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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.

**K. Weigel et al.**

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We thank referee #1 for the the time and effort spent on reading the paper and providing the comments. Below please find the reply to every comment.

**Answers to general comments:**

*The paper "CRISTA-NF measurements during the AMMA-SCOUT-O3 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 H2O 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 HNO3, PAN and CCl4 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 Without this data characterization these results should not be published.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.*

We thank the referee that he regards the manuscript as scientifically significant and AMT(D) as an appropriate choice. We will include the missing error estimates and diagnostics, i.e. the a priori profiles, error components and averaging kernels. Plots are available for all trace gases and will be included in Fig. 3, 4 and 5. Enlarged versions of the additional Figures are add to the Supplement of the reply.

As explained in more detail below several aspects of the retrieval have been with respect to Hoffmann et al. 2009. Radiance from another channel of the CRISTA-NF instrument is used in order obtain integrated spectral windows distributed over a wider spectral range. This allowed us to retrieve not only water vapor volume mixing ratios, aerosol extinction, tangent heights and radiometric offset as in Hoffmann et al. 2009, but additionally $O_3$, $HNO_3$, PAN, $CCl_4$ and temperature. We agree that these issues need to be explained sufficiently in the revised version of the manuscript.

**Answers to specific comments:**

*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*



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*on spatial resolution should be more specific rather than over-generalizing.*
We agree with the referee that the resolution is limited in the direction along the line of sight (i.e. perpendicular to the flight direction) as e.g. discussed in von Clarmann et al. 2009, and will clarify the corresponding statements. The along-line-of-sight horizontal resolution is dependent on the vertical resolution, the vertical distance to the aircraft altitude and the atmospheric refraction. It is in the order of several 100 km for lower tangent altitudes.

We will add this information at Sect 2, Page 926, Line 15: "The horizontal resolution along the line of sight is coarser for limb measurements (see e.g. von Clarmann et al. 2009). For CRISTA-NF it dependents on the vertical resolution, the vertical distance to the aircraft altitude and the atmospheric refraction. In can be in the order of several 100 km."

*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 O3 or HNO3). 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 HNO3 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 (CCl4 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*



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*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.*

The long correlation length where chosen ad hoc to stabilize the retrieval. The auto-regressive correlation model is equivalent to a smoothness constrain (i.e. 0th and 1st order Twoney-Tikhonov) (Rodgers 2000 pp.165–166) and will suppress negative error correlations. We set long correlation lengths to make it possible that the measured radiance influences the retrieval result above the measurement altitudes. We also set relatively low a priori errors for the stratospheric values to prevent unrealistic retrieval results. At the same time, the $O_3$ (from ECMWF analysis data) can be assumed to be better known in the stratosphere because of lower spatial and temporal variability. In altitudes with a resolution indicating an influence from stratospheric values this can also lead to an underestimation of the errors. For $HNO_3$ the climatological error is used above the flight altitude and a higher ad hoc choice below 18 km (see also new Figure 3d). To address these issues we will add:

"These long correlation lengths are necessary to stabilize the retrieval for emitters with a high stratospheric abundance (i.e. $O_3$ and $HNO_3$) and are equivalent to a smoothness constrain (Rodgers 2000)."

on Page 930 Line 21 and

"The small error for $O_3$ above 18 km reflects the lower temporal and spacial variability in the stratosphere and is necessary to stabilize the retrieval." at Page 935, Line 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.*



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Based on tests with different vertical grids, the one used here shows the best retrieval results concerning resolution and errors. We agree with the referee that this is not sufficient to claim that no better grid can exist and will therefore change the paragraph at Page 931 Lines 2–9 to (see also below): ”Since part of the systematic error depends on the scanning direction of the gratings (see Riese et al., 1999) the spectra of each altitude scan are separated into two profiles, each containing spectra of one scanning direction of the grating only (hereafter “forward” and “backward” spectra). The resulting vertical sampling of the corresponding “forward” and “backward” profiles is approximately 500 m. Therefore we chose a retrieval grid spacing of 500 m in the tangent altitude range. In our analysis, this retrieval grid was found to be a well-working pragmatic ad-hoc choice.”

