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# *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**

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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.

1) My main concern refers to the angular correction suggested in this article and its



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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 land (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).

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.

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?

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.

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?

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

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.

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.

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

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

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.

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