Interactive comment on "S5P/TROPOMI NO$_2$ slant column retrieval: method, stability, uncertainties, and comparisons against OMI" *by* Jos van Geffen et al.

Anonymous Referee #1

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Any early assessment of the data quality for a major remote-sensing mission (such as TROPOMI) is always welcome, and the study in review, "S5P/TROPOMI NO 2 slant column retrieval: method, stability, uncertainties, and comparisons against OMI", by van Geffen et al., delivers a timely and conscientious report on the particulars of the NO2 slant column density retrievals from the TROPOMI data. The article is well-written and reasonably compact. I perceive no major problems that may delay a prompt publication. My comments are mostly of a clarifying or editorial nature.

Specific comments:

P.8,l.11: The smaller-pixel size doubles the percentage of flagged NO2 retrievals. This

leads to additional ~2*10ˆ3 flags/orbit compared to the larger-pixel case. Please comment, if you may, on the possible cause of this doubling. Are these flagged retrievals randomly distributed across the orbit? Are they mostly related to the saturated pixels?

P.9,l.16. Please comment on the evolution of the radiance-irradiance wavelength difference along the TROPOMI orbit.

P.9,l.23: "A similar seasonal variation, though larger in magnitude, is seen in the OMI wavelength calibration data...". Fig. 34 from Schenkeveld et al. (2017) shows comparable (early-mission OMI data) magnitudes of the seasonal <w_s> variability, without any detectable long-term trends in the OMI VIS channel. Please re-phrase ll. 23-27 accordingly.

P.11, Eq. (9) As written, the intesity offset implies a [potentially] non-linear change of the wavelength sampling for radiances. Please check.

P.12,l.1. "For OMI/QA4ECV both a shift and stretch are fitted;..." Is the stretch term essential for OMI? Please comment.

P.12, Sect.3.4 - Please specify whether OMNO2A v2.0 uses the intensity offset term.

P.12, Sect.4.1, 1st par. - Please comment on how representative this particular TROPOMI orbit is. Have similar comparisons been done for different seasons? Can the shown trends be safely extrapolated on a ~year-round sample? This in particular applies to the tests shown in Fig.3b,e. To some extent, this question is answered in P.19. It is worthwhile to make a general statement earlier on.

P.14, last par. Perhaps it would be worthwhile to point to the big difference in the striping patterns delivered by various OMI algorithms, also mentioning that the forthcoming Sect. 4.3 quotes exclusively the OMI/QA4ECV stats.

P.15,l.5. "The increased SCD error visible in the TROPOMI data of Fig. 3d-e around $\theta \approx +20$deg is related to the presence of saturation effects above bright clouds along this particular orbit." Nevertheless, most of the corresponding saturation-affected GCD val-

ues seem normal. Does this imply that the 405-465nm retrieval range retains enough of the saturation-free data to provide ∼normlal-looking SCDs? Is there anything else helping to stabilize the saturation-affected retrievals? Please comment on.

P.16,l.18. The 4-times difference in the QDOAS/TROPOMI RMS estimates is hard to overlook, indeed. Considering the typical S/N∼1500 in the TROPOMI data, one may side with the RMS∼8*10ˆ(-4) provided by QDOAS. Similar- magnitude RMS∼0.5-1.0*10ˆ(-4) is frequently quoted by various groups working with ∼similar S/N data sets from various spacecrafts. The cited (Table 2) RMS∼0.2*10ˆ(-4) seems like an overly optimistic assessment. Any comment?

P.18,l.19 "Fig. 4c shows this RMS...". Please provide more details on how this value was calculated.

P.19,l.9 Is de-striping applied both to TROPOMI and OMI? Please clarify in the text.

Fig.6. Since the intensity offset has the highest impact over the cloud-free, clear-water ocean areas (Sect. 5), one may test this by segregating the data in Fig.6a into two cases (cloudy and cloud-free; probably, selecting even more extreme cases than in Fig.8) and commenting on. I consider this as an important test, in light of the findings from Oldeman (2018).