*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.*

Scaling is applied above the flight altitude to assure a smooth transition between the measured CFC-11 abundances and the climatological profile. Below, we are certain that there is no more accurate information than the HAGAR measurements during ascent, descent and dive of flight L5. Not only the climatological value, but also its uncertainty is included above the flight altitude, which should assure that existing deviations to the real value are covered in the error budget (i.e. the top column error is included). For the temperature CFC-11 is i.e. responsible for up to 1.5K error caused by trace gases (see new Fig 4c for the error budget of the temperature, the temperature error due to trace gases is about 2K in maximum).

A 'downwelling factor' would be a valid assumption for arctic latitudes, where the air is descending. Though subject to transport the CFC values in the tropics are mainly governed by the abundances in the troposphere (which are measured by HAGAR) and decomposition due to photolysis above, i.e. the age of air. It would probably be possible to model the decrease of CFC-11 with altitude based on this information but this is beyond the scope of this work. For future studies we consider to use a chemistry model or satellite measurements to get more accurate information about CFC-11 in the stratosphere.

*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 CO2 radiance alone is under-determined, 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?*

Although they are not large, there are differences in the Planck radiance as well as the temperature and pressure dependence of the CFC-11 cross-sections and $CO_2$ line parameters. This helps to distinguish the influence of temperature and altitude also where the trace gas abundances do not change with altitude. To strictly link the $CO_2$ ISW with temperature and the CFC-11 ISW with altitude is somewhat oversimplified, the retrieval is run on all ISW simultaneously. They are not always independent but the retrieval showed more realistic and stable results for temperature and altitude than without this information. This is also mentioned, i.e. in the discussion about temperature deviations, P393 L10–14. We will therefore change both entries in Table 1 to Temperature/Altitude.



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*Sect 4.4 (This is my most serious criticism of the manuscript): I miss the error estimation of HNO3, PAN and CCl4. 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, CCl4, and to a certain degree also upper tropospheric HNO3 is quite tricky due to spectral interferences. In a retrieval based on integrated radiance 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.*
We will include the other trace gases, temperature, and aerosol extinction to Fig. 3, 4, and 5 (additional Figures at the end of the reply) and add the following text to the manuscript:

The sentence at P930, L10–12 will be changed to:
"More details about the selected a priori data can be found in Table 3 and 4."

In Sect. 4.:
P934, L17–19: "Fig. 3 shows retrieved mixing ratio values of $H_2O$, $O_3$, temperature, $CCl_4$, $HNO_3$, PAN and Aerosol for forward spectra and backward spectra (see Sect. 2) together with the corresponding a priori profiles."

P935, after L6:
"Figure 3c shows the result of the temperature retrieval for forward and backward spectra. The retrieved temperatures are higher than the a priori temperature at about 11 to 15 km altitude. The result of the $CCl_4$ retrieval for profile 87 is displayed in Figure 3d. Large differences are found between the a priori profile from the Remedios climatology and the retrieval result. This most probably results from the fact, that there is no special tropical profile for $CCl_4$ in the climatology but the measurement take place



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in the tropic, i.e. the climatological profile is representative for an atmosphere with a much lower tropopause height. Figure 3e shows the result of the $HNO_3$ retrieval for profile 87. The retrieval result shows a higher $HNO_3$ abundance than the climatology. The retrieval result for PAN is shown in Fig. 3f. A zero profile is used as a priori while the a priori error is estimated from Glatthor et al. (2007). PAN volume mixing ratios up to about 280 pptv are found between 10 and 15 km altitude. In Figure 3g the retrieval result for aerosol extinction is shown together with the a priori profile."

P935, L7 will be changed to:
"The errors of the retrieval results are shown in Fig. 3."

In P937, L10 we will add:
"The AVKs, vertical resolution and measurement content for temperature, $CCl_4$, $HNO_3$, PAN and aerosol extinction are shown in Fig. 5c–g."

*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.*
We agree with the referee that the measurement contribution alone is not sufficient to estimate the data quality. Nevertheless it provides additional information about the total impact of a priori influence on the results. Therefore the measurement contribution provides the possibility to exclude data which are significantly influence by a priori information. Due to the changing measurement grid, mainly caused by aircraft movements, spectra excluded due to spikes and clouds, it is not possible to define a fixed altitude range where the retrieval can be assumed reliable. Hence, we use both resolution and



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measurement contribution to estimate the data quality for each profile. For a detailed discussion see Hoffmann et al., 2008.

