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Interactive comment on "Comparison of CO_2 from NOAA Carbon Tracker reanalysis model and satellites over Africa" *by* Anteneh Getachew Mengistu and Gizaw Mengistu Tsidu

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Authors' response to interactive comment on "Comparison of CO2 from NOAA Carbon Tracker reanalysis model and satellites over Africa" by Anonymous reviewer

We thank the anonymous reviewer for the valuable comments and time spent to provide the important comments and suggestions. They are enormously constructive and are used to improve the quality of the manuscript significantly. We will respond to all the comments in details as follows.

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Reviewer's comments: The authors evaluated two versions of the Carbon Tracker inversion system by comparing its posterior simulation with satellite XCO2 retrievals over the African continent. This regional focus is interesting but the paper is loosely written and often reads like a technical report. Throughout the text, there are many vague or awkward expressions that induce misleading or erroneous statements. There are both some small repetitions and some out of scope paragraphs (e.g., the parts about growth rate).

Response We thank the reviewer for realizing the importance of this work, in particular, the focus on regional aspect. The revised manuscript have been edited substantially following the reviewer's comment. However, it is difficult to pin-pointedly indicate where these changes are made since we edited the whole manuscript because of the fact that the reviewer made generic comments to improve the overall manuscript quality.

The reviewer indicated presence of many vague expressions. We hope that some of those might have been already taken care of in the process of substantial editing as indicated in the previous paragraph. In the case of the content on the growth rate, for instance, we would like to emphasis that it is important as it shows the degree of agreement between the model simulation and satellite observations at interannual time scale. In other words, one can also argue assessing whether model prediction of growth rate is comparable to that captured by satellite retrievals is important. This means that growth rate in our context is meant for comparison purpose rather than understanding dynamics/processes that led to a specific growth rate. Therefore, we believe that the paragraphs on growth rate is within the scope of the paper which is to assess how CT model simulation compares with satellite observations.

Reviewer's comment: The content of the cited papers does not always correspond to what is said of them in the citing paper.

Response We have checked all our references and determined whether they are properly cited for the conclusions/results included in the cited papers. We thank the reviewer for pointing out cases of inappropriate citations which are now removed. For example, on page 2, line 15, Deng et al. 2016 was cited as if it shows historical changes in XCO2 from pre-industrial period. This was wrong and it has been now replaced with two new references namely Tsutsumi et al. (2009) and Allison (2015). There was also few other cases that are also corrected.

Reviewer's comment: Retrievals are used and cited without any reference to specific versions while they may be quite different from one to the next (e.g., are the authors using v7 or v8 OCO-2 retrievals from NASA?). I argue that much effort is needed to check the content of each sentence and improve the general presentation, before the paper can be published. I am therefore skipping the numerous details in order to concentrate here on general issues

Response We thank the reviewer for pointing luck of specific information on the data version used in this study to us. We apologize for the oversight. Now, changes have been made in this particular case. Specifically, we want to indicate that the version of OCO-2 used is OCO-2 V7 lite level 2 products. This version of OCO-2 was compared to Carbon Tracker release CT2016 for 2015 and version CT-NRT.v2017 for 2016. Moreover, the version of GOSAT XCO2 used in the manuscript is ACOS B3.5 Lite. This information is incorporated in Section 2.2 and 2.3 of the revised manuscript.

Reviewer's comment: The introduction (p.2-3) is unnecessarily long and convoluted, and it poorly motivates the study. For instance, the paragraph about TCCON is disconnected from the rest. More importantly, there is a silent shift of meaning for "model" from "Earth system model" to "transport model" without any concern about consistency in the logic flow.

Response We have tried to shorten and add clarity to the two paragraphs mentioned by the reviewer's. Although the statement on the necessity of models to compliment observations is equally good for both kind of models namely complex 'Earth system model' and simple 'transport model' in the context of paragraph 2 (line 20-22), we

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rephrased it such that it now refers to transport model to avoid similar misunderstanding by general reader. We also amended paragraph 3 to improve the flow of ideas reflected in it.

