We would like to thank the referees for their comments. Before proceeding with the response I would like to note that we received an updated climatology for N_2O from the WACCM model to use for the N_2O correction of the methane estimate, as described in the text. By using this updated a priori and the use of a new quality flag we believe we have greatly improved our methane estimates. The validation of the methane is addressed in a recently submitted publication by Kevin Wecht (a co-author on this paper) using data from the NSF HIPPO aircraft campaign and the agreement shows improvement between this revised TES CH4 and HIPPO aircraft profile data over the previous TES CH4. I have updated the text accordingly along with addressing the comments by the three reviewers.

I would first like to address comments from Dr. Schneider.

Comment: Applying a broad spectral region and fitting all absorbers simultaneously instead of the former microwindow approach is a good idea. I well believe that it produces profiles with an increased vertical resolution. However, the by most important modification is the changed a priori constraint. Weakening the constraint naturally increases the theoretical profiling capability of the remote sensing system. I wonder whether the effect of applying a broad spectral region can be completely neglected if compared to the effect of reducing the a priori constraint.
Did the authors simulate the effect of applying a broad spectral region? By how much increase the DOFs if you apply the broad spectral region instead of the microwindows but keep the constraint constant? A table where the effects of the different modifications are documented would be nice: change to broad spectral region means additional xx DOFs; change of hard constraint means additional xx DOFs; change of soft constraint means additional xx DOFs. I have the feeling that the change of the soft constraint is clearly dominating. If so, it should be made clear in the manuscript!

Response:
Dr. Schneider is correct in that the looser constraint and the increased number of radiance elements are both responsible for the increased vertical resolution. I did not however create a table showing which is “more responsible” for the increase because it varies as a function temperature, H_2O, land type, clouds etc. However, as an example for two specific cases, clear sky tropics and clear sky summer arctic, I would obtain the following:

	DOFS Tropics Old Constraint	DOFS Tropics New Constraint	DOFS Arctic Old Constraint	DOFS Arctic New Constraint
Old ν	1.1	1.55	0.43	0.75
New ν	1.2	1.91	0.68	1.0

As demonstrated by this calculation, the DOFS increase is quite different for these two scenes. In addition, it does not show the improvement in the separation of the averaging kernels that allows for profiling of the HDO/H_2O ratio from the boundary layer through the middle free troposphere; a capability that the previous retrievals did not have. Because of the variability in the change in precision and sensitivity, we have added into the text that the increase in DOFS is approximately half due to the constraint and half due to the increased number of spectral points used in the retrieval and added text indicating emphasizing the separation of the HDO averaging kernels (last part of Section 4.2).

For methane we have also loosened the constraint in the boundary layer as well as increased the number of retrievals; however, we find that the increased number of methane lines provides most of the increased sensitivity to the middle and lower troposphere. Note that this should also be the case for IASI retrievals as I have seen a poster by Dr. Waterfall (Rutherford Appleton labs) showing similar methane averaging kernels but with larger errors likely because of the poorer spectral resolution compare to TES. I should also note that while the methane errors and sensitivity appear to be well characterized (via comparisons to HIPPO data); we now believe (as discussed subsequently) that we should change the methane retrieval strategy in a subsequent release so that it does not have as much sensitivity because the estimated errors are much larger than the observed variability.

2) **Comment** starting with: By weakening the a-priori constraints one assumes and ending with: "**In summary:** The error estimation is made for the new Sa (obtained from the new measurements and model calculation cited by the authors). It is only valid if the new Sa is the right Sa. I think this should be made clearer in the error discussion. Since the improvement of the retrieval depends on the validity of the new Sa I would also like to recommend changing the title to something like: "Using new a priori assumption for producing profiles of CH4, HDO, H2O, and N2O with improved lower tropospheric vertical resolution from Aura TES radiances"

