

Interactive comment on “Information-based mid-upper tropospheric methane derived from Atmospheric Infrared Sounder (AIRS) and its validation” by X. Xiong et al.

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We thank the reviewer for constructive comments and suggestion on our paper. Following these comments/suggestion given, we revised the paper accordingly.

To answer the question “How much better is this approach than others?”, we added the comparison of AIRS retrievals with aircraft measurements on a fixed geographical layer at 300-500 hPa in the bottom panel of Fig.4, from which we can see the correlation is much less than using the mid-upper tropospheric CH₄ derived in this paper. Considering the difficulty in the retrieval in the HNH and the lack of aircraft measurements, the improvement in the correlation is significant.

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The maximum sensitive layer (>0.95) is defined instead of the maximum sensitive level. To justify the use of 50-250hPa layer below the tropopause, the histograms of the lower and upper boundaries of the maximum sensitive layer (MSL) is plotted in Fig. 2. Fig.3 is added to demonstrate the upper and lower MSL vary a lot in the HNH, so to use the AIRS retrieval products at a fixed pressure layer to analyze the spatial and temporal variation of CH₄ is inappropriate in the HNH.

All these typos and grammar errors have been corrected and double-checked. We really appreciate the time of the reviewer to point out the errors in language.

DETAILED COMMENTS

The measurement of CH₄ from satellites is complicated by the fact that no satellite instrument makes a simple measurement. No satellite instrument measures the mixing ratio at a single level and no satellite instrument (indeed no instrument that this reviewer is aware of using any technique or platform) measures the total column amount. All satellite instruments have some weighting function or averaging kernel associated with the measurements and this is an integral part of the result. Thus the statement that SCIAMACHY is sensitive to total column amount (16336) is incorrect – SCIAMACHY has an averaging kernel as every other instrument. That averaging kernel may be nearer a vertical line than other instruments, but it is not a vertical line from the surface to the edge of space and therefore it does not measure a true column amount.

Thanks for pointing out this mistake. This sentence is reworded as:

In contrast to SCIAMACHY which uses the absorption spectra of solar radiation in the near-infrared and has a better sensitivity to methane in lower troposphere, AIRS is . . .

Retrievals can be further complicated by additional assumptions about the profile, whether these are contained in a priori information or in the details of the constraints in retrieval. A major issue therefore is the statement on page 16338 that the averaging kernel for this AIRS product is not distributed with the data. If this is true, then it is a

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major defect of the data and should be rectified. These kinds of measurements without the corresponding AKs are very hard – almost impossible – to understand. However in the next paragraph the authors produce these averaging kernels in Fig. 1. The reviewer assumes that this is a result from a more specialized retrieval code. The lines in Fig. 1 should be defined. Reference is made to 11 trapezoidal functions and a 100 level grid, but it is not clear how these work together.

It is right that the averaging kernel for this AIRS product is not distributed with the data V5. However, data of the AK are included in the NOAA products and these products are used in this paper. Data of the AK will be delivered in the products of version 6 by NASA.

Fig.1 is re-plotted, in which the maximum sensitive layer is defined when the AK's area is larger than 95% of its maximum. Due to the limit of information content, 11 layers are used in the retrieval and the corresponding averaging kernels are plotted in different colors. The radiative transfer model has 100 levels. To obtain the maximum layer we first interpolate the AK's area (marked in black dash line in Fig. 1) in the 11 levels into the 100 levels of radiative transfer model.

It seems that the fundamental point of this paper is that referring the problem to the tropopause level makes things more consistent than referring them to the surface. Fig. 2 is the key figure in this respect, but it is confusing. The last statement of section 3.1 is that "we can see that the maximum sensitive layers are mostly located between 50 and 250hPa below the tropopause". The figure lower panel certainly indicates that the level is below the tropopause, but the results at higher latitudes seem to be grouped into two: 0-130hPa and 150-300hPa with an interesting minimum between them. There is also some confusion as to whether the discussion refers only to the NH or to both hemispheres.

As the reviewer suggested, we plotted the histogram of the upper and lower boundary, i.e. the upper/lower maximum levels relative to the tropopause. From the histograms, it

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is clear that the maximum sensitive layers are mostly located between 50 and 250hPa below the tropopause Our analysis focuses on the high northern hemisphere, but it should work in the high southern hemisphere.

