Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-293-AC1, 2018

Biogeosciences Discussions

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

Mingjie Shi et al.

erbrzostek@mail.wvu.edu

Received and published: 12 October 2018

We are grateful to the reviewer for their thoughtful comments and we have addressed them below. We have also included a revised manuscript file which is uploaded here so that the direct changes that we made to the manuscript.

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.

ERB: In the materials and methods section, we have added significant detail on the model simulations, the initial conditions, and the assumptions that we made. This includes both a better introduction into what each model does, more detailed information on the coupled CAM-CLM and CAM-CLM-FUN runs, and the initial conditions and forcing data for the models. In the results section, we have included two new global map figures in the supplemental information that show the absolute values of NPP, LAI, and ET in CAM-CLM and CAM-CLM-FUN.

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.

ERB: The FUN model predicts the C cost of N acquisition from the soil by ectomycorrhizal, arbuscular mycorrhizal, and nonmycorrhizal roots based upon root biomass (a proxy for access) and soil nitrogen concentrations (a measure of availability of N for plants to take up). Previously, the parameter controlling the sensitivity of the C cost of N acquisition to root biomass was low. As such the C cost of N acquisition showed little to no sensitivity to variability in root biomass across grid cells and the ECM cost of N acquisition was always lower than the AM cost of N acquisition even in high N biomes. We have included a figure in the supplementary material that shows how modeled NPP changes with the new parameters as well as a table that shows the parameter changes. The parameter adjustment reduces global NPP by 1.5Pg or $\sim$3%. Finally, we include text above in the material and methods in lines 130-149 that discusses this figure and the rationale behind the parameter adjustment.

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.

ERB: We have added text in lines 178-182 that states the model spinup and configuration files are the default inputs that NCAR provides with the model. Both model configurations thus start from the same initial conditions and then diverge as FUN downregulates NPP in CLM based upon the C cost of acquisition.

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 CO2 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 CO2 growth rate, which is around 2 ppm per year. Hence the derivation of the yearly atmospheric CO2 increase should be described very detailed.

ERB: In CLM-FUN heterotrophic respiration was 3.3 Pg C yr-1 less than in CLM without FUN. This represents about 40% of the reduction we observed in NPP. While not including this reduction in heterotrophic respiration may impact the results we present, empirical and modeling evidence suggests that including the C cost of N acquisition likely enhances heterotrophic respiration. This would occur through C being sent belowground to rhizosphere microbes which enhances their ability to prime soil organic matter decomposition. This is why we highlighted the coupling of FUN with a microbial decomposition model in lines 368-370. We have also added text in the discussion to highlight this assumption as well as the lack of ocean uptake of CO2 in the model in lines 383-391. We also acknowledge that our estimate of climate impacts represents an upper boundary condition that may be mediated by heterotrophic respiration and ocean processes. Also note that there was a rounding error in the NPP reduction and we have revised it down to 8.1 Pg C yr-1.

In addition, we assume that all of the C from the reduction of NPP is transferred to the atmosphere. We have included more detail on this calculation in lines 195-204. We have added text to the discussion to describe how these assumptions influence our results.

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 atmo- sphere (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 CO2 increase, because that should evolve internally by itself.

ERB: Due to complexity of running the fully coupled model of CAM with CLM in which the terrestrial biosphere impacts on C cycling dynamically interact with the atmosphere, we instead used an offline CLM-FUN run to calculate in experiment 1 the down regulation in NPP and assumed that this carbon that did not go into biomass instead went into the atmosphere. In experiment 2, we then run CAM with CLM or CLM-FUN. We then prescribe a CO2 increase in CAM-FUN and compare it to CAM with CLM only. Despite the lack of C cycling coupling, the resulting impacts of LAI or ET on energy budgets does influence radiative forcing. We have added text to clarify and justify this approach in lines 192-215 as well as text to state that CAM and CAM-FUN start off with the same initial conditions.

If only the atmospheric CO2 concentration in CAM-FUN is increased compared to CAM, the analyzed effects might rather depend on the CO2 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 atmospheric CO2?

ERB: Due to the computational cost of running the fully coupled model, we used the prescribed CO2 approach to assess the sensitivity of the climate system to the C cost of N acquisition. However, in the CAM-CLM and CAM-CLM-FUN runs we were able to assess the impacts of changing climate conditions on NPP, LAI, and ET.

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

ERB: We did not use the slab mixed-layer ocean model and have removed this from the text.

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'?

ERB: We have clearly defined CAM as the Community Atmosphere Model and have provided text to describe its function in lines 150-171.

L122-L125: double reference to forcing data set L124: spatio-temporal vs L139 spatiotemporal

ERB: We have corrected these errors.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-293/bg-2018-293-AC1-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-293, 2018.