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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Improving algorithms and uncertainty estimates for satellite NO_2 retrievals: Results from the Quality Assurance for Essential Climate Variables (QA4ECV) project" *by* K. Folkert Boersma et al.

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We thank the reviewer for the positive and useful comments, and for careful reading of the paper. We have addressed the questions as follows, with our response in blue.

Reviewer Comment 1

The manuscript summarizes the long-term effort in building a consistent, multi- decade record of NO2 columns based on the practically all available data sets acquired from space. The article offers a plenitude of technical details aiming to improve the trace-

ability of the described products as well as provides valuable recommendations that should help improving quality of the NO2 retrievals. The article deserves a prompt publication.

Thank you for this comment.

I would like the authors to consider the following corrections/amendments:
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The main text: I think that Section 2, especially Sect. 2.1, should be substantially shortened, since, by the author's remark, the survey's outcome was published elsewhere. I am not sure Fig. 1 adds any valuable content to the main objectives of the article. I would consider its removal. Section 2.1 could be shortened to 1-2 paragraphs by mentioning only the user's suggestions that have been implemented in the current version of the products). I would leave out all 'things-to-be-done' or 'things-to-be-considered'.

The same applies to Sect. 2.2 - I would concentrate exclusively on the already implemented items.

If the authors perceive the full review of the user's comments as very important, then I suggest moving it to Appendix.

This is a good point. We feel it is important to document in an easily accessible manner the outcomes of the surveys, and this is now done in the Supplementary material. We shortened the text and removed original Figure 1 accordingly.

Also, consider putting the text from p.8, l.25 through p.10, l.21 into a new Sect. 2.3, thus ascribing the current Sect. 2.3 to 2.4. This particular text, [p.8,l.25 - p.10,l.21], does not belong in Sect. 2.2.

Agreed. We now introduce a new section 2.3 titled 'QA4ECV consortium activities'.

The Footnote 2 to Table 1 does not help understand the meaning of the quoted 2

We have rephrased this as follows: “According to GCOS, the user requirement for stability is a requirement on the extent to which the uncertainty of a measurement remains constant over a long period (GCOS-200, 2016).”

Table 2: is GOME v5 L1 used in the article different from the most recent L1 version described by Coldewey-Egbers et al. (AMTD, 2018)? Adding a footnote may help to remove the ambiguity. Also, Shah et al. (2018, AMT, 11, 2345) describes SCIAMACHY V8 while the authors use v7. This should be commented on just as well.

Thanks for the opportunity to clarify this. In our GOME data product, we worked with what was available to us at the time of project finalization, which was v5 level 1 data. This is not exactly the same as in Coldewey-Egbers et al. [2018], which is v5.1. The main difference between v5 and v5.1 is the consistency of orbits, and not the radiances themselves [Dehn, personal communication], which is not a strong concern.

For SCIA, we indeed use V8 level 1 data, so our original manuscript wrongfully stated that it was v7.04-w. This has now been corrected.

p.15, l.5. OMI shows different degradation rates in radiances and irradiances. Moreover, various instrument-performance metrics show far superior stability than the quoted 2

Done.

p.15, l.11. '... directly from the Sun...'. Please re-phrase, since 'directly' does not apply to the sunlight presumably scattered by a peeling piece of insulation.

Done.

Fig. 2. The percentages shown at the upper axis do not correspond to the RA-marked areas in the plot. Either correct or clarify.

The percentages in the upper x-axis do not correspond to (#badrow/#allrows)x100% but to the “percentage of affected pixels” meaning: (#badpixels/#totalpixels)x100%

from all rows. For some rows, the row anomaly does not affect the entire orbit. This is now clarified in the caption of the figure concerned.

p.16, l.1. Replace 'detector degradation' by 'optical throughput changes in the irradiance channel'. The CCD detector does not change at the quoted rates.

Agreed. We updated the sentence.

p.18, l.9. In addition to the mentioned time-dependent slit-function changes, the GOME-2A slit function varies with the scan angle and along the orbit, though at a much lower level compared to the dominant long-term changes. Are these cross-track and along-orbit changes accounted for? Please comment on.

