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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.

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Thank you to all of those involved, and to the referees for their helpful and insightful comments, which have been taken onboard. We have carefully considered the questions and suggestions of the referees, and detailed responses to these questions and suggestions are indicated. Reviewer comments are quoted using [...].Some changes in the manuscript have resulted from the referee's suggestions, resulting in us submitting a revised paper for publication in AMT. This is included as a supplement to this comment.

Anonymous Referee #1

[General comments: This manuscript addresses an important subject of understanding the differences between the products of two satellite sensors. The techniques are sound and related formulations are well described. I particularly like the use of GOES-CHEM model as a tool in the inter-comparison. However, I have a few major concerns that I believe should be addressed before it is accepted.]

We thank the referee for the positive comments. The purpose of this paper is to provide a treatment of the technique for comparing CO satellite products; making use of theoretical insights, data retrieved with common a priori, and model data sets to provide a calculation of differences that would be expected even for 'perfect' instruments. Our methodology provides a mathematical and practical basis which we believe will be of great benefit to future studies by the community. We believe that our revised manuscript emphasizes these aspects in a much stronger manner.

We understand the concerns but believe these are mostly due to the referee not appreciating that our purpose is to provide a demonstration of a technique that will enable future comparisons to be performed by the community with greater rigour. We should have made this point much more clear in our manuscript and have now done so. We answer the points one-by-one below.

[In addition, the algorithm description for the IASI CO product was published in a separate manuscript at AMTD "A new optimal estimation retrieval scheme for carbon monoxide using IASI spectral radiances – Part 1: Sensitivity analysis, error budget and simulations", but not yet at AMT, even though the referee comments were posted on 3 Sept, 2010. The acceptance of this inter-comparison manuscript should be pending on the outcome of the algorithm paper.]

This paper has now been published. The reference is: 'ULIRS, an optimal estimation retrieval scheme for carbon monoxide using IASI spectral radiances: sensitivity analysis, error budget and simulations', Atmos. Meas. Tech., 4, 269-288, 2011, and has been updated in the paper.

AMTD

3, C2970–C2979, 2011

[Specific comments: 1. Not enough spatial and temporal coverage for this subject. The advantage of intercomparing different satellite datasets is in its statistical properties. There are multiple years of observations from both sensors, and they both provide daily global coverage. I'm not sure if I understand about the choice of location and date included in this paper. I could understand it if this choice was to support a field campaign, to test their new products with a specific in situ dataset, or to address a unique science problem. To understand the quality of a new satellite product, validation should be provided to cover a reasonable range of scenarios. Even if the authors stated that "the continuation of this work is to extend the intercomparisons over a larger temporal range, and to different regions," why not wait to publish so that this manuscript can represent a much more comprehensive study.]

This paper presents an initial study into the comparison between the IASI and the ULIRS product. It is by no means a global comparison, but rather it aims to establish a methodology for such a comparison, and to present results over a specified region, over which the ULIRS has been characterised (see Illingworth et al, 2011). In our opinion, it is important that the technique is well understood for this complicated problem, instead of people performing validation comparisons on a wider basis without the theoretical foundation. We have strengthened the manuscript in this regard less readers should not fully understand the implications of what we have done.

Two aspects are particularly noteworthy. Firstly, the use of a small region allows a much tighter definition of a priori mean and errors, which is important to the near-optimal nature of the comparison. In this sense, our study is more akin to that of a specific field campaign which allows a more precise study because of the close attention to regional specificity. Furthermore, the mathematics and our results suggest that actually global studies need to be performed in a different way than might be expected, and might be better as an aggregate of smaller regional studies with common a priori data developed specifically for each region. (we now note this in our conclusions). This is a surprising result most likely, and important to bring to the community. It is beyond the scope of

AMTD

3, C2970–C2979, 2011

our study to perform a global comparison for both reasons of effort and practicality (see second point).

Secondly, the regional constraint was imposed by the availability of data generated using ULIRS which was deliberately conceived as a slow but accurate retrieval, and also by the need to re-process MOPITT V3 data using the ULIRS a priori (a time limited option before the V3 processor was switched off).

[2. Not enough description on the IASI algorithm that is been evaluated. Even if an algorithm description paper was referenced, which is not yet a refereed publication, the major components in the algorithm should be summarized for completeness. What I don't understand is how much description that was given on MOPITT in contrast. MOPITT is well documented in a large number of publications such that the community is well aware what are all included in the algorithm even the latest updates on the forward model and V3 vs V4 features. There is much discussion in a whole paragraph about the MOPITT forward model MOPFAS, but only one sentence on the forward model that is used in the ULIRS IASI CO retrievals. Details about the ULIRS IASI CO retrievals need to be provided such as, but not limited to: the noise treatment, cloud treatment, the sources of the temperature, moisture, and ozone profiles used by the forward model.]

We agree that the balance of description is not as good as it could be. We include a longer description of ULIRS as well as reference to the now published retrieval paper.

[3. Not enough reviews of other people's similar works and there is very limited discussion/review of other IASI CO products.]

