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# *Interactive comment on* "Evaluating two soil carbon models within a global land surface model using surface and spaceborne observations of atmospheric $CO_2$ mole fractions" *by* Tea Thum et al.

**Anonymous Referee #2**

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Thum et al. present an interesting approach for the evaluation of soil carbon models in a land surface model by using atmospheric CO2 observations. I like the basic idea of the study and the work is methodological sound. However, I actually got bored and disappointed when I was reading the paper. This is too a large degree caused by the presentation (text and figures) of the material (I agree with all the points by reviewer 1):

1) It is not clear what the purpose of the study is. Only in the discussion it's written that the "aim was to use atmospheric observations to benchmark soil carbon models". If this was the aim, the evaluation of just two modules of JSBACH is insufficient (and causes



my disappointment). It would be better to clearly state already in the introduction that the aim was to evaluate two soil modules of JSBACH. However, if this is the case, the manuscript would do better as a model evaluation paper in Geoscientific Model Development. Generally, the text is written as a model evaluation study and I don't find important results for the general Biogeosciences. My feeling is that the paper should go beyond a model evaluation and include some more substantial scientific questions, hypotheses and findings in order to fit into Biogeosciences.

2) If the "aim was to use atmospheric observations to benchmark soil carbon models" (as stated), I would expect a more detailed description of the assumptions and a detailed analysis on how to use atmospheric CO2 observations for the benchmarking, including how to disentangle the contributions of GPP and Reco on CO2, the role of uncertainties in observations and atmospheric transport, and how different regions contribute to the CO2 seasonality. Especially the later points would help to potentially benchmark soil carbon simulations if different parts of the world.

3) As already stated by reviewer 1, the text needs substantial rewrite. The text has no clear structure, topic sentences are missing, some chapters are too long (especially 1 and 3.1). For example, the first section of the results (3.1) report mainly minor results (including references to the supplement) but does not report the most important results. In addition, I recommend to split this section in further sub-chapters to improve the structure of the text.

4) Figures: I'm sorry, but reading the figures in the main text and in the supplement was a nightmare! The figures are too small and the grey colours make it almost impossible to distinguish the different model runs and observations. Please improve all figures.

5) The discussion of GPP is over-simplistic. JSBACH overestimates GPP and has in some regions shifts in the seasonality. Hence it remains unclear which soil carbon model is the better one because the comparison of CO2 seasonality is also affected by wrong simulations of GPP. Could it be an option to force more realistic GPP estimates

into JSBACH or mix GPP from data-driven estimates with Rh from JSBACH in TM5?

6) Please describe if permafrost was simulated in the JSBACH runs and how the simulation or non-simulation of permafrost contributed to soil carbon simulations.

- Figure 1: Explain the numbers in the caption

- Figure 2: Even if the soil carbon stocks have been already evaluated, it would be still helpful to add 1 or 2 maps from an observation-based product for comparison.

- Table 2: There seems to be a mistake in the results for TER, as those can't be the same numbers.

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