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Atmospheric Chemistry and Physics Discussions

Interactive comment on "IASI carbon monoxide validation over the Arctic during POLARCAT spring and summer campaigns" *by* M. Pommier et al.

Anonymous Referee #1

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General comment:

This paper aims at validating CO retrievals from the IASI spaceborne instrument using aircraft observations taken during scientific campaigns in the Arctic. While TIR sensors don't have their highest sensitivity in Polar Regions due to the cold surfaces and low thermal contrasts it is important to validate the data in the Arctic. The publication of this manuscript is therefore worthwhile. Nevertheless, the manuscript needs major improvements both in its structure and in its content before being published.

I have listed below major points to be improved and/or modified in my point of view.

Major comments:

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Interactive Discussion

Discussion Paper

Confusing structure

The description of IASI CO retrievals should be done properly at the beginning of the manuscript in a dedicated section. Indeed, we find partial description of IASI CO retrievals in Section 3.1.1 with a vague introduction to the notion of averaging kernels without proper illustrations. Furthermore, in section 3 the IASI measurements, at the heart of the study, are presented on the same level than ACE-FTS or in-situ observations which are ancillary data. Section 4.2 deals with "performance of the IASI retrieval..." without presenting averaging kernels. Finally, we can find some averaging kernels plotted in section 5 that is at the end of the manuscript, while a good understanding of the comparison results need a previous good description of the IASI CO retrievals. Furthermore, a complete description of the retrievals should introduce error covariance matrices for the profiles and scalar errors for the integrated columns. The primary aim of the paper is the validation of IASI data in the Arctic. Nevertheless, before validation is performed, the paper deals with "long range transport during the 2008 Arctic campaigns" (4.1) and "analysis of IASI information along selected flights" (4.3.2). These 2 sections provide interesting results, but they should come after the validation results provided in section 5 in order to be given some credit.

Section 3.1.1

The ref. describing the FORLI retrieval algorithm is "in preparation". The authors should therefore give some details about this algorithm and its performances. The description of the a priori data lack of details but these data are crucial for the regularization of the retrievals. The authors should give some information concerning the way MOZAIC/ACE-FTS and model outputs are mixed and sampled. A plot of the covariance matrix (not shown in Turquety et al., 2008) would be of interest.

Section 4.1

This section dedicated to LRT is interesting but it should be given some more care. IASI observations alone are not enough to characterize LRT and the study should rely

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on complementary data such as backtrajectories or at least an analysis of the wind fields and meteorological conditions corresponding to the LRT events described in the paper (Asia to US west coast and across the North Pole).

Section 4.2

The results provided here should be somewhat summarized and presented in the section dedicated to the retrieval description. In particular, all the details about the retrieval RMS are not necessary and table 2 could be shortened. I didn't understand to which bias the author refer in the last part of this section.

Section 4.3

In section 4.3.1 the authors describe the collocation criteria between IASI and aircraft data: about 20x20 km and 1h. These are very stringent criteria. Could the authors give evidence of the worsening of the comparisons when the criteria are relaxed to 50x50 km and 2hours or more ? This should clearly improve the statistics.

It seems that comparisons provided in section 4.3.2 don't give highly satisfactory results. This is not completely surprising to me because (i) IASI sensitivity is rather low in the Arctic (ii) the sampled plumes maybe too thin to be detected by the spaceborne instrument. Nevertheless, when the results are presented the way they are, they give a rather poor idea about the ability of IASI to measure tropospheric CO. If the data were properly described and validated previously, the large IASI/aircraft discrepancies could be better explained by the authors and understood by the readers.

In this section, the authors often refer to sea-ice or snow cover to explain loss of information with IASI data and explain the discrepancies. To give credit to such an explanation, the authors should (i) show averaging kernels over snow and sea-ice to show at which altitude sensitivity is lost (ii) explain how the surface emissivity database account for sea-ice and snow cover interannual variability and whether "bad" emissivity is not a better explanation for "bad" retrievals.

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Describing the comparisons displayed in Fig. 6, the authors focus on the lower troposphere where the IASI data lack of sensitivity (as shown in Figure 8). Important discrepancies are also found in the free troposphere around 5-6 km but they are not enough discussed. It seems that they are also attributed to a lack of sensitivity (especially over sea-ice) while the IASI observations have the best sensitivity in this altitude range according to the averaging kernels (Figure 8). Information about the impact of sea-ice upon CO retrievals and averaging kernels (as required above) would really help to answer.

Section 5:

As mentioned previously this section should come earlier in the manuscript. Furthermore, equation (1) will be more understandable if the general retrieval equation (x=xa+A(x-xa)) was introduced earlier. Here again, relaxing the coincidence criteria may improve the statistics. As shown in Figure 6 and 7 and discussed above, thin CO plumes are not detected by IASI anyway.

In 5.2, sea-ice is again mentioned as an explanation for IASI/aircraft discrepancies following a lost of sensitivity. If it was the case, the effect would be accounted for by the smoothing and there would be no differences. An emissivity problem may be a better explanation. Furthermore, the incriminated profile 9(b) has a larger DFS (1.3) than 9(a) and 9 (c) (1.0) which both show better agreements. Same comment for 9(e) and the snow cover. . .

An important problem of section 5 is that it doesn't provide its valuable information in a concise way. Instead of compact plots, very detailed descriptions of the comparisons are given in the text.

For instance, Section 5.3.1 is dedicated to "comparison by aircraft". Is it meaningful to make such an exercise? Looking at Fig. 10, 11 and 12, we would learn more (and remember what we have learned) if we had averaged profiles with spring and summer differentiated. We also need the averaged relative differences together with the RSD to be plotted instead of long descriptions of the biases in the text. The scatter plot should be with a single color and single CC! Does it mean much that R(P-3B)=0.73738 (0.74 would be fine!) in spring ? The important figure is that R=0.37 in spring. The issue of the altitude reached by the different aircrafts could still be discussed in the text. Another point in differentiating the aircraft would be to deal with biases between in-situ measurements. The authors mention “a 7 ppbv negative difference between the ATR-42 and the Falcon 20”. . . but this is not discussed in section 5. Is this bias too low to be meaningful when comparing with IASI?

Section 5.3.2 comes at the end of the manuscript while the surface type is mentioned within the whole manuscript as a major source of discrepancy between in-situ and IASI CA data! This section is therefore very important in this paper and should be dealt with more in depth and earlier (as the whole section 5). As previously mentioned, we really need to know how the emissivity problem is taken care of concerning sea-ice and snow and whether or not this is an issue. It would also be a good idea to have maps of sea-ice and snow cover if such things are available when dealing with “impact of surface type”. The summer averaging kernels are “not shown” while it is very important that the reader could see them. Results shown in Figure 13 are interesting but the authors should add plots of the relative differences and RSD as mentioned above. The differences between retrievals above different surface types are described but not really analyzed and explained. An analysis of the different surface emissivities and the way they are accounted for would help to understand why the bias is negative in spring over sea and positive in summer for instance (if the spring/summer difference is significant).

The English has to be checked thorough fully throughout the manuscript by a native English speaker (such as one of the co-authors).

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14445, 2010.

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Interactive Comment