The referee has not gone into specifics as to what other works need to referenced, but we believe that we refer to, and summarise the findings of, a number of studies which have been carried out and which are similar to this work, in particular the studies carried out by Ho et al. (2009) and Luo et al. (2007). We have specifically but briefly included the FORLI-CO algorithm as it is a full physics algorithm as we would understand it and

AMTD

3, C2970–C2979, 2011

is most relevant to the paper in the sense of producing data with the required averaging kernels. We do not believe it necessary to review all other IASI CO products since this is a techniques paper about how comparisons should be performed for low vertical resolution.

[Technical corrections: 4. The panel marks “A, B, C, D” are difficult to see for Fig. 5, 6, 10, and 11.]

These figures have been changed accordingly.

Anonymous Referee #2

[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 independent 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.]

We thank the referee for the positive comments. The purpose of our paper is exactly this, i.e. to provide a clear exposition of the technique with both a theoretical basis and

AMTD

3, C2970–C2979, 2011

improved practical implementation. We believe that our demonstration of technique is valuable to the community and provides techniques which can be followed in more wide ranging and more extensive comparisons of satellite data. We have emphasized these aspects more in our revised manuscript.

[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.]

The figures are indeed smaller for ocean daytime but we include other cases in the Tables and we feel it is prudent to quote the conservative values for all cases. Nonetheless, we have updated our quoted error estimates to give a better sense of the comparisons. The Tables show standard deviations and N, allowing error on the mean to be computed.

[Specific comments: 1. It is referred 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.]

This paper has now been accepted for publication.

[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 tailored 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

AMTD

3, C2970–C2979, 2011

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?]

This paper presents an initial study into the comparison between the IASI and the ULIRS product. As noted in the response to referee 1, it is by no means a global comparison, but rather it aims to establish a methodology for such a comparison, and to present results over a specified region, over which the ULIRS has been characterised (see Illingworth et al, 2011). Given the methodology used in this paper, whereby the use of different a priori products is accounted for, we believe this is most appropriate for a demonstration of technique.

We feel it is important that this demonstration of technique is published first prior to global comparisons which might be performed by many people in the community, as it changes our understanding of the way in which we should intercompare satellite measurements(particularly in the troposphere). The referee's comment betrays the fact that the real implications of the techniques in the paper have not been fully understood by the referee and we have therefore strengthened the manuscript in this regard (see also comments to referee 1). The comparison reveals systematic components of error which are due to instrument and forward model errors; the a priori effects have been largely removed by the techniques of section 5, and a priori variations are therefore not so relevant. We also re-state that our purpose is not the global mapping of CO but in fact the reverse which is to confine the region of study to demonstrate the technique and results in a region where a detailed and specific a priori is available. This allows the comparisons to be near optimal.

[MOPITT v3 and v4 differ in CO load but also in the cases where retrievals were performed/successful, 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.]

AMTD

3, C2970–C2979, 2011

Only data which has been flagged as cloud clear has been used in this comparison. The IASI data uses a cloud filtering algorithm outlined in Illingworth et al (2011), and the MOPITT data uses a cloud filter as outlined by Warner (2001). Text has been added to Section 4 to make these points clear.

The abstract has been changed to state more clearly that only the ocean day-time configurations are discussed and presented in details in this paper. However, as the scenarios of nighttime ocean and land are also considered in the paper, the conclusions are believed to be a good representation of the findings of this work. We have emphasized that we perform the retrieval for these cases because likely forward model errors are smaller, thereby allowing us to test more effectively for instrument bias, as opposed to a priori bias.

[Minor comments: a. lines p4895.8-13: The repetition in the two consecutive sentences gives an unnecessary insistence on the mathematical trick to get positive CO VMR and more importantly 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 very relevant information for the reader in the context of this paper at this stage already.]

The repetition has been removed, and a sentence explaining that the covariance of CO profiles in logarithm space describes the relative or fractional VMR variability has been added.

[b. Equations 2 and 4 are the same.]

AMTD

3, C2970–C2979, 2011

Equation 4 has been removed.

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

The sentence on p4898.24-26 has been removed.

[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.]

Yes, A_IASI has been shown on its full native grid, but only the AK profiles for the levels closest to those of MOPITT V3/4 have been plotted. This has been made clearer in the text.

[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 time wise matched MOPITT data. Although given in Table 1 later, it would ease the reading to know already the sample size (about 1000 cases here).]

A sentence has been included to clarify this point.

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

The heading 5.1 has been removed for clarity.

[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.]

AMTD

3, C2970–C2979, 2011

This has been expanded on in the text for clarity.

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

Via a simple interpolation; a note has been included to make this point clear.

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

Yes, this has been changed in the equation and the text.

[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.]

This has been corrected in the text.

[k. p.4907.6 & 17. I cannot 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?]

This is a mistake. They should read 6.4% and 2.9%, thus corresponding to the values in the tables. The text has been changed accordingly.

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

The answer is MOPITT V3, and this has been clarified in the text.

[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.]

This figure has been changed accordingly.

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