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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessment of the calibration performance of satellite visible channels using cloud targets: application to Meteosat-8/9 and MTSAT-1R" *by* S.-H. Ham and B. J. Sohn

Anonymous Referee #2

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General comments

The authors describe three methods to assess the calibration accuracy of the visible channel of MET-8/9 centred at 646 nm and of MTSAT-1R centred at 724 nm and present an application to some months of data. The methods explot 1) collocations between these two geostationary satellite sensors and the polar-orbiting MODIS instrument; 2) radiative transfer simulations of satellite reflectances starting from MODIS cloud properties; 3) deep convective cloud targets. As a result, relative accuracies are found to lie within 5–10 % for MET-8, 4–9 % for MET-9 and up to 20 % for MTSAT-1R.

This interesting paper addresses an important topic not only for climate research but also for process studies related to cloud physical properties. Although it is well written, important changes must be carried out. Thus, this paper should be published in ACP but it must first go through major revisions (see below).

Specific comments

The main deficiencies of the paper consist in

1. the illustration of the methods
2. the application of the methods
3. the missing evaluation of the results against literature results.

In the following, I will go through these points.

Method 1

The theoretical relationships between the two sensors' reflectances that are used to take into account differences in spectral response functions are a very important tool to correctly evaluate calibration uncertainties. However, their derivation is only roughly sketched and necessary details about these radiative transfer calculations are missing and should therefore be added to the manuscript. First of all, not only the range of the sun-satellite geometry should be given but also the way how this range is sampled (How many sample points have been used? Are these sample points equally separated or random distributed?). Since only ocean pixels were collocated, I imagine that an ocean surface was assumed for the simulations. Is this really so? Which parameterisation was used for the ocean BRDF? If the BRDF depends on wind for the consideration of white caps, which wind speed was used? The cloud phase was also not mentioned: are you considering only water clouds or also ice clouds? In case:

Why did you select only water clouds? How did you parameterised optical properties of ice crystals? While you talk about a RTM, you do not mention which RTM you used. Is it SBDART? Furthermore, gas absorption must be correctly accounted for, usually by means of a correlated-k method. Which one did you use? How many sample points were considered for the single satellite channels? Since ozone absorption and water vapour absorption affect measurements in the given spectral intervals it is also important to mention which atmospheric profiles were used. Although not essential, the indication of the vertical cloud location inside the atmosphere (cloud bottom and cloud top heights) are also desirable. Finally, the overall number of radiative transfer calculations performed should be mentioned. Please clarify these issues.

While MODIS is calibrated against reflectances (MODIS is a reflectometer) and these reflectances are directly available in the MOD02/MYD02 products, SEVIRI reflectances must be computed starting from radiances under the assumption of some solar constant value. Please specify the values used in this study.

An indication about the spatial (and temporal) distribution of the collocations as well as their number should be given.

Furthermore, it should be explained why MODIS band 1 and not MODIS band 15 (743–753 nm) was selected for the calibration of the MTSAT-1R visible channel centred at 724 nm.

Please provide a plot of all three (four) spectral response functions (MODIS, SEVIRI-8/9, MTSAT-1R).

Method 2

The relatively simple use of CTT thresholds for the distinction between water and ice clouds seems not very reliable. In principle, a MODIS cloud top phase product is also available and represents the basis for the determination of cloud optical thickness of the MODIS algorithm. Thus, this quantity should have been used instead of or in addition

to the CTT threshold tests mentioned in the manuscript. Please comment on this.

Effective radius of clouds, which is also produced by the MODIS team, does not have a large impact on reflectances in the visible spectral range but for such an application concerning calibration accuracy it would be recommendable to make use of this quantity. Please comment on this.

As for Method 1, a complete description of the RTM and all the input data including radiative transfer solver, ocean BRDF and so on should be given. Furthermore, the choice of the tropical atmospheric profile should be better justified. The ozone absorption band in SEVIRI's 646 nm channel has a small influence on the measurement but in the worst case it can range between 5–10 %, which is also the range of the effect that the authors want to investigate.

