

# ***Interactive comment on “Accounting for Carbon and Nitrogen interactions in the Global Terrestrial Ecosystem Model ORCHIDEE (trunk version, rev 4999): multi-scale evaluation of gross primary production” by Nicolas Vuichard et al.***

## **Anonymous Referee #1**

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Review of manuscript gmd-2018-261 “Accounting for Carbon and Nitrogen interactions in the Global Terrestrial Ecosystem Model ORCHIDEE (trunk version, rev 4999): multi-scale evaluation of gross primary production” by Nicolas Vuichard, Palmira Messina, Sebastiaan Luyssaert, Bertrand Guenet, Sönke Zaehle, Josefina Ghattas, Vladislav Bastrikov, and Philippe Peylin

The manuscript describes the new version of Global Terrestrial Ecosystem Model ORCHIDEE with Nitrogen interactions integrated into the trunk. The paper is very well written and very clearly presents the description of the nitrogen cycle and its interac-

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tion with the photosynthesis and carbon/nitrogen allocation, and shows the results of the model validation and sensitivity analysis.

I think the manuscript describes significant contribution to the field of modeling of carbon and nitrogen interactions in the terrestrial biosphere, and deserves to be promptly published.

I do, however, have few minor remarks and questions, outlined below.

Page 3, line 1: typo, “pionneering” should be “pioneering”

Page 4, line 34 “Nitrogen inputs in the soil/plant system . . . (ii) biological nitrogen fixation and nitrogen fertilisation over managed grasslands and croplands. . .” I think this phrase needs some disambiguation – does it mean biological nitrogen fixation everywhere and fertilization over managed grass/crop lands, or both over managed grass/crop lands?

Page 5, lines 6-7: “Furthermore, the present study considers biological nitrogen fixation rates invariant in time and computed them as a function of evapotranspiration . . .” – if the nitrogen fixation rates are a function of evapotranspiration, and in experiments with elevated CO<sub>2</sub> transpiration drops, does it mean that the nitrogen fixation drops as well? Based on observational evidence, is there a reason to believe that this effect is real and nitrogen fixation will drop in the elevated CO<sub>2</sub> world? Naively, I would think that the opposite is true – more available carbon in CO<sub>2</sub>-rich world may lead to the plants being able to spend more on symbionts, increasing fixation. How does this fixation treatment effect the differences between CNfix and CNdyn experiments presented in this paper?

Page 9, lines 13-16: What is the length of the in-situ meteorological data? Is it enough to sample representative interannual variability? Under-sampling climate variability might lead to biases in the base state of the vegetation, and perhaps also to the biases in the responses to model treatment.

Page 16, lines 1-4, Figures 5 and 6: The enhanced interannual variability on BoENF sites in CNfix simulations (and lack of this variability in respective CNdyn) looks very interesting, especially what looks like long-term oscillations in CNfix output. What can be the cause of that, in the system with less degrees of freedom than CNdyn configuration?

Page 16, lines 26-29. What is the reason for the large GPP biases of different signs in two tropical forest regions (Africa and Amazonia)?

Page 41, line 9: typo, “S1-CNdy” should be “S1-CNdyn”

A general question: How does geographical distribution of GPP biases compare with the original ORCHIDEE model? How does it translate in the biases in other biophysical characteristics, such as biomass or LAI? I understand that the main focus of this manuscript is GPP, but I think it would be beneficial to the reader if some other results were shown too, at least from the global simulation. Unless the authors plan further publications which would address validation of the presented model version in a broader sense, of course.

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