Atmospheric
Chemistry
and Physics
Discussions

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 #2

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General comments:

In their work "The consistency between observations (TCCON, surface measurements and CO2 models in reproducing global CO2 growth rate", submitted to ACP, the authors investigate the agreement of the atmospheric CO2 (annual) growth rates from several data sources: total column CO2 from the network of ground-based Fourier Transform Spectrometers (TCCON), inverse model estimates from Carbon-Tracker 2017 and CAMS, and reported growth rates from the Global Carbon Budget as well as total column CO2 from satellites. While quantifying the atmospheric CO2 growth rate using different data sources might be a topic suitable for ACP, the scientific

questions that this paper attempts to address are not sufficiently clearly defined. Thus, the research presented in the paper seems to somewhat suffer from this throughout the manuscript. The concepts, ideas, tools and data in the paper are not novel, and the methods used for data analysis are not entirely valid or sufficiently described. I consider that the paper should not be accepted for publication in its current form. A thorough revision starting from re-formulating the research questions would be necessary. In what follows, I will point out more specifically my concerns and comments on the manuscript, focusing more on the early sections as they naturally affect the rest of the manuscript. Because the comments are quite extensive, I will exceptionally not list technical corrections nor specific comments related to the English language in my review.

Specific comments:

Lines 64-89: The main message of this paragraph is unclear to the reader: are we interested in the global or local CO2 growth rate in this paper?

The authors start by highlighting the need for an accurate knowledge of the global CO2 growth rate; however, their analysis focuses on both local and global analysis without a clear focus or guidance to what essentially is important in the results and why the analysis has been made. On lines 97-104, the authors state the three objectives of their study. Regarding these objectives (a)-(c), (a) I don't think that aggregating all TCCON data would represent the global CO2 growth rate because of several reasons: even though the TCCON is in principle a (near-)global network, the instruments are mainly located in North America and Europe. In addition, several of them are affected by local sources (e.g., Paris, Tsukuba, Pasadena) that make them interesting for the evaluation of satellite CO2 retrievals in urban circumstances but maybe less representative of the global CO2 background for the purposes of this study. Regarding (b) and (c), the estimation of spatiotemporal inconsistencies between inverse models has already been carried out in a number of studies. It is not clear whether this study brings anything new to the discussion. In particular, I found the analysis regarding (c) a bit rushed and

shallow, and lacking important references to earlier studies that consider either these particular models or regions of interest (e.g., Lindqvist et al., 2015; Palmer et al., 2019).

The analysis focuses on the atmospheric CO2 growth rate and several times mentions a "permanent" growth of CO2, which may be misleading to readers, considering the seasonal cycle of CO2.

Section 2.1.1 (TCCON): The TCCON data policy requires that the authors contact the TCCON PI's in the preparation phase of the paper in order to guarantee that the data are used and interpreted correctly and also to agree on a potential co-authorship in case the TCCON data have a central role in the manuscript. Since the TCCON PI's have already commented on this issue separately, I do not focus on this more. I do want to add, however, that several issues in the manuscript regarding the interpretation of the results at specific TCCON sites would have been clarified in the preparation process of the manuscript in case the TCCON PIs had been contacted for the work.

Sections 2.1.2, 2.1.3, and 2.1.4: These sections lack plenty of relevant details, such as version numbers of several of the prior flux components and a more detailed description of the satellite data, even though these data were cited ("CO2 observations from SCIAMACHY and GOSAT" is not sufficient).

Section 2.2.1: Description of the methodology is not sufficient for reproducing the results. For example, it is not clearly described how the gaps in the data are considered. Are there any criteria for including or excluding some of the TCCON sites? Exact methodologies should also be described for comparisons of model and TCCON data (e.g., spatiotemporal interpolation of the gridded model data, averaging kernel corrections etc.).

The results and discussion sections suffer from a very scattered analysis which is rich in details but not in content, and lacks focus. Correlation analysis is not sufficient in case of time series: for example, a phase difference in the time series would result in a relatively weak correlation but the reason for the weak correlation would not be

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identified. At least some representative cases of the XCO2 time series should be presented. The discussions and conclusions drawn on the claimed "biomass burning regions" seem particularly rushed and would have been relatively straightforward to check by the authors because at least CarbonTracker 2017 provides the imposed fire fluxes as a separate data field.