Response: Yes this is correct; I have therefore modified my conclusion that the smoothing error has decreased, but not necessarily the total error, because the increase in sensitivity in the PBL will reduce the smoothing error regardless of my choice of Sa. Because the global distribution of the HDO/H_2O ratio is still being characterized with satellite and in situ data, it is difficult to arrive at a covariance which is the best estimate of the "true" Sa. However, while we cannot precisely estimate the smoothing error, we can calculate the precision errors. We can also determine if these calculated errors are consistent with the actual errors or alternatively whether the estimated atmospheric variability is due to noise or represents real variability. For example, we estimate about a 30 per mil (3%) precision error in the tropical oceanic boundary layer. We find that the variability in the boundary layer is also about the same; Because I would not expect too much variability in the tropical oceanic boundary layer (Lawrence et al., 2004), I would expect that most of this TES estimated variability is simply due to noise; these conclusions are already stated in the text. However to address Dr. Schneiders comment I have added language in Section 4.2 that the measured uncertainty indicates that our retrieval is not optimal for tropical oceans but is appropriate for continents and high latitudes where the isotopic composition of the boundary layer is more variable; in the future we will need to use a variable prior that depends on latitude and an Sa that is formally derived from independent measurements or using the latest model estimates.

Comment 3:

By the way: Our Schneider and Hase (2011) IASI thermal nadir retrieval is very similar to the new setup reported by the authors. We fit a broad spectral region (1190-1400cm-1) and simultaneously retrieve H2O, HDO, (and HDO/H2O), CH4, N2O, HNO3, and CO2. The paper is on APCD since May 2011 (http://www.atmos- chem-phys-discuss.net/11/16107/2011/), and the authors might just have overlooked it. Today it should go online on ACP and due to the similarity to the author's "improved retrieval setup" I think it our paper should be cited.

Response:
Yes ☺. This was a silly mistake on my part.

Comment: Section starting with:
IV: Sensitivity with respect to HDO/H2O: In Section 4.2 the authors state that "[...] the HDO averaging kernel best describes the vertical sensitivity for the HDO/H2O estimate [...]". I disagree! In the following I will show that using the HDO kernels as a proxy for the HDO/H2O kernel significantly overestimates the HDO/H2O sensitivity. Actually the HDO/H2O sensitivity is smaller than the HDO sensitivity: The reason is that the space spanned by the HDO kernels is no sub-space of the space spanned by the H2O kernels.

Response:
Wow! First, I would like to thank Dr. Schneider for the time he spent on this analysis and discussion. I have been struggling with this problem for some time and I agree that since the HDO averaging kernel does not completely span the H_2O averaging kernel it does not completely represent the variability of the HDO/H_2O estimate. On the other hand we know from the analysis in the Worden et al. 2006 paper, as initiated by the second referee Dr. von Clarmann; that there is no unique HDO/H_2O averaging kernel because it depends on both the HDO and H_2O sensitivity. I would prefer not to include the discussion from Dr. Schneider in this paper because I believe it would distract from the main point of this paper which is to document the error characteristics and sensitivity of the new TES retrievals. Therefore, in order to address this comment I have changed the sentence in Section 4.2 "Consequently, the HDO averaging kernel best describes the vertical sensitivity for the HDO/H_2O estimate characteristics" to "The HDO averaging kernel is a good approximation of the vertical sensitivity for the HDO/H_2O estimate because the sensitivity to HDO always overlaps that of H_2O. On the other hand the true sensitivity of the HDO/H_2O estimate is likely smaller than that of the HDO estimate because the HDO averaging kernels do not perfectly span the altitude range of the H_2O averaging kernels.

Comment: **V:** Section 4.3.4, a posteriori correction of CH4 by the retrieved N2O: This is very interesting. In addition I think it can be further improved. Instead of correcting the CH4 a posteriori you could a priori introduce a ln[CH4]-ln[N2O] inter-species constraint thereby constraining against a CH4/N2O ratio similar to what is done for HDO/H2O. I am not sure but maybe this will reduce the jumps you are talking about. You might think about mentioning such ln[CH4]-ln[N2O] inter-species constraint retrieval and say that it would be a interesting future development of the CH4 retrieval.