To clarify that the measurement contribution is not used to evaluate the retrieval quality allone we will modify Page 937, Line 5 to:"This is used as additional indicator for the vertical range where the $H_2O$ retrievals are reliable (e.g. Hoffmann et al. 2008)."

*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 HNO3, but what is its meaning given the fact that the vertical resolution exceeds the scanning range?*
The admittedly high value of 20 km is used as an upper threshold to filter unreliable data. In the presence of measurements the resolution is often better, see Fig 5b an example. For clarification, plots displaying the resolution for all profiles for the retrieved trace gases will be added to the supplement (see reply to referee #2 for the Figures). To clarify this we will change Page 937 Line 13 to: "The upper threshold for the resolution is set to 20 km for HNO3 and O3 and 3 km for all other retrieval variables; the lower and upper thresholds for the measurement contribution are 0.8 and 1.2, respectively. In the following, only retrieval results meeting these quality criteria are displayed."

*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.*

We agree with the reviewer that the claim is not valid for all altitudes and all trace gases and will use “high” instead.

*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.*

The effect was not considered in the given errors. It will differ for each profile and trace gas and we do not have an in situ profile which corresponds equally well to all measured profiles due to the spatial extend of the flight. Further, a different temperature has a complex indirect effect, because it will mainly influence the result of the altitude retrieval. These changes in altitude will lead to changes in the trace gases. To estimate the effect we rerun the retrieval without a joint temperature retrieval and use the averaged a priori temperature below the flight altitude for the forward spectra of profile 87. The error for the temperature was set to 5K and 2K. The result for the different trace gases can be seen below, in Fig. 6. The different Temperature has the largest effect on the altitude retrieval (up to about 0.8 km) and therefore also on trace gases where the abundances vary strongly with altitude. Additionally the measured radiance is reproduced better in the original retrieval for this profile. It is not certain, that the in situ temperature is the “better” choice for this particular profile due to the spatial distance to decent, ascent, and dive locations where the in situ temperature is measured below the usual flight height but this example should visualize the effect of the temperature on the retrieval result. We will add: ”If temperature and altitude



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retrieval are not completely independent this can also influence the retrieval results for the trace gases, especially when they vary strongly with altitude." on Page 939 Line 14 to address this issue.

*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.*
The impression that the error bars are larger than the a priori errors is not correct. This impression is probably caused because the deviation from the mean is larger than the a priori which is plausible because all errors are given as $1\sigma$ values.

*Fig 3, left panel: Is the sharp change in a priori uncertainties at 17 km realistic?*
The change reflects the lower spatial and temporal variability of water vapor in the stratosphere. It can be argued that the sharpness and the altitude could be chosen different but the influence on the retrieval result is low because $H_2O$ usually reaches its detection limit at 15 km (as mentioned in the discussion) and the retrieval itself is stopped at 17 km. We will add:"This leads to a low error estimate above 17 km to reflect the lower stratospheric variability. Below a higher, conservative error estimate is used." at Page 934 Line 22 to explain this.

*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.*
As mentioned above the relative low stratospheric errors for $O_3$ are necessary to stabilize the retrieval. At the altitudes above the flight altitude there is in most cases not enough measurement information for the retrieval. Nevertheless this should decrease the effect of the stratospheric values of $O_3$ on the retrieval results below if



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the stratospheric $O_3$ can be adjusted by the retrieval to a certain extend. At the same time the $O_3$ (from ECMWF analysis data) can be assumed to be better known in the stratosphere because of lower spacial and temporal variability. In altitudes with a resolution indicating an influence from stratospheric values this could also lead to an underestimation of the errors. To address that problem we will add: "The small error for $O_3$ and $HNO_3$ above 18 km reflects the lower temporal and spacial variability in the stratosphere and is necessary to stabilize the retrieval." at Page 935, Line 1.

**Answers to 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.*
We will remove the word "successfully" (see below) from Page 924, Line 2 and add the sentence: "The new retrieval scheme is based on 9 integrated spectral windows allowing to retrieve an extended set of trace gases and temperature fields with high vertical resolution (up to 500 m). Retrieval results are shown for temperature, water vapor ($H_2O$), ozone ($O_3$), nitric acid ($HNO_3$), peroxyacetyl nitrate (PAN), carbon tetrachloride $CCl_4$), and aerosol extinction." Additionally we will explain the differences in the Introduction (see below).

