

Interactive comment on “Global evaluation of nutrient enabled version land surface model ORCHIDEE-CNP v1.2 (r5986)” by Yan Sun et al.

Yan Sun et al.

yansun.ni@gmail.com

Received and published: 31 December 2020

Referee #1 [General comment] Sun et al., performed a comprehensive evaluation of the nutrient-enabled version of the ORCHIDEE model (ORCHIDEE-CNP). The evaluations were made for biosphere carbon fluxes, N and P cycles, leaf and soil conditions and plant resource use efficiencies. Based on this, the authors were able to provide clear recommendations for future development. The extensive set of observational data that the authors use, together with the evaluation of different metrics is very powerful. The work provides a complete picture of the model performance. My main concern, however, is that due to the comprehensiveness of the work it is at the same time difficult to grasp the main messages when reading the results and discussion section. When I read the manuscript for the first time I was overwhelmed by all the

C1

data and comparisons. I did not get a clear picture of the key messages in between the discussion of all the different metrics. The length of the manuscript contributes to this as well (27 pages of text, 18 figures and a table, plus several supplementary documents). Instead of showing comparisons of all available observational datasets, the authors could choose to only show those figures that help to illustrate their conclusion, and move other comparisons to the supplementary material and discuss them only shortly. For example, the authors discuss in sect 5.1.2 that it is difficult to falsify one model version over another based on the comparison of the de-trended anomalies in Fig 4, due to uncertainties in the observed GPP. As this figure did not contribute much to their final conclusion, the authors could choose to leave Fig. 4 out of the main manuscript. I would advise the authors to have a critical look at their figures, and the messages that they convey, to see which figures are really key to bring their message forward in order to condense (and thereby improve) the manuscript. Moreover, section 2 “Model description” gives an overview of the modifications that were made compared to ORCHIDEE-CNP v1.1, but it does not provide an overview of the nutrient flows of N and P. A brief description of steps in the N and P cycles would be helpful to understand the processes in this nutrient-enabled model version of ORCHIDEE. When such processes are introduced this would also help to understand the evaluation with observations later on. A discussion of Fig. 1 could serve this, as it is a nice illustration of such processes, but this figure is currently hardly discussed in the text. [Response] Thank you very much for your careful review and the positive comments with regard to our manuscript. We have revised the manuscript according to your suggestions and hope the readability of the manuscript has improved substantially due to focusing on key aspects. According to your suggestion on making the main text ‘more clear and concise’, we reconstructed the Result and Discussion sections with focusing on the evaluations for 4 key nutrient-related emerging properties of the model simulations which are linked to ecosystem gas exchanges and carbon storage: (1) vegetation resource use efficiencies, (2) the response of GPP to increasing CO₂, (3) ecosystem N and P turnover and openness, and (4) large-scale pattern of ecosystem stoichiome-

C2

tries. Point (1) and (2) control the response of vegetation carbon uptake which operates on timescales of years to decades, while point (3) and (4) control the response of the ecosystem carbon storage potential which operates on timescales of centuries and longer. This has been stated in the revised introduction section (Page 3; Lines 101-109). The choice was further based on the availability of observational data. To do so the following changes have been made which condensed the main text to 20 pages and 10 figures. First, information on processes which underlie the 4 key properties was moved to a large part to the SI. For example, we moved the detailed evaluation of single nutrient fluxes (e.g. BNF, P leaching etc.) and budgets into the SI (Sect. S2-S7 in supplement) which are now cited in the main text in the discussion of patterns in ecosystem nutrient turnover. Second, we condensed the information which was previously shown in three figures into one single figure by focusing on two statistical indexes (i.e. R2 and relative mean square error) between model and reference datasets (Fig. 10). The original figures which contain additional information (spatial patterns, temporal evolution) are now shown in the SI. According to your suggestion on providing the overview of nutrient flows in the 'Model description', we added the brief description of steps in the N and P cycles for the important N and P fluxes: 'ORCHIDEE-CNP simulates the cycles of C, N and P which are described in detail elsewhere (Krinner et al., 2005; Zaehle and Friend 2010; Goll et al., 2014, 2017a, 2018). We here give a brief overview. P enters the ecosystem by release from minerals into the soil solution, whereas N is biologically fixed from an ample reservoir of dinitrogen. Dissolved nutrients are either taken up by vegetation, converted into soil organic matter or absorbed onto soil particles. Losses occur as leaching of dissolved nutrients, gaseous soil N emissions, or occlusion of P in secondary minerals. When nutrients are taken up by vegetation they are either stored internally or used to build new plant tissue driven by the availability of C, N and P in vegetation. The nutrient concentration of plant tissue varies within a prescribed range depending on the relative availability of C, N and P. Before plant tissue is shed, depending on the tissue a fixed fraction of the nutrients is recycled. The nutrients contained in dead plant tissue and organic matter are miner-