Response: I have thought of adding these correlations; what prevents me is that I don't yet trust the modeled correlations between CH_4 and N_2O because the distribution of emissions of these trace gasses are not well known. What primarily correlates atmospheric concentrations of these trace gasses is the atmospheric transport which results in both having a north/south gradient. We can think about ch4/n2o correlations for the next release.

Comment:
Page 6683, Eq. (1): a "+" is missing in the third term
Response: Looks like a transcription error. I will ensure that the next iteration is correct

Comment:
Page 6684, Eq. (2) and (3): what is the difference between Λz and Sa-1?-1From explanation in the text I got the impression that you apply as constraint Sa , i.e., Λz=Sa-1.

Response: The Λz is the constraint choice (e.g., Tikanov) and is equal to Sa^{-1} if the constraint choice is based on a climatology. I have added this comment into the section: Note that Λ_z can take on different forms such as Tikanov, a hybrid constraint (e.g., Kulawik et al., 2006a) or the inverse of a climatology (Rodgers 2000).

Comment: Page 6684, Eq. (3): the third term should be SM or Gz * Sm * GzT. Please correct.
Response: Fixed

Comment: Page 6685, line 1 and 2: I would relate Sx1 and Sx2 to Sa and Stot: Sx1= Sa and Sx2=Stot, right?
Response: This is correct if you are starting from Sx1 = Sa; However this need not be the case. That said, I have added this comment to the text: Typically, $\mathbf{S}_{x1}$ is the *a priori* covariance $\mathbf{S}_a$ and $\mathbf{S}_{x2}$ is the *a posteriori* covariance.

Comment: Page 6685, line 16 and 17: two times "illustrates"
Response Fixed

Comment: Page 6685, line 23: remove "the"
Fixed

Comment: Page 6687, line 13: "[...] covariances from these models are not typically invertible.": this is no good argument because one can perform a "pseudo" inversion via a singular vector decomposition.

Response: I believe you have just made the same argument we make in the manuscript ☺, that you use the climatologies as a guide but then adjust them so that they provide a stable retrieval with well characterized errors.

Comment: Page 6688, line 7: what are "observation covariances"? Please define. It is explained in the caption of Fig. 3, but I think it should also be explained in the text. (added language that observation covariance is combination of measurement plus interference error)

Comment: Page 6689, line 18: Worden at al. (2010) does not appear in the reference list. Fixed

Comment: Page 6691, line 12: Worden et al. (2011) does not appear in the reference list. Fixed

Comment:
Page 6692, line 16-19: "[...] this increased sensitivity to the lower and middle troposphere is due to use the methane lines around 1230 cm-1.": This is interesting! For methane changing from the microwindow approach to fitting the broad spectral region significantly increases the sensitivity, whereas for the H2O and HDO the increase in the sensitivity (or vertical resolution) is mainly due to the "relaxed" soft constraints. As already mentioned in my major comment (I), I think that a table describing how the DOFs for the different absorbers change due to the different modification (broad spectral region, hard, soft constraints) would be very useful for the reader.

Response: (still need to check this???)
While we have slightly loosened the methane constraint in the boundary layer and lower troposphere, we fine that most of the increase in the methane sensitivity in the lower troposphere is due to the increased number of lines. As noted in the text, the reason the previous spectral windows were used was to avoid spectral interference from H_2O. The new TES Level 2 retrieval approach mitigates errors due to H_2O (and other species). I have added language to this effect to the manuscript.

Comment: Page 6692, line 24: I guess you mean here Fig. 2 instead of Fig. 5.
Response: Fixed

Comment: Page 6692, line 25: isn't an assumed a priori variability of methane of 5% a bit too large? The peak-to-peak amplitude of the seasonal cycle is only about 2%, right?

Response: We have added language to Section 4.3.2 explaining how we arrived at our choice for the methane variability. We also note that our covariances and constraints used for the subsequent algorithm release will likely change based on experience using the new TES methane product with global models. Note that the covariance (and hence the constraint) is not currently optimal but should be well characterized. Most likely, we will change the constraint in the next release to only allow 1 DOF in the troposphere in order to mitigate systematic errors associated with the observed bias in the CH4 estimates.