In the GOME-2 NO2 fits, no slit function fit was performed so in-orbit changes are not accounted for. In the visible, the change of the GOME-2(A) slit function is not important (unlike in the UV-range). If any cross-track changes in the slant columns would still persist, these are likely dampened by the stripe-correction, which was applied on GOME-2A data in the same fashion as for OMI.

Figure 3. In the present format some details are inevitably lost due to the limited resolution of a printout. The same applies to the screen viewing, no matter the zoom. I may suggest: 1. Substantially enlarging the upper section. 2. Substantially overlapping two lower sections (these are practically the same) and expanding both of them at the same time.

Thanks for this good suggestion. We followed up on it.

p.23, l.4. Do you include or reject the RA-affected retrievals in your stats? Please clarify.

We have applied XTrack flagging using the mask provided in the lv1 files, which means that the RA-affected retrievals have not been included in the stats.

Fig. 4. Define the units of the color bars under both plots.

Done.

p.23, l.13. The intensity offset proves to be very important, so this particular subject deserves more detailed discussion, either in the main text or in a separate Appendix. In particular, the authors should provide a mathematical description and discuss the particulars of the 'best-practice' implementation of the offset. Indeed, the cited Müller et al. [2016] provides valuable information, but it is not possible to conclude which form of the intensity offset is considered as the best-practice by the authors. I suggest, besides providing mathematical description in the text, putting some specifics of the applied approach in Table 3. Currently the Table carries 'yes' or 'no' entries in the respective column. Does this mean that all 'yes' entries ascribe to the same intensity offset mathematical form and implementation?

We now refer to the mathematical description of the intensity offset which has been applied. The selected form of the intensity offset was driven by the optical density nature of the fit.

Table 4. Describe the 'undersampling' and 'Eta' corrections. These weren't mentioned in the text.

Good point. We now elaborate on these corrections in the margin of Table 4.

p.31, Footnote 6 : replace '...behavior of the red line...' by '...behavior of the black line...'

Done.

p.32, l.1. The 1st sentence mentions AMF_strat in Fig.5, while Fig.5 caption says AMF_total. Which one is true?

Thank you for pointing this out. We corrected the first sentence on page 32.

Table 5. If all listed values are derived on a pixel basis, then I would put a note in the caption and remove 'per pixel' from the fields. If not, then each field must carry either 'global' or 'per pixel' notation. Or, better yet, the table could be segregated into the

'pixel' and 'global' parts.

Thank you for this valuable suggestion. We followed up on this suggestion and introduced a category indicating whether the estimate is pixel-specific, 'global', or a mix of these categories.

Fig. 9. All the colors but blue and light-blue are described in the caption. Either add these two descriptions or remove all and mention that additional info is provided in the figure legends.

Done.

Fig.13. The caption quotes P < 850 hPa while the text (p. 50) says P < 875 hPa. Which one applies?

875 hPa applies. Corrected.

p.52, l.7. The common Gauss-process error-combination formula gives 'sigma=sqrt(sum(sigma_i^2))'. The text mentions 'sigma = 1 / sqrt(sum(sigma_i^2))'. Please explain the difference.

This was corrected and now reads 'sigma=sqrt(sum(sigma_i^2))'.

p.50 quotes -2% bias and 16% RMS, while Summary provides -2% and 13%, respectively. And, the same applies to DOMINO results: [+11% and 35%] in p.51 vs. [+11% and 28%] in Summary. Please clarify.

Corrected. The numbers in the text apply, and those in the summary has been corrected.

The list of references: a quick spot-check shows the cited in the text, but missing in the list - Boersma et al. [2004], Irie et al. [2009], Krotkov et al. [2016]. Thus, I suspect such omissions to be more numerable. Please double-check the list of references.

Done.

Also, please add letters b through f to the Boersma et al. [2017] works, to match the citations in the main text.

Done.

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Additional corrections: p.23, l.15 - "... results in larger NO2..."

Done.

p.24, l.10 - "The inter-comparison of preferred-setting SCDs..."

Done.

p.29, l.4 - remove the 2nd 'agree reasonably well'

Done.

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