Biogeosciences

Discussions

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Interactive
comment

Interactive comment on "Plant-microbe Symbioses Reveal Underestimation of Modeled Climate Impacts" *by* Mingjie Shi et al.

Anonymous Referee #1

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General Comments

Shi et al. address the effect of nitrogen (N) limitation on the land carbon (C) uptake and climate change by estimating C costs for N acquisition. They coupled the Fixation and Uptake of Nitrogen (FUN) sub-model, which directly calculates C costs for different N acquisition strategies, to the Community Land Model (CLM) and run CLM, and CLM-FUN respectively, to estimate the reduction of net primary production (NPP) under N limitation, firstly. Secondly they used CAM, an atmospheric circulation model that includes CLM (or CLM-FUN), to take impacts on climate change into account. They show that C costs for N acquisition lower NPP and the Leaf Area Index globally, what has implications for the global C budget as well as for evapotranspiration and surface albedo. This influences the global radiative forcing and water balance and leads to

changes in surface temperature and precipitation. Shi et al. summarize that Earth System Models that do not take C costs for N acquisition into account might over-estimate the land C sink and following under-predict climate change, but they also emphasize that P and water limitations play a role as well.

Overall, I think this is an interesting study and fits thematically the scope of BG quite well, but I have some major issues regarding the built and implementation of the study that make the evaluation impossible.

Specific Comments

I think, the 'Material and Methods' section is to short. Based on the given information it is impossible to reproduce the study, because it is unclear, what the authors have actually done and under which conditions the simulations run. The 'Results' part is very short, too, and does not show any base values, but only absolute and relative changes between model simulations. These short preparations of the following discussion make it impossible to fully understand the study and evaluate the paper.

Comments on FUN model

I miss more information about the modified parameters for N uptake by ECM / AM infected roots. Changes in orders of magnitudes require some more information. Not only, why the parameters were changed, but also how this effects the results compared to previous studies and/or a sensitivity analysis.

Comments on CLM vs CLM-FUN simulations

Regarding CLM-FUN vs CLM a proper set-up description is missing. If the reader wants to reproduce the study, he needs to know, if there is a spin-up done and how, how the models are initialized etc. From given information it is impossible to know for example, whether both models (CLM and CLM-FUN) start with the same biomass, or if they already differ in the beginning of the analyzed period, because of different spin-up results.

Secondly, the models calculate a global NPP difference of 8.2 PgC/yr. As consequence biomass in CLM-FUN should be lower as in CLM and thus also heterotrophic respiration should change. Do the authors consider that? If not, they might over-estimate the effect of N acquisition costs. Same for just looking at the land surface. As soon as the global land C sink is lowered, the ocean will take up more C, and not all additional C will remain in the atmosphere to force climate change. Over all it is completely unclear, how the authors derive the yearly CO_2 increase of 3.8 ppm from the calculated NPP difference. Moreover the increase of 3.8 ppm per year just because of taking C costs for N acquisition into account seems very high compared to the actually measured atmospheric CO_2 growth rate, which is around 2 ppm per year. Hence the derivation of the yearly atmospheric CO_2 increase should be described very detailed.

Comments on CAM vs CAM-FUN simulations

First of all, it is unclear, whether FUN is actually coupled to CAM (and the abbreviation CAM-FUN indicates that somehow), or if only the additional C release to the atmosphere (3.8 ppm/yr), which is calculated be CLM-FUN, is added to the atmosphere of CAM.

If FUN is coupled to CAM, the reader needs again a proper set-up description as required already for CLM-FUN. For a fully coupled CAM-FUN model, I don't understand the reason for the atmospheric CO_2 increase, because that should evolve internally by itself.

If only the atmospheric CO_2 concentration in CAM-FUN is increased compared to CAM, the analyzed effects might rather depend on the CO_2 forcing of CAM in general than on C costs for N acquisition. From the manuscript it is unclear, if any other changed values/fluxes from CLM-FUN are introduced to CAM-FUN, for example NPP to outbalance the additional C input to the atmosphere or is the total amount of C in CAM-FUN increasing. If NPP in CAM-FUN is constrained by CLM-FUN, how does that influence vegetation dynamics and development in CAM-FUN under increased atmo-

spheric CO2?

Besides all that, the introduction of the optional slab mixed-layer ocean model is misleading, since it is not used. Is it?

Technical Corrections

L111: CAM is introduced as abbreviation of CAM version 4. Is there any other name for CAM than 'an atmospheric general circulation model that includes CLM'?

L122-L125: double reference to forcing data set

L124: spatio-temporal vs L139 spatiotemporal

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