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Atmospheric Measurement Techniques Discussions

Interactive comment on "Methane vertical profiles over the Indian subcontinent derived from the GOSAT/TANSO-FTS thermal infrared sensor" *by* Dmitry A. Belikov et al.

Anonymous Referee #1

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Review of the manuscript "Methane vertical profiles over. . ." by Belikov et al. submitted to the journal Atmospheric Measurement Techniques

The article "Methane vertical profiles over. . ." by Belikov et al. presents the vertical distribution of methane (CH4) retrieved from the GOSAT TIR measurements focussing on the Indian subcontinent over the period 2009-2014. Coupled to a model (MIROC4), the seasonal variations of CH4 in the lower, middle and upper troposphere are analysed in terms of transport and sources. The sensitivity of the GOSAT CH4 retrievals into the lowermost troposphere is discussed in order to quantify a surface emission of CH4 over India of 51.2 +/- 1.6 Tg yr-1 in 2009-2014.

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Discussion paper

It is a potentially very interesting paper that may help in the quantification of the CH4 flux over a key source area, namely India and its surroundings. Nevertheless, the article has too many weaknesses to properly address this fundamental issue. In my opinion, the manuscript has a too broad (not precise and/or not rigorous enough) approach of the scientific and technical issues related to CH4 in the observation and in the modelling aspects. These aspects will be dealt in detail below but major ones are: 1) a title that is too vague but consistent with the content of the manuscript, in other words the paper is a mixture of observations, validation, modelling and process studies, but none of them are carefully addressed; 2) the lowermost tropospheric sensitivity of the CH4 observations in the TIR is not satisfactorily addressed/proved since it is too much impacted by the dynamical a priori information used; 3) the 2 model outputs in the lowermost troposphere are compared to observations of GOSAT without using averaging kernels (Fig. 13), consequently no quantitative information can be derived on the CH4 emissions from this. I would propose to the authors to focus on one important aspect of their study, namely the CH4 emissions over India, and to clearly show that GOSAT TIR observations can deal with that. Unfortunately, for all these reasons, I cannot suggest to major revise the manuscript but rather to reject it, and to resubmit a version focussing on one single aspect of their analyses.

MAIN POINTS

1) TITLE & CONTENT

The title, very consistent with the content of the study, is too vague to properly address the rationale and the scientific outcomes of the study. Although the geographical extent of the project is clearly presented, the analysis shows CH4 fields analysed: 1) in three different layers from the lowermost to the uppermost troposphere (800, 500 and 200 hPa), 2) over different time periods: from 2009 to 2014, but focussing on 2011 (with no explanation to why focussing on this particular year) and highlighting climatological fields over 1981-2010 and 2009-2014 underlining the lack of consistency in the data analyses used in the overall study (Figure 2), and 3) combined with 2 model outputs

that are not clearly presented in terms of differences of processes impacting CH4 emissions (section 3.1). This wide range of studies are not rigorously addressed and, by the end of the article, the conclusions are not supported by the presented analyses.

2) LOWERMOST TROPOSPHERE

The analyses of CH4 in the lowermost troposphere are probably the most interesting results presented in the manuscript. Unfortunately, the authors fail to convincingly show that the GOSAT TIR CH4 observations are actually sensitive to 800 hPa. One of the main reasons is the use of an a priori information that is issued from a model, that is to say, that is dynamically evolving in time and space. As a consequence, the CH4 retrievals (whatever the layers considered) are contaminated by this dynamical a priori. Figure 3 left shows typically the obvious relationship between retrieved CH4 and a priori CH4 around 800 hPa (no differences) that is also shown in Figure 3 right where the averaging kernels are peaking at 300 hPa and, for few of them, very difficult to examine since they are labelled in levels and not in pressure, around 500 hPa. Maps of CH4 in the lowermost troposphere at 800 hPa (Figures 4 and 5) also show the strong a priori contamination to the GOSAT CH4 observations over India with almost similar fields in GOSAT and in the a priori CH4, that is not the case at 500 and 200 hPa. The vertical distribution of CH4 (Figures 9 and 10) also highlights this point between GOSAT, a priori and the 2 model outputs at 800 hPa. I would suggest to particularly focus on this issue and carefully show to the reader that GOSAT TIR can actually observe in this layer.

3) CH4 EMISSIONS

The two CH4 emissions used in the article will need to be properly presented and explained in what and why they were employed for the study. Differences in the modelled CH4 fields are clearly shown although some Figures will need to be better presented as for instance Figure 12 that does not present the period over which the data are highlighted. The most important issue in my opinion comes from Figure 13 for which

the modelled CH4 (ACTMCao and ACTMWH) are not convolved with the a priori information using equation (1), that is to say, it is impossible to quantify (as the authors do) the South Asian regional CH4 emission simply based on this Figure since models and observations are not in same space. A more rigorous comparisons will need to be done in order to infer such an important value.

Minor Points

I will not list in detail all the minor points that will need to be improved but only the key ones for a future version.

1) In general, please quantify (absolute and relative values) when making comparisons (differences for instance) and avoid terms as “good agreement”. This happens several times in the manuscript.

2) Clearly present which version of the GOSAT CH4 retrieval you have used, whether this version has been validated or not and, above all, if the validation has also focussed on the lowermost troposphere (800 hPa).

3) Explain whether you have used daytime or night-time or both CH4 retrievals since this may impact on all of your results, particularly in the lowermost troposphere.

4) Prophet should be presented in the section “Method” and we should understand why you have used this model. Prior to show the results, you should clearly highlight your methodology to achieve your scientific goal(s).

5) Why did you use so many periods? If important for you, please explain.

6) The Degree of Freedom (DOF) of the CH4 TIR retrievals is “around 1” (L. 192), that is to say, you only have access (in theory) to a columnar information of CH4 from TIR. So, explain how you can still have a sensitivity in the lowermost troposphere with DOF=1.

7) The colour Table (yellow-red) showing the Number of observation points in Figures

4 and 5 does not really highlight the dynamical range over the Indian subcontinent. I would propose to modify it.

8) Some sentences are difficult to understand (English and/or science): L. 204, 243, 257, 260, 356, 366, 413.

9) In general, I would propose a section dedicated to the discussion of the results obtained at least to confront your results to those of other studies.

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