RESPONSE TO THE REVIEWER #1

The manuscript "The consistency between observations (TCCON, surface measurements and satellites) and CO_2 models in reproducing global CO2 growth rate" from Labzovskii et al., submitted for publication in Atmos. Chem. Phys., presents and discusses atmospheric CO_2 growth rates from different observational data sets and CO_2 inverse models. While the topic is in principle important and appropriate for Atmos. Chem. Phys., I see several major shortcomings and cannot recommend publication – at least not without major modifications – as explained in the following.

Response 1.01: We thank the reviewer for the valuable suggestions based on which we have substantially improved the manuscript. Point-by-point responses are given below and marked by green color. Please note that the definition of GR, AGR and MGR terms are given in the article. We underline that our manuscript has undergone major modifications based on versatile comments/suggestions of the reviewers and also by the TCCON community (see their collective comment at the ACP page of this revision).

The authors frequently cite Buchwitz et al., 2018, which is a recent publication addressing essentially the same topic. In the Labzovskii et al. manuscript, the method of Buchwitz et al., 2018, is used to compute growth rates and also a similar analysis is presented. However, I find the presented analysis quite weak and it remains unclear if and if yes where this publications goes beyond the state-of-the art including the method, results and discussion as presented in Buchwitz et al., 2018.

Response 1.02: Due to prevalence of TCCON-oriented analysis and according to the comments from the reviewers, we reformulated the research aim and the objectives of the manuscript.

- New research aim is, to quote *"This study aims to understand whether TCCON aggregated observations are currently suitable for robust estimation of the global CO_2 growth rate given the existing spatio-temporal gaps of the network"*
- Updated objectives are "*(a) to estimate the robustness of AGR_{TCCON} due to the data sampling, measurement gaps or difference in time series across the sites"*. Secondly, (b) "*to examine the AGR_{TCCON} agreement with the existing CO_2 growth references and its sensitivity to external factors".* Additional objective includes *(c) "to assess the exposure of CO_2 growth estimates at each TCCON station to external factors"*

This publication does not present any new method since we use Buchwitz et al., (2018) as the methodological reference for AGR calculation. We made it more clear in the manuscript by stating *"More specifically, we use exactly the methodology of Buchwitz et al. (2018) whereas their method originates from the commonly-accepted GR calculation approach (Thoning et al., 1989)"* in section 2.2.1. Meanwhile the main new finding is that *"We showed that the current estimates of CO_2 annual growth obtained*

from the TCCON aggregated observations are adequate and are in reasonable agreement with the existing references even when the simple methodology (Thoning et al., 1989) applied." This main finding is mentioned in the conclusion where the new implications from the study findings are given in the second part of conclusions (after line 956).

Also the English needs to be significantly improved. I strongly recommend that the authors consult an expert to improve the English as in the current version there are several errors but often it is also not entirely clear what the authors mean. In the abstract the authors write: "This study is aimed to advance our knowledge about temporal and spatial variations of annual CO2 growth rate (AGR) by using CO2 observations from the Total Column Observing Network (TCCON), CO2 simulations from Carbon Tracker (CT) and Copernicus Atmospheric Monitoring System (CAMS) models being compared with the previously-reported global references of AGR from Global Carbon Budget (GCB) and satellite observations (SAT) for 2004-2019 years."

Response 1.03: We applied the suggestions for language corrections from the TCCON community collective feedback. As several TCCON PIs signed this document are native English speakers, we hope that the English language is significantly improved. The new experienced coauthors included in this study have hopefully improved the level of English language as well. If the level of English remains unsatisfactory at this stage, at the next stage of revision, we will either ask one of our native English-speaking coauthors or editing company for thorough grammar and stylistic check of the article.

From the methods used and results presented in the manuscript, I cannot see that the goal to advance our knowledge has been achieved.

Response 1.04: We agree that the previous study goal was too vaguely formulated (see Response 1.02 for new research aim and objectives).

In the Conclusions section the authors underline that they have primarily found three results: (i) different CO2 growth rate estimates are consistent, (ii) conclusions w.r.t. modelled and TCCON derived growth rates which I find a bit confusing and (iii) conclusions w.r.t. CO2 from biomass burning and fossil fuel emissions which – I think – are based on a very weak analysis.

Response 1.05: The conclusions are now based on three reformulated objectives.

- *(a) "to estimate the robustness of AGR_{TCCON} due to the data sampling, measurement gaps or difference in time series across the sites".* This objective is now supported by expanded evidences about daily, monthly, annual and seasonal stability of TCCON AGR estimates, objective
- *(b) "to examine the AGR_{TCCON} agreement with the existing CO_2 growth references and its sensitivity to external factors".*

- This is basically similar to what was stated for objective (i) from the reviewer's comment about but complemented with more results (Figure 7, Figure 9, Figure 10, Figure 11, Figure S.2.4).
- We agree that (iii) objective and conclusions were based on weak analysis. We reformulated this objective to be (c) "*to assess the exposure of CO_2 growth estimates at each TCCON station to external factors"*. We included an extended analysis of fluxes from CarbonTracker (Figure 14) and their relationship with the model disagreement. Moreover, we analyzed whether the model disagreement was driven by the vertical mixing (Figure 15 where model-to-surface CO_2 data is analyzed).

Concerning (i) I am not aware that inconsistencies had been previously identified as a major issue, which needs to be addressed.

Response 1.06: We agree that this point should not be the main factor driving the research from our study. We reformulated the main motivation of this study as *"The CO_2 growth rate (GR) is one of the key geophysical quantities reflecting the dynamics of the climate change but there are still few global observational approaches for quantifying global GR*" which is given in the abstract. However, some inconsistencies between different models in reproducing CO_2 growth rate do exist according to Gaubert et al., (2019) reference given in this study.