Comment: Page 6694, you mention that the bias might be caused by an anti-correlation between upper and lower/middle tropospheric methane. You say such an anti- correlation is suggested by the kernels of the new retrieval (negative values of lower/middle tropospheric kernels in the upper troposphere, right panel of Fig. 8). In old kernels there are no negative values of the lower/middle tropospheric kernels. In consequence there should be no bias? Is this the case?

Response: The old retrievals did not show anti-correlations because the older retrievals could not distinguish the upper troposphere methane variability from the middle/lower troposphere. However, the old retrievals were also biased high. On the other hand we do find that the averaging kernels show anti-correlations and one can observed the consequences in the vertical distributions (e.g., high methane in the upper troposphere, low methane in the low troposphere) which are unphysical. For this reason, I suspect that the anti-correlations make the bias worse. Note that I have not definitively prove this hypothesis, which is why I use the extremely watered down language (e.g., suspect, suggest etc) in the text.

Comment: Page 6696, Eq. (11): I think you should write this Equation similar to Eq. (3). Writing it different is an unnecessary source of confusion. Therefore, I suggest modifying Eq. (3) a bit. Change the last two terms of Eq. (3) to: GR *Sm*GRT +GR *(Σi Ki *Sbi*KiT)*GRTThen you can also mention that writing here GR=GC-GN instead of GC (or Gz) makes the difference.

Response: I have expanded the systematic error term in Equation 3 so that it is of the same form as Equation 11.

Comment: Page 6696, last line: I do not understand what you mean with "[...] the bias error described in Eq. (9). Do you mean "[...] work for correcting the bias error shown in Fig. 11."?

Response: I have removed this sentence from the text.

Response to Reviewer 2 (Dr. von Clarmann)

We would like to thank Dr. T. von Clarmann for his comments.

Comment: p. 6680 l. 20 and elsewhere: MIPAS retrieves CH4 in a similar spectral region and has also a problem with a high bias in the upper troposphere and lower stratosphere (von Clarmann et al., AMT 2, 1-17, 2009). This supports the hypothesis that there indeed is a problem with spectroscopic data.

Response: We have added the reference to the MIPAS data; thanks! I wonder though whether both the TES and MIPAS CH4 retrievals also have issues with the cold-space calibration as opposed to errors in spectroscopy because the N_2O correction to CH4 appears to mitigate some of the upper tropospheric bias. I suspect the remaining, approximate 2% bias observed in the TES CH4 data after correction is due to spectroscopy as it is within the uncertainties of the data (Linda Brown, private communication). All of this is speculation which is why I group all three potential issues together (spectroscopy, calibration/temperature, retrieval jack-knifing/anti correlations).

Comment: p. 6680 l. 16 “greater resolution”: If the number becomes larger, the resolution be- comes worse. It is the “resolving power”, not the “resolution” which becomes greater (at least this is the terminology I have learned, I may be wrong). If you replace “greater” by “better” the statement will be unambiguous.

Response: Fixed in three locations

Comment: p. 6668 l. 4: It is a good idea to discuss the scientific relevance of the gases retrieved also in a technical paper like this. However, the inclusion of Fig. 1a might be a little bit too much, particularly because it is never referred to the contents of this figure in other parts of the paper.

Response: Ill remove and renumber the figures. I included them because I used them for a talk where I discussed these retrieval results as the figures provide context. However, I agree that these are probably not relevant as I do not show in this paper how these data improve our understanding these processes.

Comment: p. 6681 l. 18: You might wish to include MIPAS in this list, in order to achieve a better correspondence between list of instruments and the scientific studies mentioned in lines 13-16.

Response: Thanks for pointing this out! I had the Steinwagner reference in there but not the instrument.

Comment: p. 6681 l. 21 Fig 1b: same as for Fig 1a. The text is fine but I do not consider the inclusion of such a schematic figure necessary in the context of this paper.
Response: Fixed.

