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Interactive comment on "In-orbit Earth reflectance validation of TROPOMI on board the Sentinel-5 Precursor satellite" *by* Lieuwe G. Tilstra et al.

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The paper by Tilstra et al. on the In-orbit Earth reflectance validation of TROPOMI on board the Sentinel-5 Precursor satellite is an important contribution to the meanwhile significant history of knowledge in characterising this class of instruments, since the launch of GOME-1 on ERS-2. The paper is well written and organized and it shows robust and convincing results on the assessment of the TropOMI instruments radiometric in-flight calibration accuracy using independent radiative transfer (RT) forward modelling of the expected Top-Of-Atmosphere (TOA) radiances in the region between 328 and 2314 nm.

The fact that the results are convincing and robust with respect to the provided sta-

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Discussion paper

tistical analysis of the reflectance correlations (between simulated and measured results) is not a given, since previous attempts to use RT model results in evaluating the in-flight calibration accuracy and performance of this class of sensors were limited predominantly by uncertainties in model inputs.

In this respect the statistical significance of the presented results, and the fact that they may be used as a robust evaluation of the sensor performance for most of the large spectral regions investigated here, makes it a unique contribution, generally in the field of post-launch high-spectral resolution sensor calibration in the UV to the near infra-red (NIR).

In this respect, and in view of future missions in development (Sentinel-5), or planned mission like the proposed High-Priority Copernicus Candidate CO2 monitoring mission (HPCC CO2M), it is unfortunate that the short-wave infra-red (SWIR) band evaluation had to be excluded from the results, probably because of systematic error contributions in the RT model surface reflectance input. In view of the significantly stronger focus of S5 and CO2M on the SWIR, improving the analysis and associated methods in this spectral region would be very welcome (and as suggested by the authors they consider this possible provided a better surface albedo input becomes available).

The authors address the two key error-sources for the overall approach in quite some detail. One is the selection of real clear-sky Rayleigh scattering scenes, with no or negligible residual cloud or aerosol scattering contribution. The other is the accuracy of the used surface reflection data, and in particular their angular and spectral variation (spectral BRDF), which is a large contributor - in addition to the accurate knowledge of the surface albedo itself - to the total budget of the forward modelling RT error and has significantly hampered previous attempts for carrying out such studies.

Cloud screening is predominantly done using co-located VIIRS cloud detection, and aerosols are detected by deriving an Aerosol Absorbing Index (AAI) from the measurements themselves. Here the authors state that they avoid a vicious circular problem by “correcting” the reflectances. However it seems not quite clear if the way these valuse are corrected is then actually implying an overall iterative approach, i.e. going through the full procedure, coming up with an radiometric offset (or residual between measurement and RT result), then use these results to correct the radiances, and finally repeat the procedure until some convergence criterion is fulfilled. This aspect should be, from my point of view, better described or clarified in the paper.

The accuracy of the used surface reflectance is for sure the most critical aspect for the performance of the RT forward model results in the described validation approach. The author’s present 4 methods to validate the Lambertian Equivalent Reflectance (LER) database derived from OMI and SCIAMACHY, the latter being probably used in order to cover the SWIR. The missing angular variation is considered as one of the largest errors in the existing OMI and SCIA LER databases, especially over vegetation, and this is why the chosen filtering on low quality LER values makes use of BRDF and albedo information as provided by Modis the Terra or Aqua satellite platforms. This however raises two questions: 1) Since the problem in the missing angular information in the LER has already been described for the existing LER databases derived from GOME-2, and therefore an angular dependent version of the GOME-2 LER database has been developed in the context of the Atmospheric Composition Satellite Application Facility (ACSAF) activities for Metop, the question arises why this database has not been used here. Especially since, in the end, the SCIA LER database in the SWIR shows deficiencies, and the analysis of this region had to be excluded anyway. 2) In the context of TOA test-data simulations of this kind of spectrometers, the MODIS surface albedo and BRDF is frequently used in conjunction with MODIS surface type characterisation and the ADAMs (A surface reflectance Database for ESA’s earth observation Missions - https://nebula.esa.int/sites/default/files/neb_study/1089/C4000102979ExS.pdf) spectral database, which provides the possibility to calculate the angular dependent BRDF at any wavelength from the UV to the SWIR (using principle components of spectral vectors for various surface types). This proved to provide realistic BRDF values in the wavelength region covered here in simulation studies for that type of sensors. Why has this option not been considered? Since this approach might turn out to be useful to solve the issue for the SWIR band radiometric performance validation for TropOMI.

I am sure the authors will have some convincing answers on the few issues I have raised here, in which case I can highly recommend the paper for publication in AMT.

Minor and editorial issues:

p.2, l. 13: The reference to GOME-2 on Metops could be associated to the relevant paper by Munro et al.

General, the exclusion of band 8 should probably be motivated more towards the beginning of the paper.

Section 3.4. The temporal aspects on using a database derived from SCIAMACHY and its application to a recent missions, should probably be mentioned and/or addressed, especially for vegetation and crop surfaces.

p. 8, l.3. “From Fig. 1 it can be inferred…”. I find it actually quite difficult to infer it from the Figure if the differences are small. From the Figure 1 one can only for sure infer that they are in the right overall magnitude and spectral relation.

p. 10, l.17ff: How is the inhomogeneity of the target area actually determined? Is it just derived from coastline maps and surface type database or from the TropOMI radiance variance itself - which would be the best option I guess? In this respect, can the surface albedo as derived from the OMI and SCIA databases can be considered a true average over the 1 by 1 box as used here as a target?

Figure 3 shows the location and number of measurements over the defined period and number of days for clear sky scenes. However, it is not clear if this is the final statistics for all 56 days applying method 4 for cloud screening. The distribution of the locations of the latter, which actually go into the results, would here be of highest interest.

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