Method 2 is based on radiative transfer simulations of SEVIRI reflectances starting from optical thicknesses derived from MODIS. Data is collected to 0.5° boxes and compared with measured SEVIRI reflectances averaged over the same box. The step from the high resolution MODIS optical thicknesses to box averages of simulated SEVIRI reflectances is discussed in detail. Two approaches are conceivable: the first one takes the average of the MODIS optical thicknesses over each box and uses it as input for the RTM to compute the SEVIRI reflectances at the box level. The second one first makes a RT computation for every MODIS optical thickness to compute high resolution SEVIRI reflectances and then exploits some sort of averaging procedure to obtain SEVIRI reflectances at the box level. Due to the nonlinear dependence of reflectance on cloud optical thickness, these possibilities produce different results. It is shown that the first procedure produces biases (the PPH bias). The second procedure, where high resolution SEVIRI reflectances are averaged over the entire grid box, is more accurate, but its accuracy also depends on the way how reflectance averages are computed. The authors propose the use of the LN-ICA method (Oreopoulos and Davies, 1998) and show, with the help of an extended MODIS dataset, that it is more accurate than the first procedure. At this point two considerations must be made.

1) The first consideration is of general nature and regards the definition of the PPH bias and the method proposed to create simulated SEVIRI box reflectances. One usually wants to derive accurate low resolution (in this case for the grid boxes) reflectances starting from a high resolution spatial distribution of optical thicknesses (τ) making use of a 1D RTM. To this end, the errors produced by the neglect of 3D effects must be assessed. Thus, both 1D and 3D RT calculations for the same cloud field are performed and results compared at the box level. Here, however, the context is slightly different. The τ distribution is derived from space-borne (MODIS) measurements, i.e. each single optical thickness has been derived from a 3D (nature is 3D) reflectance measurement by means of a 1D RT model. Thus, shadowed (dark) cloudy pixels will presumably possess lower τ than in reality while illuminated (bright) cloud sides will probably show too large optical thicknesses. The occurrence and magnitude of these effects depends of course on the given sun-satellite geometry and on the cloud structure itself. Any way, these optical thicknesses are determined in such a way that a 1D RTM is able to accurately reproduce the (3D) measurements. This means that the authors use an optical thickness field that is affected by 3D effects determined by the MODIS characteristics to first compute a 1D reflectance field for another satellite sensor with different viewing geometry. I think that this method could produce systematic differences between the simulated SEVIRI reflectances and the measured SEVIRI reflectances because the input optical properties are not the "real" cloud properties in the given boxes, but the cloud properties as observed by MODIS under a given sun-satellite geometry that is different from the geostationary one. Exactly because of 3D radiative effects, systematically higher or lower cloud optical thicknesses could lead to biased SEVIRI reflectances. In order to minimise these effects it is very useful to screen the data in the way the authors do: the use of fully cloudy grid boxes with small internal variability should minimise these problems. However, it is very difficult to say whether this procedure is bias free. Please comment on this.

2) The second consideration refers to the example summarised in Table A1. The authors show that the lognormal averaging procedure using simulated high resolu-

tion MODIS reflectances produces better agreement with the measured MODIS reflectances averaged over the given grid box than the PPH computation where the averaged MODIS optical thickness over the grid box is used in one RT calculation to derive the MODIS grid box reflectance. First of all, it would be interesting to know how the simpler ICA averaging procedure performs. Second, this MODIS-related example (see Table A1) cannot be directly used to exemplify or support the method proposed. Here, there is no reason for expecting significant deviations between the simulated and the measured MODIS box reflectance. If the MODIS derived optical thickness (1D) can accurately reproduce the MODIS measurements (3D), already and maybe especially the usual ICA average should give accurate coarse MODIS reflectances that are similar to the average of the real MODIS measurements. Furthermore, the bad performance of LN-ICA against PPH in October 2006 and January 2007 should be explained. As far as Fig. A1 and Table A1 are concerned, the authors should specify the details of the MODIS data used, e.g. whether the investigation was at the global scale and how many fully cloud covered grid boxes could be evaluated.

Again, for the quantification of the ICA error the Monte Carlo model should be referenced and details of the simulations (correlated-k, cloud optical properties...) should be given. A justification for the choice of the extinction coefficient value of $0.005\,\mathrm{m}^{-1}$ is also expected: is this a typical value? The extension of 2D profiles to 3D profiles should be explained in more detail. According to Fig. A2 it seems that the authors mainly simulated clouds with iced tops while Method 2 mainly refers to water clouds, at least as far as SEVIRI is concerned. Does this affect the evaluation of the ICA error? It is also stated that the ICA bias represents a random noise that can be reduced by averaging over time or space. This contradicts also Zinner et al. (2006, Fig. 9a) where depending on cloud cover the ICA bias is plotted. Please check or explain results concerning this issue.

