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Biogeosciences Discussions

# *Interactive comment on* "A model investigation of vegetation-atmosphere interactions on a millennial timescale" *by* N. Devaraju et al.

**Anonymous Referee #1**

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The work presented in this paper is very interesting for the debate about vegetation-atmosphere feedbacks. As albedo and transpiration can both have positive vegetation-atmosphere feedbacks, it is a valid question whether these positive feedbacks are strong enough to switch to different steady states. Modeling studies with different model setups and terrestrial schemes are very much welcome to find out if different steady states will be possible. As different outcomes are expected depending on the type of terrestrial models, it is important to understand how vegetation is modeled. In this manuscript I have two major issues which should be solved, and a number of more specific comments that could improve the manuscript:

1) NPP calculation: The part what I do not understand is why the NPP is immediately at the level of a mature forest. Apparently there is no succession in the model and the



NPP is independent to the amount of biomass present. The authors say that (pg 8768, L8-10), the step like behavior of NPP at year 100 is due to rapid increase in GPP since tree PFTs have higher LAI).

a) I can not see why the authors say a rapid increase in GPP, as it is I think a direct step.

b) This direct step need to be explained. From an ecological perspective, I would expect that GPP (and therefore also NPP) is density dependent, so dependent on the amount of biomass. It now seems to me that if climate conditions are optimal for trees, then NPP is directly in its maximum even if trees are not yet present (see the low biomass). The authors should make this clear and add the equations in the manuscript. (In the Kucharik et. al (2000) paper they use an approach that canopy photosynthesis is proportional to absorbed PAR. Did the authors use this approach and if so, what are the limitations?)

2) Anomalies and uncertainty plots (fig 2 and 3). The seasonal anomalies in temperature and precipitation shown in Figure 2 are quite large while only a few cells show significant differences. From the text I understood that the authors have made a significance test on seasonal averages, which is appropriate because we want to understand the effect on vegetation growth (seasonal to centennial effect). However, the anomalies are quite large. For instance an anomaly of 5 mm/day (in the ITCZ zone) in precipitation for both winter and summer is very large but not significant different? This while on average 2 m/year (about 5 mm/day) of precipitation in the ITCZ zone is recorded. If the two model experiments with these large anomalies have no significant differences, this could only be explained if the standard deviations between the years are very large. However, on yearly average the fluctuations in precipitation and temperature are quite low (see figure 1). Please can you clarify this? Or is the significant test based on daily values (so the standard deviations between the days)? If so, I would say that this is not an appropriate term as we are interested in the vegetation growth. I would suggest to make an extra panel with absolute values of temperature and precipitation and if the

two model runs are significant the same, also show maps of the corresponding large uncertainties.

Next to these two major issues, I have a couple of questions that could be used to improve the manuscript and discussion on climate-vegetation interactions and feedbacks:

3) The objective given at pg 9764 is a bit strange: Our study is the first that performs a millennial time scale simulation using a comprehensive coupled model. . . Dekker et al (2010) did a similar kind of experiment, although a simpler GCM was used, so this paper is not the first. Of course you can debate about the word comprehensive, but the authors also say that their model is not a comprehensive one (pg 8768). Second Brovkin et al. (2009) did use a comprehensive model, but the period of interactive vegetation was 500 years. I would say that it is not the simulation length or the complexity of the model that matters and is interesting for this paper. A better objective could be that it is needed to use multiple models with different ways of modeling the vegetation to understand the when and how positive climate-vegetation feedbacks will lead to multiple steady states.

4) The paragraph at page 8771 (L20-25) is unclear to me. Is it that if you have less PFT's the climate conditions do not overlap and the multiple steady states are due to sudden switches in PFT's? Do the climatic envelopes between the PFT's have big differences? Then, of course it is true that if you have more PFT's, the sudden shifts are less in magnitude, so then you will have less chance of multiple steady states. This paragraph does only make sense if it is clear what the authors and Kleidon et al. imply.

5) Figure 1: are the values of temperature and precipitation global averages or averages above land? In this paper I would be interested in the annual means of the land cells. The differences in annual precipitation values at YEAR 100 between the two runs are low (2.76 and 2.88 mm/day, on annual basis 1.0 m/y and 1.05 m/y). As the differences in biomass are large (fig 1c) it means that the moisture recycling due to vegetation is very low in this model setup. It would be nice to elaborate this in the



discussion and compare this with other models. Did you also find relative small differences for the tropical regions in moisture recycling? If so, then I would conclude that CAM-IBIS simulates low moisture recycling and therefore has low sensitivity to the precipitation-vegetation feedbacks.

6) P8769L5. What do you mean with 1.6 and 1.7%. Which is 1.6 and which is 1.7? Is this the percentage of all cells or only land cells? As the experiment is focused on the land, I should calculate the effect only on land cells.

7) In the introduction, the authors explicitly want to address the question of the time scales (P8763, L16). In the abstract the authors say that the regrowth takes such a long time that other processes will be involved. However, I can not see how the time scale of regrowth is tested. Is the current parameterization of the model appropriate to say something about these time scales? If not, then do not mention these two questions (how long will it take for the climate system to reach a new equilibrium in such a case? What determines the time scales?). If yes, discuss these questions in more detail.

8) P8770, L17. Why is it that Tundra performs poor? Is it that only a small change in climate will switch from a tundra PFT to another one? Please explain

9) The fact that IBIS2 is used, is only mentioned in the discussion. Please also mention this in the Methods

Typing errors: 10) Change MPIESM into MPI-ESM

References Brovkin, V., Raddatz, T., Reick, C.H., Claussen, M. and Gayler, V., 2009. Global biogeophysical interactions between forest and climate. Geophys. Res. Lett. 36, 5. Dekker, S.C., de Boer, H.J., Brovkin, V., Fraedrich, K., Wassen, M.J. and Rietkerk, M., 2010. Biogeophysical feedbacks trigger shifts in the modelled vegetation-atmosphere system at multiple scales. Biogeosciences. 7, 1237-1245.

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