Atmos. Chem. Phys. Discuss., 9, C1688–C1691, 2009
www.atmos-chem-phys-discuss.net/9/C1688/2009/
© Author(s) 2009. This work is distributed under
the Creative Commons Attribute 3.0 License.

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Carbon monoxide distributions from the IASI/METOP mission: evaluation with other space-borne remote sensors" *by* M. George et al.

Anonymous Referee #1

Received and published: 10 June 2009

General comment:

The paper "Carbon monoxide distributions from the IASI/METOP mission: evaluation with other space-borne remote sensors" by George et al. provides a first evaluation of the global distributions of atmospheric CO retrieved from the Metop/IASI sensor by comparisons with 3 other spaceborne sensors. The CO IASI distributions are new and therefore exciting and the results from this paper are interesting for the scientific community using CO data from spaceborne sensors. The paper is therefore adequate for publication in ACPD. The aim of comparing instruments is to provide an evaluation of the data and to explain possible discrepancies in order to improve future processing

and to help data users. Nevertheless, the analysis of the presented material is not going deep enough and the novelty of IASI is not a sufficient argument to warrant publication. The results presented are too close to descriptive statistics and the paper needs some revisions to strengthen the analysis before publication. My suggestions to improve the content and analysis of the paper are detailed by the following specific comments.

Specific comments:

1.The radiative transfer and retrieval tools used for IASI CO are only mentioned in the introduction with a reference to Turquety et al. (2009). Because they are important for the analysis, the main ingredients, principles and assumptions of both the radiative transfer and the retrieval software should be described in this paper.

2.We also need more details about the a priori used for the retrievals with at least the vertical profile and associated standard deviations displayed and discussed. The a priori profiles differences may be important to understand discrepancies between sensors (see 3.a).

3. The methodology used for the comparison of the spaceborne data is a basic direct comparison. Nevertheless, "when intercomparing measurements made by remote sounders, it is necessary to make due allowance for the differing characteristics of the observing systems, particularly their averaging kernels and error covariances". This is the first sentence of Rodgers and Connor (2003) who have developed the mathematical and theoretical basis to compare remote sounding instruments. It is therefore surprising not to find any mention to this work in the present paper. The methods described in Rodgers and Connor (2003) may seem complicated but they are useful to make quantitative comparisons and to have a better understanding of discrepancies. In particular, I suggest to take the following simple recipes into account to improve the comparisons:

a.section 3 and equation 10 of Rodgers and Connor (2003) allow to "adjust [the different retrievals] for different a priori".

b. section 4.3 should improve comparisons of the different estimates of the total columns. In particular, Equation 24 will enable a quantitative estimation of the impact of the differences in vertical sensitivity upon the variances.

4.The water vapor and temperature profiles are EUMETSAT level 2 products. There is a reference to Schlussel et al. (2005) for a description of the product. We need more up to date information about these products! What is the status of their validation? How are the pixels selected or rejected by EUMETSAT and therefore by FORLI (quality, cloud filtering...)? Do you reject other pixels and on which criteria?

5.In section 3.1, we need a plot with the a priori vertical profiles and standard deviations of the covariance matrices compared even if it may be complicated with TES.

6.The comparisons of the averaging kernels of section 3.2 are not correct because, as mentioned by the authors, the retrieval levels are different for the different sensors. Therefore Figure 4 is not really useful to compare the vertical sensitivity and has to be modified. First, the best data provide about 2 independent pieces of information (except for AIRS). The authors should therefore choose one more scene with higher information content. Second, from the averaging kernels they should analyse what partial columns are best representative of those 2 independent pieces of information (0-X km and X-Y km). Third, they should compute and compare the averaging kernels for the corresponding partial columns and also for the total column. From such an analysis, they may reach a better conclusion about the sensors vertical sensitivities and about their impact upon the discrepancies (see 3.b).

7.In section 3.3, retrievals above North Africa and the Arabic peninsula are mentioned as problematic for IASI and AIRS. What is the difference with MOPITT and TES? Are those problems related to the surface emissivity used for the retrievals? to the reflected solar light ? What are the assumptions made about those aspects in the different algorithms?

8.In the conclusions:

a."sensitivity varies as a function of ... local emissivity": this is not detailed in the core of the paper.

b."in the SH... IASI is...15% lower ...than AIRS and MOPITT": why? Does it have anything to do with the a priori (see 3.a)?

c."column comparisons... biased by different a priori": yes, see 3.a in order to have a better idea.

d."profile comparisons... will account for averaging kernels and a priori differences": column comparisons also have to (see 3.a and b).

Ref: Rodgers, C., D., and Connor, B.J., Intercomparison of remote sounding instruments, J. Geophys. Res., 108, doi:10.1029/2002JD002299, 2003.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 9793, 2009.