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

Hi there,

I have received and read the authors reply/rebuttals (amt-2019-170-AC1-supplement.pdf and amt-2019-170-AC2-supplement.pdf) to reviewer's comments on the manuscript amt-2019-170.pdf.

Unfortunately (and not taken lightly) I cannot recommend the current altered manuscript to be published. There are still too many mistakes. Reviewers comments were not adequately addressed.

Concerns:

-Many of the replies to both reviewer's comments and stated manuscript changes were incomplete with spelling mistakes (not a good start). For example: amt-2019-170-AC1-supplement.pdf P5, L1 reply: incomplete.

P5, L1: Why different versions of HITRAN were used. Please explain and also, what versions were used for gases
Response on P5, L1: As the referee suggested, we added sentences that description which versions of HITRAN data were used for gases. Different versions of HITRAN were used as the new updated HITRAN……………….
Required

-The authors were inconsistent in illustrating manuscript changes. Sometimes the authors stated the changes verbatim, along with page and line reference (as per protocol) but other times the reply only stated changes were made, not the actual change and location. This makes it hard to conceptualise all the changes to the manuscript or find out the actual changes.

S5/ Pg4, L28: Apriori is mentioned. Are the apriori profiles used static? i.e. unvarying, or are they changing seasonally, yearly, or daily? If the apriori is static, then how is it constructed, a mean over XX years? Is the apriori based on a certain global region?
Response on S5/pg 4, L28: It was static, mean of 40 years WACCM for tropics was used.

-Reasons were not given to some questions. Example below:

P5, L15. It is mentioned that the retrieval strategy is optimized using a single spectra: : : please expand this description, what do you use a criteria for optimization? Is it consistent for all months, zenith angles?.
Response on P5, L15: The optimization of the retrieval strategy starts from the selection of the micro windows which are somewhat different from the recommended by NDACC.

-There was a lack of adequate response to very important questions. Examples:

-How the Tikhonov regularization scheme was formulated and the requirement for extra Tikhonov details:

S8/ Pg 5, L15. The authors mention an "optimised retrieval strategy" but only give a passing mention to the Tikhonov retrieval regularization scheme. This is an important part of the retrieval; influencing overall information content and interlayer correlations of information content. Could the author please describe the Tikhonov regularization parameters. Why was the Tikhonov scheme implemented instead of using apriori uncertainties? What type of smoothing constraint is used (L1, L2 etc..), were the smoothing constraints normalised using layer thickness? what is the alpha

parameter used? and how was the alpha parameter selected? is the alpha parameter static? or varies per retrieval?
Response On s8/page 5, L15: detailed description of retrieval strategy has been added after L10.
Methane and nitrous oxide vertical profiles over Addis Ababa have been obtained by fitting five and four Micro windows respectively. The retrieved state vector contains the retrieved volume mixing ratios of the target gas defined in 41 layers of the tropical atmospheric conditions. The retrieved profiles were derived using a Tikhonov-Phillips method on a logarithmic scale.

-The reasons for different micro-window selection to that of standard NDACC IRWG practice was not given:

T38/ Pg, 5, L9. Modified Microwindows: why is this?
Response on T38, P5, L9: The reasons why we modify the micro windows are due to high residuals obtained between the measured and synthesized spectra at the Addis Ababa site. Thus, the micro-windows recommended by the NDACC might be useful for the other FTIR sites found in mid and high latitudes. The references from where the micro windows are adopted has been added at P5, L8.

-Addressing the concerns around profile comparisons when there is only ~2 DOFs:

Section 5.2. It is not explained why authors compare FTIR vs satellite vertical profiles. The FTIR information content is limited to 2 DOFs (tropospheric and stratospheric columns) but main figures for the comparison are shown as profiles.
Response, Section 5.2.: Since those results are the first for Addis Ababa.

-Investigating spatial-temporal co-location criteria on the measurement comparisons which could possibly affect comparison results:

P10, L8. Explained why only satellite data between March 2009 to Dec 2010 is used for MIPAS?, what about the other satellite measurements (it is not mentioned)? why this wide range is used. I would try other distances as well. How do you assess the spatial-temporal variability of both CH_4 and N_2O.
Response on P10, L8: We have added the statement to explain the period time used to validate FTIR with MLS.
The comparison of FTIR with MLS for a period time of May 2009 to February 2013 has also made. Here, in this paper, we did not put anything about the spatial-temporal variability of both CH_4 and N_2O.

-To a lesser extent, I also feel the authors have missed an opportunity to explain the aims of the study better, and the importance of the Addis Ababa site location and measurements made.

The authors state that the comparisons at the "Addis Ababa station is good to study tropical atmospheric processes" (Pg 19, L12). „Good" in what context? Given the comparison results, will the ground-based CH_4 and N_2O measurements capture seasonal cycles and multi-year trends? Will biomass burning or other episodic events most likely be seen, and from what part of the tropics (the tropic is a large place)?
Response on S2: Since this result is the first to the Addis Ababa site, it required validation to assess the quality of the ground-based FTIR measurements. The motivation is to fill the gap in understanding the atmosphere over Addis Ababa, tropical region of the globe. Moreover,

there are scores of measurements in tropical. The expression below has been added in section 2.1 to elaborate more about the measurement site.

Ethiopian is characterized by high rainfall and temperature variability on both spatial and temporal scales. The variability in distribution is related to altitude, latitude, humidity and winds, which are the significant factors in affecting the weather system of the country.

I am still willing to review/comment on any future manuscripts. It is a shame as the content and overall aim and methodology of the manuscript is robust and would be a welcome addition to the literature.