C3

alized and released back into the soil solution.' (Pages 3-4; Lines 106-117). All of the specific comments and suggestions have been addressed and implemented in this revised manuscript. Responses to the specific comments can be found below. [Comment 1] P2, Line 49: "this direction of future carbon storage", what direction? [Response to #1] It indicates the overestimation in C storage. This sentence has been changed as: "Empirical stoichiometry observations were applied in the posteriori estimates of future carbon storage from land surface models (LSMs) lacking an explicit simulation of N and P biogeochemistry, which led consistently to an overestimation of future carbon storage in LSMs (Hungate et al., 2003; Wang and Houlton, 2009; Zaehle et al., 2015; Wieder et al., 2015)." (Page 2; Lines 46-50). [Comment 2] P2, line 70: "should look for" = needs. [Response to #2] We replaced "should look for" by "needs" according to your suggestion. [Comment 3] P2-3, line 75-81: Shorten or break up this sentence for readability. [Response to #3] According to your suggestion, we broke up this sentence as: "The evaluation for N and P together with carbon cycling in global LSMs remains very limited (Wang et al., 2010; Goll et al., 2012) but recent advances in ground-based measurements, ecological datasets and process understanding have made a better evaluation of C, N, P models feasible. The available nutrient datasets have allowed for meta-analyses of site-level nutrient fertilization experiments (e.g. Yuan and Chen, 2015; Wright, 2019), data-driven assimilation schemes to constrain nutrient budgets (Wang et al., 2018), new knowledge about the critical P-processes of sorption (Helfenstein et al., 2018; 2020) and phosphatase-mediated mineralization (Sun et al., 2020), global datasets of leaf nutrient content (Butler et al., 2017), and empirical constraints on the CO2 fertilization effect on land carbon storage (Terrer et al., 2019; Liu et al., 2019)." (Pages 2-3; Lines 73-81). [Comment 4] P4, line 118: give full name of SOM. [Response to #4] The full name of SOM "soil organic matter" was given in this sentence. [Comment 5] P4, line 140-142: mention here the resolution of the ORCHIDEE run (0.5 degree) that differs from the 2.0 degree for ORCHIDEE-CNP. Otherwise this is only mentioned in the caption of Fig. 3, but it is relevant information. [Response to #5] According to your suggestion, we added one sentence to state the informa-

C4

tion of spatial resolution for ORCHIDEE as: “To disentangle the effect of introducing nutrient cycles into ORCHIDEE, we performed the same simulation with ORCHIDEE (revision 5375) which has no nutrient cycles and a comparable parameterization for other processes. ORCHIDEE was run at a higher spatial resolution (0.5x0.5) than ORCHIDEE-CNP. Prior to the analysis, the data from ORCHIDEE was remapped to the resolution of ORCHIDEE-CNP.” (Page 5; Lines 155-159). [Comment 6] P8, line 277-279: check correctness of this sentence. [Response to #6] This sentence was corrected and removed into SI according to our re-constructions. The revised sentence is: “Thus, the annual soil P loss via surface runoff (kg P km⁻² yr⁻¹) from the ORCHIDEE-CNP output were extracted, and were compared with the GlobalNEWS2 load rates (load).” (Sect. S5 in the supplement). [Comment 7] P8, line 287: its uncertainties were calculated [Response to #7] We corrected it as “its uncertainties were calculated”. [Comment 8] P8, line 290: based on [Response to #8] We corrected it as “based on”. [Comment 9] P8, line 291: remove “speaking” [Response to #9] We removed the word “speaking” from this sentence. [Comment 10] P9, line 311: twice higher = twice as high [Response to #10] This sentence was deleted according to our re-constructions. [Comment 11] P9, line 316-318: The markers in Fig. 2b for temperate and western Europe don’t seem to show this? [Response to #11] Thanks for pointing this out. We revised the text to be in consistent with the figure (Fig. S6 in the revised manuscript) as: “ORCHIDEE-CNP simulated comparable GPP values for most parts of the globe (Fig. S6a), and comparable NPP values for most of northern high-latitudes (Fig. S6b), which lie within the range given by the data-driven products.” (Page 13; Lines 446-448). [Comment 12] P10, line345- 347: Over the whole range, Eco2 seems to be quite similar for ORCHIDEE-CNP and ORCHIDEE. Is the ORCHIDEE Eco2 higher only because it has more data points in the lower GPP range where Eco2 is clearly higher than those based on Campbell et al. (2017) and Ehlers et al. (2015)? What is the role of the different resolution of ORCHIDEE-CNP and ORCHIDEE here? Does the resolution explain that there are data points in the ORCHIDEE plot below 400 GPP-296 and in between 300-1000 GPP-396, that are not there for ORCHIDEE-CNP? [Response

