

Interactive comment on “In-flight calibration and monitoring of the TROPOMI-SWIR module” by Tim A. van Kempen et al.

Tim A. van Kempen et al.

t.a.van.kempen@sron.nl

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Dear Mrs./Mr., We would like to thank the referee for their time and effort in reading and reviewing our paper with constructive comments. Please find our answers to your comments below in italics.

General comments: The paper gives a good overview of the in-flight calibration and monitoring of the SWIR module and it is good to see that the SWIR module of TROPOMI is that stable over time.

Thank you

Specific comments:

Page 2, line 7: 'dedicated the out-gassing the instrument' Is it not more likely for preventing contamination of the instrument from e.g. the platform, multi-layer insulation mainly water outgassing by making sure the instrument - especially if cooled SWIR - detectors do not act as a cold traps and get contaminated? Usually much care is taken with the instruments during integration, assembly and testing under cleanroom environments that the outgassing of the instrument itself is minimised. Suggestion to rephrase.

We agree, rephrased.

Page 2, line 12: 'During nominal operations'. Not only during nominal operations, but the whole life time, even on-ground instruments should be monitored. Suggestion to rephrase.

We agree, rephrased.

Page 2, line 18: The word 'dark-flux' is used here and throughout the document, isn't it rather dark current? 'CKDs for . . . were also derived on-ground.' Since some CKDs may also be derived on board, and may be updated, is the 'also' meaning also possible updated in flight? Suggestion to rephrase to make the statement more explicit and add also which CKDs are updated in flight.

We agree that the difference in terms between dark flux and dark current is rather confusing. This is caused by a rather complicated issue. Dark current is defined as the current produced by the detector at its operational temperature. However, at the SWIR wavelengths, the measured dark current is always an addition of the true dark current (produced by the detector itself) and the signal from an external thermal background (i.e. anything the detector looks at in the dark). Even if something like a baffle is covering the detector, this will produce a signal equal to the Planck curve at the baffle temperature in addition to the true dark current. Internally we defined and used dark flux to distinguish it from a true instrumental dark current. During the instrument development phase this proved to be very valuable. The 'also' was in reference to the

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Discussion paper



straylight and ISRF CKDs. All CKDs currently used are derived on-ground. Rephrased slightly for clarity. We have the capability to derive and update some of the CKDs in-flight (dark flux, offset, Quality mask, noise), but have so far not seen a reason for this. This capability has not been clearly described, but is now included in the start of section 2.

Page 2, line 19: 'Signals of the sun as seen over the two diffuser' Please refer to paper with instrument design and/or add sketch of light path via diffuser for better understanding for the readers. 'internal lamps' Please explain which kind of lamps, refer to instrument design and add sketch of light paths.

The instrument design, which includes the light path description and its requirements on the solar irradiance path via multiple diffusers fall under commercial control of Airbus and TNO and not to SRON or any of the authors. They were sadly not published. We have included an overview figure with the location of the on-board light sources with the light paths of the SLS and WLS. The location of the CLED is shown, but as it is not usable, no light paths are indicated. The DLED is located inside the module next to the detector.

Page 3, line 4: Isn't there also a CLED in the SWIR path? see e.g. Kleipool [2018] calibration unit description. Please add and shortly explain the light path.

There is, but the emission properties of the CLED (i.e. the amount of photons produced) do not extend into the SWIR wavelength range. This was checked during on-ground calibration. We added a footnote to explain this.

Page 3, line 14: 'due to operational restrictions' this part is not understood, can you please detail what the restrictions are or why they are there or refer to another publication.

This has two reasons. First, the amount of time designated for calibration with one of the on-board lights (DLED, WLS or lasers) during the eclipse side of an orbit is only

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a few minutes. This has been rephrased and referred to van Hees et al., 2018, which detail the operational restrictions.

Page 3, line 18: according to Hees et al. [2018] this was already planned "However, as the diffuser mechanism is a life-limited item, only during the on-ground calibration campaign and during the in-flight commissioning phase, measurements will be performed with a moving diffuser." The way it is written now, it seems as if it was a later decision during operation. Suggestion to rephrase according to Hees et al. [2018].

Rephrased

Page 3, line 29: 'digitized typically with 12, 000' Is this binned on non-binned?

unbinned

Page 4, line 1: 'Solar irradiance or signal from the on-board lights' this incl. straylight.

