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Atmospheric Measurement Techniques Discussions

Interactive comment on "In-flight calibration results of the TROPOMI payload on-board theSentinel-5 Precursor satellite" *by* Antje Ludewig et al.

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The paper by Ludewig et al. on the In-flight calibration results of the TROPOMI payload 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 follows the individual calibration steps in a systematic way. This is important in order to be able to compare the presented results and calibration approaches to past instrument in-flight calibration efforts, and to compare their associated calibrated level-1 radiance product quality performance. This may then inform both, ongoing reprocessing efforts for

the derivation of fundamental climate data-records from instruments like SCIAMACHY, GOME-2, OMI, OMPS, as well as the preparation of future instrument on-ground calibration and commissioning campaigns, and their continuous like Sentinel-4/5, TEMPO, but also the High-Priority candidate Copernicus CO2 monitoring Mission (CO2M).

I can recommend the paper for publication in AMT but would like the authors first to consider and address the following issues (accompanied by a list of minor remarks) Irradiometric calibration, observed degradation and its correction.

Section 12 and 13 describe the approach taken to correct for some partially significant, observed degradation effects especially in the UV. The overall approach seems sound (section 12). However it is not obvious for me how the degradation model approach and application in section 12 is related, or better decoupled, from the correction of the observed, partially quite significant offsets (up to 15%) in the absolute irradiance calibration of the solar port (section 13).

My understanding from the paper is that the derived spectrometer component (from the 312 to 330 nm region) has been accounted for by a degradation correction, which is, again to my understanding, applied spectrally neutral to the full UV detector irradiance. Is this correction then also applied for Earthshine measurements, as one would expect it to be, because it is considered an effect of the common optical path? In case yes, I guess that the normalization day/orbit 2818/2819 is then used for an adjustment to OMPS, such that any likely degradation happening to the irradiance signals until this point is corrected for by reference to OMPS. Again, one expects an unknown degradation to have happened also to the Earthshine path until orbit 2818/2819, which would then lead to a differential degradation in reflectance after adjustment of the solar irradiance, and especially in case nothing is done additionally for the Earthshine data (and probably there are also some finite yet different accuracies for the radiometric key-data to be taken into account).

The choice of OMPS seems also very subjective. While it is stated that OMPS irradiance has been ”independently calibrated”, it is not stated what “independently” would mean in this context (without adjustment to reference spectra? If yes, then this should be stated). I would maintain that it remains just a choice. The results show a close to 3% difference with the Dobber et al. spectrum after adjustment. In contrast, all three GOME-2 instruments shave shown smaller residuals than 3% to the Dobber reference spectrum, above 300 nm at the beginning of live, without (!) adjustment (so using the on-ground derived key-data only). So this choice of a reference solar spectrum would leave a potential unknown "offset" of 2 to 3% with respect to other instruments and their absolute calibration after degradation correction. Since 2 to 3% accuracy is effectively the current limit on the knowledge of the solar irradiance accuracy in the UV and VIS wavelength region in general, such a choice for sure can be made, but it should be presented as the limit of the knowledge in the absolute calibration accuracy then also for this mission. Moreover, this would then also be the limit of knowledge on the Earthshine radiance accuracy, with a potentially even larger error on the reflectance.

In this respect, the question is why an independent Earthshine degradation modelling has been ruled out. For previous missions GOME-1, 2 and SCIAMACHY degradation modelling using global averages of cloud free Earthshine data showed quite some success, and also Libyan desert degradation modelling should not be ruled out.

Finally, the derived spectrometer component in Section 12 seems to be on the order of 1% per 1000 orbits (Figure 11). In contrast the observed WLS and LED signal degradations seem to be lower or on the same order. I am wondering why the use of the internal light sources then have been ruled out for degradation monitoring or even correction, or how their “output degradation” could have been identified as such, when the identified spectrometer component is on the same order or even more significant. Is there an optical component in the path (like another folding mirror) between the spectrometer and the WLS, such that any direct Earthshine degradation modelling using these sources cannot easily be done? It might be interesting to look at the ratio of calibrated SMR and calibrated WLS, and their (differential) evolution over time and

spectrally in this context.

Additional comments

Section 3, l 80ff: How exactly non-linear is the observed decrease of the light sources and can this decrease be attributed to the sources or is it already part of the optical chain for WLS? It should not be ruled out that this is simply a consequence of the spectrometer degradation observed in Section 12 (see before).

Section 5, l.90ff: I would assume the temperature dependency of the dark current has been measured on-ground. From these measurements it could be stated here what is the projected dark current orbital dependency using the observed orbital detector thermal stability from HKTM.

L.110ff: The change in the gain during manoeuvres is not further explained. Can any reason be given for this?

l. 145ff: It would be interesting (and helpful for future missions) to get an idea (statistically) on the extend of blooming in pixel space. E.g. by providing a histogram (or table) on the number of occurrences over the number of pixels affected per event. Does such a statistic exist?

Section 8 on geo-referencing: Has any attempt be made for geo-rectification using VIIRS data? This should provide very accurate geo-referencing knowledge also on the point-spread function. Can anything be said about the alignment of the other bands not used in the geo-referencing analysis? Or can some qualitative assumption be derived from the optical setup (telescope) and alignment? A discussion would be needed here I think.

Section 10 on slit irregularities: From Figure 6 it looks like the WLS exhibits significant spectral structure. Why is this? Actually, wouldn’t a highly structured spectrum like the solar lead to a better correction?

Section 11 on goniometry: The azimuthal maximum variation of the sun should be reported in this Section in order to motivate/justify the restriction to 10 degrees, even though 15 degrees have been measured. Is the orbit stabilized, and for how long in the mission? Or in other words, is there any restriction in future ground track drifts concerning the validity range of this data?

Section 11, on the origin of the remaining residuals in the goniometry key-data derived in-flight: I would guess that they are probably a combination of diffuser features, speckles, and especially instrument drifts between individual measurements and the temporal position of the normalization measurement. In addition, one should find the pattern of the observed degradation correction residual in such a potential drift, I would assume. Since the measurement period was quite long (400 orbits), and it was in an early state of the mission, can effects like gain drifts during this period, and as reported in the earlier sections, be ruled out? It would be good to discuss the status of the mission at the time of the dedicated measurement period (start orbit, overall platform thermal stability etc...), and if the measurements have been filtered for outliers.

Section 12, degradation model: Why would one expect that all components are “perfectly exponential”. At least in the long-run. Since this is not what is observed with other instruments, and for sure not in case of a potential mirror contribution. Is there a long-term trend observed in the Rk and Pk components?

l. 364: “but especially in the UV range it is unclear if it is reliable”: Which spectrum is referred here to? Since we have observed that the Dobber et al., spectrum shows clearly better results for GOME-2 for wavelength below 300 nm at the beginning of the mission and without any adjustments than all other available reference spectra. I fear that at this stage this is no discussion about the truth, but probably more about inter-instrument consistencies.

Editorial comments

General: Although it has been describe multiple times elsewhere, a table of band numbering associated with source region "UV", "UVIS, "NIR" "SWIR" and associated wave-

length ranges would be of help for the reader to have at hand up-front. Since band numbers, detector labels and source regions are used multiple times in exchangeable ways in the paper.

Figure 5: "...within the requirements" -> add black lines in brackets p. 10ff: The plots in Figure 6 and the reported row numbers in the text (e.g. line 208) are different. The caption indicates the Figure shows the binned count. Somewhere at least a written translation should be made. E.g. in the caption: bin x corresponds to pixels yy. or similar.

p14, ;l263: Check sentence: "For double processing, so (?) ..." l.380 switch -> with

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