Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2016-79-RC2, 2016

Atmospheric Measurement Techniques Discussions
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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 #2

Received and published: 6 June 2016

Major comments:

1. The major concern in this article is the inclusion of the ground station analysis. The article should not be considered fit for publication with these results included. I would suggest removing these results (reasons provided below) and with careful attention to other comments below and from other reviewers it would be fit for publication after major revision.

Although a few authors have used SURFRAD and other FLUXNET ground stations for LST 'validation', detailed analysis of these sites by Wang et al. 2009 and Guillevic et al. 2012 show that they are in fact unsuitable for validation of sensors at the kilometer or more scale resolution. Fluxes from these sites are measured with pyrgeometers on

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10m towers giving them an effective spatial footprint of 30-45 meters, which compared to GOES at 4km (scaled to 8km) is essentially a point measurement. Considering that surface skin temperatures can vary a few degrees over distances of a few meters, this is simply not a valid comparison. e.g. Wang et al. 2009 found that large surface heterogeneity at these sites (e.g. Bondville surroundings go from fully veg to bare soil within a few meters of the tower, and Desert Rock has clumps of much warmer dark maffic rocks a few hundred meters from the tower) accounts for 60–70% of the error when making comparisons between ground-based measurements and LST retrievals from ASTER (90m). Guillevic et al. 2012 concluded that only by using an upscaling model to account for these heterogeneities was it possible to make any kind of validation assessment with kilometer-scale data. Wang et al. 2009 concluded SURFRAD sites should only be used with nighttime data from high-res sensors at 100-m scale.

Therefore claiming that there is an VZA improvement at these sites is really quite meaningless considering the overwhelming number of uncertainties based on site variability, scale difference, and emissivity estimation. There is no discussion on how emissivity was estimated at the SURFRAD sites? It is critical to get in situ emissivity measurements from the PI's themselves at these sites, given their fine-scale variability, and a simple assumed land cover classification will not suffice.

2. To first order the average view angle dependent correction is a move in the right direction but without dependence on elevation and surface type information it is hardly a complete correction, and may result in compensating errors dependent on pixel location.

3. Pinheiro et al (2006) and Guillevic et al (2013) have already shown that the nighttime LST is independent of the viewing considerations, so the relevancy of bias correction at night with this methodology is questionable. You are likely compensating for GOES-MODIS time differences or spatial aggregation of MODIS LST.

4. There is no discussion or mention of the possible effects of aggregating MODIS

1-km to the effective GOES pixel resolution (assumed 4 km here?). Temperature does not scale up in a linear fashion (emissivity does), so you are introducing an additional uncertainty in your VZA corrections from the scaling.

5. Eq. 13. Without temperature/emissivity separation you can not simply imply an emissivity dependence from the surface leaving radiance fraction, which makes this section and all accompanying figures invalid assumptions.

Please double-check references (e.g. Wang et al. 2014) is not included.

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