

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

**Anonymous Referee #2**

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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. A previously-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 CO<sub>2</sub> 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.

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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?

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

### **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 CO<sub>2</sub>). At present it highlights the changes in "high-latitude" temperature and precipitation, but there are no other numbers.

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.

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.

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

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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).

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.

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.

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 CO<sub>2</sub> calculated from offline runs of CLM-FUN". Either way it needs to be clarified. Why is CO<sub>2</sub> ramped up, why not just start from a higher value? I guess the point is that N-acquisition gradually leads to enhanced atmospheric CO<sub>2</sub>...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 CO<sub>2</sub>. Why is the full 8.2 Pg C yr<sup>-1</sup> added to the atmosphere? In reality only a fraction (40%) of anthropogenic emissions of CO<sub>2</sub> 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 CO<sub>2</sub> used here, and this is another aspect. From the description it appears that CO<sub>2</sub>

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is prescribed and not interactive in CAM(-FUN) (i.e. CLM-calculated fluxes of C do not change the atmospheric CO<sub>2</sub>) but this should be clarified. Do both CAM and CAM-FUN 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.

## 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.

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<sup>-2</sup>)? 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.

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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<sub>2</sub> (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!

#### **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...".

L153 - if the units of dF are W m<sup>-2</sup>, those of alpha should be the same (not g m<sup>-2</sup>).

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