16340 The statement is made that comparisons are limited to data above 500-600hPa – the reviewer would have expected a value and not a range here.

The first two sentences have been revised as: Aircraft measurements of CH₄ from the NOAA /ESRL/GMD Carbon Cycle Group were used for a preliminary validation of AIRS CH₄ in a few layers (Xiong et al., 2008). In this paper we limit the comparison to data in the layer 50 to 250 hPa below the tropopause, and this layer is mostly above 500-600 hPa. The mean mixing ratio in this layer will be compared with AIRS.

16341 The simultaneity condition for the AIRS profiles is stated here as within 800km and 24hr. The authors should specify whether that is +-800km and +-24hrs and also clarify the statement on page 16339 where the time criterion is specified as "the same day".

These sentences are reworded as: The AIRS retrieved profiles within a radius of 800 km from the aircraft measurement in the same day are selected for validation.

16341 Comparison is made with a previous paper by Xiong et al.. Although that paper is available, the reviewer would advise including the relevant figure in this paper so that the improvement can be directly judged. There is a further problem when doing the comparison in that Figure 9b does not appear to be the correct figure number. The comparison with the 22 sites is in Figure 6b and in that case the correlations indicated are 0.79 for 459-596 hPa and 0.77 for 358-459 hPa, significantly higher than the 0.64(0.57) in this study in contradiction with the text.

Figure 9b is changed to Figure 6b. As suggested, the comparison of the retrievals with aircraft measurements in the geographical layer 300-500 hPa is added. The following sentences are added:

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Compared to the previous validation at the layer 358-459 and 459-596 hPa by Xiong et al. (2008) (lower left and upper right panels of Fig. 6b), the correlations which are 0.79 for 459-596 hPa and 0.77 for 358-459 hPa seems significantly higher than the 0.64(0.57) in this study. However, if we restrict our analysis to include only the data in the mid-to-high latitude regions in Fig.6b in Xiong et al.(2008), the correlation between AIRS and aircraft measurements is much smaller. This is evident from the comparison of the AIRS retrievals vs aircraft measurements at the layer 300-500 hPa using data in the regions above 25N, as illustrated in the lower panel in Fig.4. The correlation coefficient is only 0.4-0.47.

16341 There is a statement made about extending the profile above 350hPa, but no reference to how often this is a significant issue. If the reference is to the tropopause, perhaps the upper limit of the profile should also be specified wrt the tropopause.

This sentence has been removed to Sect.3.3, and reworded as:

To convolve aircraft measurements, we need to extrapolate the in-situ aircraft data to the top of atmosphere at 0.005 hPa, which can be done by using the monthly average of model simulated data from TM3 (Houwelling et al., 2006). This will lead to some additional uncertainties to the convolved values, particularly for NOAA/ESRLGMD data as the samplings are mostly below 300-350 hPa [Xiong et al., 2008].

16342 Reference made to a number of aircraft measurements and then to a specific in situ measurement system. Was this used in INTEX A? B? Which aircraft? Are these the only results being compared in the following discussion?

The following sentence is added: The CH4 aircraft measurements in INTEX-B were carried out in the same way as in INTEX-A.

16342 The reviewer considers that the use of the term “truth” is inappropriate. A number of measurements by different techniques that are being compared. None of these is “truth” – each has its errors and biases to consider.

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All the words “truth” have been changed in the whole context as suggested.

16343 The discussion of “tuning” of the absorption coefficients is disquieting. What effect does this have on the results? If the retrieval is tuned to a particular dataset, how does it behave with other datasets?

The following sentence has been added: As detailed by Xiong et al.(2008) this tuning was implemented in the radiative transfer model calculations in version 5, and would impact the retrieval results in both the tropics and mid-high latitude regions, so some adjustments of the turning will be required for the future improvement of CH4 retrievals from AIRS.

16344 The the measurements in Fig 6 lack error bars. There is no explanation of the figure. As far as the reviewer can determine the vertical lines are the of cases for the NOAA data, but this is not explained and they look like error bars.

