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Atmospheric Measurement Techniques Discussions

Interactive comment on "CRISTA-NF measurements during the AMMA-SCOUT-O_3 aircraft campaign" *by* K. Weigel et al.

Anonymous Referee #1

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The paper "CRISTA-NF measurements during the AMMA-SCOUT-O_3 campaign" by K. Weigel et al. reports on measurements of several trace species with the CRISTA spectrometer. The paper focuses on the retrieval itself rather than new insight into atmospheric processes; thus AMT(D) is the appropriate choice for this manuscript. The authors publish a new data product; thus the manuscript is scientifically significant. CRISTA-NF H_2O retrievals have already been published by Hoffmann et al., ASR, 2009. Improvements in the retrieval and/or data product since then need to be clearly outlined in order to avoid multiple publication of the same issue. Results are presented also for HNO_3, PAN and CCl_4 but error analysis and diagnostics are missing. Without this data characterization these results should not be published. However, if these species are removed from the paper, the content of new results remains quite limited. I recommend publication of the paper in AMT after major revision with respect to

scientific quality and presentation quality, as detailed below.

Scientific issues:

Sect 4 p929 l7: At various places in the manuscript the authors emphasize the good spatial resolution of GLORIA-NF. However the use of 1-D retrievals suggests that the along-line-of-sight horizontal resolution might be quite poor. Is there any information on the along-line-of-sight horizontal resolution available? In any case, the statements on spatial resolution should be more specific rather than over-generalizing.

Sect 4.1 p930 Eq 1: This ad hoc assumption on the error correlations will always lead to positive inter-hight correlations. Assume the profile of a stratospheric gas with a pronounced maximum (like O_3 or HNO_3). If the maximum of the profile is assigned to an incorrect altitude, then positive errors in one altitude go along with negative errors at another altitude, i.e. any effect which shifts the profile in altitude instead of scaling it will cause negative error correlations. The auto-regressive correlation model still may be an appropriate ad hoc approximation but I think this issue deserves some discussion, particularly since for these gases extremely large vertical correlation lengths have been chosen.

Sect 4.1, last par of p930: I wonder why ozone and HNO_3 require such large correlation lengths. The characteristic feature of these species is that they have their maximum in the stratosphere. Why should the stratospheric part of the vertical profile of a gas with nearly zero concentration in the mid/upper stratosphere (CCl_4 or PAN) need less regularization? Is this because the diagonals of the related covariance matrices are so small that results are actually constrained towards zero (This is what I suspect)? Or is there another explanation? Without the variance data the reader can only speculate what the reason might be. This issue needs some discussion. See also presentation issue w.r.t. Table 1.

Sect. 4.2: Why is the retrieval performance on a retrieval grid spaced according to the nominal tangent altitudes best? The retrieval grid must be finer than the finest structure to be resolved. Since the actual tangent altitudes are not known, the grid on which atmospheric state variables are retrieved might be out of phase with the actual tangent altitude grid, and vertical resolution is lost. This grid may be a well-working pragmatic ad hoc choice, but evidence of superiority over other grids must be provided if claimed.

Sect 4.2 p932 l14: Is it appropriate to scale the CFC-profile? Since the tropospheric values are quite constant, wouldn't it be more appropriate to use something like the 'downwelling factor' (Toon et al., Evidence for subsidence in the 1989 Arctic winter stratosphere..., JGR 97 7963-7970, 1992) to adjust the profiles? Scaling to me seems more appropriate for a chemically reactive species than for a transport tracer. Beyond this, what about CFC-11 above flight altitude? Its abundances are low but the profile shape may still be important for the uppermost tangent altitudes.

Another question w.r.t the use of CFC-11 as tangent altitude pointing tracer: I understand that the need for such a tracer arises from the fact that the simultaneous retrieval of temperature and tangent altitude pointing from the CO_2 radiances alone is underdetermined, and that the CFC-11 profile shape adds additional independent information to the system. But what about the lowermost, tropospheric tangent altitudes, where CFC-11 probably is also constant with altitude? Where does the complementary information come from in this case? Are the integrated radiance increments responding to a tangent altitude increment on the one hand and a temperature increment on the other hand really linearly independent?

