Biogeosciences Discussions

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Comment on bg-2022-130

Anonymous Referee #1

Referee comment on "Global evaluation of terrestrial biogeochemistry in the Energy Exascale Earth System Model (E3SM) and the role of the phosphorus cycle in the historical terrestrial carbon balance" by Xiaojuan Yang et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-130-RC1, 2022

General comments

In this study X. Yang and co-authors show global evaluation of ELMv1-CNP-model, which has fully prognostic carbon (C), nitrogen (N) and phosphorus (P) cycles. The model evaluation is done using the ILAMB benchmarking system, GOLUM-CNP data derived product and meta-analysis from elevated co2 manipulation experiments. Also the published values in literature are used in evaluation of simulated global C, N and P pools and fluxes. After the model has been proven to have good performance against evaluation metrics, influence of the P cycle on historical carbon balance is discussed. The main findings here are widespread co-limitation of N and P as well as a prominent influence of P on the historical C balance. The authors also go in detail to the causes of differences between GOLUM-CNP and model results and they also discuss the downsides of the benchmarking data used in ILAMB. They also cover the development needs that their model has.

The paper is well-written and figures are clear and demonstrative. The topic is wide and many of the global/biome scale evaluation results shown are not discussed in detail, which is justified, as a lot of things are covered. However, now in some occasions it’s mentioned that the model is not doing so good in some respect e.g. in one biome and this fact is not re-visited later in the text. It would be interesting for other modellers working with these issues to hear more insight from the authors what they think the reasons are. But the model performance overall is good, so this is just a suggestion.

Overall the paper is of high quality and I recommend its publication in Biogeosciences after some comments below (mainly very minor) are addressed.

Specific comments

Model overview & simulations: It was not clear for me that the fire module was activated before the discussion. Maybe this could be mentioned here already. Few points that would also be interesting (also in the light of rest of the manuscript) to mention, what was the soil depth and if fixed stochiometry was used and if the leaves were the only pool where PFT-specific stochiometric ratios were used.

l. 299: Do any other of the models shown in Fig. 1 have CNP-cycles enabled?

l. 314: Is CNP-version always better than CN? If I'm not mistaken, the FLUXNET (for NEE, respiration & GPP) is better captured by CN-version? Also Precipitation/GPCP2?

l. 337: Are these annual mean LAI values or mean LAI values for the time when there are leaves?

l. 352: Are you referring here to both temperal and tropical grasslands or also tundra? The TEG seems to have high NUE and PUE values in the distribution. Why do you think that occurs?

l. 394: Are you also simulating peatlands in your model?

l. 397: Sorry, what was the value for your top 1 m soil carbon? (I found it it Table 2, but that had not been referenced yet here.)

l. 398: The estimation you refer to from Todd-Brown is originally from the HWSD? Would it be fair to mention also that source?

l. 401 & 403: Are the estimates from Pan (2011) really exactly the same for litter C and CWD?

l. 419: Is the Xu and Prentice paper having two different estimates for vegetation N, the other one agreeing exactly with the Zaehle et al. estimate?

l. 452: In the caption of Fig. 9b you say that values close to 1 show co-limitation. Could you be more specific and say how close to one the values need to be that co-limitation is prominent?

l. 527-528: Should you clarify here, that the W-E gradient is referring to Amazon? Another point: I didn't find Quesada -paper in your references.

l. 589-590: Is this true also for tropical grasslands, or only tropical forests?

l. 593: Also for tundra?

l. 596: Have you mentioned earlier, how you defined you stochiometric ratios? To my understanding they were PFT-specific, but that was only for the leaves?

Fig. 1. You have some datasets in green boxes, some in orange boxes. Is there a difference between these datasets/variables?

Fig. 7: Unclear, which is model and which is observation, since in the caption only circles are mentioned, but also triangles are shown. Since there are two observations for NSC (these should be also clearly denoted, which one is which), it's clear that the green triangles are from the model. The NPP vs. GPP response in the simulations shows quite similar response (or NPP response is larger), whereas in the observations the NPP response seems to be lower compared to GPP response. Would you like to comment on that? In Fig. 6b there is more pronounced co2 effect seen in NPP than GPP in Central Canada. Would you like to explain a bit more what is happening there?

Fig 9b: In this case the values close to 1 are interesting, but unfortunately very similar in color to regions without vegetation. Whereas the extremes of the color scheme perhaps partly replicate the information already visible in 9a. Would it be possible to modify this figure to show the areas of co-limitation clearer?

Fig. 11. The units are not now clear for me. They should be added. I'm pondering on the color scale for subplots b, d, and f. The below zero values show here the constraint caused by P to these variables, if I understood right. What is the unit on this effect? The color bars have exactly the same values for all the different variables, so has this effect been normalized? Why do the color bars stretch to the positive side? Are there any over zero values in these plots and if there are, how are those to be interpreted?

Technical comments, typos

-co2 missing subscript in several places

l. 442: Missing unit here.

l. 637: eCO2 has not been introduced.

Fig. 1. Caption, typo: JSBACH and MPI-ESM.

Fig 5: denote subplots

Fig. 8: Could you explain in the caption the acronyms for the heterotrophic and autotrophic respirations?

Fig. 10. Could you add the units?

Table 1: Typo in “transient” (LULCC column)

Fig. S2: denote subplots

Fig. S7: Remove the subplot mark. (If ‘b)’ stands here for that...)

Fig. S8: Are the subplots marked? In S8a the highest latitude point for model is not seen.

-It seems that S8 & S9 are referenced to before S7. Could the order of the plots be swapped?

Fig. S9: Could you clearly denote the lat, lon -values for the sites? What are the grey boxes the cycle-plots? Denote the subplots. Include units. Replace ‘var’ with the variable name.

Table S1: It’s a bit mystery for me, why this table is called PFT-specific parameters, but only leaf parameters seem to be changing between PFTs...

Table S4: typo ‘Global net ecosystem’

Supplement – references: Richardson paper not in alphabetical order.