

# Methane profiles from GOSAT thermal infrared spectra

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## Abstract.

## 1 Introduction

### General

We would like to thank both reviewers for the constructive comments that aided us to improve our manuscript. In this document we provide our replies to the reviewer's comments. The original comments made by the reviewer are typeset in italic font. Following every comment we give our reply. We provide a new version of the manuscript but in our replies to the comments we provide line numbers, page numbers and figure numbers referring to the original version of the manuscript, if not stated differently.

### Response to Referee #1

#### 10 Minor Comment 1

*P1, L6: Make clear the MACC model profiles are scaled to match the TCCON observations. i.e. "scaled to the TCCON total column". The phrasing on P2, L30-33 is more clear.*

Changes: Wording of P1, L6 is now in line with phrasing on P2, L30-33.

#### Minor Comment 2

15 *P1, L20-21: Some wildfires are also "natural" sources of CH<sub>4</sub>, so this discussion should be reworded.*

Changes: Clear distinction is made between natural and anthropogenic sources (P1, L20-21).

#### Minor Comment 3

*P1, L25-26 and P2, L3: Please provide a few references for these statements – I agree with them, you just need references to support them in the text.*

20 Changes: Two references have been added to support both statements (P1, L25-26 and P2, L3).

#### **Minor Comment 4**

*P2, L4-9: Also include the Cross-track Infrared Sounder aboard the Suomi-NPP satellite.*

Changes: At the end of P2, L9, CRiS aboard Suomi-NPP has been added to the listed satellite instruments.

#### **Minor Comment 5**

- 5 *P2, L35: I don't think you need to note that the correction scheme helps here, as if it didn't you probably wouldn't be publishing it.*

Changes: P2, L35-P3, L2 have been re-worded.

As a stand-alone sentence the referee would be right. However, in the article this particular sentence is used as a bridge to the following line. To prevent confusion, the two sentences have been re-worded.

#### **10 Minor Comment 6**

*P3, L6-19: I would appreciate more discussion of GOSAT in the main text of the paper, specifically on the spectral resolution of the infrared band and on any known instrument or retrieval issues with the thermal infrared observations.*

Changes: We have added the spectral resolutions of the bands (P3, L11). Regarding instrumental issues, there is the concern that a small non-linearity is unaccounted for in the L1b version used in this study, which is also added to the text (P3, L18).

- 15 We are not aware of specific GOSAT retrieval issues. There is, however, a general observation of a (positive) bias in methane TIR retrievals that is not fully understood, as already mentioned in the text (P2, L13-22).

#### **Minor Comment 7**

*P4, L14: What version of the MT\_CKD continuum did you use? The reference discusses several different versions.*

Changes: Version MT\_CKD\_2.5 has been added (P4, L14)

#### **20 Minor Comment 8**

*P4, L20-22: Can you provide more details on how the actual line-by-line calculation is done?*

Changes: The line-by-line calculation follows the RemoTeC implementation and this remark has been added to the text with the proper reference (P4, L19).

#### **Minor Comment 9**

- 25 *P8, L10: This is not true as stated, as you have just shown in Figure 1 that the sensitivity of the methane retrieval varies quite a bit with altitude. I think you are trying to say that, after the averaging kernel of GOSAT is applied to the MACC/TCCON columns, they have similar sensitivity and thus can be compared? If so, that is not currently clear in the text.*

Changes: For clarity SWIR is now explicitly mentioned in P8, L8.

I believe there is a misunderstanding here. Figure 1 refers to TIR averaging kernels, but the statement (P8, L10) refers to SWIR retrievals and Figure 1 is therefore not relevant in this discussion (nor mentioned in the text). For the SWIR retrievals the statement is, to a very high degree, true. To avoid confusion SWIR is now explicitly mentioned in the text.

#### **Minor Comment 10**

5 *P9, L16-19: I'm not convinced that this cloud-clearing algorithm is sufficient for the type of validation study you are doing here. Did you make any independent checks to confirm that the cloud-filtered profiles were likely cloud free, say using independent observations from other bands for the daytime cases? How large of a cloud AOD can your procedure miss?*

No changes.

In this study we actually used the method proposed by the referee to check the cloud filter against the independent observa-  
10 tions from the SWIR bands (P20, L3-11) and the quality of the filter is summarised in Table 1 on P20.

The suggestion by the referee to identify the detection limit in terms of cloud optical depth, sounds reasonable and is actually a quantity provided by the SWIR filter. However, there are two reasons for not including this in the current study: First, the cloud optical depth from the SWIR filter is not a validated product, but merely a filter quantity, and secondly, the cloud optical depth alone is of limited use only since the vertical sensitivity of the TIR retrievals. For instance, a thick low cloud may be  
15 missed by the TIR filter, without impacting the retrievals because of the loss of sensitivity towards the surface.

