

### **Comments on Authors reply/changes to the manuscript: amt-2019-170 for the Editor.**

Yirdaw berhe, et al., have resubmitted their manuscript comparing ground-based MIR-FTS measurements of atmospheric CH<sub>4</sub> and N<sub>2</sub>O at Addis Ababa, Ethiopia to that of three satellite (MIPAS, MLS and AIRS) data products.

I have received and read the authors reply/rebuttals (amt-2019-170-author\_response-version2.pdf), along with the new supplement, amt-2019-170-supplement-version1.pdf. The authors supplied an updated/revised manuscript: amt-2019-170-manuscript-version5.pdf. The authors have made substantial changes to the manuscript to address reviewer's concerns.

Unfortunately, the revised manuscript presented at the end of the rebuttal document amt-2019-170-author\_response-version2.pdf (which has highlighted changes, thanks, very handy) and the revised manuscript amt-2019-170-manuscript-version5.pdf (with no changes highlighted) are different. For example, section 2.2 is different between these two manuscript versions. Which is the correct version? This makes reviewing difficult. All commentary below is based upon the document: amt-2019-170-manuscript-version5.pdf

Unfortunately (and not taken lightly), again, I cannot recommend the current altered manuscript (amt-2019-170-manuscript-version5.pdf) to be published. There are still too many spelling, grammatical and reference citation. Yirdaw berhe, et al. have addressed many of the initial reviewer's comments but I still find some critical replies/rebuttals are inadequate. Such inadequacies refer mainly to technical details, and once these are addressed (along with the mistakes) I would recommend publication. I do not see a major revision is required.

#### **Spelling, grammatical and reference citation mistakes:**

Throughout the revised manuscript amt-2019-170-manuscript-version5.pdf and the reply to authors document (amt-2019-170-author\_response-version2.pdf) there are numerous grammatical and spelling mistakes. The formatting of cited references still uses more than one type of style. I recommend the authors perform more intensive copy editing themselves or employ a copy-editing service.

#### **Technical concerns:**

1/ The figures in amt-2019-170-supplement-version1.pdf are of a low visual standard. Could the figures be replaced with those of a correct aspect-ratio?

2/ Retrieval strategy and quality:

The theory of the Tikhonov retrieval strategy is provided in the supplement and (starting) on pg. 5, L22 of the revised manuscript. The details supplied do not address the questions of the reviewers on this topic. It is stated at pg. 6, ln 1 in the revised manuscript "An optimised retrieval strategy for tropics has been established", but there are no details given on the method or results of the optimised strategy. For example, what is the value of the Alpha parameter and how was this value decided upon. What is the S<sub>e</sub> value used? Also, the retrieval strategy is also optimised for Addis Ababa, not the for tropics in general.

On pg. 6, ln 7 the manuscript states "The magnitude of the residuals indicates that measured spectra... of both CH<sub>4</sub> and N<sub>2</sub>O was quality as they are less than 1". Apart from grammatical mistake, the authors apply a QC/QA filtering limit of '1' to the residuals to assess retrieval 'quality'. There are no units of this value, or a description of the meaning of the value. Could the authors please explain in more detail the methods assessing retrieval quality.

On pg. 4, ln 25 the statement “The quality of the measurements during the time period of May 2009-February 2011 has revealed by Takele Kenea et al. (2013).” is out of place in this section and has no context. This statement should be at the end of section 3.1 and the authors should also give the readers more insight as to what the actual criteria are used to assess quality.

3/ I think the authors response and manuscript correction to the reviewer’s question below is inadequate.

My understanding is that measurements started in 2009 to present, is this correct? Why you use limited number of years? I highly encourage to include more years and if possible trend analysis. Do the trend make sense?

Response on P10, L8: The statement at P 10, L 8 has been deleted and taking the information to P 12, L16.

- The FTIR measurement was not functional from March, 2011 to November, 2012.
- I think the instrument is not functional starting 2015.

Response: We have added the following statement in Pg 12, L 16 to explain the period time considered during the intercomparison of FTIR CH<sub>4</sub> and N<sub>2</sub>O with MIPAS, MLS and AIRS. The quality of the FTIR CH<sub>4</sub> and N<sub>2</sub>O for a period that covers May 2009 to March 2013 is assessed through comparison with data from MIPAS (May 2009 to December 2010), MLS (May 2009 to March 2013) and AIRS (May 2009 to March 2013) sensors on board satellites.

The authors need to explicitly state why certain periods were excluded, i.e. the instrument was not functional between March 2011 and November 2012. Also, why comparisons halted at March 2013. Was the instrument not working from 2013 onwards? Or alternatively, why the period May 2009 to March 2013 was the period selected for satellite comparisons.

4/ Coincidence criteria.

The authors state “The more stringent latitudinal criterion has proven to be a good choice for all comparisons, since latitudinal variations are, in general, more pronounced than longitudinal ones” (pg. 12, ln 9). Could the authors explain why the chosen limits are more stringent (and compared to what?) and why they are a good choice.

5/WACCM citable reference: Pg. 4, ln 31. The WACCM model needs a citable reference. [http://www2.cesm.ucar.edu/working\\_groups/?ref=nav](http://www2.cesm.ucar.edu/working_groups/?ref=nav) does not refer to WACCM.

6/ pg.12 ln 18: “Hence, the profiles from MIPAS, MLS and AIRS have been degraded to make a comparison between the FTIR and satellite observations.”

I think degraded is the wrong word, ambiguous. Could the authors rewrite or explain the term ‘degraded’ in a better way.

7/ Partial column altitude range in section 5.4 and Fig 11. Caption

I infer the partial column range is 15-27km? is this correct? Could the authors please explicitly state the exact range in section 5.4 and fig 11 caption.