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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on “The consistency between observations (TCCON, surface measurements and satellites) and CO_2 models in reproducing global CO_2 growth rate” *by* Lev D. Labzovskii et al.

Anonymous Referee #1

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The manuscript “The consistency between observations (TCCON, surface measurements and satellites) and CO2 models in reproducing global CO2 growth rate” from Labzovskii et al., submitted for publication in Atmos. Chem. Phys., presents and discusses atmospheric CO2 growth rates from different observational data sets and CO2 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.

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

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Discussion paper

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.

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

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

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.

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.

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

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). 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.

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). How do the growth rates for the different sites compare ? How have the authors dealt with different time periods covered by the different sites ? 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 ?

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 ?

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