

Interactive comment on “Improved slant column density retrieval of nitrogen dioxide and formaldehyde for OMI and GOME-2A from QA4ECV: intercomparison, uncertainty characterization, and trends” by Marina Zara et al.

Anonymous Referee #3

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The manuscript provides an overview of spectral fitting and its uncertainties for trace gas retrieval from ~ 0.5 nm resolution UV/visible earthshine measurements. The authors report the uncertainty of NO₂ and HCHO retrieved from several spectral fitting procedures and compare these results to empirically determined uncertainty estimates. The empirical uncertainty estimates are determined by evaluating the spatial variation of the retrieved parameter with the assumption that the parameter should not vary over the spatial scales evaluated (2deg x 2deg). Because the paper summarizes years of work, covering topics ranging from fit algorithms to instrumental degradation, it is also

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able to serve as a review of literature. The manuscript is within the scope of AMT and should be considered for publication once comments below are addressed.

The paper should consider improving discussion of the relevance of the study for monitoring trace gas trends. I am most concerned with the closing statement that “the SCD uncertainty increases at a slower pace, suggesting the GOME-2A HCHO retrievals will allow the detection of trends.” Given that HCHO trends are likely very small relative to NO₂ trends and that HCHO measurements rely on weak absorption features nearby larger O₃ and BrO features, I am not convinced that this paper has demonstrated the suitability of fit quality for computation of HCHO trends. For example, there are potential interferences of large scale geophysical variations such as stratospheric O₃ or BrO variations on reported HCHO trends, which may depend on decadal and multi-decadal climate variability, and would not be well represented in the 2deg x 2deg empirical uncertainty estimates. Please consider and discuss this potential in more detail.

Regarding readability, I would find ways to condense what has been written by decreasing repetition. E.g., The final paragraph of the introduction does a better job of communicating the three goals that are listed above. E.g., Figures, Figure captions, Tables and text all report essentially the same information – I recommend removing all figures showing Gaussian distributed observations and representing with text or tables. The readability of the paper and figures would also be improved if the “statistical uncertainty” was replaced by something along the lines of empirical uncertainty and DOAS uncertainty was referred to as fit [parameter] uncertainty.

Major: P7 L15: Early in the manuscript, please consider discussing how the results presented for GOME-2A do or do not apply to GOME-2B, in general terms.

P13 L5: What does this paper find regarding systematic variation of SCD with fit window size? Are there systematic differences between retrievals that use larger and smaller fit windows?

P21 L2: (Figure 4) This figure, and others like it in the manuscript, can be better repre-

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sented in table format or text. As an example, the caption contains all of the pertinent information and the figure does not provide additional insight. In fact, I would think that table 4 also reflects the same information, but the 1-sigma uncertainty estimates are reported as different in table 4 and the caption of figure 4.

P22 L17: Is there a “partial cloud” impact on noise? I.e., is there more noise at some intermediate crf value? – After reading the paper in completion, I find that this discussion and results section is repeated in more detail later in the paper. E.g., you answer my above question in later section.

P24 L8-16: Again, as with the above comment, results presented later in the manuscript provide a means to address what you have described here: you can compare OMI and GOME-2 uncertainty estimates in 2007 before GOME-2 degradation has accumulated. Please comment and consider arranging the text to combine the analysis (e.g., Figures 11-12, year = 2007).

P33 L21: Please report the HCHO behavior more precisely. This information would be useful to evaluate the discussion in p24 L8-16. See also Figure 12, Panels 1,3, year = 2007 (before degradation of GOME-2A)

Figure 12 Panels 1-3: Please comment briefly on why the OMI-BIRA has more uncertainty and GOME-BIRA has less uncertainty than its QA4ECV counterpart

P36 L15-L20: (a repeat of top-level concern) This paragraph is one of the more important in the manuscript. It should be moved to its own section or combined with 4.3.4 “Implication for stability of long-term tropospheric measurements” and include more discussion. In particular, I am interested in the potential interferences of large scale geophysical variations such as stratospheric O₃ or BrO variations on reported HCHO trends, which may depend on decadal and multi-decadal climate variability, and would not be well represented in the 2deg x 2deg empirical uncertainty estimates. P38 L20-22: This proposed test would seem to fit well in this paper.