Reviewer's comment: Some statistics are given without any reference to specific retrieval versions. The method (p. 5) does not mention the retrieval averaging kernels. The authors need to clearly state the fact that they have used them, or redo their study if they have not used them yet.

Response We appreciate the reviewer for reminding us about the averaging kernel. In fact, we have ignored the difference in the resolution between the model and satellite XCO2 observations in the study after simple inspection of model and satellite vertical grids. As a result we did not use averaging kernel to smooth CT XCO2. We have now smoothed CT XCO2 as per procedure described by Rodgers and Connor (2003) and then compared the smoothed XCO2 with the observations from satellites. This has led to significant changes improving the agreement between the model simulations and satellite observations leading to lower RMSD, bias, high correlation as well improvement in the categorical statistics. Therefore, the analysis in the manuscript is updated (see the revised manuscript). We would like to thank the reviewer for the valuable inputs. Now, Section 2.4 in the revised manuscript includes the procedure on how to smooth the high resolution data to low resolution data using averaging kernel of low resolution data, a priori profile and the weighting functions.

Reviewer's comment: Statistical quantities are not mathematically defined and there are many in this paper. For instance, the False Alarm Ratio (FAR) is simply defined by "identifies the fraction of events captured by simulation but not available in reference observation", but there is no obvious "event" for XCO2 (in contrast to rain for instance). An equation would explain what the authors mean, but at first glance FAR seems ill-suited for a continuous variable. From the values they find, the authors seem to be concerned by the fact that the model does not capture the higher end of the retrieval distribution, but what does it mean? The model could, e.g., slightly misplace fire plumes in time or space without affecting its overall realism. A simple scatter plot could efficiently replace the series of categorical indices that the authors use and simplify the message.

Response We have included equations that describes some of the known statistics such as RMSD, bias and pattern correlation including the categorical metrics although we felt that doing so does not add much value as there are ample references with details on these statistics cited in this manuscript. However, the reviewer has also questioned the importance and relevance of the categorical metrics. As correctly pointed by the reviewer, the scatter plots can provide qualitative information on the discrepancy between CT and satellites XCO2. However, we want to go beyond qualitative analysis such that we have some kind of metrics to assess the level of agreement between model and satellite observations in the extreme ends of XCO2 distribution. Indeed, it is true that XCO2 is a continuous variable whereas the categorical metrics apply to categorical variables. However, when we use quantiles as a threshold to determine whether the model predicts accurately the observed XCO2 lies above the given quantile threshold or not, we are not looking at the actual value. Instead, we are investigating whether the model and observations are in good agreement in indicating the correct category of the observed and model XCO2 i.e., higher or lower the quantile threshold. Details of the calculations are given in Section 2.4 of the revised manuscript. In this regard, we have also made some changes in the discussion of results to improve clarity of the manuscript.

Reviewer's comment: Also about statistics, it is not even clear whether they are computed over 5 years, or over 5 years and 3 months. Last, some error bars are put on monthly-mean and area-mean retrieval values in the figures. We may suppose that they are standard deviations (the legend does not mention them), but they seem to be way to large for that (only random errors are given in satellite products and by default they should decrease as 1/sqrt(n)).

Response We used 5 years and 3 months data in computing the statistics in the old manuscript, while comparing ACOS GOSAT B3.5 and CT2016 covering the period from April 2009 to June 2014. However, comparison on the seasonal basis may affect the statistics between seasons due to unequal number of data points within seasons due to the extra three months as correctly indicated by Dr. Baker, the second reviewer. Therefore, we have used only 5 years in the revised manuscript. The error bars over the spatial mean and monthly mean are the corresponding spatial mean and monthly mean XCO2 posterior error of satellite which is a combination of instrument noise, smoothing error and interference errors (Connor et al., 2008; O'Dell et al., 2012). Their value ranges from 0.74 to 0.88 ppm. Since the error bar does not include the complete systematic errors that arise, for example, due to forward radiative model error and Line of sight (pointing) error, our estimate is rather conservative. According to O'Dell et al., 2012 , the true error could be as large as twice the posteriori. Therefore, we have highlighted this drawback of the error bar used in this manuscript in Section 3.1 on page 10, lines 2-3.