Error bar is added in Fig.6 (now Fig.7). The whole paragraph has been revised as below:

In order to compare the difference of validation results using these three different aircraft data sets and check the dependence of the retrieval error with season, Fig. 7 plots the biases and RMS errors of AIRS retrievals using data from INTEX-A, -B and NOAA/ESRL/GMD in different months. The sample number using NOAA aircraft measurements for each month is also plotted in the bottom panel. From the biases for different months estimated from NOAA/ESRL/GMD data, it is evident that the bias in summer is smaller than in other seasons. This is expected as the moist air during summer pushes the MSL of AIRS to a higher level than in other seasons, and the lapse rate and the degree of freedom of the retrievals in summer is relatively larger (Xiong et al., 2008). The bias of retrievals validated using the INTEX-B aircraft data is negative and is in a good agreement with that using NOAA aircraft data, but the bias using INTEX-A data is positive and about 0.5% higher than that using NOAA aircraft data. Further examination indicated that in INTEX-A, there are 9 aircraft profiles in which AIRS have

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a positive bias larger than 1.0%, and the mid-upper tropospheric CH₄ mixing ratios for these profiles are less than 1780.0 ppbv from aircraft measurements. These cases with low CH₄ mixing ratios suggest the intrusion of air masses from stratosphere to troposphere and were sampled by aircraft measurements. However, AIRS covers a larger area than the aircraft measurements, so it is the difference in the spatial coverage between AIRS and aircraft measurements that leads to a large positive bias in INTEX-A. We also noted that there were large forest fires in Alaska in 2004 during the campaign of INTEX-A. Differences in the spatial sampling of AIRS and the in situ measurements might be an additional source of uncertainty.

16345 “Due to the large difference in the sampling time and location..” It is not clear whether this is the 800km/24hr criterion or something else. One alternative would be to tighten the criterion to produce fewer matches, but more relevant ones.

Not exactly. Sampling time here is referred to the sampling interval of aircraft measurements are too sparse. This paragraph is removed to the section of discussion.

16346 There actually little in the paper to show that the tropopause correlates “well” with the maximum sensitive layer. Figure 2 is not convincing. Some simple histograms showing the pressure level of the maximum sensitivity and the delta pressure from the tropopause would be useful. Presumably the latter would show a much tighter distribution than the former. The improvements cited in the correlations and biases are there, but are not outstanding. The comparisons show that the AIRS data are consistent with the aircraft measurements – but as to showing “valuable information” – there has to be an application where this is significant – can AIRS measurements help with diffuse sources such as the wetlands/permafrost? These large-area diffuse sources are the hardest to determine by any method.

Fig.2 is replotted as suggested. Fig.2 shows the variation of tropopause and the maximum sensitive layer in spatial domain, Fig.3 is added in the time domain. This sentence about the “valuable information” has been revised as:

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These results suggest that this new information-based product provides a better way to use the retrieved CH₄ from thermal infrared sounder to analyze the spatial and temporal variation of CH₄ in the mid-high latitude regions where the satellite observation is very difficult.

16347 The discussion of the possible errors and biases in the measurements should not be made in the summary and conclusions section. The discussion is unfocused and confusing. A much better assessment of how (for example) the sampling bias of the aircraft measurements might influence the results should be made. What are the relevant error sources and what can be done to reduce them? The correlations recorded in Figs. 3-5 are not large – the best is 0.72 – and some explanation of the reasons for the variance and how this might be reduced would be appropriate.

As suggested, the summary and conclusions have been revised. Uncertainties in the validation and the use of this product are discussed. The discussion of the detail of retrieval uncertainty has been moved to the Sect.3.3.

16348 An attempt has been made for the case for the use of the tropopause as the reference layer in this paper. This method might well be appropriate to well-mixed gases such as CO₂ and N₂O, but is unlikely to be successful for CO where the changes in the shape of the vertical profile are very significant. In that case, the averaging kernels, that should be used in any case, are going to be absolutely necessary for the interpretation.

Thanks. This sentence has been added in the summary and conclusions.

MINOR CORRECTIONS:

Figures have been re-plotted as suggested.

We appreciate the reviewer to point out all the grammar error. All these typos and grammar errors have been corrected as suggested.

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