*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.*
We will remove the word "successfully".



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*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).*

Not the retrieval method but several aspects of the retrieval have changed with respect to Hoffmann et al. 2009. Now, radiance from another channel of CRISTA-NF are used in order obtain integrated spectral windows distributed over a wider spectral range. This allowed us to retrieve not only water vapor volume mixing ratios, aerosol extinction, tangent heights and radiometric offset as in Hoffmann et al. 2009 but additionally $O_3$, $HNO_3$, PAN, $CCl_4$ and temperature. We agree with the referee that this needs to be clarified. Additionally to the change in the Abstract (see above), we will explain the differences in the Introduction (Page 925, Line 25): "The new retrieval scheme uses 9 ISWs from channel L6, in contrary to the retrieval scheme presented by Hoffmann et al. 2009, which uses 3 ISWs from channel H5. The higher number of ISWs from a wider spectral range allows us to retrieve not only water vapor volume mixing ratios, aerosol extinction, tangent heights and radiometric offset as in Hoffmann et al. 2009 but additionally $O_3$, $HNO_3$, PAN, $CCl_4$ and temperature."

*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.*

We will add the complete definition on Page 924, Line 26.

*Intro p925 l8–9: probably 'particular' is more appropriate in this context than 'special'.*

"AMMA Special Observing Period" is the name of a particular phase of the AMMA campaign (Cairo, 2010). Therefore we will correct this and write it with capital letters.

*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.*
We agree with the referee that we should rather refer to the sampling at this point and change the sentence to: "CRISTA-NF measures thermal emissions (4 to 15 $\mu m$)" with a dense vertical sampling.

*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.*
We agree with the referee and will change the term to spectral resolving power.

*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'.*
We will replace the term "optimal estimation retrieval" with "retrieval".

*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.*
We will include the content of the supplement in the main part and include all trace

gases in Fig. 3, 4, and 5.

*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.*

The term will be replaces by "measurement contribution" in the figure caption and becomes obsolete after reformulating this paragraph (see below, i.e. replies to the comments about the retrieval grid and tangent altitude spacing).

*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?".*

The term nominal was used because the "real" tangent altitude spacing is influenced by the movements of the aircraft. To clarify this, regarding also an earlier comment of the referee we will change the paragraph to (The first sentence will be removed): "Since part of the systematic error depends on the scanning direction of the gratings (see Riese et al., 1999) the spectra of each altitude scan are separated into two profiles, each containing spectra of one scanning direction of the grating only (hereafter "forward" and "backward" spectra). The resulting vertical sampling of the corresponding "forward" and "backward" profiles is approximately 500 m (dependent on the movements of the aircraft). Therefore we chose a retrieval grid spacing of 500 m in the tangent altitude range. In our analysis, this retrieval grid was found to be a well-working pragmatic ad-hoc choice."

*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.*



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We agree with the referee and will replace the term "averaging kernel" by "sum over the rows of the AVK matrix"

*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.*

We will change the sentence to:"In order to not restrict the altitude range for comparison, the profile of in situ measurements is extended with the a priori value used for the CRISTA-NF retrieval above the flight altitude in Eq. 3."

*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 authors did not divide the sum of variances by the number of error components.*

The referee is right and we will correct the sentence.

*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.*

The cloud detection is an important prerequisite for the retrieval but not a direct part of the retrieval itself, it also provides a stand alone result. We will move the description of the cloud index to the end of instrument section (Sec. 2, P927, L6):

"An important prerequisite for the retrieval is to filter the data for clouds and optically thick conditions. Therefore the cloud index (see Spang et al., 2008) is calculated as color ratio between the ISW from 791–793 and 832–834 $cm^{-1}$ at the measurement locations. A cloud index lower than 3.5 is indicative for a significant influence of clouds (or optically thick conditions due to the water vapor continuum at the lowermost altitudes). Corresponding spectra are excluded from the retrieval. Figure 6 shows the cloud index of all flights where retrieval results are available with the scheme presented here."

*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.*
We will move Fig. 6 to the instrument section and will not mention the other flights in the results.