C5

to #12] Thanks for this question. We are lack of the simulation for ORCHIDEE with spatial resolution of 2o x 2o, which make it is hard to separate the role of different resolution. To keep the consistency of comparison, we resampled the ORCHIDEE outputs to 2o x 2o resolution before comparing the ECO2. This information is also provided in the main text: “All the gridded datasets with high spatial resolutions (Table 1) were resampled to the 2o x 2o resolution of the model output using area-weighted mean methods.” (Page 7; Lines 261-262). Then, we found that ORCHIDEE still overestimated the Eco2 for low GPP region. Besides, the spatial pattern of ECO2 (Fig. S5) shows that ORCHIDEE simulate a much more higher value than ORCHIDEE-CNP in northern high latitudes. [Comment 13] P11, line 379: refer to Fig. 6a here already [Response to #13] This sentence has been deleted in the revised manuscript. [Comment 14] P11, line 381: “... but is close to JENA-inversion estimate during this period”. Can you give the value for the CTracker atmospheric inversion? [Response to #14] This sentence has been deleted in the revised manuscript. To make the comparison for NBP clearer, we show the matrix of statistic indexes between ORCHIDEE-CNP and inversion data and mean value across Trendy ensemble (v6) (see the Response to the General comment) in the main text (Fig. 10). [Comment 15] P11, line 381: Global simulated NBP from ORCHIDEE-CNP. . . [Response to #15] This sentence has been deleted in the revised manuscript (see Response to #14). The statement for the evaluation on NBP was revised as: “Net biome productivity (NBP) is defined as the net C exchange between the atmosphere and the terrestrial biosphere, that is the sum of net primary productivity, heterotrophic respiration and emissions due to disturbances; positive values denoting a land carbon sink. Compared to the three sets of atmospheric inversions (CAM5, JENA and CTracker), ORCHIDEE(-CNP) performs slightly worse than the mean of predictions from 16 land surface models from Trendy ensembles (v6) (Fig. 10c). ORCHIDEE-CNP shows a worse performance in inter-annual variability of NBP than ORCHIDEE when compared against inversion datasets at global scale and for the Northern Hemisphere. However, ORCHIDEE-CNP improved the performance of inter-annual variability of NBP against inversion datasets relative to ORCHIDEE for

