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Comment

Interactive comment on “Mid-upper tropospheric methane retrieval from IASI and its validation” by X. Xiong et al.

Anonymous Referee #2

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The manuscript describes the validation of the IASI methane retrieval product from product from the National Oceanic and Atmospheric Administration (NOAA). Retrievals have been validated against aircraft profile measurements from the HIPPO campaigns, which are well suited for application to validation of satellite measurements. The material is within the scope of this journal. The data is new, and should be of interest to the atmospheric community. The approach is clearly described and the results are sufficient to support the conclusions. In my opinion, the paper is suitable for publication in AMT after minor revisions.

General comments:

On balance, the paper reads well, but there are some important areas where the analysis and discussion are lacking.

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The manuscript is rather vague when it comes to the discussion of systematic errors. I would like to see more discussion of this. The authors state that channels were selected to be sensitive to CH₄ but not to N₂O or HNO₃. No mention is made anywhere in the paper that the spectral region used for this CH₄ retrieval is strongly affected by water vapor interference. (There is no way to avoid that.) Errors in the water vapor (and the temperature) that were retrieved in previous steps can propagate through to the CH₄ retrieval. The impact of temperature and water vapor errors ought to be estimated.

The discussion of the empirical bias correction to the radiances is lacking in information. Are the correction coefficients available anywhere? Is the bias correction really applied to the radiances and not to the absorption coefficients in the RTA? How can the same radiance correction be applied to AIRS and IASI, when the two instruments have different spectral resolution? This is an important factor in understanding the observed bias.

Users of the data will wish to know whether the bias with respect to aircraft data is constant, or whether it varies in latitude and/or time. This is not discussed in the paper. It should be.

This is not so much a comment on the manuscript, but on the data products themselves. The authors state that the averaging kernels are not supplied with the data products. It will be extremely difficult for the user community to make proper use of this product without the averaging kernels. I hope that this group will consider supplying the averaging kernels with these NOAA CLASS products.

Specific comments:

The list of references (page 2504, lines 14-15) for GOSAT CH₄ retrievals ought to be expanded/updated. For example, the existing reference to the retrievals from the Japanese group is for an AGU abstract from 2008. This should at least be updated to the following: T. Yokota, Y. Yoshida, N. Eguchi, Y. Ota, T. Tanaka, H. Watanabe,

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and S. Maksyutov, “Global concentrations of CO₂ and CH₄ retrieved from GOSAT: first preliminary results,” *Sci. Online Lett. Atmos.* 5, 160–163 (2009) I would also suggest including a reference to the work of the group at the University of Leicester: Parker, R., H. Boesch, A. Cougan, A. Fraser, L. Feng, P. I. Palmer, J. Messerschmidt, N. Deutscher, D. W. T. Griffith, J. Notholt, P. O. Wennnberg and D. Wunch, Methane observations from the Greenhouse Gases Observing SATellite: Comparison to ground-based TCCON data and model calculations, *Geophys. Res. Lett.*, vol. 38, L15807, doi: 10.1029/2011GL047871, 2011 Page 2507, line 24: “near 7.66 microns” – previous references to spectral region have used wavenumber units. It would be helpful for the reader to follow if either microns or cm⁻¹ were used consistently (or the numbers in the alternative units are provided in brackets).

Figure 2 needs improvement. The level index for the 101 forward model levels is not helpful for the general reader, and in my opinion, does not add value to this figure. It should be removed. The authors might consider using a larger number of colors instead of using the dashed lines. Also, the numbers/text on the right of the figure overlap in places. This should be fixed (or these numbers/text should perhaps simply be removed altogether).

In the discussion of Fig. 5 (page 2508), the reference to such wide layers when referring to “peak sensitivity” is misleading. The text implies that the retrieval sensitivity has a very wide peak. In fact, Fig. 5 shows that the peak sensitivity is somewhere around 250 to 300 hPa in the tropics, not 100 to 600 as stated in the text. The retrieval sensitivity at 600 hPa is in fact quite low, and a statement that 600 hPa has any relation to the peak is inaccurate. The same comment applies to the description of “peak sensitivity” at other latitudes.

In Figure 6, there are some isolated dots at the top of the figure that do not look like places where the aircraft flew. Are these supposed to be there?

Figure 7 needs improvement. The legend should be positioned so that the text does not

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spill over the plot axes. Also, I see that an explanation of the blue profiles is provided in the caption, but it is a little confusing that there is nothing on the legend to explain what the blue profiles are. Is it really necessary to show the profiles that did not pass quality control? I would suggest removing them.

Figure 9(a) could be improved by adding a zero line.

Page 2512: “A larger retrieval bias than the first guess. . .” Are the authors referring to the upper atmosphere in Figure 9? If so, please make this clear.

Page 2513, lines 5-6: The authors state that the error resulting from the time difference between the IASI and aircraft measurement is expected to be small. Wecht et al. [2012], in their validation of TES CH₄ against HIPPO, actually examined this in detail. The authors could reference that study here.

Discussion and summary: “To help users utilize this product appropriately. . .” What would really help users utilize this product appropriately would be to supply the averaging kernels as part of the product.

In the discussion of possible reasons for the observed bias, the authors do not mention possible errors in the temperature or water vapor profiles. Those also could have a strong effect on the retrieved methane. The channels used in the CH₄ retrieval are strongly affected by interference from water vapor.

Technical corrections:

Page 2502, line 8: “The degree of freedom of” should be “The number of degrees of freedom for” Page 2502, line 9: “The most sensitivity layer is between. . .” should read, “The retrievals show greatest sensitivity between. . .” Page 2504, lines 16-17: The Payan et al. paper should be dated 2009, not 2007 Page 2505, line 8: “detail” should be “detailed” Page 2505, line 13 states “a nadir resolution of roughly 50 by 50 km” while page 2506, lines 12-13 states “a nadir spatial resolution of about 45 km”. This seems inconsistent. Page 2506, lines 4-5: “can be referred to” should read “can

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be found in” Page 2506, line 23: “triangle” should be plural (triangles). Page 2508, line 11: “can be referred to” should read “can be found in” Page 2508, line 18: “HNN” has not been spelled out anywhere in the manuscript. Page 2508, line 20: “Fig. 5 plots. . .” should read “Fig. 5 shows” Page 2509, line 14: “FOR” has not been spelled out anywhere in the manuscript. Page 2510, line 1: “NSF” has not been spelled out anywhere in the manuscript. Page 2514, line 9: “The reasons for this negative bias might be due to..” is grammatically incorrect. I suggest using “Possible reasons for the negative bias may include. . .”

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