*Sect 5.2, last paragraph: Unless thorough error estimation is included also for HNO3, PAN and CCl4, this paragraph and related figures should be deleted, as should be Fig. 14 (see also related comment under 'science issues').*
The other trace gases will be included in Fig. 3, 4 and 5 (see above).

*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.*
We will include the content of the supplement in the main part.

*Fig 6: Since nearly all results are from L5 flight, the purpose of this figure is not quite clear to me.*
The figure shows all flights where retrieval results with the presented scheme are available. We will clarify this and move the figure to the instrument section.

**Answers to technical issues:**



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*Sect 3 p928 l3: unresolved reference.*
The referee is right, the references (to Offermann et al.(2002) and Schaeler and Riese (2001)) are missing. This was caused by a spelling mistake in the label and will be corrected.

*Sect 4 p929 l2: 'retrievalS processor' the 's' is probably obsolete.*
We will correct the text.

*Sect 4.3 p934 l4 in order not to restrict... (flip 'to' and 'not')*
We will correct the text.

*Sect 4.4 p934 l18: blank after O3.*
We will correct the text.

*Sect 4.4 p934 l19: volume mixing ratios (plural)*
We will correct the text.

*Sect 5 p941 l14: differences ARE found (plural)*
We will correct the text.

*Figures: all the two column figures should be reproduced larger.*
We will improve the readability of the figures.

**References**

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Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/3/C765/2010/amtd-3-C765-2010-supplement.pdf

