. These may translate into a simple model based on viewing angle deviations from a reference view, changing with local seasons. As such, "The lack of a daytime VZA dependency in January" (lines, 17-18, page 9) is in fact not surprising, as temperature differences among sunlit/shadowed surfaces are usually much lower in winter than in summer. If no other variables are taken into account, I do not see how a simple angular correction as that proposed in this article may be derived for the full GOES disk, including pixels with the same viewing angle in the Northern and Southern Hemispheres.

In line with our previous response, the purpose of this paper is to introduce a simple, first-step empirical model that is based solely on VZA. Although including more variables (e.g., relative azimuth, solar azimuth, land cover type, surface elevation, hemisphere, etc.) would certainly strengthen the model, such further subsetting with the present dataset would reduce sample size beyond reliability. Therefore, variable-based subsetting is more appropriate for future improvements. Discussion of the need for this kind of subsetting has been added in Section 5.1 (page 9, lines 26-29), and is also reiterated in the Conclusions (page 16, lines 37-38).

Regarding the VZA dependency in January. This sentence was poorly stated. We in fact agree that the lack of dependency is NOT surprising, as suggested by our conclusion statement, "A January daytime VZA dependency was also not found, indicating the chief importance of emissivity differences resulting from terrain and canopy-surface configurations that change with VZA." To draw from our own conclusions, therefore, we changed the sentences to now read, "The lack of a daytime VZA dependency during January (Fig. 7a) may seem surprising given that the average SZAs are larger in the Northern Hemisphere during January. That effect, however, is counteracted by the fact that sun/shade discrepancy is the dominant driver of LST anisotropy," (page 9, lines 22-24).

3) Following the point above, the results presented in fig. 11 (GOES versus MYD11), which I suppose cover the whole GOES disk, suggest that no night-time correction is needed; this is in line with fig.8. However, comparisons made for ARM and SURFRAD sites show improvements after angular corrections performed for night-time LST, which often surpass those observed for daytime. Were such night-time corrections made using the "daytime adjustment"? How can we physically explain such outcome?

First, we have now clarified at the start of Section 5.2 (page 10, line 10) that GOES and MYD11 LST are matched between 60°N and 60°S, not the full disk.

We acknowledge that using the daytime model at night may seem suspect, especially considering the results of Figs. 8 and 11 (see Section 5.4, page 13, lines 8-12). We can't ignore, however, that the GOES-East/West LST comparisons of Section 5.3, and the resultant Fig. 13, suggest that nighttime VZA dependency does matter. Furthermore, we cite Minnis and Khaiyer (2000) and Vinnikov et al. (2012), who note that nocturnal anisotropy can be induced by differential cooling between lower- and upper-level surfaces and different

fractional amounts of vegetation in the sensor FOV (page 9, lines 17-19). For now, we can only use the daytime model to assess nighttime validation, because we were unable to resolve a nighttime dependency with our standard methodology (i.e., Fig. 8). We present the results of using the model for nighttime validation versus ground sites, which overall show that its application is beneficial. For example, relative to the ARM IRT, absolute daytime bias reduces from 1.39 K to 0.20 K, and absolute nighttime bias reduces from 0.95 K to 0.16 K. The SDD reduces in both cases as well. Taking all ground site comparison results into account, the average absolute bias reduction at night is 0.1 K, and the average SDD reduction is 2%. These overall nighttime improvements are now mentioned explicitly in the final paragraph of Section 5.4 (page 15, lines 23-24). Furthermore, we've added a sentence in the same paragraph, cautioning users that, although there is improvement, the nocturnal correction is nevertheless based on daytime observations (page 15, lines 24-26).

4) AVHRR results are never shown separately from GOES-13 results. The article must include a description of the "AVHRR-only" validation and on the applicability of the angular correction to AVHRR observations, particularly to those obtained with larger (e.g., > 40_) view zenith angles. The same period of data should be used for validation of GOES and AVHRR LST to exclude the influence of inter-annual variability when comparing those results. The stability of angular corrections over different years should also be taken into account.