Agree. Rephrased

Page 4, line 4: 'amount of light lost' suggestion to change to degrading due to light loss, contamination

Rephrased

Page 6, figure 2.: The unit used here is Spectral Photon Radiance, why not use spectral radiance unit in $[W m^{-3} sr^{-1}]$? Is this the unit used for the L1b products in the SWIR? As first image Iraq is shown, was there a special reason to select Basra? Why not e.g. another big city? or volcano?

Spectral Photon radiance is indeed the unit used in L1b products and adopted here for consistency with the L1b released product. Basra was selected as it is the clearest example of the contamination. The signal also originates not from Basra itself but from the many oil drilling sites near the city. These burn off excess gas at night, and the fire is the origin of the signal. We do not see the city itself. Signals of big cities are typically not seen at the eclipse side. Other sources seen are natural wildfires. Volcano's have

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not detected., but in the limited available data (background measurements with the FMM open during the E1 phase) we have not actively looked for them.

Page 7, line 7: 'in form of blue bands' these blue bands in Fig.4 have negative numbers, is there less light in the background measurement with the absorption lines than during the on-ground measurements? was the temperature the same? Please detail in the text.

Yes, During the on-ground measurements there was no air between the light source and detector.

Page 8, figure 4.: Suggestion to add also the on-ground result as graph for better understanding.

Respectfully disagree: given the very small relative differences (<0.2%), plotting the on-ground results on absolute scale is not going to be very illustrative.

Page 8, table 2: It seems difficult to compare FMM open with closed, since there are more than 1500 orbits difference, is it possible to compare open/ closed effects closer in time together? Also to have similar thermal conditions since the seasons are completely different. Otherwise there may be too many effects intermingled.

In answer to your first question: sadly no. Due to the schedule of the E1 period, open and closed comparison will always have a minimum 1200 orbits in between (orbit 2240 is about the earliest where enough useful measurements were obtained. see Fig. 6). During orbit 1100-2200 new calibration measurements for the relative irradiance were obtained. As such, closer in time can only be done by about 25%, which is not significant. However, it is our opinion given the results of the closed FMM monitoring (see Fig. 6) and the very low variance seen there, that any closed FMM measurement can be compared to any open FMM measurement.

On your second question: Thermal conditions of the SWIR instrument and all components in the optical path are not influenced by the seasons. The thermal conditions are

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nearly identical between all of the presented orbits.

Page 9, figure 6.: Why is the uncertainty varying? Are e.g. different temperatures measured during these 6 weeks? Or are different amounts of measurements taken? please add text.

Text added. It is the amount of measurements.

Page 10, line 10: 'clearly absent when the FMM is closed' Is then the conclusion to only measure background with the FMM closed? In case this is the conclusion, has the operational manual been adapted?

Text added. And the answers are yes, and yes.

Page 10, line 24/25: 'no correction is applied for the SAA. . .' is there any conclusion about this? Currently it may be interpreted that the author would prefer to have a correction, but it might also just be a fact, that the SAA is flagged as in many other processors.

Rephrased.

Page 12, line 4/5: 'Note that there appears a small systematic difference between the ADCs.' Detail what is meant here, which different ADCs, where is it visible, has it to do with the positive numbers in figure 7 up to column 500 and negative numbers from 500 to 1000?

Yes this has been rephrased.

Page 12, line 14: 'should not be used for retrieval of CO or CH4' this could be flagged, in case it is flagged and the processor is accounting for it accordingly suggestion to change to "is not used for retrieval of CO and CH4"

Done

Page 12, line 17/18: 'Therefore, calibration measurements to determine noise levels

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Discussion paper



should also be executed with the FMM closed' see previous comment: is this now operationally implemented? what are the issues with mission budget related to amount of movements of the FMM?

Correct. This was implemented by the team responsible for operations. FMM closing is indeed not done every orbit. However, since it was already planned for every other orbit, no additional FMM movements had to be scheduled so there is no effect on the number of movements of the FMM wrt originally planned operations schedule. This has little effect on the calibration quality.

Page 12, line 23: 'cosmic ray impacts' aren't these flagged and can then be excluded from the processing?

These are indeed flagged. However, flagging is done much later in the processing chain than typically processing level for noise measurements. In addition, the noise must also be able to distinguish cosmic ray impacts from actual noisy pixels. False positive/negatives or too rigorous flagging may obfuscate noisy pixels. Therefor a choice was made not to rely on flagging. Noisy pixels remain noisy for all measured frames, while cosmic ray impacts do not.

