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## ***Interactive comment on* “Optimization of the seasonal cycles of simulated CO<sub>2</sub> flux by fitting simulated atmospheric CO<sub>2</sub> to observed vertical profiles” by Y. Nakatsuka and S. Maksyutov**

**Anonymous Referee #2**

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**Summary:** This paper discusses the use of two observational data sets to improve simulated seasonal and annual cycles of atmospheric CO<sub>2</sub> concentrations in the lower troposphere: first, the observed CO<sub>2</sub> concentrations in a vertical column extending from the surface ground to about 6000 m agl, and surface layer observations only. An inverse model (CASA) was used to simulate the seasonal cycles and the results are discussed in the context of model runs with different parameter sets including a) a ‘generic’ data set taken from aggregating literature values, b) optimization using the vertical column data, c) optimization using the surface data, and also compared to direct observations. Although the subject of this study is of genuine interest to the biogeochemical and atmospheric communities and inverse modeling techniques have

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become a promising tool in the toolbox of scientists linking observations on different space and time scales with atmospheric transport and biogeochemical process models, the manuscript has severe shortcomings. Foremost, it lacks a rigorous description of the methodology, and the conclusions are not supported by the results. Specifically, it remained unclear to me what parameters were optimized, and how the ingestion of the observations was linked to improving the vertical mixing in the model. To my understanding, no modifications have been done to the model to improve the mixing scheme. The authors should also make a more careful review of the available literature on inverse modeling. The length of the manuscript is appropriate, the language acceptable. The paper would definitely benefit from a thorough reorganization of its sections. I cannot see how the conclusions on the vertical mixing scheme are connected to the results. I recommend rejecting the current version of the manuscript for publication in Biogeosciences, while encouraging the authors to submit a more carefully prepared and complete manuscript.

Major comments: a) The manuscript lacks a clear list of parameters that were optimized, a comprehensive description of how parameters were selected for optimization, and as well as how the optimization was done. Eq. 1 is a classical representation of a Bayesian approach, but the linearization of the flux model (B) needs further justification and explanation. Based on the provided information, it was not clear to me how the presented and discussed results were connected to the model optimization. b) The connection between the improved model-observation match by using partial column data of CO<sub>2</sub> and the improved vertical mixing remained unclear. The only obvious connection is that the observed concentrations represent a certain degree of mixing inherently contained in the data, but that doesn't allow for any inferences about the mixing scheme in the atmospheric transport model. Despite the better match, the authors argue that their transport model has an insufficient mixing scheme (Page 10, mid page). The better match of observed versus modeled seasonal concentrations may simply be an artifact of nudging the model in one direction, but possibly at the expense of losing accuracy in other areas. One parameter of interest is the height and dynam-

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ics of the mixing layer that significantly determines the vertical extent and time scale at which concentrations from ground sources are mixed. The latter would be at least relevant for the optimization with surface CO<sub>2</sub> concentrations only. Detailed comments: 1) Page 2, 2nd paragraph: NEP is no physical process. 2) Page 3, bottom section: this paragraph doesn't belong in the introduction, as it contains conclusions. 3) Page 6, 3rd paragraph: The author state the their Rh model is driven by soil moisture and temperature, yet no soil moisture dependence is included in their model (Eq. 8). I believe the LHS of Eq. 8 is incorrect and should read  $R_h = \dots$  4) Page 6, Section 2.3: Does that mean you prescribe a monthly mean PBL height? Isn't that one reason for deploying an atmospheric transport model to estimate the mixing layer height? 5) Page 7, Section 2.4: How often were individual levels sampled, over what period? I realize that the description of the this data set can be found elsewhere, but sufficient information needs to be provided here to be able to understand what data were used for the optimization. The meaning of the sentence "We used weighted mean of the ..." was unclear to me. 6) Page 3, 2nd paragraph: The acronym 'FTS' is used several times without defining it. 7) Page 8, Section 3.2: I am confused as to why CASA was designed to have an annual net carbon flux of zero. This statement puts a big question mark on the suitability of this model for this study. How is the zero flux condition achieved in the model? 8) Page 9, Section 3.3: The authors need to comment on the temporal dynamics of model-observation fit: the match seems to be better for winter periods when the vegetation is dormant and fluxes inactive than for the summer months. Error bars should be provided also for the model runs, I assume the given traces represent ensemble averages? 9) Table 2: What are the values given in parentheses? 10) Figure 2: The authors discuss differences between results from the two different observational data sets despite the fact that most of them are statistically not significant. 11) Figure 3: Each caption and figure needs to stand on its own, so please explain and define all parameters and acronyms. 12) Figure 5: The unit of partial CO<sub>2</sub>-column data remained unclear to me. Are you plotting relative differences? Data must have undergone some sort of normalization. What height level do the results represent?

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