

Atmos. Meas. Tech. Discuss., 5, C1905–C1908, 2012

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AMTD

5, C1905–C1908, 2012

Interactive
Comment

Interactive comment on “Retrieval of tropospheric CO column from hyperspectral infrared sounders – application to four years of Aqua/AIRS and MetOp-A/IASI” by T. Thonat et al.

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We'd like to thank referee#2 for his/her comments. Here are our answers.

Abstract, line 9: do the authors mean FWHM?

→ Yes, we refer to FWHM. We have modified the abstract to make it clear.

Abstract: I believe it should be colocated not collocated.

→ It has been corrected in the whole manuscript.

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Abstract and throughout the paper: I'm not sure why so many decimal places have been used, e.g., 3.42%? At the very least the authors should be consistent.

→ We have reduced to 1 decimal.

Abstract, line 18: would be good to show some metrics that exemplify the statement about "good agreement."

→ The average difference between AIRS and IASI is 0.5 ppbv with std of 3.5 ppbv. This has been specified both in the abstract and in Section 5.3.

Abstract, line 20: this is to be expected? If yes, the authors should say so.

→ Done.

Introduction: While the history behind measuring CO from space is interesting I'm not sure it is strictly necessary in this paper. I suggest the authors consider shortening the introduction, staying focused on the subject matter.

→ We think it is important to highlight previous studies dealing with CO retrievals and since it concerns only one paragraph of the introduction, we would prefer keeping it.

Introduction, line 18: the authors should state the most important trace gases.

→ Done.

Introduction, line 25: The authors should clarify whether the method presented works outside the tropics.

→ Here we limit our study to the tropical region because of the availability of the used cloud and aerosol detection mask.

How does IASI/AIRS compare with MOPITT, a very different instrument? I'm sure the authors have done this work but it's worth briefly summarizing the findings.

→ As said in the conclusion, the comparisons with other sounders/retrieval methods is the next step of this work. The objectives of the paper are to describe the retrieval

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method, compare to in situ measurements and analyse the results highlighting the impact of biomass burning on the evolution of CO in the tropics. Comparisons with MOPITT as well as with other instruments/retrievals are in progress.

If these data are to be adopted by the wider atmospheric community to estimate CO sources from biomass burning, for example, they would have to be accompanied by averaging kernels, single measurement uncertainties, etc. Are these data available?

→ The averaging kernels are available; the single measurement uncertainties can be computed by randomly modifying the input data and calculating the subsequent CO change (as described in section 5.1).

Section 5.2: the authors should explicitly state how they have sampled the satellite data in the vertical to match with the aircraft data. Is the aircraft altitude reasonably constant?

→ We haven't sampled the satellite data in the vertical as we have only an integrated content, but, as described in section 5.2, we have made a selection on the aircraft data keeping only the points above 5 km. We have checked that limiting the aircraft altitude doesn't change the conclusions and to have better statistics we have used the range 5-12 km. The text has been modified to explain this.

Section 5.2: What method have the authors used to estimate the slope? They should not have used a simple linear regression that assumes a causal relation between the two variables.

→ By mistake, we have put the slope values instead of the linear correlation coefficient values. It has been corrected.

Section 5.3, page 3880, line 22: I don't see this sharp increase in CO.

→ We have rephrased: "an increase in maximum CO values". It is true it is not as "sharp" as in South America but it is still important (~20 ppbv).

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I didn't find the data shown in Figure 10 very useful as it is plotted. I strongly suggest the authors find another way to reduce their data.

→ Fig. 10 is plotted the same way as Fig.1 of Matsueda et al. (2002) to compare with it. As said, the evaluation of our CO is here based on aircraft measurements, and in this part of the world data are not yet available from the CONTRAIL team; that's why we did it this way. It is a particularly interesting area, with biomass burning and ENSO influences, with a significant seasonality.

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