We have shown that the performance of the TIR and SWIR filters are almost identical in terms of biases (Table 1, P20), but the spread is larger in case for the TIR cloud filter (Table 1, P20). Therefore we believe that this (admittedly crude) cloud filter does not introduce additional biases (which is the focus of the current study) and we propose to leave the text as is.

#### **Minor Comment 11**

20 *P9, L21: Is the chi-squared check considered part of the cloud filter?*

No changes.

Yes, the chi-squared check is indeed considered part of the cloud filter, which is in correspondence with the cloud filter from the SWIR bands. This test identifies the failure of the forward model to capture all spectral features of the observation. The forward model does not account for clouds, and a large chi-squared may therefore indicate the occurrence of clouds in the  
25 observation.

#### **Typos and Style Suggestions 1**

*P1, L18 and L20: I'd say "the year 1750" in both places, as the first time I read this I thought you were saying this was the pre-industrial concentration of CH4 in ppbv.*

Changes: As per suggestion (P1, L18 and L20).

## **Typos and Style Suggestions 2**

*P2, L24: Check the format of these references.*

Changes: The references are now correct (P2, L24).

## **Typos and Style Suggestions 3**

5 *P7, L18: typo in “A priori”*

Changes: As per suggestion (P7, L18).

## **Typos and Style Suggestions 4**

*P9, L12: I’m not sure what “we pertain to” means here, I think you mean something like “we focus on”*

Changes: As per suggestion (P9, L12).

10 **Typos and Style Suggestions 5**

*P11, L6: This is a bar chart, not a histogram.*

Changes: As per suggestion (P11, L6).

## **Response to V. Payne (Referee #2)**

### **Comment 1**

15 *In general: How good are the MACC CH4 profiles? Can the authors provide any references to model validation? Are the profile comparisons sensitive to uncertainties in the model representation of the stratosphere?*

Changes: A second verification study has been added to the text (P10, L1)

The quality of the MACC fields are being discussed in lines P9,L29-P10,L3. In these lines we refer to a study verifying that the model delivers methane fields within 1% uncertainty, albeit with different input data as used in the current setup  
20 (NOAA-ESRL as opposed to GOSAT-SWIR). For completeness, we have now added a second reference in which the MACC fields (based on SCIAMACHY data) are verified to be again within 1% with independent observations. We have estimated the uncertainty in the current setup to be 2%, which seems therefore to be on the safe side.

### **Comment 2**

*Abstract, lines 14-15: “This filter. . .is consistent with the cloud filter based on the GOSAT-SWIR measurements, despite the  
25 fact that the TIR-filter is less stringent”. I was not clear on what this means. When you say the filter is consistent, do you mean that the bias in the retrieved profile does not change according too which filter is used? Consider changing the wording to say that the bias (rather than the filter) is consistent?*

Changes: Abstract L14-15 have been adapted to clearly state that a) no additional biases are introduced by the TIR-filter (wrt. SWIR) and b) the acceptance rate of observations is higher for the TIR-filter but the uncertainty as well (wrt. SWIR)

### **Comment 3**

Page 2, lines 1-12: Please also list the Cross-track Infrared Sounder (CrIS). There is a CrIS flying on the Suomi-NPP satellite, 5 launched in 2011, and there will be follow-on instruments on the JPSS satellite series. I am not aware of a publication on CrIS CH4 retrievals to date, but there are definitely people working on those.

Changes: At the end of P2, L9, CRiS aboard Suomi-NPP has been added to the listed satellite instruments.

### **Comment 4**

Page 2, lines 23-29: This discussion of previous work is a little hard to follow and would benefit from some re-wording for 10 clarification of various points. Papers from Saitoh (2012) and Holl (2016) cannot both present “first results”. It would be helpful to clarify that the Saitoh (2012), Holl (2016) and Zhou (2016) papers all discuss GOSAT TIR results from the same algorithm, and that algorithm is different from the one that you are using here.

Changes: "First" has been omitted (P2, L24). The statements that these papers stem from the same algorithm and we use a different one, are included (P2, L30).

### **Comment 5**

(Degree of signal should be degrees of freedom for signal?) When you say that the degrees of freedom for signal are significantly 15 lower than 1, are you referring to the degrees of freedom for signal in that other algorithm? This was not totally clear from the text.

Changes: It should indeed be "degrees of freedom for signal" and has been adapted (P2, L26). Two other occurrences in the 20 text have been adapted as well (P2, L30 and P6, L3). We were indeed referring to the other algorithms and this has now been made explicit (P2, L26).

