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

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ERB: We are grateful to the reviewer for their thoughtful comments and suggestions for improving the manuscript. In the supplement, we include a track change document that shows all of the edits and revisions that we made to the manuscript.

This is a study of the implications of the fact that most (or all) conventional modeling studies do not represent the expenditure of energy (C) by plants on the uptake of N. Apreviously-developed model of plant uptake (FUN) is used with the CLM land surface model to estimate the reduction in NPP as a result of N acquisition. This reduction in terrestrial uptake of C is then converted to a corresponding increase in atmospheric CO2 which is fed into the CAM atmospheric model. Simulations of CAM with and

without the FUN sub-model are used to quantify the impacts of N acquisition on global climate.

Although the manuscript is well-written in terms of the language used, I have serious concerns over the methodology and the information presented. As such I suggest that it requires major revision before it would be acceptable for publication.

At the very least the manuscript needs to do a better job at explaining what has been done (and possible limitations), but it is also possible that further simulations are required (particularly to clarify if the signal is robust).

General comments

One of my main concerns is that I am not sure I understand what the authors did – there is a need for more material in the methods section. A series of complicated modeling systems has been used but few details of the configurations and simulations are provided. I am not looking for 100% reproducibility - that is very difficult to achieve unless the author's github site includes all the configuration files, which I haven't checked – but the paper should provide more details than it does. For example, what were the initial conditions, was there a spin-up phase, what additional inputs are provided?

ERB: We have added more detail to the materials and methods section to address these concerns. We now have text in lines 170-174 and 199-201 that states that the initial conditions, spinup configuration and other necessary conditions needed to run the simulations for both CLM and CAM. All of the configuration and spinup files are the default model inputs that are provided by the National Center for Atmospheric Research. We have also clearly defined the scope of our model experiments to alleviate confusion regarding the coupling of the two models.

The discussion of the results is also very brief with only 35 lines in the Results section.

ERB: In response to Reviewer 1, we have added in global maps of the absolute values of NPP, ET, and LAI for CAM-CLM with and without FUN to the Supplementary Material in lines 244-256. We have also highlighted the stronger impacts of the C cost of N acquisition for temperature than precipitation and greater uncertainty in precipitation estimates in lines 270-279.

Specific comments

Abstract - I would like this to be more quantitative and also give some indication of the nature (and limitations) of the experimental design (e.g. ramped CO2). At present it highlights the changes in "high-latitude" temperature and precipitation, but there are no other numbers.

ERB: We have added in more quantitative information into the Abstract. In addition, we have added a sentence that describes the experimental design.

L67 - It might be useful to add a line or two about the approach used in most climate models, e.g. N is "free" and NPP is simply "snipped" to match the N availability, to contrast with the approach used in FUN.

ERB: We have added text in lines 76-78 to state how typical climate models work per the reviewer's suggestion. In addition, we have added text to lines 80-82 to show how our previous work with FUN contrasts this common approach.

Section 2.1 - I don't expect full details of CLM (those can presumably be found in the literature that is cited) but a brief overview would be useful, particularly for people who have little or no idea what a land surface model is.

ERB: We have added text to provide a brief overview of the CLM and CAM models in lines 124-129 and lines 150-171, respectively.

L102 "we updated the parameters" - It appears that the values of two parameters were changed by about 4 orders of magnitude and this is justified by a description of how the new model is better, but I would like to see more detail/evidence/justification. I haven't read all the literature cited for FUN but I am left wondering why it was necessary to adjust the parameters by so much - or is it just that the results are not very sensitive

to these values? In this area it might also help if the previous work with FUN was summarised - e.g. this is what has been done and found using FUN (coupled with other models?) previously. Can we see "before and after" patterns of, say, NPP, to show the improvements produced by changing the parameter values? If possible the names of the altered parameters should also be given (even if it is possibly obvious to anyone who reads the cited papers).

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 gridcells 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.

L111 CAM - I think this stands for Community Atmosphere Model, which should be explained. "optional slab mixed-layer ocean model" - I'm not so bothered that it is optional, but I do want to know if it is used here. L137 suggests prescribed SSTs were used and if that means no slab model then don't mention it. Is it relevant that CLM and CAM are part of CESM? Again, if not, don't mention it.

ERB: We have deleted this text from the materials and methods as we used prescribed sea surface temperatures as the reviewer noted and did not use the slab ocean model.

Experimental Design - CLM - how was the initial state of CLM prescribed? Was there a spin up? Was land use change included? Again I'm not looking for every detail so that I can definitely reproduce the results, but the reader should get a pretty good idea of what was done - which they don't at present.