Reviewer's comment: Table 1 is very intriguing. First, the focus area is made of 428 grid points and the data spans about 5 years, but the statistics are made over 750 data points only. Second, if we assume that GOSAT errors and CT errors are uncorrelated, we deduce a 1-σ error for CT XCO 2 over Africa of (3.47 2 -0.9 2) 0.5 = 3.4 ppm, marginally smaller than the variability of the retrieved XCO2 (σ = 4.3 ppm). Such a poor skill is hard to believe. By comparison, I downloaded the CAMSv17r1 data (http://apps.ecmwf.int/datasets/data/cams-ghg-inversions/) and compared it with the ACOS GOSAT v3.5 retrievals for the same period (5 years and 3 months) with the proper averaging kernels. For individual values, I find a model-minus-retrieval bias of -0.41 ppm (similar to what is shown in the paper if we except the sign, that is undefined in the table legend) and a standard deviation of 2.2 ppm, for a number of data points of 266,662. That makes a model uncertainty of (2.2 2 -0.9 2) 0.5 = 2 ppm, ie 35% less than for CT. If we account for the fact that the estimated retrieval precision may be wrong by a factor of ÌČ1.5 (see O' Dell et al 2012 for a previous ACOS release, http://dx.doi.org/10.5194/amt-5-99-2012), we find a model random uncertainty a bit better than the retrievals, as expected. The correlation also rises from 0.73 in the paper to 0.87. CAMS and CT are different products, but we do not expect such a difference in quality. Talking about CAMS, there was a comparison between MACC13r1 and ACOS GOSAT v3.5 a few years ago with some focus over Africa savannahs, that suggested deficiencies in the retrievals (in terms of systematic errors and in terms of averaging kernel shape) (Chevallier et al. 2015, http://dx.doi.org/10.5194/acp-15-11133-2015). If the authors use a more recent version, these artifacts may have disappeared, but this needs to be looked at. If the authors use the same version, this needs to be accounted for when using the retrievals as a reference.

Response We apologize for the oversight. The 750 indicated as being used in calculation was wrong. It is the initial number of grids that have coincident satellite observations when data over Africa is extracted from the global data set. However, our analysis focuses only on African landmass which reduces the 750 grids to 428. In addition, we excluded grids with less than 10 observations in the final analysis. This means that the actual analysis was based on 426 grids excluding 2 grids with less than 10 observations. Nevertheless, the actual analysis was not affected by the oversight and was based on the 426 grids in the old manuscript as well as in the revised manuscript. What was wrong is our erroneous reference to 750 and 428 in the different part of the manuscript. The statistics in section 3.1 of the paper were performed based the available time series data that passes the selection criteria as indicated above. The distribution at each pixel was indicated in the paper (Fig. 1d) and it was discussed in the manuscript. The aggregate number of data used in the statistics is 472,821 over 426 pixels covering the study period. We now also included the aggregate data points used in the revised manuscript in Table 1 caption of the revised manuscript.

As indicated in one of the previous responses, initially we have not used averaging kernel of GOSAT CO2 retrieval to smooth CT data. After applying smoothing using retrieval averaging kernel, the results of statistics shows a substantial improvement. The correlation was changed from 0.73 to 0.83 which is improved by 13%, and RMSD

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is changed from 3.4 to 2.3 ppm. Implying a model uncertainty of (2.32 – 0.92)1/2 = 2.12 ppm shows a 52% improvement. The bias is also changed from 0.43 to -0.28 ppm. The negative sign indicates that CT is lower than GOSAT by 0.28. Similarly, comparison between CT and OCO-2 shows improvement in bias and RMSD. The bias was changed from 0.93 to 0.34 and RMSD was changed from 3.77 to 2.57 ppm. These changes are quite significant and inline with what the reviewer expected. These changes are now reflected in various parts of the revised manuscript.

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3. Rodgers, C. D. and Connor, B. J.: Intercomparison of remote sounding instruments, Journal of Geophysical Research: Atmospheres, 108, 2003.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2018-84/amt-2018-84-AC1-supplement.pdf

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