**Biogeosciences
Discussions**

Interactive comment on "Constraints from atmospheric CO$_2$ and satellite-based vegetation activity observations on current land carbon cycle trends" *by* D. Dalmonech and S. Zaehle

Anonymous Referee #2

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1. Does the paper address relevant scientific questions within the scope of BG? yes

2. Does the paper present novel concepts, ideas, tools, or data? yes

3. Are substantial conclusions reached? yes

4. Are the scientific methods and assumptions valid and clearly outlined? yes

5. Are the results sufficient to support the interpretations and conclusions? yes

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? yes

8. Does the title clearly reflect the contents of the paper? Partly

9. Does the abstract provide a concise and complete summary? yes

10. Is the overall presentation well structured and clear? yes

11. Is the language fluent and precise? yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? some of the text needs clarification, see specific comments below

14. Are the number and quality of references appropriate? yes

15. Is the amount and quality of supplementary material appropriate? yes

general comments The manuscript presented by Dalmonech and Zaehle presents a elaborated methodology to evaluate terrestrial biosphere model performance with respect to vegetation performance and carbon dynamics. The merits of the approach are (i) the investigation of the interaction of climatic drivers and biosphere response in order to avoid poor evaluation scores due to underlying climate distortions in coupled earth system models, (ii) the analysis of 2 biosphere properties with independent data sources, and (iii) a focus on dynamics rather than pool sizes. Evaluation of terrestrial biosphere model performance, especially in coupled mode in earth system models, is of high relevance for science in climate, ecosystem and biogeochemistry fields. I think the work presented here is a highly welcome contribution to TBM evaluation and it is well structured and analyzed. Given the complexity of their approach, the current manuscript is sometimes confusing despite the authors' attempt to clearly structure

the text. These smaller parts of the manuscript require more explanation to be also readable to non-experts. The only major concern I have is that I miss a discussion of the approach's limitations to some extent. Often, climate and biosphere traits are only weakly correlated, which has implications for the interpretation of results. Similarly, uncertainties from other sources are often listed but could be better addressed in the interpretation of results; the uncertainties from the transport model are explicitly discussed at appropriate places, but ocean uptake, land-use emissions, satellite data interpretation could be extended. The identification of suggested mechanisms in JSBACH responsible for analyzed discrepancies between observations and model is not always clear and could be explained a bit more. The presented evaluation scheme should be discussed more in the context of existing evaluation schemes (site-level dynamics for water and carbon, global pools and dynamics, space for time etc.). Almost all figures could be improved with respect to explanations in text, legends and captions.

specific comments

* title: the paper is not on constraints on current trends but on evaluation of current projections, I'd suggest to title the paper "Atmospheric CO2 and satellite-based vegetation activity observations in evaluations of carbon cycle projections"

* 16088/20 (and elsewhere): model failure is a strong word: I'd suggest "model deficiencies"

* 16091/23ff: I don't know too much about the details of TMs but I would imagine that uncertainties of reanalysis wind fields is higher in remote areas?

* 16091/26-16092/8: This needs a bit more explanation. After reading it several times, some understanding dawns but it remains unclear how the robustness is assessed here.

* 16092/13: what is the "collection g"? Can't find it in Tucker or Beck

* 16093/21: how was the aggregation performed?

* 160595/3-4: It would help my flow of reading if you'd specify the actual years used: 80s (1982-1991), 90s (1992-1997), 2000s (1998-2006)

* 16095/6-7: confusing: "by mean of normalized"; rephrase sentence.

* 16096/10: This trait checks for large regional inconsistencies between...?

* 16096/22: I'd suspect that the original atmospheric CO2 time series has multiple zero-crossing dates: how was the data aggregated (running mean, trend?) to identify a single zero-crossing date? BTW: Section 2.1.1 does not specify the temporal resolution of the CO2 data.

* 16097/21: what is the zero-crossing point in fAPAR data which I would expect to be always positive?

* 16097/22: I'm very much in favor of evaluating correlations between climate and vegetation signals, as climate in models can be substantially skewed as you write earlier. Why do you here use linear differences between observed and modeled months of t-onset/t-max, even though these should directly reflect biases in driving climate data seasonalities?

* 16102/18: wouldn't it rather be "the east and south of the North America Temperate region"?

* 16102/9-14: I feel that the "data not shown" would be better suited to support the claimed overestimation of the amplitude than the latitudinal gradient shown in Fig 3b. If not, this requires more explanation.

* 16102/26 "owing to" instead of "responsible to" or do you mean "responsible for"?

* 16103/10: how does this conclusion refer to selecting only areas with one vegetative season (16097/10)?

* 16106/1: instead of "results not shown" the previous studies could be cited I guess?

* 16107/4: NDVI in regions with. . . is mainly driven. . .

* Figure 2: LTT in right-hand panel should be names V-LTT as in Table 3 to avoid confusion

* Figure 2: what is the meaning of blue/red/black traits?

* Figure 3a: what is the meaning of the color coding?

* Figure A2: what is the horizontal line?

* Figure 8 seems to use at least 2 different tones of blue – does it have any meaning?

* Figure 7/8: it seems the areas masked out differ (Greenland) – and why would northern Greenland not be masked out?

technical corrections

* 16091/26: sects 2.4.1 and 2.4.5

* 16094/2: Hurt et al. 2006 is missing in reference list – and I believe you mean Hurtt et al. 2006?

* 16094/8 "(Thoning et al.," should be "Thoning et al. ("

* 16094/21: long-term

* 16096/20 and 16097/1: consistency in extra-tropical

* 16107/1: NDVI?