### **Comment 6**

Zhou et al. (2016) compare results from AIRS (not IASI) and GOSAT. The statement about “A prevalent bias. . .between 25 both satellite retrievals” is confusing. Since the Zhou et al. paper does not appear to include any independent validation measurements and deals only with a comparison between two satellite retrievals, I assume that you are referring purely to the difference between the retrievals, in which case, you should state clearly which one is biased high relative to the other. As an aside, a point that is not discussed in the Zhou et al. paper, but which has been referred to in AIRS papers and presentations (for example, in Xiong et al. [2008]) is that in those AIRS CH4 retrievals, absorption coefficients are tuned within the radiative transfer algorithm in order to produce better agreement with validation data (a different form of correction). Xiong, X., C.

Barnet, E. Maddy, C. Sweeney, X. Liu, L. Zhou, and M. Goldberg (2008), *Characterization and validation of methane products from the Atmospheric Infrared Sounder (AIRS)*, *J. Geophys. Res.*, 113, G00A01, doi:10.1029/2007JG000500.

Changes: The confusion between IASI and AIRS has been cleared (P2, L28). It is now explicitly stated that AIRS is biased high wrt. GOAST (P2, L27-29). The bias correction approach in the AIRS retrievals has been added to the discussion  
5 (P2, L14).

#### **Comment 7**

*Page 2, lines 34-36: I think it would be good to refer here to the use of a similar correction approach using empirical orthogonal functions within the OCO-2 Level 2 algorithm. To my knowledge, the OCO-2 approach is not discussed in any journal papers to date, but you can find discussion of the use of empirical orthogonal functions in the OCO-2 Algorithm Theoretical Baseline Document (ATBD), available at: [https://docserver.gesdisc.eosdis.nasa.gov/public/project/OCO/OCO2\\_L2\\_ATBD.V6.pdf](https://docserver.gesdisc.eosdis.nasa.gov/public/project/OCO/OCO2_L2_ATBD.V6.pdf)*  
10

Changes: We agree that the approach by the OCO-2 team is very similar and we have added this to the text (P3, L2), including the reference.

#### **Comment 8**

*Page 3, line 27: It is not clear to me what is meant by “an effective H2O column to calculate the water continuum independently from the water vapor absorption lines.” Can you please expand on this point?*  
15

Changes: P3, L27 has been re-worded.

It turned out that a single water retrieval parameter was not sufficient to capture the spectral features of both the water vapour absorption lines and the continuum contribution. Therefore, two parameters are in the state vector incorporated to account for both contributions respectively. The text has been expanded to make this point clear.

#### **Comment 9**

*Page 6, Fig 1: Please label the altitude axis on the right hand side of the figure.*

Changes: Fig 1 (P6) has been changed. For similar reasons Figs 2 (P8), 5 (P12), and 6 (P13) have been changed as well.

#### **Comment 10**

*Page 7, line 1: Suggest removing the word “reduced”.*

25 Changes: As per suggestion (P7, L1).

#### **Comment 11**

*Page 7, lines 18-19: “the fact that the null space contribution of the integrated methane column is typically in the order of 30 %”. Did you show this somewhere? Please elaborate.*

Changes: An explicit explanation has been added to the text (P7, L18) with a reference to Figure 2 (P8).

## **Comment 12**

*Page 9, line 7: Suggest replacing “cloud clearing” with “cloud screening”, since the term cloud clearing has a particular meaning to some members in the TIR sounding community (Susskind et al., 2003). Susskind et al., IEEE TRANSACTIONS ON GEOSCIENCE AND REMOTE SENSING, VOL. 41, NO. 2, FEBRUARY 2003*

5 Changes: As per suggestion (P9, L17). An other occurrence has been updated as well (P20, L5).

## **Comment 13**

*Page 10, line 9: "Indicatory" is not a word that would be commonly used. Suggest saying instead that the difference is representative.*

Changes: As per suggestion (P10, L9).

## 10 **Comment 14**

*Page 11, Figure 4: Why choose this order for the TCCON stations? Consider arranging them by latitude.*

No changes.

It is the processing order and we did not put much thought into the arrangement of the figures. Rearranging them would only make sense when the order is rearranged in all figures, charts and tables. However, this needs to be done manually and is therefore error prone. We would like to request if keeping the current, admittedly peculiar, order is acceptable, as it will not lead to different conclusions or insights.

## **Grammar/typographical errors 1**

*Page 3, line 14: Suggest splitting the points about the 10 km footprint and the sparse spatial sampling into two separate sentences for clarity.*

20 Changes: As per suggestion (P3, L12-14).

## **Grammar/typographical errors 2**

*Page 3, line 15: Coarse should be course.*

Changes: As per suggestion (P3, L15).

## **Grammar/typographical errors 3**

25 *Page 3, line 16: Should this be v160160?*

No changes.

During the reprocessing of the data under v160160, a small update was incorporated leading to v161160. It was decided to only process the remaining data under v161160 and keep the already processed data under v160160. Therefore the version is generally indicated as v16x160.

## **Grammar/typographical errors 4**

*Page 2, line 15: Tropospherical should be tropospheric.*

Changes: As per suggestion (P2, L15).

## **2 Conclusions**

## References