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Interactive Comment

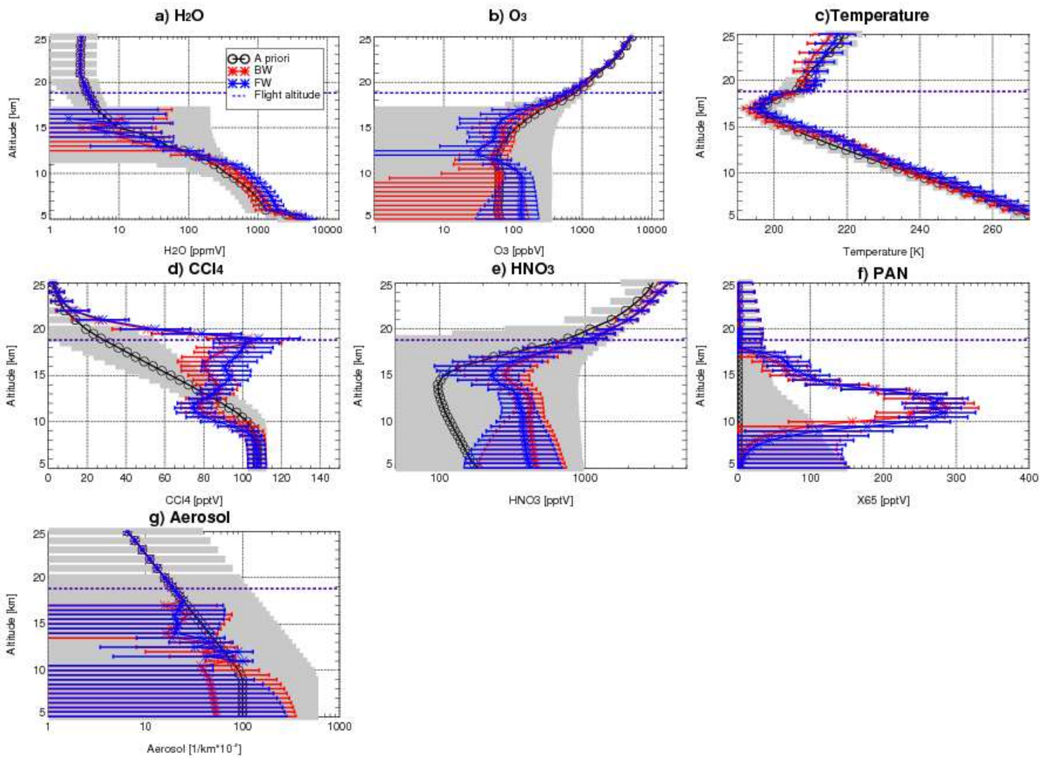


**Fig. 1.** Renewed Figure 3 including the other trace gases and temperature

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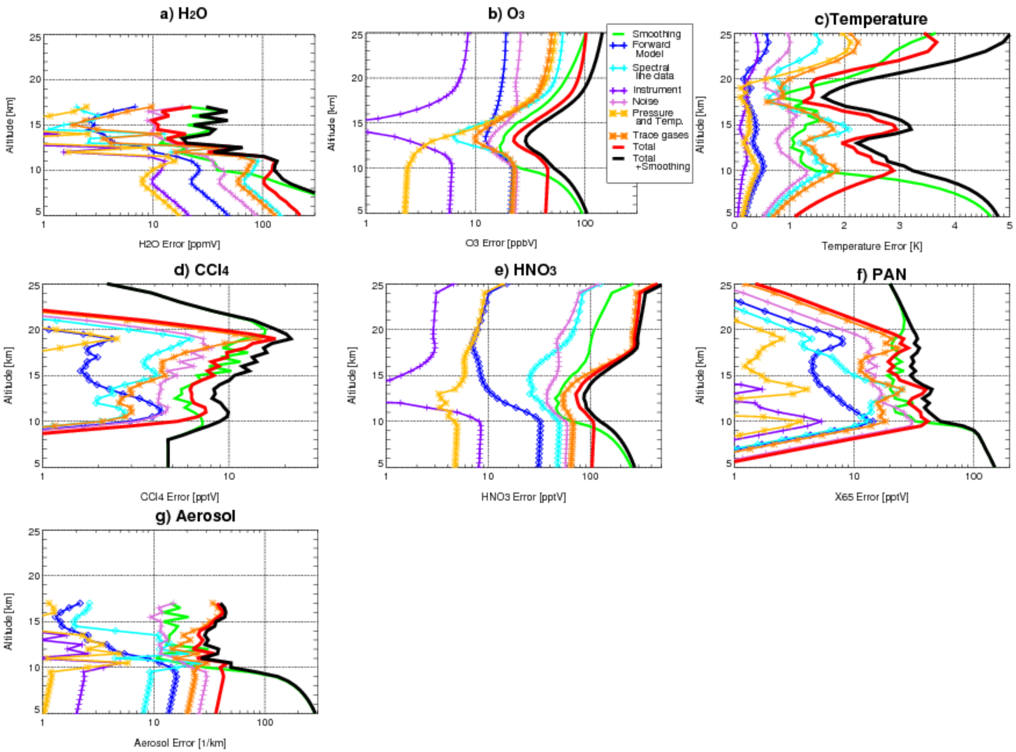


**Fig. 2.** Renewed Figure 4 including the other trace gases and temperature

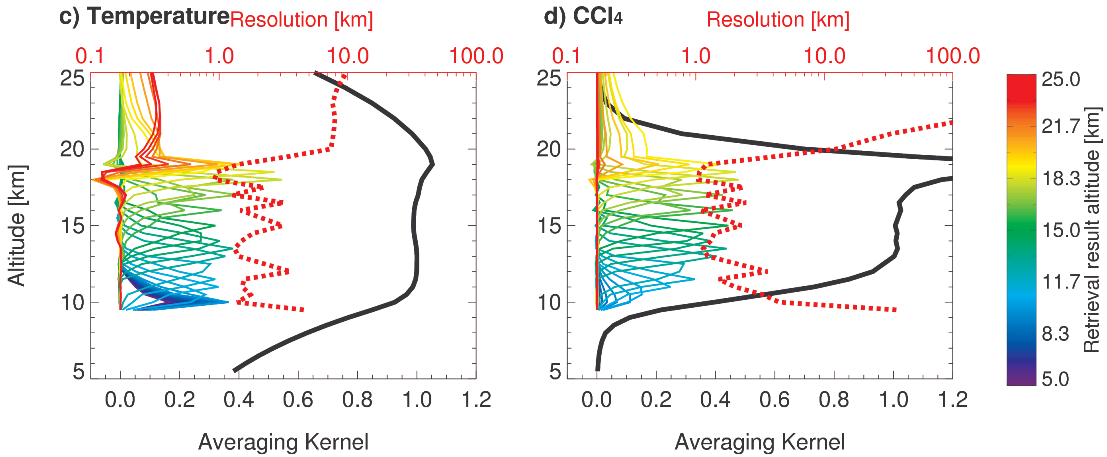


**Fig. 3.** New Fig. 5 c,d: Averaging kernel matrix, measurement contribution and resolution for temperature and CCl$_4$ for profile 87 (forward spectra), flight L5.