NOAA-19 AVHRR data for 2013 replaced the 2008 NOAA-18 AVHRR data for all LST analyses. We feel it is important to use NOAA-19 rather than NOAA-18 AVHRR in 2013 given the orbital drift NOAA-18 underwent by the latter date. That is, NOAA-19 is much closer to the 13:30 LT orbit in 2013 than NOAA-18 is for that same year. Furthermore, the AVHRRs are of the same series and design, with onboard blackbody calibration, which we have found to be stable and accurate via simultaneous nadir overpass comparisons with Aqua-MODIS. Tables 2 and 3 were changed to incorporate NOAA-19 AVHRR and AVHRR-only results. Figures 17, 18, and 19 were replaced, now using 2013 NOAA-19 AVHRR results. Discussion throughout Section 5.4 was added/changed to accommodate the use of NOAA-19 AVHRR and AVHRR-only results (highlighted in blue). Results for the 2008 AVHRR-only analysis are preserved in Table 3, with discussion of their similarity to 2013 results added to Section 5.4 (page 15, lines 13-15). The AVHRR-only angular corrections relative to SURFRAD are more often larger than their GOES counterparts owing to correction being allowed for a wider range of VZAs. GOES views are fixed based on the ground station position, and the assumed ~53° angle for SURFRAD does not allow much viewing difference for assessing the angular correction. This fact is discussed in Section 5.4 (page 13, lines 27-28, and page 14 lines 7-8).

5) Minor/editorial comments:

a. Why using MODIS-Aqua as opposed to characterizing the angular effects with MODIS-Terra or with both Terra and Aqua?

For this initial attempt, we chose Aqua-MODIS just as a matter of consistency with other CERES products. The skin temperature algorithm relies on reflectance at 0.65 μm, and CERES Edition 3 visible products are scaled to Aqua-MODIS, and CERES Edition 4 uses Aqua-MODIS band 1 directly as the visible calibration reference. Aqua rather than Terra is used because, according to a study by Wu et al. 2013 (Characterization of Terra and Aqua MODIS VIS, NIR, and SWIR spectral band calibration stability, IEEE Trans. Geosci. Remote Sens., vol. 51, no. 7, pp. 4330–4338), Aqua-MODIS is the better characterized MODIS instrument. Although we expect that the incorporation of Terra-MODIS would prove beneficial regardless, doing so

will have to be left for future work.

b. Fig. 3c, 4c, 5c: the scale does not seem adequate to SST comparisons. Please reduce the scale max/min to at least +/-5K.

The scale was originally based on absolute minimums and maximums found across all figures, but given that these extremes are very localized, contrast across most of the image is diluted. Therefore, we followed your suggestion and changed the scale to +/- 5 K.

c. In section 5.4, it is indicated that the matchups for some ground stations consider the nearest pixel only (heterogeneous areas), while for others a 3x3 array is used. For the sake of simplicity, I strongly suggest the same criterion to be used for all.

We agree that this mixing of techniques adds needless complexity. Therefore, we now present ARM comparisons based on the nearest pixel as is done for SURFRAD comparisons. Text related specifically to 3x3 array validation results was either changed or removed (page 13, lines 1-2). This change affected Figures 15 and 16, Table 3, and results/discussion text in the first three paragraphs of Section 5.4 (highlighted blue).

d. Fig. 14 clearly shows the influence of illumination angles. Given the large difference between UTC and local time, can you indicate night-time periods (e.g., east and west of 105, respectively)? It would also be interesting to see these differences for other periods of the year in a multi-panel fig.

Vertical lines were added to Fig. 14 that indicate the start of night and day periods, as you suggest. Also, plots were added for January, April, and October 2013.

e. Table 2: Correct TBL sfc emissivity for 11 micro-m.

Corrected the typo. Doing so led us to double-check the other E11 values, and it was discovered that the wrong line was copied from our reference. All E11 values are now accurate; the typos did not influence the calculated results.

f. Missing references: Ghent et al. (2010); Duan et al. (2014); Wang et al. (2014); Coll et al. (2009); Williamson et al. (2013); Wan et al. (2002); Li et al. (2014); Yu et al. (2010); Sobrino and Raissouni, 2000

Thank you. We have added the missing references. The Yu et al. (2010) reference was actually an earlier-version citation to the Yu et al. (2012b) ATBD (Version 2.0 vs. Version 2.5). However, we had forgotten to change Yu et al. (2010) to Yu et al. (2012b) in the text. Also, a Li et al. (2013) citation was mistakenly written as Li et al. (2014) and is now corrected.

g. Please discriminate the two references to Yu et al (2012) in your reference list and in the text; Sobrino et Romaguera (2004) not referred in the text.

The Yu et al. IEEE paper is now referred to as 2012a, and the ATDB as 2012b. The Sobrino and Romaguera (2004) reference is now correctly cited in Section 1 following the mentioning of SEVIRI-based LST studies (page 2, line 14).