Comment: p. 6682 l. 7-20: I find this paragraph confusing because it describes TES retrieval issues in pretty much detail before TES has even been introduced. I suggest to shorten this paragraph considerably or even to delete it. The reader who is in a hurry finds this information in the abstract, and the more interested reader will find this information below, where it is placed much better into context.

Response: Agreed… paragraph deleted.

Comment: p. 6683 Eq. 1: I find this equation confusing because it is not clear to me which values are vectors and which are scalars. Shouldn't 'x' be bold face because it is a vector (i.e. a profile)? Or do you really refer to one element of the profile? Axy is italic in the Equation but bold face in the text. Please take care to use consistent type-setting and in addition clarify in the text for each symbol if it is a scalar, a vector, or a matrix.

Response: I made the assumption that the math would be typeset correctly from the uploaded text. I will check that the revised text has consistent indices. ????

Comment: p. 6684 Eq. 3: and related text: Attention: there is a trap in the smoothing error, because it depends on which altitude grid it is evaluated. Evaluation of the smooth-C2186ing error for the old retrieval with the coarser retrieval grid will ignore smoothing error components related to small-scale variation which can only be presented on the finer grid. Thus smoothing errors may not be intercomparable. The more formal problem mentioned later, that a priori covariance matrices are often singular just reflect this problem: These might have been evaluated on a too coarse grid. TSVD inversion as suggested by Mathias Schneider solves the problem only formally. The core of the problem, however, is that no information on climatological small scale variability and correlations is available, and this leads to an inappropriate estimate of the smoothing error. The problem with the smoothing error is two-fold: First, a priori variability on small scales may be unknown, and second, the estimate of the smoothing error de- pends on the grid on which it has been evaluated. For this paper, it is only important to make sure that smoothing errors of the two intercompared retrievals are evaluated on the same altitude grid, using the same a priori covariance matrix, and that the latter actually contains real information on the variances and covariances on a grid as fine as the retrieval grid.

Response: We evaluate the errors and averaging kernels on the same forward model grid using A = MGK, where M is the mapping matrix from retrieval levels to forward model levels. I have also used the same covariance for evaluating the smoothing error. As you note, it is not necessarily fair to compare smoothing error reduction if the covariance has changed between retrievals. That said, I do not compare the smoothing error between retrievals for the reasons you stated; I instead compare the change in precision, which is quantifiable. However, to address this concern I have changed the text in Section 4.3 to say "On the other hand, the smoothing error in the boundary layer has decreased because of the increased sensitivity" which is an accurate statement.

Comment: p. 6685 l. 19: This is interesting because also for MIPAS it was found that in this spectral region joint retrievals (in this case: N2O and CH4) perform better than single species retrievals (A blind test retrieval experiment for infrared limb emission spectrometry, T. von Clarmann et al., J. Geophys. Res., Vol. 108, No. D23, 4746, doi:10.1029/2003JD003835, 2003.)

Response: This is possibly because N_2O and CH_4 vary more in the stratosphere then in the troposphere which means that a joint estimate of CH_4 and N_2O will reduce total uncertainty better than assuming a fixed N_2O; whereas, I am using the N_2O estimate to correct unphysical variations in the CH_4 estimate. Consequently, I think the Echle paper reference is probably more relevant for this reason? If you disagree then I can add your paper reference in the next iteration.

Comment: p. 6685 l. 24: Is it really a CFC line? Heavy molecules have their lines so close together that I suspect it is rather something like a Q-branch.

Response: Yes it's a CFC absorption feature at 1280 which we will add in the next algorithm release. There is also the methane Q branch at 1308 which we also avoid; I have added text indicating that we also avoid the Q branch.

Comment: p. 6686 l. 4: where the COLUMN vectors x... (This is because people not familiar with this formalism tend to build a matrix when several vectors are put in a matrix, but you build a column vector of several column vectors).

Response: Fixed!

Comment: p. 6687 l. 10-24: Is this new Sa matrix used also to evaluate the smoothing error? If so, is it also used to evalute the smoothing error of the OLD retrievals? If smoothing errors
between the old and new retrievals are compared, it is essential that both smoothing errors are evaluated on the same grid, using the same Sa.