Page 13, line 7: 'This includes pixels outside of the effective area, which are no illuminated.' But this does not mean they are not functional, they are/could still be used. Suggestion to reword.

Agreed. Rephrased.

Page 14, figure 10: 'similarity' why should these be the same, this is not understood? One is a difference between two methods on the same data, while the other is a difference in time and pre/post launch? Please detail.

The difference between the on-ground and in-flight mean is dominated by impacts of cosmic rays. During on-ground calibration there are no cosmic ray hits, while in-flight measurements are riddled with them. The bi-weight spread method effectively removes

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the influence of the cosmic rays on the noise calculation. As such these figures look remarkably similar. This was detailed on page 12.

Page 14, figure 11: It seems as if dead pixels came alive again over time, are these not rather 'pop-corn' pixels, thus in a way sometimes bad, sometimes good? Please consider renaming the dead pixels which become alive.

In an initial classification we adopted many more categories. This very quickly became very too complicated with many different categories if we wanted to become very consistent ("outside illuminated area", "truly dead" – 0.0 remaining 0.0. "recoverable dead" – 0.0 but can recover. "Truly very bad" (>0.0 and <0.1 remaining there), etc. etc.). In addition to this, pop-corn pixels can exist in various flavors. First there are pop-corn pixels that are bad/good every few orbits (skirting the relatively arbitrary divide of 0.8). Second are pop-corn pixels that only change the noise behavior after annealing (i.e. heating of the array, which typically happens unplanned during anomalies). Given the complexity of such a categorization, there is the possibility any pixel may still change category (e.g. a pixel that has always been 'truly dead' may still turn into a pop-corn pixel) and the relatively small number of pixels (~ 2,000 on ~210,000 in the illuminated area), a choice was made to simplify this to three categories ('dead', 'bad' and 'good'). We agree that the nomenclature of the 'dead' category can be a little misleading. As such we have added a footnote.

Page 15, table 3.: See also comment above. How can there be less dead pixels after launch than on-ground? or are these to be seen as additional number of bad/ dead quality pixels? Please detail.

See above. The 'dead' label is a category with quality <0.1 and 'dead' was chosen as a nomenclature. There are several effects in play. The most likely is the thermal environment, which is much more stable in-flight than achieved on-ground.

Page 15, line 6/7: 'Although monitoring. . .' Why can it only be approximated? Please detail.

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Because we do not have access to external lamps (outside of the irradiance). Therefore several components cannot be characterized (using an external light source other than the sun over the solar diffusers is not possible for instance).

Page 15, line 8/9: 'the calibration sources. . .and/or diffusers.' and any other optical elements in the optical path incl. the video chain can be degraded. Please amend the text.

Done

Page 15, line 20: 'hypothesized this is the ... grating.' This is not understood, since the DLED is in direct proximity of the detector and it was understood the light path from the DLED is not via the grating. Can it not be some kind of etaloning via the protective glass of the MCT detector, and a layer which might have been on it? Or is the CLED meant here? Please consider changing text.

That is indeed a possibility. The hypothesis was not the grating itself, but the thermal balance (or lack thereof) in the instrument due to the very long relaxation times of the grating. However, after further looking into this, we can confirm that the hypothesis proposed by the referee is possible as well (either glass over the MCT detector, or a lens in the optical path to the DLED). We changed the text accordingly.

Page 16, figure 12.: Please detail, e.g. top DLED 2515 - 907, bottom DLED 2515 - 2707. The difference in the bottom is basically 1, thus no significant change measurable in about 200 orbits, is this understanding correct?

Done, and that is correct.

Page 16, line 7/8: 'less stringent stability limits of the WLS system. . .due to the DLED.' Is it understood correctly, though the WLS with less stringent stability shows with the resulting expected larger error bars the same range as the DLED measurements? Please clarify.

That is correct, and we added this to the text.

Page 17, figure 14.: Would it be possible to show the differences by normalising to one measurement instead of plotting them on top of each other to better see the differences?

Respectfully, we disagree with the referee. A figure as proposed by the referee was made before submission, but resulted in a) an identical figure near the peak and b) a very confusing figure near the edges of the detector where very little diode laser light is detected. Given the necessary information of the absolute scales at various distances from the laser illuminated area, we decided on this version of the figure.

Page 18, line 1/2: See above, please plot also the ratios normalised to one measurement (similar as shown for the ISRF e.g. figure 15) to better support this sentence.

The changes shown are typical measurement errors (given in the uncertainty in the second column), which cannot be easily represented in a normalized version, see above.