C6

tropical region (higher R2 and lower rMSE) with closer or even better fitness against inversion datasets than the mean value of Trendy ensemble models (Fig. 10c).” (Page 13; Lines 463-472). [Comment 16] P11, line 392: avoid the word “cause” here. Better say something like: “Therefore, the underestimation of the global C sink in ORCHIDEE-CNP during the last decades is primarily due to a lower C sink in the NH.” [Response to #16] This sentence has been deleted in the revised manuscript (see Response to #14 and #15). [Comment 17] P12, line 410: abbreviation BNF was introduced already earlier. [Response to #17] We removed the full name of BNF here. [Comment 18] P13, line 457-468: this discussion of literature values is longer than needed, please shorten. [Response to #18] According to your suggestion, we removed this part of result for BNF to the SI (Sect. S4 in the supplement) and cited it in the section ‘5.3 Ecosystem N and P turnover openness’. [Comment 19] P15, line 537-538: it is not entirely clear to me how I can see the net N accumulation of 51.5 Tg N yr⁻¹ from Fig 14a? [Response to #19] This part was removed to the SI and this sentence was deleted. [Comment 20] Discussion: as there were so many results presented, it is important to reference to a figure or paragraph from the results section that evidences your statements. E.g. in line 631-634; 635-638. [Response to #20] Thanks for this useful suggestion. We added the reference for the figures from the results rather than only citing the section number. [Comment 21] P18, line 643: “all models”, you mean biosphere models? [Response to #21] We replaced the “models” by “LSMs”. The revised sentence is: “The strength of the fertilization effect on GPP differs strongly between LSMs (Friedlingstein et al., 2014).” (Page 15; Lines 534-549). [Comment 22] P19, line 676-677: but aren't ORCHIDEE-CNP and ORCHIDEE forced with the same meteorology, and thus the same temperature, precipitation and radiation? [Response to #22] ORCHIDEE-CNP and ORCHIDEE used the same forcing data of meteorology from CRU-JRA-55. But BESS and MTE used climate datasets from CRU-NCEP. For analyzing the sensitivity of GPP anomaly to climates, we used CRU-JRA-55 for ORCHIDEE-CNP and ORCHIDEE and CRU-NCEP for BESS and MTE. This information has been added in the supplement S11. [Comment 23] P20, line 691: you mean Fig. S7? [Response to #23]

C7

It corresponds to Fig. S10 after we reshuffled the Supplementary. [Comment 24] P20, line 711: “...of the land C sink...”, you mean “the size of the land C sink”? [Response to #24] This sentence has been revised as “Current LSM unanimously conclude that CO₂ fertilization is the main driver of the land carbon sink and its trend (Friedlingstein et al., 2014), but it remains unclear to what extent other drivers (i.e. climate change, land management, nutrient deposition) contribute to the sink as well.” (Page 18; Lines 659-661). [Comment 25] P21, line 768: is “capital” the right word here? Maybe use “pools”? [Response to #25] This sentence has been deleted in the revised manuscript. [Comment 26] P23, line 825: “due to that. . .” = “that is because” [Response to #26] This sentence was removed into SI. We revised it as “that is because”. [Comment 27] P23, line 831: model = models [Response to #27] This sentence was removed into SI. We revised it as “LSMs”. [Comment 28] P23, line 830-834: shorten this sentence for readability. [Response to #28] This sentence was removed into SI. According to your suggestion, we broke up this sentence as: “In the P-enabled LSMs, inorganic P processes operating on longer timescale (occlusion, strong sorption) are only simply represented (Wang et al., 2010; Yang et al., 2014; Goll et al., 2017b). This processes in LSMs is primarily based on calibration rather than data driven, which remain a large source of uncertainty regarding changes in P availability under elevated CO₂ (Goll et al., 2012).” (Sect. S6 in the supplement). [Comment 29] Appendices and Supplementary Material: It is confusing that the manuscript contains Appendices and two supplementary documents. It would be good if all such supplementary material is combined into a single document. [Response to #29] According to your suggestion, we combined the Appendices into Supplementary Material. [Comment 30] Tables and Figures: Table 1: CMAS inversion = CAMS inversion? Fig 3, legend: Rose lines = Pink lines? [Response to #30] We are very sorry for the wrong spelling. The text in the legend and Table 1 has been revised to be “CAMS inversion”. We corrected the legend of Fig. 5 (original Fig.3) as ‘pink lines’ instead of ‘rose lines’. [Comment 31] Fig 7, complex, consider removing it to the supplementary material. [Response to #31] This figure is very important to illustrate the simulated global C, N and P fluxes and

C8

storages by ORCHIDEE-CNP compared to GOLUM-CNP which is a data-driven modeling of steady-state of C, N and P dynamic. We simplified this figure (Fig. 2) and kept it in the main text. [Comment 32] Fig 10, Besides mentioning “model B” and “model C”, also give the reference in the legend (like for model A, Peng et al., 2019 is mentioned). [Response to #32] According to your suggestion, we added the reference ‘Cleveland et al., 1999’ and ‘Wang and Houlton, 2009’ in the legend (Fig. S16). [Comment 33] Fig 12, 16, what is red and what is black? Fig 12, is n from the model the number of grid cells with that soil type? Fig 9, 12, 16, 18 add in the legend what the width of the bars indicates. Fig 16, what do a, b, c in the figures mean? [Response to #33] We revised the legends of those figures to make it clear to understand.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-93>, 2020.