Response to comments by Dr. Tim Hewison

General Comments

The paper presents a nice example of the application of an inter-calibration technique to assess the calibration of one MODIS water vapour channel using IASI as a reference standard.
The presented analysis finds a systematic bias in the MODIS observations relative to IASI, which is found to be seasonally dependent. The authors suggest that the bias can be explained by a shift in the spectral response function (SRF) of the MODIS 6.8 μm channel. However, no theoretical background is provided to give technical support to this hypothesis. The authors should state whether the proposed 11 cm^{-1} shift is reasonable given the levels of uncertainty with which the SRF was originally characterised and why a bulk shift of SRF was selected in preference to the many other modifications to the SRF that could reproduce the observed pattern of biases.

Your concern about the lack of physical and theoretical background of the bulk shift of SRF as a bias source should be valid. On the other hand, as you should know well, main reasons of the sensor bias cannot be well diagnosed in particular after the satellite launch, because measured radiances arise from combinations of various natural and mechanical factors such as blackbody emissivity, detector nonlinearity, and spectral uncertainty. We understand that even the spectral shape can be factors, however unfortunately we are not able to diagnose such many possibilities in our laboratory. Although the bulk shift of SRF for explaining the bias was not manifested by evidences, finding MODIS water vapor channel bias itself should deserve a scientific recognition because MODIS water vapor products without bias correction can lead to a potential misunderstanding of the atmospheric processes in particular associated with UTH.

Recognizing many factors to contribute to the bias we chose a word of "possible" shift in the title. Taking your concerns into account we changed some wordings in the abstract like " possibly caused by the shift of the spectral response function". Furthermore we put attention forward to readers about limitations of our current understanding. In the revised version, the conclusion now contains following discussion. "Despite the better agreement of MODIS 6.8 μm channel with IASI through the spectral shift of SRF, it should be pointed out that the proposed explanation may not be conclusive because other physical error sources are not known. Further investigation into other possible error sources such as blackbody emissivity, detector nonlinearity, SRF out-of-band response, and spectral shape is necessary before reaching a firm conclusion."

My main criticism of the manuscript is that it does not specify the uncertainties at any stage of the analysis. While it is recognised that producing a full error budget of each component of the process is not trivial, it would still be beneficial to the readers to evaluate the uncertainty of the quoted biases of -3.06 K and -2.02 K by analysis of the regression statistics.
Also, it is apparent from the figures that the slopes of the regressions are not equal to 1. This means the bias will depend on the radiance at which it is evaluated. The relevant radiance (or brightness temperature) should be specified in the text.

We appreciate your comment. The biases are re-evaluated using a reference temperature of TB=250K. New biases are to be -2.9 ± 0.96 K for June 2007, -1.9 ± 0.86 K for December

2007. Now we provide biases and uncertainty ranges at a 250 K reference temperature.

Throughout the paper the term "up to 3 K" is used to describe the bias found. While this may be correct, it begins to sound like a newspaper headline. I suggest replacing this by "2-3 K", at least in the abstract and conclusion, as it is shorter and more accurately describes the findings.

Corrected.

While it may be beyond the scope of this paper, the authors should recognise that method presented would be equally applicable to many other MODIS channels.

You are right. Methodology developed here can be applicable to assess the calibration status of other IR channels aboard third polar orbiting satellite (here MODIS) by transferring hyperspectral information through a geostationary satellite. That can be done at least channels overlapping with ones of the geostationary platform. The reason we did not pay attention to other MODIS IR channels is that calibrations of other IR channels appears good (e.g., Tobin et al., 2006, utilizing AIRS measurements void of 6.7 μm WV spectral band) while WV channel may be in problems as described in the Introduction. We still think that description in Introduction is enough to provide backgrounds to readers.

Overall, I support the publication of the paper after minor revisions given the importance of this analysis to the application of data from MODIS, the use of which is widespread throughout the remote sensing community.

Specific Comments

Equation (2) performs a comparison based on the regression of collocated brightness temperatures. However, as both instruments' measurements are most closely related to radiances and their calibration is done is radiance-space, this would seem to be the natural choice for the comparison. (The radiance biases can still be converted to brightness temperatures for convenient presentation.) The authors should check whether similar results are produced when performing the comparison in brightness temperatures and radiances and comment on their findings.

In fact, the obtained TB relationship is based on the radiance calculation as expressed in Eq. (1). Thus, theoretically the relationship on the radiance domain should be same as one on the brightness domain, and vice versa, when the same spectral format (for example wavelength vs. wave number) is applied. We repeated analysis on the radiance domain, and confirmed the assumption we put on here. As frequently found in other literatures, however, temperatures are easy to interpret the bias and uncertainty level and thus we prefer to keep the results in brightness temperature space.

p.3282 The text describing the method is difficult to follow as it contains many terms that sound similar. The authors should consider introducing some clear symbols and using these to refer to each term.

Differing from measured MODIS and MTSAT-1R brightness temperatures (TB_{MODIS}, TB_{MTSAT} as their respective symbols), we use $IETB_{MODIS}$, $IETB_{MTSAT}$ as corresponding IASI-

equivalent TBs throughout the manuscript.

p.3282.26 Another advantage of the spatial averaging is that is can also mitigate geolocation errors.

Comment is included in the text.

p.3285.5 A reference should be cited to support the underlying assumption that there is a strong seasonal cycle in the upper tropospheric humidity - ideally specifying its relative variability.

Wu et al. (1993) showing the TB range between summer and winter is added. The seasonal dependence of the bias appears to be related to the degree of wetness of the upper troposphere; the tropical seasonal mean WV channel TB can vary from 243 K for the summer to 246 K for the winter (Wu et al., 1993).

Wu, X., Bates, J., and Khalsa, S.: A Climatology of the water vapor band brightness temperatures from NOAA operational satellites, J. Climate, 6, 1282-1300, 2003.