Page 23, figure 21.: The data seem to include 'pop-corn' pixels since number also decreasing again over time. See also comment above and consider changing the name 'dead pixels' for those who come alive again.

See above answer

Page 23, figure 23.: Suggestion to check with the SLS via diffuser if the same trend is visible of increasing signal. After all 2018 was solar minimum.

This is a very constructive suggestion and is a possibility we considered ourselves. However, due to the life limited item (oscillating diffuser) and amount of heat introduced into the system, such measurements are not carried out during regular operations and thus cannot be easily verified. Alternatively, this hypothesis can be proven correct by monitoring the diffusers over much longer times (three to four years). Note that at shorter wavelengths (UV to VIS), a degradation has been confirmed in the diffuser coating. This may very well have propagated to the SWIR wavelengths at very low

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Discussion paper



levels. Again, either measurements with an oscillating diffuser (currently unavailable) or very long-term monitoring will provide an answer.

Page 23, line 1/2: 'diffusers apparently becoming more effective' In this case this should then also be visible with the SLS via diffuser measurements over time. Has this been observed? Please detail in text/ with graphs.

The SLS signal does not pass over the same diffuser. Instead it has its own diffuser (which is never illuminated by the solar signal)

Page 24, figure 23.: Around orbit 3500 is one significant outlier, is something visible from the housekeeping data to explain this outlier looking at the telemetry? In addition can the jump around orbit 5500 be explained?

The outlier in orbit 3500 was due to a space craft anomaly in the spacecraft. This has been added to the text. The jump around orbit 5500 cannot be traced to a specific event.

Page 24, figure 24.: While most figures show error bars, they seem missing in this figure. Can they be added?

The statistical WLS error bars are omitted as they are not representative due to the uncorrected non-linearity. This has been added to the text in 3.4.2

Page 24, line 2: 'a rate 0.8 % per year' From the housekeeping data, is in the telemetry the voltage/ amps of the LED given? Are these values stable over time? Can this be checked and added to the text.

See attached figure. Voltage of the LED is (nearly) constant over time. Has been added to the text

Page: 26, figure 26.: Can the outlier with almost 3
No, it cannot

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Page 26, line 9: '(about 60 over 5 months) ' What is the reason the choose here 5 months and not either since launch or during nominal operation?

There are very little good reference measurements during the early parts of E1. We specifically chose to have the End of E1 to be the reference. The 5 months was a leftover from an earlier draft that used 5 months of data from the reference point. The current figure uses a full year of data from that reference point. That has been corrected.

Technical corrections general:

In general all technical corrections are adopted. Some comments are added to individual comments.

Figures are often in previous sections or next sections from their textual descriptions. For easier readability would it be possible to place them in the same section as where they are described?

This has been attempted, but is a limitation of the 1- or 2-column format

Abbreviations should be minimised and at least the first time of appearance be fully written. Page: 1 Line 1: short-wave infrared (SWIR) tropospheric monitoring instrument (TROPOMI)

Line 3: instrument spectral response function (ISRF)

Line 5: Change 'eclips side' to 'eclipse side'

Line 7: 'with little to no degradation' instead suggestion to provide values, e.g. smaller than $\langle x \rangle\%$

I do not think it is correct to express this into a single number as we include the detector, onboard lamps and grating. A single number can be misleading. The sentence has been rephrased.

Line 16: ultra-violet, visible and near-infrared (UVN)

Line 19: 'TROPOMI will produce' change to 'TROPOMI produces'

Line 20: Change 'yeilding' to 'yielding'

Page: 2 Line 10: 'the E1 phase' change to 'phase E1'

Line 12: 'the E2 phase' change to 'phase E2'

Line 14: 'planet each day' change to 'planet Earth each day'

Line 15/16: and...and... Is used, suggestion to use a comma and then only one 'and'.

Line 33: 'radiance' change to 'spectral radiance'

Page: 3 Line 1 'irradiance' change to 'spectral irradiance'

Line 4: Add detector LED after DLED similar to the other two sources.

Line 17: 'oscillation mode' This sounds as if there is a motor installed on the diffuser mounts? Please refer to a paper with the instrument design, where this is described in more detail or suggestion to add text.

See van Hees et al., 2018 for clearest explanation. Instrument design paper was not published and is property of industry (TNO and Airbus Defense and Space Netherlands). But yes, oscillating the mounts does require a motor.

Line 26: 'each pixel is read out individually' does this imply it is a CMOS detector? Please add a little more detail about the detector and possibly refer to another publication.