Sect 4.4 (This is my most serious criticism of the manuscript): I miss the error estimation of HNO_3, PAN and CCl_4. Without this, the reader has no chance to figure out if the results are of any significance. This is particularly true since retrieval of PAN, CCl_4, and to a certain degree also upper tropospheric HNO_3 is quite tricky due to spectral interferences. In a retrieval based on integrated radiances consideration of these interferences are even more important than in a least squares fitting concept applied to spectrally resolved measurements. Thorough error estimation is absolutely necessary for all species for which results are reported. If this is not feasible, these species should be removed from the manuscript.

Sect 4.4 p937 l4: I see that the (altitude-dependent) altitude resolution contains information about the vertical range where the retrievals are reliable, but I don't see why the 'measurement contribution' is an appropriate quantity for this purpose. First, since the quantity is an integral (correctly: a sum), where is the altitude-dependence, and secondly, a value of zero in the diagonal of the AKM and 1 elsewhere would lead to very funny and clearly unreliable profiles while the sum still could be unity.

Sect 4.4 p937 l13: The altitude resolution of 20 km is a consequence of the Purser and Huang definition and certainly is correct, but in this case the altitude resolution exceeds the entire range of limb scanning. This means that the profiles are actually not resolved. This gives evidence of the limitations of the concept of a low-flying limb sounder. This issue needs to be critically and honestly discussed. On p 941 the authors discuss the profile shape of HNO_3, but what is its meaning given the fact that the vertical resolution exceeds the scanning range?

Sect 4.4 p937: The altitude resolutions reported are in the usual order of size for limb sounding. Thus the claim of ‘unprecedented spatial resolution’ does not seem justified to me. For example, MIPAS, although not optimized towards the ultimate altitude resolution, provides vertical profiles of several trace species at a vertical resolution better than 3 km at 20 km altitude. Thus, the authors should be very careful with superlatives about resolution and should avoid over-generalized or unspecific statements with respect to this issue.

Sect 5.1 p939 l13: The CRISTA temperature bias may have major impact on the trace gas retrievals. Has this large temperature uncertainty been considered in the trace gas error estimation? Usually no parameter error estimation is made for joint-fit variables because mutual error propagation is already included in the usual noise error estimation formalism. If, however, the actual temperature error is larger than that resulting from the error propagation procedure, the impact of the excess temperature error is not included in the estimated retrieval errors of trace gases. With the temperature-constituent error correlations (accessible via the related covariance matrix), this additional trace gas error component can be estimated.

Fig. 3, left panel: In the troposphere the error bars of the violet profile exceed the range of the a priori variability. In real ‘optimal estimation’ this can never happen. This is probably due to errors beyond measurement noise and smoothing error but nevertheless deserves some discussion.

Fig 3, left panel: Is the sharp change in a priori uncertainties at 17 km realistic?

Fig 3, right panel: Are the low ozone uncertainties above 18 km realistic? If for some reasons the entire profile is shifted by only 1 km in altitude, the resulting ozone value would be outside of the error bar of the a priori profile.

Presentation issues:

The abstract contains some statements which do not really help the reader. The abstract shall not only be an ‘appetizer’ for the article but shall summarize the key information of the article, e.g. what are the characteristic features of the retrieval scheme or what are the main results. Parts of the abstract read rather like an introduction.

Abstract: l1-2 : “The ... instrument successfully participated...”; this is quite a vague statement, because it is not clear what in this context is ‘success’. I suggest to reword this statement.

Abstract: l7: “...new retrieval scheme”; not clear what is ‘new’ about this retrieval scheme; either specify or delete. By the way: Hoffmann et al, 2009, ASR have already published a retrieval scheme for CRISTA-NF. If the authors claim to use a ‘new’ scheme, differences w.r.t the Hoffmann-scheme have to be highlighted (not necessarily in the abstract).