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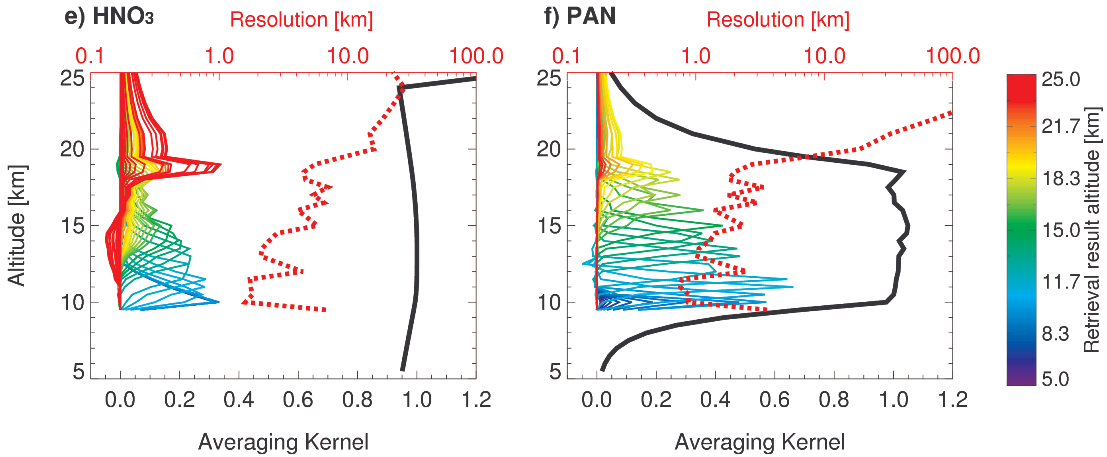


**Fig. 4.** New Fig. 5 e,f: Averaging kernel matrix, measurement contribution and resolution for HNO$_3$ and PAN for profile 87 (forward spectra), flight L5.



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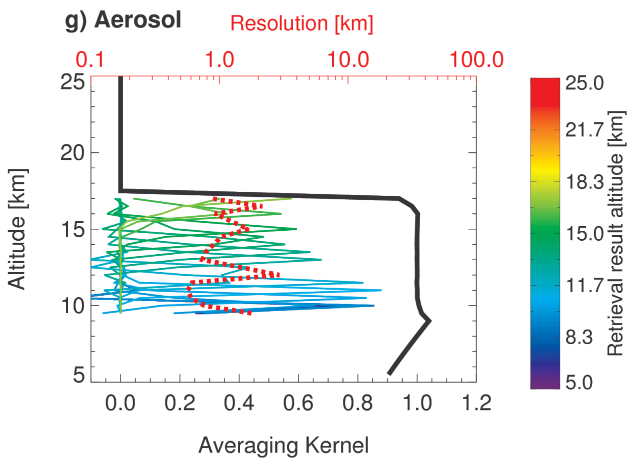


**Fig. 5.** New Fig. 5 g: Averaging kernel matrix, measurement contribution and resolution for aerosol for profile 87 (forward spectra), flight L5

