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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new insight on tropospheric methane in the Tropics – first year from IASI hyperspectral infrared observations" *by* C. Crevoisier et al.

Anonymous Referee #2

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This paper presents methane concentration distributions in the middle-to-upper tropical troposphere, acquired by the thermal infrared IASI sounder during 16 months of MetOp operations. Only channels in the 7.7 microns band were used for the analyses.

The authors have followed a systematic and very sound methodology to reach a high level of accuracy on the retrieved products (CH4 integrated columns in the middle troposphere to better than 1%), which is in turn needed to assess the role of the key climate and chemistry species in the environment. The results are discussed with focus on seasonality and geographic distributions in the tropics and are compared, as best as could, to correlative measurements (ground- and air-borne) as well as to model

distributions. These comparisons point to the good performances of IASI to capture the main patterns of methane variability in the tropics: these include a marked North-to-South gradient, a well-defined seasonal cycle in the Northern tropics, and several patterns of vertical and horizontal transport close to known source regions. Evidence of a renewed increase in the CH4 mixing ratio over the last years, as suggested in other publications, seems to be captured by IASI as well.

Overall this paper gives an excellent demonstration of the capabilities of TIR sounder to monitor methane on extended spatial and temporal scales and the results provide robust landmarks for what will be achievable on the longer term with IASI and follow-on missions. The paper is a strong contribution to the IASI special issue and no doubt deserves publication in ACP, provided the following comments are addressed.

GENERAL comments:

Although the methodology is well described and overall very convincing, some aspects are not sufficiently detailed for the reader to judge on the limitations of IASI methane retrievals. For instance, only the tropics are analyzed here, with the justification of more homogeneous temperature distributions. Whether the measurements could be extended towards the higher latitudes or not (the authors show that the correlation with temperature is properly removed by using AMSU data) is unclear. Similarly, the retrievals were only performed over sea while the CH4 channels were selected to be insensitive to surface properties. Finally, only night-time measurements were used and the reason for not using the daytime data is not addressed. The authors should at least provide indications (for example in the conclusion section) on the limitation of their approach and whether they expect it to be suitable for larger scale analyses.

The correlation between methane and other parameters and more particularly temperature and water vapor is obviously a critical aspect. The use of AMSU channels to remove the dependence with temperature seems to provide excellent results and the selection of nine specific channels for methane insensitive to other parameters is well

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explained (Figure 1). Still I am surprised in Figure1 by the weak dependence upon water vapor in the selected channels in the highly perturbed region around 1300 cm-1. Obviously interfering water lines can be avoided by a suitable selection of CH4 channels but I would have expected some dependency with the water continuum. Has the continuum been taken into account for Figure 1? Is it a negligible contributor to the uncertainty in the methane retrievals? Do we expect some impact of it when analyzing spatial and temporal trends?

SPECIFIC comments:

Abstract, page 6856, line 7: IASI spectral resolution is high for a meteorological sounder but rather medium for an instrument dedicated to chemistry. “very” could be avoided here and similarly on Page 6858, line 18.

Page 6858, line10: IMG operated for several months but only intermittently. “few operation periods of successive days” may be more suitable than “a few months of observation”

Page 6859, line 20: There are 30 scan positions making up a total of 120 measurements. This should be added and also it should be specified if all these individual measurements were analyzed or if only a 4-pixel average was considered. The coincidence between IASI and AMSU could also be specified.

Page 6860, line 4: The “estimated” accuracies referred here are more the “targeted” accuracies. This should be changed. Alternatively the authors could provide an estimate of the current performances for temperature and humidity retrievals if these are available.

Page 6863, lines 9 through 11: Although it can be understood, this sentence is not well formulated. What does a “decrease in the emission by the surface” really mean?

Page 6863, line 15: This statement is very straight. It is certainly true for the selected sets of channels and likely holds for all other possible selection of channels in the 7.7

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micron bands in the chosen conditions (sea, night). But is the sensitivity the same for, e.g. surfaces during daytime? And also the potential of using the 3.8 micron channels under favorable conditions (high reflectivity during daytime), which is more sensitive to the surface (see Razavi et al., this issue) has not yet been firmly assessed.

Page 6867, lines 11 through 14: On the seasonal cycles. Low values seem to be observed by IASI in the Northern tropics from mid-March to August without much variations, which is somewhat different than what is mentioned in the text (decrease in March with minimum in July –August)

Page 6867, line 25: "A peak of 10 ppbv is observed. . .". It is unclear what this peak refers to. One would assume this is with respect to the average values but these are nowhere mentioned in the text (and by the way these average values would be very welcome to help quantifying the North-South gradient mentioned page 6868 lines 8-9). In my opinion what is striking in Figure 4 is not only the 10 ppbv peak in January but also the low values on both sides (October and April), which seem to be fully consistent at Southern latitudes. With respect to those the increase in January is closer to 40 ppbv, which is similar to the Northern hemisphere amplitude values. Is there a reason for the low October and April values?

Page 6868, lines 15-16: The 30 ppbv difference is here for the April-September period, which corresponds to the lowest values at northern tropical latitudes. Does that point to a lower bound for the North-South gradient?

Page 6868, lines 17-19: Are the different amplitude between the middle tropospheric and surface gradients explainable by the proximity of sources and the vertical mixing?

Page 6869 lines 24 through page 6870 line 5: The positive bias between the IASI observations and the models is explained by two possible causes: larger tropical emissions than assumed in the models or too weak convection in the latter. Is one more likely than the other? This may be an important result for future analyzes.

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Page 6870, line 10 through 15: I find it hard to distinguish the plume of CH4 from central-Northern South America (August to September) from other large values at similar latitudes (being even larger for other months). Is this sufficient evidence to make the link with the tropical emission patterns suggested from SCIAMACHY measurements?

Page 6871, lines 20-21: I could not find how the values of sigma-M and sigma-V were derived.

TECHNICAL corrections:

Table 1: the omega symbol is chosen here for wavenumber whereas it is nu everywhere else in the manuscript.

BT or TB are alternatively used for brightness temperatures: e;g. axis titles in Figures 1 and 2 eq 1... (TB), different from text (BT).

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6855, 2009.