Response: See earlier response

Comment: p. 6688 l. 2-3: This over-defensive statement on validation does not help the paper. I suggest to simply remove it.

Response: Removed

Comment: p. 6688, Eq. 6: Again the formalism is a bit sloppy: What are scalars, what are matrices?

Response: As noted earlier, the formalism is (hopefully mostly consistent) in my word document; I will ensure that the copy received by the referees have the same consistency.

Comment: p. 6688 l. 20 “mean biases”: Isn’t the attribute “mean” obsolete? Aren’t biases always average differences?

Response: Removed the word mean

Comment: p. 6710 Fig 3b: “is the sum of” is misleading because it is the quadratic sum.

Response: Added covariance to fix.

Comment: p. 6690 l. 15: It is mentioned only here that a log based retrieval is used but this information is needed much earlier, eg. near Eq.5. A lot of the text and figures (e.g. averaging kernels) is easily misinterpreted when one does not yet know that the retrievals are logarithmic.

Response: Added language when discussion the radiance and Jacobian plot (now Figure 1) and after the retrieval vector (Equation 5)

Response: p. 6693 bottom: Can issues with the pressure broadening coefficients be excluded?

Comment: I don’t know. However, this would be the same as a spectroscopic error so I believe the text is sufficient for describing the potential set of errors that could explain this error.

Comment: p. 6696 l. 8 (possibly also elsewhere): “second order statistics”: Wouldn’t the correct term be “second moment statistics”? kth order statistic seems to be something entirely different (c.f. http://en.wikipedia.org/wiki/Order_statistic)

Response: This is the same language used in the Bowman et al. reference. However, I am more confused then ever whether either term is proper after reading the Wiki page above. Consequently, I have simply removed the phrase.

Comment: p. 6697 l. 11: Mathias Schneider argues that the improvement might be caused by a different Sa rather than the use of a wider spectral range. However, these choices are not independent: Certainly a weaker regularization by larger a priori variances alone will improve the altitude resolution but in turn the observation error of the retrieval will increase. I think the improvement of the altitude resolution at the cost of larger error bars can easily be predicted and does not need an additional test. Better resolved profiles at equal or better (smaller) observation errors, however, indeed can only be achieved if more measurement information is fed into the retrieval, e.g., if a wider spec-tral range is used. This is directly linked to the “law of large numbers” in probability theory. Perhaps it helps to reword the conclusion in a sense like “...by using a wide spectral range, allowing a weaker constraint without loss of precision" or something similar.

Response: I believe this is addressed in the previous comments; however as also noted, the precision error in the new retrievals is worse in the boundary layer over the tropical oceans. On the other hand, the information content over continents and high latitudes has probably increased because the a priori uncertainty over these regions is much higher (and not well known). We wont know for sure until we perform more robust comparisons against global models and in situ data sets. As an aside, I have just made the first formal comparisons of these profiles to co-measured profiles from aircraft data and the error characteristics and sensitivity appear to well describe the comparison; so I am hopeful that we have done a good job at characterizing the errors and the sensitivity of these retrievals even if they are not yet fully optimal. I have left the text unchanged as I believe the current text is describes the change in the retrieval characteristics.

Response to anonymous referee 3

Comment: Please check and correct the matrix-vector notation in equations. I believe the vectors are not in bold italics in the current version.

Response: As noted in previous comments, this appears to be a transcription error. I will verify that the equations come out correctly in next iteration.

Comment: Introduction: Figures 1a and 1b illustrating the sources, sinks, and processes controlling tropospheric H2O, CO2 and CH4 are nice, but, because this is not the topic for the paper, can be simply replaced by the corresponding references.

Response: This was also noted by Dr von Clarmann; we have remove the figures but left in the text.

Comment: Eq.(3): Should the 3rd term be **S**M?