Intro p924 l26: The acronym CRISTA-NF needs to be defined in the body of the paper. Definition in the abstract only is not sufficient.

Intro p925 l8-9: probably ‘particular’ is more appropriate in this context than ‘special’.

Sect 2 p926 l1-2: parts of this statement are redundant with p926 l1. Further, any statement of spatial resolution goes better after the diagnostics part of the paper. In the instrument section it is more appropriate to talk about sampling than resolution.

Sect 2 p927 l3: is this really the 'spectral resolution'? Shouldn't any quantity with $d\lambda$ in the denominator be 'resolving power'? The resolution usually becomes worse when the number becomes larger.

Sect 4 p929 l2: The authors call their method 'optimal estimation' but later we learn that a priori profiles and a priori covariance matrices are not those associated with a true statistical ensemble representing the measurement conditions but rather ad hoc choices. The authors should distinguish between 'optimal estimation' in a Bayesian sense on the one hand, and optimal estimation related algebra on the other hand. Zero PAN a priori profiles are clearly an ad hoc decision. It may be justified but the retrieval then should not be called 'optimal estimation'.

Sect 4.1, Table 1: It took me a while to understand that the table entries in columns 2 and 3 are meant only as links to the literature. It would be more helpful to replace the table by a plot with the profiles and their error bars. Otherwise the reader cannot verify that the standard deviations have really been chosen in a conservative way. Furthermore, I see no reason why this information is hidden in the supplementary material. I suggest to include it in the paper itself. References and/or justification for the assumptions about the uncertainty of spectroscopic data are needed.

Sect 4.2, p931 l1: The term 'measurement content' seems to be a technical term. It is used already here but defined only in Section 4.3.

Sect, 4.2 p931 l4: It is not clear what the term 'nominal tangent altitude spacing' means because the reader does not know what other tangent altitude spacings exist.

This term (without further definition) is only understandable after the discussion of the tangent altitude retrieval. Here the reader is left with the question "nominal as opposed to what?".

Sect 4.3. p933 l14: The term 'averaging kernel' is not defined here. I guess the authors mean a row of the AKM but this should be specified. Further, the term 'integral' should be replaced by 'sum' because it is dealt with discrete numbers.

Sect 4.3 p934 l8 'convolution': this wording is a bit sloppy, because convolution involves integration while Eq (3) and similar applications is just matrix algebra with discrete values rather than continuous functions.

Sect 4.4 p936 l17: This is certainly (hopefully) not the root mean square of the error components but the square root of the sum of variances. I hope that this is indeed only a wording error and that the autors did not divide the sum of variances by the number of error components.

Sect 5 p937 l24: Since cloud detection seems to be a major issue, it should be shortly described in the retrieval section. The description on top of p 938 in the results section is a bit out of context in this place.

Sect 5 p937-938: While according to the introduction the paper focuses on flight L5, here many further flights are mentioned. Please clarify. On 938 p7 again the focus on L5 is mentioned. Is it really necessary to mention all the other flights in this paper? If they are not used, I suggest not to mention them. The listing of flights is in part redundant with the introduction. I suggest to delete this here in order not to interrupt the logical flow.

Sect 5.2, last paragraph: Unless thorough error estimation is included also for HNO_3, PAN and CCl_4, this paragraph and related figures should be deleted, as should be Fig. 14 (see also related comment under 'science issues').

Supplement: I see no good reason why not to include this material in the paper itself. Three additional tables do not add excessive length to the paper but the need to switch between paper and supplement does not help the reader.

Fig 6: Since nearly all results are from L5 flight, the purpose of this figure is not quite clear to me.

Technical issues:

Sect 3 p928 l3: unresolved reference.

Sect 4 p929 l2: 'retrievalS processor' the 's' is probably obsolete.

Sect 4.3 p934 l4 in order not to restrict... (flip 'to' and 'not')

Sect 4.4 p934 l18: blank after O_3.

Sect 4.4 p934 l19: volume mixing ratios (plural)

Sect 5 p941 l14: differences ARE found (plural)

Figures: all the two column figures should be reproduced larger.

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