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Atmospheric Measurement Techniques Discussions

Interactive comment on "Near infrared nadir sounding of vertical column densities: methodology and application to SCIAMACHY" *by* S. Gimeno García et al.

Anonymous Referee #3

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Review of Atmos. Meas. Tech. Discuss., 4, 3685-3737, 2011

"Near infrared nadir sounding of vertical column densities: methodology and application to SCIAMACHY" S. Gimeno García, F. Schreier, G. Lichtenberg, and S. Slijkhuis

Garcia et al. present an overview of the currently implemented operational retrieval algorithm for CO retrievals from SCIAMACHY near-IR radiances. They introduce the theoretical background of the algorithm, introduce influences of various error sources on retrieval performance, and present results from global retrievals of CO - currently operational - and CH4 - a prototype, to be made operational in the future.

The manuscript is fairly well written, but the presentation of the material leaves much to be desired. Overall, the presentation needs focus and a clear definition of what the aim of the paper is. My main problem with the manuscript is that it does not sufficiently justify what new material is being presented, or why the material presented justifies publication. In its present form, I do not recommend the paper for publication. However, if the material is reorganized and refocused, with clear objectives and quantifiable results, the manuscript may well warrant publication in AMT.

Major points of criticism:

(1) The title suggests that this is a "retrieval methodology" paper, and the first two sections predominantly deal with the theoretical background of the algorithm. However, a good extent of section 1 has already been published elsewhere, e.g., in Schreier et al. (2007) "Intercomparison of vertical column densities ...", available at http://envisat.esa.int/envisatsymposium/proceedings/posters/2P3/462917sc.pdf from which parts are quoted verbatim, and a good portion of section 2 reviews known approaches to near-IR trace gas retrievals. This is a call for the AMT Editor to decide whether or not the amount of pre-published material is detrimental to the manuscript being accepted in AMT.

(2) Section 3 presents details on the effects of various error sources on the retrievals, but they are left at a non-quantitative level. Identification of sensitivity and error sources should ultimately lead to an error quantification for the final products - the retrieved CO and CH4 concentrations, but the discussion is left at a qualitative stage. This makes the manuscript unsuitable or, at a minimum, severely limits its usefulness, as a future reference for the BIRRA operational algorithm.

(3) The paper states that BIRRA is being used as the SCIAMACHY near-IR operational algorithm, yet the results derived with it that are presented here seem to heavily rely on external input: Level 1 data as well as the crucially important dead and bad pixel mask have been provided by the University of Bremen, which I take to mean that results

for CO are not from the operational processing stream. There is no mentioning being made how the operational products will actually be derived from end-to-end, which calls into question why they are being presented in the first place.

(4) SCIAMACHY near-IR products (CO, CH4, CO2) have been, and are being derived by the University of Bremen and SRON. If a new retrieval methodology is to be introduced, its value should be justified by comparison with these established products that are already published, peer-reviewed, and publicly available. Schreier et al. (2007) presented a qualitative comparison between CO from BIRRA CO and U. Bremen, but this was not a validation, and neither is one given in the current manuscript (as explicitly stated by the authors). Hence, the paper is not suitable as a validation reference for the operational near-IR data products from SCIAMACHY.

(5) There are no statements on public availability of either the algorithm or its products. While the publication of operational source code is most likely impractical, public distribution of a pre-compiled binary could be considered, together with a test suite of SCIAMACHY sample retrievals. At a minimum, availability of the data products should be stated. If neither the tool nor its products is available, then the value of the manuscript rest solely on the amount of new material presented, and the paper it its present form falls short on that.

Overall, the lack of a truly new retrieval approach combined with the missing justification of why a third set of CO and CH4 retrievals should be produced, and how it does compare quantitatively with established results, leads me to reject the paper in its present form. If the manuscript is revised to address the points above, it should be publishable.

Detailed points of criticism:

(a) Between Schreier et al. (2007) and this paper, BIRRA has morphed from "Better InfraRed Retrieval Algorithm" into "Beer InfraRed Retrieval Algorithm". This may just be a case of a missing/excess "tt" in one or the other, but in either case, it exemplifies

the rule that acronyms should be chosen with great care.

(b) Between Schreier et al. (2007) and this paper, the forward model used by BIRRA has changed from MIRART (Modular InfraRed Atmospheric Radiative Transfer) to GARLIC (Generic Atmospheric Radiation Line-by-line Infrared Code). The reference, however, is identical for both of them: Schreier&Schimpf, IRS 2000. Since those proceedings are not readily available, this change should be addressed to avoid confusion between different publications. If those happen to be one and the same RT code, it again exemplifies the rule of choosing acronyms with care.