P.22,l.2 "The fact that the GCD value itself (Fig. 7c) is not appreciably affected by the time difference is very reassuring..." Actually, I find this really puzzling: cf. Fig.7b and 7c. The SCD error, as anticipated, linearly increases in time in Fig.7b, but remains essentially flat in Fig.7c. Please help me (and the readers) to interpret this.

P.23,l.12 Table 3: Please specify whether the TROPOMI (presumably, yes) and OMI (?) SCDs were de-striped.

P.23,l.28 "From Fig. 8 it is furthermore clear that the statistical and the DOAS uncertainties of TROPOMI appear to be stable over the currently available data period." This is not what Fig.8 shows (the Aug. 6 jump). Please re-phrase.

P.25,l.14 "...also because instrumental effects such as straylight and dark current are adequately corrected for in the spectral calibration in the level 0-to-1bprocessor." Either remove the statement or provide the references that address the subject and are based on assessment of the post-launch data (both radiances and irradiances).

Section 5.2 I have a problem linking the statement "Since NO2 over the Pacific Ocean is primarily stratospheric NO2, validation of stratospheric NO2 essentially is also vali-dation of Pacific Ocean NO2 SCDs." Besides the point that it should be re-phrased, I do not see any connections between the discussed TROPOMI retrievals over Pacific and the ZSL-DOAS/SAOZ S5PVT network that completely avoids the Pacific basin. If the authors employed somewhat different approach than in Lambert et al. (2019), then they should provide a detailed description of the validation process. If this Section provides a summary of the Lambert et al. report and nothing besides, it should say so.

P.26,l.7 "...and the temperature dependence of the NO2 reference spectrum (usually corrected for a-posteriori in the AMF application) may show spectral structures..." Do you imply: "...and the temperature correction of the NO2 VCDs (usually introduced a-posteriori in the AMF application) may result in spectral artifacts in the fitting residuals that are linked to the temperature dependence of the NO2 reference spectrum." ? The NO2 reference spectrum always shows spectral structures in the 405-465nm range...

P.27,l.7 "Since NO2 over the Pacific Ocean is primarily stratospheric NO2, validation of stratospheric NO2essentially is also vali-dation of Pacific Ocean NO2SCDs. As reported by Lambert et al. (2019..." Again, I fail to relate the discussed Pacific Ocean retrievals to the Lambert et al. report.

P.28,l.3 There is no QDOAS Case 6 in Table 2. Please correct.

P.28,l.23 "After removal of such outliers..." Please specify how the removal is done: are the pixels corrected and re-used or completely removed from the fit?

P.28,ll.25 If this concerns the total, spatial x spectral number (I presume, though, that

the authors speak of the spatial domain only – please clarify!), ∼5% is, actually, a sizable population of the pixels. It may grow in time, eventually leading to much noisier and probably biased (depending on the preferential location of the spikes) retrievals. Any comments on this?

P.28,l.26 "...most of which have less than 5 outliers per ground pixel..." Please clarify that these 5 outliers happen in the spectral domain.

Technical corrections:

P.4,l.23: "...away from anthropogenic sources of NO2..."

P.9,l.28: "The dominant term in the overall magnitude of the radiance is the inhomogeneous illumination..." This needs to be clarified: "For a given field-of-view (row), the dominant term in the overall magnitude..." - is this what the authors meant?

P.9, l.29 "The magnitude of the day-to-day variation in the average is much smaller than this long-term oscillation..." By 'this long-term oscillation' do you mean ...the seasonal (Fig.2b) oscillation...?

P.15,l.19 "...the TROPOMI processor reports 10.2% ..."

P.15,l.27 "TROPOMI level-1b version 1.0.0 spectra..."

P.16,l.6 "...with other configuration settings as much as possible matching those of the TROPOMI processor..."

P.16,l.16 "...that the RMS definition may be different..."

P.16,l.20 "As a reference..."

P.18,l.4 "... is smooth rather than stripe-like over the non-contaminated areas..."

P.25,l.4 "...the intensity offset corrections are..."

P.25,l.14 adequately

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