Yes it is. Sadly no publication exists clearly describing this.

Page: 4 Table 1.: PRNU - pixel to pixel non-uniformity

Line 10: Add '.' after 'complex correction algorithm.'

Line 4/5: 'Straylight is defined as any outside signal that does not follow the intended path onto the detector and is thus not part of the useful signal' The 'outside signal' may be confusing, since straylight may be ghosts, in-band straylight, out of field, out of spectral band etc. straylight. Suggestion to rephrase for better understanding for the

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readers.

Page: 5 Figure 1.: Last part of flow, 'Useful radiance' : Radiance assumes already processed to spectral radiance units, with the help of CKDs, is this meant here? Possibly rephrase or add steps for generating spectral radiances.

es, this is meant here. Rephrased to indicate unit conversions also take place. These are not monitored, and as such omitted from this diagram.

Line 4: 'dark-flux' possibly other naming? Are dark current correction meant here? Flux maybe confusing.

Page: 6 Line 4: 'November 2019', is November 2017 meant here?

Page: 7 Line 4: 'dark flux' see previous comments.

Page: 8 Line 2: 'thedifferent' change to 'the different'

Table 2: 'On-ground diff.' unit missing. Please add

Page: 9 Figure 5.: 'during the commissioning phase . . .' during nominal operation, since you stated before "Nominal operations started at orbit number 2818." Please change text.

Figure 6.: 'commissioning phase' according to text above this is already nominal operational phase. Please change wording.

Page: 10 Line 18/19: 'thermal variations as a function of the orbital phase' suggestion to show a graph over the 1 1/2 year time with the thermal fluctuations from the housekeeping telemetry of the SWIR instrument.

The timescales here do not match in this comment.. Orbital phase is a changing parameter within a single orbit. A graph over 1,5 years of thermal fluctuations does not represent information on this. We added a definition for orbital phase for this.

Line 30: 'if the data taken in the SAA are excluded from the analysis' suggestion to add that this can be done due to the SAA flagging.

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*This could not be done at that stage due to the processor version. It was done by hand.
Changed wording from 'if' to 'when'*

Line 31/32, last sentence: before a statement says they are different from Hoogeveen [2013], seems contradicting : "The amount of dark flux detected differs from the measured value reported in Hoogeveen et al. (2013). " Please bring in line, or detail.

Page: 11 Figure 7.: nominal operation phase, see comment above.

Figure 8: nominal operation phase, see comment above.

These figures use data from end of E1.

Page: 12 Line 5: ADC analogue digital converter.

Line 6: 'acceptable' suggestion to change word, like this it can be interpreted as 'unacceptable', since stated to be below acceptable levels.

Page: 13 Line 8: 'impacts or other hardware degradation' suggestion to eliminate the 'other' and change to 'impacts or hardware degradation'

Page: 14 Figure 11.: Please add dead (bottom red dots) and bad (top green dots).

Page: 15 Line 15: Add '.' after sentence.

Page: 16 Line 7: 'end of E1' change to end of phase E1' Is the WLS also in orbit 2515 similar to the DLED? Please provide orbit.

It was orbit 2513.

Page 18, line 15: Add 'phase E1' instead of 'E1'

Page: 19 Line 2: 'but conclusions apply to results obtained' it is assumed 'but conclusions also apply. . .' if correct please change text.

Page: 20 Line 1/2: Please in one line 'The' with a space in front.

Page: 21 Line 15: 'from April 30th. . .' isn't this already from an earlier date, since

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Discussion paper



starting already at orbit 1800 or so. Please change.

This is correct. It has been monitored even before orbit 1800, although the backgrounds were shown to be unusable. However, the references are taken in the week before April 30th. Text has been changed to reflect this.

Line 17/18: 'Larger-scale variations seen. . . manoeuvres.' Please provide orbits when this happened linked to the figures.

Data is typically only affected by 1-3 orbits. This means that the plotted number use less data (i.e. 6 instead of 8 input orbits). Only during very significant events would it show up in the figure. The only one is during orbit 3500 when an FDIR anomaly took place.

Page: 23 Figure 22.: Instead of 'Top' change to 'left' and 'bottom' to 'right'.

In a two column version of the paper, top and bottom are correct.

Page: 25 Line 4: '5.6%' might it be better to just state 6%, since with the kind of step degradations seen in figure 24 the number 5.6 may suggest a higher accuracy of the assumed linear prediction of degradation? Consider changing.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-270, 2019.

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