The authors use TCCON data (and this is acknowledged in the Acknowledgements section) but none of the TCCON PIs is a co-author. Have the TCCON PIs been contacted prior to submission of this publication? It would be good to get confirmation that the authors have respected the TCCON data policy (see https://tccondata.org/) and the data policy of the other data sets used in the manuscript.

Response 1.07: We did have TCCON PI even in the first version of the manuscript (Young-Suk Oh plus Taeyoung Goo from the Anmyeondo measurement site). Most importantly, the oversight with the TCCON policy compliance has been alleviated as during the revision we had been contacted by the TCCON co-chair, Asia/Oceania (Nicholas Deutscher). Under his assistance, we followed the TCCON Data License and policies and shared the material of the manuscript with all PIs of the TCCON network and those who were interested, provided their interactive comments. We have also added the missing references required by the TCCON policies to the current version. Moreover, we have offered coauthorship to the TCCON researchers responsible for composing TCCON collective feedback. However, so far all the TCCON researchers decided to keep providing comments without becoming coauthor for this manuscript.

In the following, I only highlight some aspects, which I think need to be improved. I could have added more exampled but perhaps this is already sufficient to help the authors to generate a significantly improved manuscript in the future.

Line 113: The authors write: “We present the main tools for retrieving CO2 atmospheric concentration, . . .”. This sounds as if the authors have generated atmospheric CO2 data sets but if I understand correctly, they have only used (and analysed) existing data sets. If this is the case than this needs to be clearly stated in Section 2.

Response 1.08. This formulation is removed. In Section 2 we state, before describing all datasets “*It should be noted that this study does not generate any new CO_2 data and relies on the referenced CO_2 observations or simulations from the existing sources.”*

Section 2.2: The description of the growth rate computation method is very short and Eq. (1) is unclear (e.g., what is index i / which months are used to compute the growth rate for a given year ?).

Response 1.09: Please see new, more detailed formulation based on Equations 3 and 4 from the Section 2.2.1. Regarding which months should be used, see the quote from the study *“In an ideal position, there would be 12 MGRs as an input for each TCCON station (so MGR is available for every month from January to December)”*.

If the method is (supposed to be) exactly the method of Buchwitz et al., 2018, than this needs to be clearly stated.

Response 1.10: We use exactly Buchwitz et al., (2018) calculation and we once again emphasized it by stating *“In this regard, we refer to the latest outcomes from the GR research, and we use exactly the methodology of Buchwitz et al. (2018)”* in Section 2.2.1.

Figure 2 and related discussion: I find this figure too busy and therefore a bit confusing. I strongly recommend to limit this figure to panel (a).

Response 1.11: Done

The ONI / ENSO part should be shown (if really needed for this publication) separately and later in the manuscript. As discussed in Buchwitz et al., 2018, there is a time lag between growth rate changes and ENSO and this important aspect is not appropriately considered here.

Response: 1.12 ONI/ENSO part is moved to other parts of the manuscript where we investigated the agreement between AGR and ENSO strength (Figure 9). We have also considered ± 1-year time lag for this analysis (also Figure 9). Moreover, we investigated the role of ONI on the disagreement rate between AGR_{TCCON} and the references (Figure 10, Figure 11).

A much more detailed presentation and discussion of the TCCON growth rates (shown in, e.g., Fig. 2) needs to be added: Please show detailed results for at least a few representative TCCON sites (XCO2 time series and derived growth rates).

Response 1.13: Please see the analysis of the few representative TCCON sites (Tsukuba, Park Falls, Garmisch) included in the station-wise analysis (Section 3.2.1). Additional analysis is given for Ascension measurement site in the supplementary material as well.

How do the growth rates for the different sites compare?

Response 1.14: We included a new section (3.2.2 with Figure 5) dedicated to this intercomparison.

How have the authors dealt with different time periods covered by the different sites ?

Response 1.15: We analyzed the role of daily, sub-monthly and annual variability of XCO_2 and their role on single-station AGR_{TCCON} estimates. See the second part of section 3.2.2 and the description provided for Table 1 where the data abundance statistics for each TCCON site are reported.

Much more details on the dependence of the used threshold needs to be added, e.g., it is unclear why 20 is the optimum threshold ? Why not 19 or 21 not shown in (quite sparse) Table 1 ?

Response 1.16: Please see extended analysis from Section 3.1 (Figure 2) where we analyzed all possible daily thresholds could be applied for XCO_2 daily data from TCCON station. The error spread due to the daily threshold is now reported on two figures (Figure 3, Figure 7). We agree that there is no optimum threshold for the current methodology. We applied '2' as the daily threshold because the agreement with the references is the same like for stringent thresholds but we have more data to analyze. To confirm, *"First of all, the change of daily threshold from '2' to higher values (3-25) does not lead to improved correlation of* AGR_{TCCON} *with either SAT (*$r \leqslant 0.60$ *for any threshold) or GCB (*$r \leqslant 0.56$ *for any threshold). At one hand, these findings confirm the correctness of our approach of saving as much data as possible by using daily threshold '2'. At other hand, it recognizes once again that the disagreement is not driven by data quality or sub-monthly structure."*

Figure 4: The correlation is often quite low, especially for TSU. Is it clear why this is the case ? Is this related to length of time series ?

Response 1.17: It is unlikely due to the time series length (see Figure 4 and the respective section 3.2.1). We found interesting pattern that is related to the vicinity to

large cities (see section 3.4.2, Table 2 and also Figures S.2.4 and S.2.5 in the supplementary for the additional information).