

Second review of Shi et al., 2018, “Plant-microbe symbioses reveal underestimation of modeled climate impacts”, Biogeosciences.

In general I find that the revised manuscript is a substantial improvement on the original submission, and I consider that it is suitable for publication after minor corrections. In particular the “Material and Methods” section has had material added to better describe the series of model simulations. The revised title is also clearer.

Although the authors have introduced various caveats about the limited nature of their climate model runs (2 runs starting from the same initial conditions are compared over a 10 year period), some of the discussion is still rather limited in this respect (e.g. no explicit mention of internal variability of the model, nor of possible trends related to a common initial state). This might be a deliberate attempt to suit the intended audience, or might possibly reflect the authors’ backgrounds and interests. Given the intended audience and the stated aim being to provide a first estimate of the implications of neglecting the C costs, I consider that the level of detail provided is probably sufficient. Although we need to be wary of the details of the 10yr (climate) changes described (wary of the precise quantification) I have no reason to doubt that the effects are real (possibly with different magnitude).

Minor comments

L89-91 “we imposed a simplification” – this sounds like you actively modified CAM but I think what is meant is that you did not use interactive ocean or sea ice models (rather you prescribed SSTs and ice amounts). Rephrase along the lines of: “we used CAM with prescribed sea surface temperatures and sea ice, and introduced symbiotic processes...”.

L146 – Ideally we might also get more details of the experimental setup for these early simulations of CLM-FUN, though I suspect they followed the configuration used later in section 2.2. As a minimum I suggest adding something to indicate that the downregulation of NPP referred to contemporary or recent conditions, or 1995-2004, or whatever. Or if both sets of CLM simulations shared common details, move them to here .

L209 and following – Again, I assume that the CAM runs were for 1980-2004, as was used for CLM – but this should be stated. And so all later averages from CAM refer to the years 1995-2004?

L262 – What are the bounds (latitudes?) of the various areas used to calculate averages, e.g. high-latitude, mid-latitude, tropical low latitude? Add these after each, e.g. high-latitude ecosystems (60-85N). Otherwise we have these rather vague descriptions of areas next to precise statistics of changes.

L305 and others – I don’t find these statistics of regional changes in precipitation convincing, or useful, given that they are based on only two 10-yr simulations. Personally I would consider removing much of L300-305. However, as the aim of this paper is to provide first estimates, not a detailed account of atmospheric changes, it is probably acceptable to leave the statistics in the manuscript.

L312 – I think this is the first mention of using 10yr statistics from CAM (see earlier request for clarity on this). Also rephrase along the lines of “temperature increased by 1degC over 10 years and precipitation increased by 9 mm yr⁻¹”.

L341 – -5.2 W m⁻² should be -0.52 W m⁻².

Figures – captions should say that they refer to 10 year averages (or whatever).

Table S1 – I can't see where footnote a is referenced. I suspect it applies to (at least) the middle column.