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Minor P4 L29: Is OMI a push-broom instrument? I thought it was a 2-D CCD.

P5 L26: Does the row-position of stripe maxima vary over time? If so, with what time constant? Each orbit? Each day?

P6 L15: AMF = 4 is very large for HCHO and tropospheric NO₂. I suggest use of a smaller value.

P6 L28: Is there a reference literature on the GOME instrument to include here?

P8 L21: “sensitivity to the absorber of interest” Add reference that does a good fit window optimization.

P12 L24: “better represent the across-track average” Does the updates algorithm use a unique slit function for each cross-track position? It seems like this sentence is saying that one slit function is used for all cross-track positions.

P12 L32 “but is largely identical to the approach taken” Please clarify. Also, please either remind reader why different parts of data record are processed with different algorithms (NLIN and QDOAS), or if it is not important, omit.

P14 L12: Why are cross-sections dynamically convoluted with slit function for HCHO fit but not for NO₂?

P14 L22: Please briefly expand on “E/W bias in the extended fitting interval.” I do not understand as written.

P17 L2: Please remind the reader more precisely what is meant by “background correction.” Is this the correction for using a radiance reference spectrum?

P18 L18: How are clouds treated in this analysis? Are all data included? You describe in more detail later, but a brief comment here would be useful.

P19 L7-10: Why does this discussion begin with NASA NO₂ product? It’s the fourth panel of the relevant figure and is the last item for discussion at other points in the

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paper. It makes for a rough transition. Please better reference Figure 3 panels in the text to help the reader. Also, please comment briefly on the suspected cause of the 0.5×10^{15} systematic difference between v3.0 and v3.1 as this is a large value relative to the errors discussed in this paper.

P21 L17-21: This sentence should be moved to section 4.2. Alluding to results discussed later in the paper distracts from the narrative. I also was unclear what was meant by “on a global scale”

P33 L13: How is ISRF different from slit function? Unless this difference is important to outcomes of this paper, please avoid introducing extraneous jargon and acronyms.

P37 L10-19: The GCOS discussion of error and uncertainty estimates may be more useful to the reader if moved earlier in the manuscript.

P37 L24: “If we consider the SCD uncertainties to be completely systematic in nature” Please clarify this discussion. Based on the empirical analyses, we know that there is some large fraction of the uncertainty that is randomly distributed. I would prefer if the authors referred to “SCD uncertainties” that are “systematic in nature” to be “systematic errors.”

P38 L28: “structural uncertainties may increase up to 30-40%” as evaluated over what time period?

P38 : The conclusions are very well written.

Suggested edits for word choice, word order and grammar (non-exhaustive list) P2 L21: “reliable and traceable information on data quality”

P3 L3: “Here, we quantify . . .”

P5 L14: Replace “jumps” with “non-physical variation”, “variation” or “cross-track variation”

P5 L33: “We exclude the affected rows . . . from our analysis”

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P6 L8: Delete “Together with . . . performance, its”. Add “OMI”

P6 L24: “node” – “orbit”

P8 L15: Here and through the manuscript, I do not understand the parantheses around certain adjectives. “Signal” or “observed signal” are better than “(observed) signal”

P11 L16: repeated use of “stretch”

P16 L9: Here and throughout, I recommend using consistent nomenclature in both equations and text, replacing all instances of SCD in the text with N_s and vertical column with N_v.

P17 L11-14: Please clarify this sentence. What is “this routine”? and what is a “mostly linear fit”?

P24 L19: “pronounced absorption signatures” + “large abundance in the atmosphere” and “relatively small differential optical depth” contradict one another. The latter is the important point to communicate.

P33 L14: “ISRF changes are strongly weakened” - evaluate word choice.

P37 L21: “(e.g., the systematic reductions in SCDs by +/- 1.2×10^{15})” Is the plus minus sign incorrect or is the sentence intended to say SCD uncertainties?

I do not necessarily see the value of including footnotes. In most cases in this manuscript the footnotes do not add value (footnote 1, 2,) or could be better addressed in line with the text (footnote 3) .

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