A posteriori, looking for instance at Fig. 2, a very similar performance of Method 1 and Method 2 is observed which seems to confirm that the conditions used in Method 2

to minimise 3D effects (both in the MODIS retrieval and in the RT simulations) were effective. However, it should be stated that Method 1 and Method 2 are related to each other because cloud properties input to the RTM have been derived from the reflectance measurements that are used in Method 1 to find collocations. Ham et al. (2009) show for instance that detailed 1D RT calculations are able to reproduce the MODIS shortwave band reflectances with an uncertainty of 5%. However, the computations in this paper could not be as accurate as those in Ham et al. (2009), because ancillary information (like atmospheric profiles) are missing, such that one could expect a similar or worse accuracy than for Method 1. Thus, the benefits and characteristics of this method should be clearly stated and emphasised with respect to Method 1. Possible characteristics of Method 2 can be the fact that it only considers areas with homogenous and complete cloud coverage. Furthermore, this method enables to take into account the different spectral response functions in a more detailed way than in Method 1.

The discussion of 3D effects and possible measures to minimise them is an important topic that is rightly pointed to in the manuscript. In this context, the plane-parallel error (PPH) and the ICA error are discussed, mainly in Appendix A. Because it represents an important aspect of the method, this discussion should be moved forward to Sect. 2.2 under consideration of the above comments.

Method 3

The authors cite Sohn et al. (2009) where the DCC detection applied to MODIS is explained. Adaptation of this method to SEVIRI and MTSAT-1R is presented, but it is not clear whether the same threshold values are applied to all three sensors. Please specify. Furthermore, it should be pointed out that MODIS is not used here at all.

As for Method 2 and Method 1, more details about the RT calculations should be given. All considerations related to 3D effects as well as all measures used to minimise them should be provided in this Section (Sect. 2.3) and not postponed to the Appendix.

Application of the Methods

For all methods proposed in the paper four or seven months of data were used to assess inter-calibration differences between SEVIRI and MODIS and MTSAT-1R and MODIS. First of all it should be explicitly mentioned in the text how many targets could be used in each method. Furthermore, and most importantly, the given months should be separated. Of course, enough targets must be still available to obtain statistically significant results. If this is not possible with one month, then two consecutive months could be considered. The point is that due to sensor degradation (as explained in the manuscript) calibration accuracy could change with time. Mixing up different months from different years increases the chance that measurements with different calibration accuracy are used. As far as both Method 1 and Method 2 are concerned, I recommend to consider Terra and Aqua separately. Although both have a radiometric accuracy of $\leq$2 %, they can still have different calibrations inside this range.

In general, it would be helpful not only to see scatter plots, correlation coefficients and biases, but also standard deviations. As far as biases are concerned, one should distinguish between relative and absolute biases, and between relative difference and bias (a relative difference for one pixel is not a bias: see for instance Fig. 3 where histograms of relative differences and not histograms of biases are plotted). Furthermore, the authors could consider the use od 2D histograms to indicate how the points are distributed along the $x = y$ line: in scatter plots it is not possible to distinguish where the data points are concentrated.

Finally, a comparison to the literature like Doelling et al. (2004); Govaerts and Clerici (2004) is missing. In particular, since the presented methods yield different results the consideration of additional calibration sources will be very interesting. In this context, is the observed difference in calibration accuracy obtained from Method 1 and Method 3 for SEVIRI really related to a saturation effect? How is the design specification of the channel range? Does the 10 bit SEVIRI digitalisation of the signal play any role?

Further comments:

Abstract: Please mention the time periods investigated for the two satellite instruments.

Abstract, page 12630, line 11–13: "In the simulation, the three-dimensional radiative effect of clouds was taken into account and was subtracted from the simulated reflectance to remove the simulation bias caused by the plane-parallel assumption.": In principle, the authors do not do a full 3D radiative transfer simulation and they do not know the magnitude of these 3D effects in detail, but they try to minimise them. Please reformulate this sentence.

Abstract, page 12630, line 18–21: Method 2 and Method 3 provide very similar results (see above), while Method 3 yields differences that are not fully understood. Thus, I would say that the three methods can yield valuable information to monitor the calibration performance, but I am not sure whether a combination of them is really necessary.

Page 12632, line 7: Please explain why the target for the maximum calibration uncertainty is 5 %.

Page 12632, line 13: Please cite older authors like Vermote and Kaufman (1995); Govaerts et al. (2001); Hu et al. (2004).

Page 12634, line 13: "to reduce calibration errors." –> "to reduce calibration errors and parallax effects."

Page 12636, line 21: How do the authors come to this value of 227 K?

