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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 #1

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Thum and colleagues present an interesting study looking at how different soil models influence atmospheric CO2 fluxes, which can be compared with ground and space-based observations. The study is thorough and well written, but in my estimation, it somewhat glosses over some of the caveats that should likely be considered with trying to evaluate component fluxes (here HR), with atmospheric CO2 observations that serve as a proxy for NEE.

Major concerns: Introduction, I found the basic primer on the global carbon cycle a little pedantic at times, with phrases like "Photosynthesis takes place in the green plant

parts". As well as wandering, combining information on soil respiration and soil C stocks without really developing these ideas in a focused way that supports the direction or intent of the paper. I realize this is more stylistic that substantiative comment, but would encourage the authors to revise the introduction with focused topic sentences for each paragraph that introduces background literature and develop ideas in a way that focuses attention on the work to be presented.

I'm trying to wrap my head around how biases in the magnitude (and timing) of GPP simulated by JSBACH confound results presented here and I fundamentally disagree with the statement on Line 392. âĂć Should SI4 and Fig 2 be combined into one display item in the main text, using the same y-axis units for both? âĂć Could you show the annual cycle of NPP or HR globally, in addition to the latitudinal bins shows in SI2? [maybe this goes into SI]. âĂć Would similar plots of regional & global AR or NPP also be helpful (in SI)? âĂć It's not really clear that the magnitude seasonal cycle of HR with YAS is so large in mid latitudes (30-60N; Fig 4), and is of equal and nearly opposite magnitude to the high GPP biases in this region, resulting in a lower than observed NEE (Fig 6-7). âĂć I'm wondering how any version of JSBACH captures appropriate seasonal peak to trough dynamics of NEE Fig 6 given biases in the magnitude of GPP fluxes? This means that the timing and or magnitude of AR or HR fluxes must be compensating to generate reasonable seasonal cycle of NEE. Given known biases in the seasonal cycles of GPP (Fig S2), what modifications are needed to improve the representation of plant and soil dynamics in JSBACH? âĂć In summary, could one erroneously chose an inferior soil biogeochemical model that give 'better' NEE fluxes with atmospheric observations, but that's fundamentally just masking over / compensating biases in GPP? This is, 'getting the right answer for the wrong reasons?

Throughout on display items, the shades of grey make it kind of hard to distinguish models and observations. This is especially true for Figs 5-7). Is there any harm in using colors?

Minor and technical concerns: There are enough abbreviations in the text that it somewhat distracts from the readability of the manuscript. I'd encourage removing some of these less standard ones if possible (e.g. SCA, GAW).

There are also several very short paragraphs (even just one sentence long), that should either be further developed or merged into related paragraphs.

L19: the IAV of fluxes are never communicated here (although they could be easily brought into text and display items.

L26: Maybe remove 'natural'

L39: I might include van Gestel et al's 2018 critique of the Crowther paper to make this point.

L56: The connection between benchmarking global soil C models (the topic of the last paragraph) and global CO2 flux observations is a little rocky and unclear. Reading between the lines, I think it's a very good idea- but the connections about how / why it may be considered a useful way to evaluate soil biogeochemical models should be clarified. That said, others have recently used a similar approach (Basile et al 2020), which could be useful in contextualizing the introduction of the present work.

I'm also assuming the authors will discuss some of the assumptions being made in evaluating HR fluxes with atmospheric CO2 observations that may include biases in the timing and magnitude of GPP and AR fluxes from (JSBACH), potential errors imparted by the atmospheric transport model (TM5), or challenges in interpreting total column CO2 observations- especially from space. Maybe it's worth foreshadowing some of these in the introduction? See also refs from Keppel-Aleks et al (below).

Line 67: this sentence seems awkwardly phrased, maybe drop 'in' and 'far'.

Line 84: Is there reference for TM5, or example of where / how it's been used?

Line 97: Randerson et al 1997 found that the timing of litterfall was important for controlling the timing of HR fluxes. Is the same true in JSBACH? How well does the model

simulate this phenology?

Line 140, I realize it's likely in your previous publications, but is it worth noting how litter chemistry from JSBACH PFTs is translated onto the YASSO litter quality definitions? The way this is presented in kind of confusing & disconnected

Line 143 define PFT

Line 159 constraints for what?

Line 175 is 'atmospheric' redundant here?

Line 205: Should this be S2.

Line 250: Why run statics on YAS fluxes and alpha, when the model uses precipitation to moderate decomposition rates (eq5)?

Are the correlations between environmental drivers and HR fluxes really that surprising or interesting (Table 3, Line 241-254 & 366)? The models have these assumptions apriori (Methods, Fig. S1). In places with large seasonal variation in soil or air temperature (arctic), temperature is important control over seasonal HR fluxes. In places with little annual variation in temperature (tropics) moisture is a more important control. I'm not really sure what readers are supposed to learn from this analysis?

Fig 6: Dotter is not a word.

Fig 6 & 7, can uncertainty estimate (or interannual variability) be shown for observations?

Table 3, why not just report r values, so negative correlations can be more clearly illustrated. Can't statistically significant correlations be highlighted (not just results with high r2)?

L345, I'm not sure better agreement with CMIP5 models is necessarily a good thing, based on Kathe's work. Moreover, the calculation of global turnover times seems to mask important regional patterns. Instead, see Koven et al. 2017.

L395: where are results supporting these claims shown?

L405: This doesn't seem like a standalone paragraph, nor is it really clear how it relates to the results being discussed regarding carbon tracker and JSBACH land C uptake.

L408: what biases are not important here?

L414: while I agree with this statement, however it's not done in the work presented. Multiple benchmarks, however, could be presented, again see Koven et al. 2017, or Todd-Brown's work that's already cited). Indeed it seems for a flux based analysis like this some more rigorous evaluation of upstream fluxes (e.g. GPP) is pretty important.

References: Basile, S. J., Lin, X., Wieder, W. R., Hartman, M. D., and Keppel-Aleks, G.: Leveraging the signature of heterotrophic respiration on atmospheric CO2 for model benchmarking, Biogeosciences, 17, 1293–1308, https://doi.org/10.5194/bg-17-1293-2020, 2020.

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