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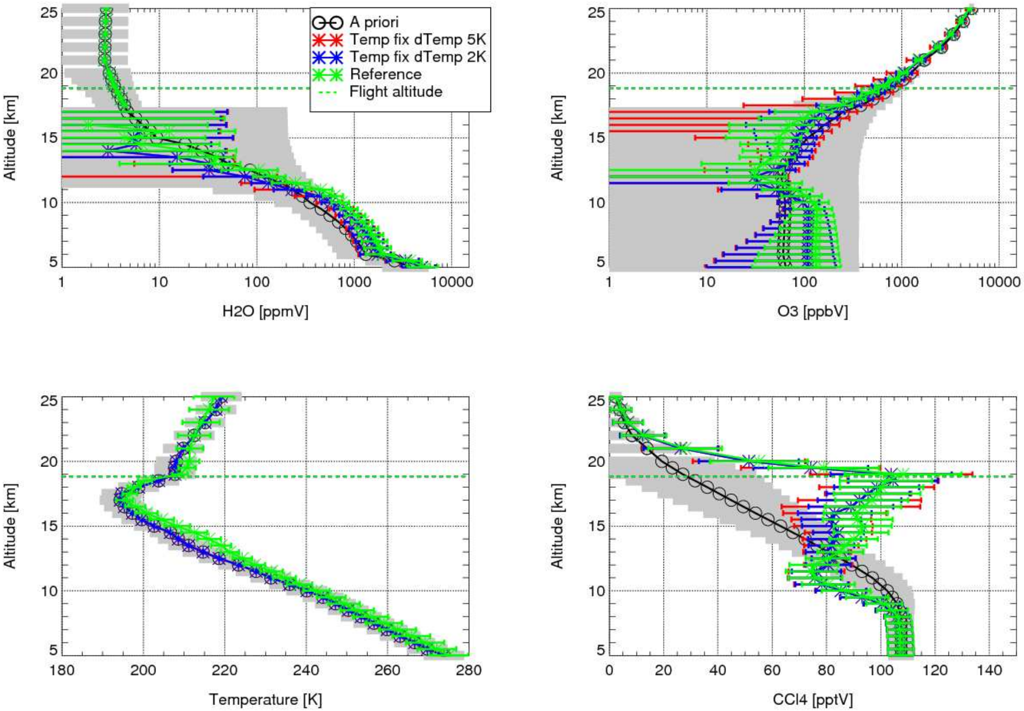


**Fig. 6.** Retrieval result for H$_2$O, O$_3$ and CCl$_4$ if the temperature is set to the average in situ value compared with the original retrieval result.



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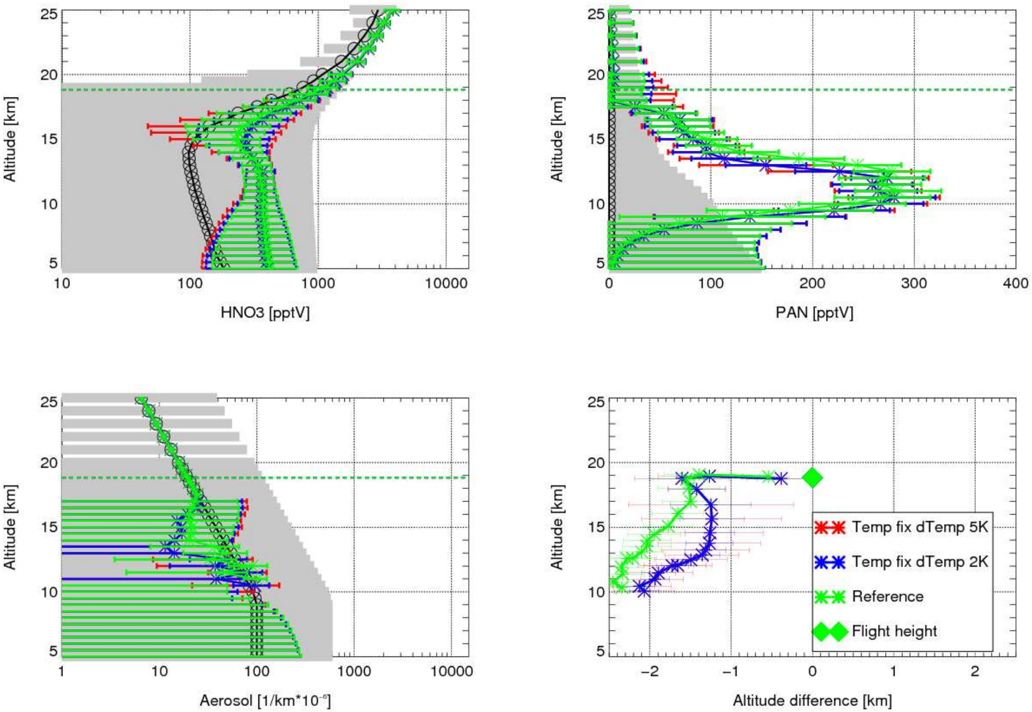


**Fig. 7.** Like Fig. 6 but for HNO$_3$, PAN aerosol extinction and view point altitude.



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