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Atmospheric Measurement Techniques Discussions

Interactive comment on "A comparison of OEM CO retrievals from the IASI and MOPITT instruments" *by* S. M. Illingworth et al.

Anonymous Referee #2

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The purpose of the work presented in "A comparison of OEM CO retrievals from the IASI and MOPITT instruments" by S.Illingworth et al. is to calibrate the IASI CO products obtained at University of Leicester with the ULIRS algorithm (presented in another dedicated paper, not yet accepted) against two independant data sets, namely the MOPITT CO products versions 3 and 4. Reciprocally, the ULIRS CO products are proposed in this paper as an external dataset to characterise the differences between MOPITT v3 and v4. Eventually, this paper claims to be the first such intercomparion of a IASI CO product with both the MOPITT CO v3 and v4.

This paper, presenting and discussing a new instrument product, is found very relevant in the scope of AMT. The paper is well articulated and the language is of a high

standard.

It highlights how important it is for products validation and exploitation that their vertical sensitivity to the true state as well as the 'a priori' knowledge of the atmospheric state that supported the retrieval are provided along with the products themselves. The ideas, assumptions, experiences and interpretations are clearly enounced, which makes this paper very didactic for the interpretation of retrievals obtained with the optimal estimation method by Rodgers.

The work concludes on a better agreement between ULIRS CO and MOPITT v4 than with v3, with a mean bias smaller than 7%. This is a general figure although the discussions in the paper restrict to ocean daytime cases only (and for which smaller errors could be reported, couldn't they ?). No mention is made to the standard deviation relative to that mean, which is a missing important component of the error.

Specific comments: 1. It is refered to 10.5194/amtd-3-3747-2010, 2010. 4891, 4893, 4898 for the presentation of ULIRS which acceptance should condition the publication of this paper.

2. The scope of the paper is global mapping of CO, which is recalled in the abstract and introduction, and the purpose is the calibration of a new product. Thus the data used to support this appear limited in comparison even though the conclusion paragraph opens on to the necessary perspectives of an extended validation exercise.

In particular: p4893.11. The a priori has been taylored for this particular case study, which is stressed out as a key point but somewhat contradicts the philosophy of the statement in previous sentence: "A constant a priori product, i.e. one which is the same for every retrieval, is used to ensure that any spatial or temporal features observed in the retrieved CO product are not symptomatic of features in the a priori." How can the reader extend the conclusions on the accuracy of ULIRS to the rest of the World ? How different would have they been with a not-so-specific static a priori ?

MOPITT v3 and v4 differ in CO load but also in the cases where retrievals were performed/sucessful, which is mostly obvious over land. Is the cloud detection involved here ? This subject has not been touched in this paper: how are the respective retrievals/analyses sensitive to cloud contamination ?

The abstract and conclusions should state more clearly that only the ocean day-time configurations are discussed and presented in details in this paper.

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Minor comments:

a. lines p4895.8-13: The repetition in the two consecutive sentences gives an unnecessary insistance on the mathematical trick to get positive CO VMR and more importantely misses to explicit that the covariance of CO profiles in logarithm space describes the relative or fractional VMR variability [www.acd.ucar.edu/mopitt/v4_users_guide_val.pdf]. The latter has the advantage to be less variable from site to site and season to season as confirmed with in-situ measurements ["Retrievals of carbon monoxide profiles from MOPITT observations using log-normal a priori statistics," M. N. Deeter, D. P. Edwards, and J. C. Gille, J. Geophys. Res., 112, doi: 10.1029/2006JD007999 (2007)]. Therefore, a constant and global background covariance matrix of ln(CO_ppmv) can be used although the a priori mean profile is variable. This is believed to be a very relevant information for the reader in the context of this paper at this stage already.

b. Equations 2 and 4 are the same.

c. lines p4897.15-19 are a repetition of p4898.24-26

d. lines p.4900.26-27 and p.4901.1-2 andFigs.5&6: please explicit what is meant by "A_IASI at retrieval pressure levels close to those of MOPITT V3(4) are shown in Fig.5(6)". It seems to me that A_IASI is shown on its full native grid in fact.

e. p4901. It is carefully and wisely repeated that the statistics apply to ocean daytime

cases which IASI retrievals passed the residual and chi2 tests and spatially and timewise matched MOPITT data. Although given in Table 1 later, it would ease the reading to know already the sample size (about 1000 cases here).

f. p.4902-4904: section 5 has only one subsection: 5.1. Was a 5.2 intended at some point ? => Reorganise this section.

g. p.4904.26-28: these differences are indeed of a very similar magnitude in the upper troposphere. They are as far as I can read actually somewhat different in the lowest layers, which is consequently also reflected in the differences in the total column: only in the stddev for v3 (20.4% vs 6.1%), both in the bias and stddev for v4 (5.6% vs 18.4% and 20.3 vs 9.3%, respectively). The following conclusion that many of the observed differences "can be explained by the smoothing bias", although not challenged by this comment sounds too fast to the reader. A short discussion would certainly support it better, especially since it serves as introduction to the section 6.

h. p.4906.4: how the conversion of an AK from a pressure grid to the other was performed ? Simple interpolation ?

i. eq.9: litteraly, shouldn't the A_MOP' factor of A_IASI and eps_IASI rather read A_MOP" ?

j. p.4906.15 Fig.10 actually says that DFS for A_MOP' - A_MOP'.A_IASI is 0.08, not 0.07. Does not change the interpretation but confuses the reader.

k. p.4907.6 & 17. I can not find the values of 6.32% and 4.21% in Table 2. Could you please clarify in what these biases are different from the numbers presented in Table 2 ?

l. p.4908.5-6: for clarity, explicit which one from IASI or MOPITT v3/v4 sees a higher CO load.

m. Fig.4: Display A,B,C,D on each quadrant. Would gain in clarity/efficiency for the reader's direct comparison by eyes if the products were plotted on the very same lat/lon.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 4889, 2010.