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.

Experimental design - CAM - I think that CAM-FUN means CAM with CLM and FUN...but I am not 100% sure. Another possibility is that it means "CAM with extra CO2 calculated from offline runs of CLM-FUN". Either way it needs to be clarified. Why is CO2 ramped up, why not just start from a higher value? I guess the point is that N-acquistion gradually leads to enhanced atmospheric CO2...but on the other hand that is not something that started in 1980 and, ideally, one might have started both runs from a pre-industrial CO2. Why is the full 8.2 Pg C yr-1 added to the atmosphere? In reality only a fraction (40%) of anthropogenic emissions of CO2 remain in the atmosphere, with ocean drawdown a large part of the story, so one might expect that something similar would apply here. I'm a bit confused by the whole approach to CO2 used here, and this is another aspect. From the description it appears that CO2 is prescribed and not interactive in CAM(-FUN) (i.e. CLM-calculated fluxes of C do not change the atmospheric CO2) but this should be clarified. Do both CAM and CAMFUN start with the same amount of vegetation? Clarify what fluxes CLM exchanges with CAM, what is prescribed and what is interactive. All in all the design has to be better explained and justified.

ERB: The reviewer is correct in how we configured the model runs. 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 195-204 as well as text in lines 207-209 to state that CAM and CAM-FUN start off with the same initial conditions.

Results

Are the changes in modeled climate (particularly temperature and precipitation) statistically significant? It is many years since I was involved in a paper that presented changes in modeled climate, but at that time it was considered essential to use an ensemble of runs (e.g. using different initial states) to quantify internal variability, and maps of changes would indicate the statistical significance of the change at each location. The widespread areas of increased temperature in Fig.3a are consistent with the "expected" change and are likely "meaningful", but the much more patchy changes in precipitation (Fig.3b) are less obviously signal rather than noise. If there can be no estimate of significance I think the discussion of changes in atmospheric hydrology have to be couched in much less certain language, with the limitations of the method flagged up. This becomes even more important at regional level.

ERB: Given that we did not do an ensemble of runs, we are not able to evaluate significance. As such, we have added text in the results in lines 273-282 to couch the precipitation results and to acknowledge the low signal to noise ratio in the precipitation results.

Fig.2 and related discussion - I am not very familiar with how radiative forcing is used or calculated, but I am confused by the discussion! How is the radiative forcing from reduced evaporation calculated? Is this just the reduction in the latent heat flux (W m-2)? The caption "warming...was offset..by..reduced evapotranspiration" is rather confusing - with reduced evaporation one might expect increased sensible heat flux (all else being equal) which would have a warming effect. L224 suggests that the ET change resulted in reduced water vapor and implies that that is where the radiative forcing comes from. I think we need better discussion of the energy balance and clarification of the radiative forcing/mechanisms. It might be quite correct but I am sure many readers of Biogeosciences are not familiar with the ideas of radiative forcing.

ERB: We have added text in the methods to explain why we were doing this analysis which lets us see which of these factors had the biggest impact on climate in lines 231-233 and also in the results to state that ET had a cooling effect due to reductions in water vapor in lines 267-268.

I can see that the study represents a "first look" at the implications of the C cost of N uptake on modeled climate - but it is unclear whether the methodology used allows for a meaningful estimate of the impact. Improved description and justification of the experimental design would clarify this, and at least improve the reader's confidence in the design, but at present I am left wondering what the experiment with a relatively rapid ramping up of atmospheric CO_2 (3.8 ppm per year) from an arbitrary start year (1980) actually tells us about the "real world". The authors conceded in L276 that there might be limitations to their method but do not properly enlarge on this. Convince me and I will be happy!

ERB: We have increased the text describing the limitations as well as benefits of our approach in the Discussion in lines 383-391. In addition, we have made substantial changes to the methods to help clarify and justify our approach as highlighted in responses above.

Further details

Title - I don't like this. "Plant-microbe symbioses reveal underestimation" suggests that the symbioses were somehow active or involved in the study. I would rephrase it as something like "Neglecting symbioses leads to underestimation of modeled impacts...".

ERB: We have changed the title to: “Neglecting plan-microbe sysmbioses leads to

underestimation of modeled climate impacts.”

L153 - if the units of dF are W môĂĂĂ2, those of alpha should be the same (not g môĂĂĂ2).

ERB: We have corrected this mistake.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-293/bg-2018-293-AC2-supplement.pdf

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