(c) I missed a discussion on molecular line-mixing. What is the strategy to account for or avoid those spectral regions?

(d) "unphysical results, e.g., non-negativity" are avoided by regularization, and the improvement in the results is clear. However, negative results are only unphysical if they remain negative within the error bars. Non-linear least squares retrievals are a statistical approach, and negative values for near-Zero VMRs are to be expected. True, they are unphysical in the real word, but as long as they are >=0 within the uncertainties, they are acceptable.

(e) The last half of the last sentence in section 2.3.1 appears garbled.

(f) For retrievals over ocean, have glint observations (improved S/N) been considered?

(g) Figure 2 a shows the dead and bad pixel mask up to 06/2009. We are now in 2011. What has happened in the meantime? Is there a date after which SCIAMACHY near-IR retrievals become impractical due to advanced degradation?

(h) The first sentence in section 3.3 reads funnily. Isn't the aim of an inversion process to gain information on a parameter governing the atmospheric state based on model results fitted to observations?

(i) What exactly is a "Sun Mean Reference", i.e., where does the "Mean" come from?

(j) In section 3.5: The Kurucz solar spectrum is NOT a model spectrum but a high spectral-resolution measurement made from Kit Peak. What is the reason to use an extracted, obsolete version from MODTRAN 1999? The most recent version is from 2010 and is described in

An improved high-resolution solar reference spectrum for Earth's atmosphere measurements in the ultraviolet, visible, and near infrared, K. Chance and R.L. Kurucz, JQSRT, 111, 1289-1295, 2010.

(k) Consider "Spectral Calibration" for the header of section 3.6.

(l) Is spectral calibration (or "pixel-to-wavenumber correction") considered for the SMR? If not, why not?

(m) Section 3.9 ("Least squares settings") is either too much information, or too little. "relative convergence" and "x/y tolerance" all go with numerical parameters set in the solver. Either quantify or omit these details; refer to the publication of the solver routines for details.

(n) What exactly is meant by "a survey" in the heading of section 4?

(o) Section 4.1 should be rewritten to state clearly what parameters are monitored and why. "evidence for a possible causal relationship" conveys "approximately not very much information".

(p) The exact way of derivation of the "dynamic bad and dead pixel mask" would be of interest for a better understanding of the workings of the retrieval process, and for an estimate of how it will be implemented in the operational processing. Also, this would be the point to acknowledge "private communication" from U. Bremen to make it clear where the inputs to the retrieval are coming from.

(q) Stating that "Satellites [...] see in general a higher portion of the atmosphere" than ground-based spectrometer is a confusing statement. What is meant here, I believe, is that they cover a larger horizontal area. But satellites tend to see less of the VERTICAL

air mass than the GB spectrometers.

(r) The Summary states that "Pre- and postprocessing of the data turned out to be crucial", but the paper fails to mention how this is being implemented in either the operational or scientific data stream. While that might be acceptable for a non-operational approach, this amount of hand-cranking makes one worry about the operational data product. Clearly state how the processing stream has been implemented, so that the reader/user can get a firm idea on how the data products have been derived. This holds for both the L1-to-L2 processing (BIRRA), and the L0-to-L1 processor. If U. Bremen will supply L1 data for the operational L1-to-L2 data stream, state so. If not, explain how it is being done.

(s) Figures:

[i] Most figures require larger fonts in title, axes labels, and legend.

[ii] Fig 2.a contains very little practical information. Consider deletion. Also, the panels are "top" and "bottom", so there is no "left diagram".

[iii] Fig 5 is too small to show the enhanced CO over South-East Asia; China seems much more pronounced.

[iv] Fig 5: Why does the exclusion of strong H2O loading lead to higher coverage (more data points, i.e., less white space) of CO VCDs? The color bar starts at 0.0, and negative values should have been regularized.

[v] Fig 6: What are the outliers? They seem to appear in both spectra.

[vi] Fig 7: The plot range should be confined to positive values. Extending the color bar to negative values is pointless when there are no negative values to plot.

[vii] Fig 7: Change "Kurusz" to "Kurucz".

[viii] Fig 9: "IUP mask" in the title is not explained anywhere in the text. Only from the Acknowledgment does it become clear that U. Bremen has supplied the DBM.

[ix] Fig 9: The top panel title says "\delta \lambda", which the bottom panel says "\delta \nu". Which one is it?

[x] Fig 10b: Those 4 plots could be replaced by 4 correlation coefficients. There is little information content in the scatter plots beyond that.

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