Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-281-RC2, 2020

Atmospheric Measurement Techniques Discussions

Interactive comment on "Methane retrieved from TROPOMI: improvement of the data product and validation of the first two years of measurements" *by* Alba Lorente et al.

Anonymous Referee #2

Received and published: 8 October 2020

Review of "Methane retrieved from TROPOMI: improvement of the data product and validation of the first two years of measurements" – Alba Lorente et al.

General comments I found this paper to be of good scientific significance since it details the progress of the RemoTeC full-physics algorithm for TROPOMI data. This includes the use of a new bias correction routine to account for albedo biases which is independent of other sources of validation data; as well as the evaluation of spectroscopic databases and comparisons of the data to both TCCON and GOSAT data. The authors also evaluate biases which they see at higher latitudes over potentially snowy scenes. The new bias corrected TROPOMI data that they present is a good improvement over

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the previous data without the correction. The scientific quality of the data is good, using valid methods and approaches that are discussed and analysed appropriately and with appropriate references. The presentation is very good, the English is very good, the figures and tables are clear and understandable. Overall I recommend the manuscript to be accepted for publication in AMT after the authors have address my comments below.

Specific comments 1. Page 4, line 5. How do you determine the position of the 12 pressure layers? Are they fixed for every scene or are they calculated with respect to the surface pressure or tropopause height? If the location of the tropopause isn't accounted for in the construction of vertical layers, have you considered the uncertainty this could cause in calculating the total column XCH4, compared to a method which aims to put a pressure layer boundary at the tropopause height?

2. Page 4, line 12. Could you please be more specific in which ECMWF data you are using. Is it ERA-5 or IRA-Interim for example.

3. Page 4, line 21. How well do the results of the back-up filter compare to the VIIRS cloud filter data when you try to use it for scenes where you do have VIIRS to validate it? How many scenes in total for your two years of data use the VIIRS cloud clearing method, and how many use the back-up H2O retrieval method?

4. Page 4, lines 24-26. I am a little confused by how you cite a paper from 2019 (Hasekamp et al. 2019) to say that results of version 1.2.0 from June 2020 of your algorithm largely comply with mission requirements. Please could you elaborate on this.

5. Page 4, line 27. You call this new version the beta version here, but do not refer to this again. However, on page 6, line 20 you say the updates to the algorithm correspond to v1.3.0. Is there a difference between this beta version and 1.3.0? If not then it might be clearer to call it v 1.3.0 here on page 4.

6. Page 5, line 8. Could you please comment on why you chose to use daily averaged TCCON instead of averaging only data which is within a shorter time frame. I understand that TROPOMI has 14 orbits in one day so I would assume it likely that more than one orbit may intersect the 600km diameter co-location criteria. Do you think there is merit in being stricter in your temporal co-location as a result so you are only matching TCCON at a similar time to an overpass?

7. Page 6, line 4. I think it's potentially misleading to say that the GOSAT swath is 790km with a 10.5km resolution without saying that its measurement method is different to TROPOMI's and that it usually only makes 3 of those 10.5km measurements across its swath. I think an additional sentence here on the sampling pattern of GOSAT would be helpful.

8. Page 6, line 16. You use a full-physics method for TROPOMI because the proxy method cannot be applied, and go on to say that the full-physics and proxy methods were found to perform similarly for GOSAT. What you don't explain in the paper is why you don't use the GOSAT full-physics data as this feels like a more natural comparison. Could you please comment on why you used gosat proxy over gosat full-physics?

9. On Section 3.1. It makes sense to me that using a constant gamma reduces the overall dispersion of the data, improving your results. But please can you comment on the theory behind why you think calculating gamma per iteration should result in a less accurate result than using an average value.

10. Page 7, line 12. The reduction of 9% going from 19.7 ppb to 24.5 ppb doesn't make sense to me since it's becoming larger instead of reducing and the difference between these numbers is larger than 9%.

11. Page 7, line 25. In your section on the TCCON validation you say that the overall bias with respect to HITRAN 2008 is +15.5 for HITRAN 2016. Table 2 shows that the difference between HITRAN 2008 and HITRAN 2016 is 20.3 ppb for TCCON.

12. Page 7, line 31. Please could you give the global numbers as referred to here which show that SEOM-IAS has a significantly improved RMS and chi-squared over the other two.

13. On section 3.3. I like the discussion on the differences of greater than 45m and 50m, but in figure 2 there are a lot of smaller systematic differences of 10-20m to be seen in the Eastern US which lead to a net positive XCH4 difference over this half of the country. Firstly, please could you comment on why you think the higher resolution DEM would be on average higher elevation than the lower-res DEM over this region. And following on, could you please comment on the change to XCH4 overall as a result of any mean altitude difference between DEMs on a global scale (if one exists). I ask since you only focus on outliers between the DEMs in the paper and don't talk about any systematic differences.

14. On the Small Area Analysis. Could you please elaborate on how and why you chose the areas which you did. How dependent on your method is the choice of SAAs.

15. On the bias correction method, page 10, line 1. You only apply a bias correction on the surface albedo and say that the other retrieved parameters show negligible dependence, showing surface albedo, AOD and SZA. For OCO-2 the parameter dP (the difference between the a priori and retrieved surface pressure) shows the largest dependence on the bias. Is this a parameter you have looked in to?

16. Page 16. Lines 5-6. Have you tried comparing with snow cover data to verify how reliable this method of detecting snow actually is?

Technical corrections âĂć Page 5, Table 1. Caption missing versions for the instrument. âĂć Page 10, line 7. Typo with full stop. I assume you wanted a capital T or a semi-colon. âĂć There are multiple instances throughout the paper where you've misspelt ppb as pbb. Such as Page 7 line 25, page 9 line 8, page 20 line 16 and twice on page 14 line 4.

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