Response: Fixed ordering

Comment: Smoothing error: the authors state that the term (**A**xx-**I**)**S**a(**A**xx-**I**)T in Eq.(3) represents the smoothing error. However, the smoothing error is (**A**xx-**I**)**S**e(**A**xx-**I**)T, where **S**e is the covariance of an ensemble of real atmospheric profiles about the mean profile (detailed discussion of this issue can be found in the book [Rodgers, 2000]). When true **S**e is unknown, using a priori covariance matrix **S**a instead of **S**e introduces the corresponding limitations in estimates of the smoothing error. I absolutely agree with T. von Clarmann that the smoothing error in new and old retrievals should be estimated on the same grid using the same **S**a. Please stress this and clarify what **S**a have you used.

Response: We have added discussion on the smoothing error for both HDO and CH_4 and how we make the choices to calculate the smoothing error. I have also expanded the description of how the smoothing error is calculated as discussed in your comment.

Comment: I think, it is important to demonstrate that the improved vertical resolution is not at the price of degraded accuracy, but the new retrieval is indeed presents the better estimates. For this, I suggest the following modifications in Figures 3b and 5b: - Left subplot: combine two current subplots into one. Mirror on negative part is not needed, use only positive part, and use different line notations (solid- dashed or similar) for old and new retrievals.
Right subplot: present the error estimates for old and new retrievals, when profiles are presented in the same vertical resolution. The presenting profiles in the same vertical resolution can be done via (i) convolving low-resolution profile with the averaging kernel of a high-resolution profile and the other way round, or (ii) degrading high-resolution profile down to a resolution of low-resolution profiles by corresponding smoothing. The first approach is more accurate.

Response: As discussed in the text, the precision of the HDO/H_2O estimates are probably not improved in the tropical oceanic boundary layer. However, as also discussed in the text; we are confident that our errors in the precision and interferences are properly quantified and that the new and older estimates are consistent (within calculated uncertainties) in the overlapping altitude regions; these are more important statement for users of this data. For the higher latitudes there is definitely an improvement because the previous retrievals could not provide information about these regions due to very low sensitivity. Finally, the separation of the averaging kernels for the HDO and H_2O estimates improves the ability to distinguish between the free troposphere and PBL as long as the uncertainties are small relative to the variability between these regions; which they are for this new version of the data.

Similarly, the new methane profiles are now sensitive to variations in the middle troposphere and the previous methane estimates were not.

As noted in the text, these improvements in the vertical resolution were due approximately half by using the nearly the full spectrum at 8 microns and half by adjusting the constraint with the proportions changing depending on temperature, clouds, etc. We also note that we will likely change the constraints further (if they are sub-optimal) for the next release after more careful comparison between these data and in situ data in the altitude regions and locations where these new data show sensitivity.

I would like to keep the figures as they are now as they are self-consistent with respect to the altitude grid and it is also how we have been showing the averaging kernels and uncertainties in previous papers.

Comment: Fig.3b and Fig. 5b: Please indicate units on horizontal axis.(Relative error?)

Response: Text added indicating this is approximately a fractional error.

Comment: Figure 10: Above 100 hPa, the total error is larger than a priori uncertainty. It looks very strange for me: measurements worsen a priori knowledge? Please provide a comment/explanation.

Response: You typically see this behavior at altitude regions where the sensitivity is low and has cross-dependency on other altitudes.

Comment: Figure 6, bottom: the figure is hardly readable. Please try to improve vertical scaling or consider including the data statistics.

Response; I have increased the size of the figure. I will also work with the editor to improve the figure if that is insufficient.

Comment: P.6684, l. 24: ""to a decreases uncertainty"" -> please change into ""to a decrease in uncertainty"" or ""to a decreased uncertainty""

Response: Fixed

Comment: P.6688 line 19: Please explain/discuss shortly the increased uncertainty at 700 hPa.

Response: Added comment about change to the constraint.

P.6690, line 13: I think it is better to define delta-D immediately after the first appearance of this notation in line 13, and simplify the text below.

Response: Move the definition up to where we first mention "per mil"

Comment: P.6690, line 15: It is written that the data with DOF>1.0 are used, while the caption of Fig.6 states that DOF threshold is 0.7. Which one is correct?

Response: 1.0 is correct

Comment: P.6690, line 18, ""than""-> ""then"
Response: Fixed