Page 12636, line 27: "optical properties of mixed-phase" –> "optical properties of potentially mixed-phase": one could have supercooled water droplets.

Page 12639, line 4–5: Please make a complete list of all the conditions used: SZA$\leq 40^\circ$ (why "e.g.,$\leq 40^\circ$"?), which homogeneity checks and so on?

Page 12639, line 20–24: Why does the bias depend on the magnitude of the reflectance?

Page 12643, line 7: Please quantify the scatter mentioned here.

Page 12654, Table 1: Please explain the meaning of "varying from 0.1 to 1.0 with a 0.1 increment". If you binned the MODIS data, does 0.1 mean that you took MODIS reflectances between 0.0 and 0.1 ($0.0 \leq refl < 0.1$)? In that case, the mean values of the MODIS reflectances in the given intervals should be given as well.

Page 12655, Table A1: How does the ICA perform?

Page 12659, Fig. 3: "Monthly frequency histograms of measurement biases from simulated values at SEVIRI 0.640-μm channels of (a) Meteosat-8 and (b) Meteosat-9 form Method 3. Relative errors are given for the measured reflectances as percentage errors from simulated values. Mean biases inferred from the ray-matching method (Method 1) are also given as vertical grey solid lines." –> "Monthly frequency histograms of relative differences between measured and simulated reflectances for SEVIRI 0.640-μm channel of (a) Meteosat-8 and (b) Meteosat-9 from Method 3. Mean biases inferred from the ray-matching method (Method 1) are also given as vertical grey solid lines."

Page 12660, Fig. 4: The x axis range selected makes it clear that high reflectances are observed here. However, a smaller scale (e.g. 0.8–1.1 in the x axis) could enable

to observe some more detail of the data cloud. Please consider whether it could be more useful to zoom into the graph and change the scales.

Page 12662, Fig. A1: It is not really clear from this figure whether the biases become smaller when the grid spacing is increased. In particular, for the 0.3° grid size, it seems that new larger biases appear that were not present in the 0.2° grid size plot. Why is it like this?

Page 12664, Fig. A3: Is the ICA bias here an absolute or relative bias?

Technical corrections

Please replace the use of the past tense in expressions like "In this study, we explored..." with the use of the present "In this study, we explore..." throughout the whole paper.

Please check the usage of "grid" and correct it when needed: in many cases ("at each grid...") a grid box is meant and not the grid itself.

Page 12631, line 4: "may be changed" –> "may change".

Page 12631, line 12: "The inter-satellite calibration method" –> "Inter-satellite calibration".

Page 12631, line 23: "the vicarious calibration" –> "vicarious calibration".

Page 12631, line 27: "surface properties, desert" –> "surface properties, deserts".

Page 12631, line 28: Please cite one of the older papers like Fraser and Kaufman (1986).

Page 12631, line 28: "Knapp and Haar" –> "Knapp and Vonder Haar".

Page 12633, line 1: "solar channel" –> "solar channels".

Page 12634, line 20: "(SZA)" –> "SZA".

Page 12635, line 5: "MTSAT" –> "MTSAT-1R".

Page 12636, line 6: “As in the Method 1” –> “As in Method 1”.

Page 12636, line 7: “data remained” –> “data was considered”.

Page 12636, line 12: “of less than” –> “smaller than”.

Page 12637, line 18–20: “The SBDART model considers the multiple scatterings by atmospheric particles under the assumption of the plane-parallel atmosphere.” –> “The SBDART model considers multiple scattering by atmospheric particles under the assumption of a plane-parallel atmosphere.”.

Page 12638, line 5: “two homogeneous conditions” –> “two homogeneity conditions”.

Page 12638, line 10: “to monitor” –> “to ensure”.

Page 12639, line 14: “were selected” –> “was selected”.

Page 12640, line 20: “smaller those” –> “smaller than those”.

Page 12640, line 27: Please remove “The biases were given in a percentage ratio.”.

Page 12641, line 5: “in the Method 3” –> “in Method 3”.

Page 12641, line 13: “However, explanation” –> “However, a more detailed explanation”.

Page 12642, line 1: What is JAMI?

Page 12642, line 15: “may due to the scan problems” –> “may be due to scan problems”.

Page 12642, line 26: What does “monthly modes” mean?

Page 12642, line 29: “from SEVIRI visible” –> “for the SEVIRI visible”.

Page 12643, line 17: “covert” –> “convert”.

Page 12644, line 5: “small SZAs” –> “moderate SZAs”.

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