

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

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Received and published: 25 November 2011

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:

We thank the reviewer for appreciation of our modelling study and for helpful com-

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ments. The comments and suggestions helped us to improve the manuscript. The detailed point-by-point response for the comments is given below.

(Also, Please see the supplement for the revised manuscript and figures).

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 cannot 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?)

Reply: a) Agreed, we changed “rapid increase” to “step-like increase”

b) We modelled the evolution of NPP using exponential fit for LCC simulation and found a fast time scale of about 2 years (Also, see the Reply to Reviewer #2 comment 2 on evolution of tree cover fraction). This is related to the time scale of establishment of biomass in leaves in IBIS2 which is 2 years. This is now discussed in 3rd paragraph of the results section.

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

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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.

Reply: Many thanks to reviewer for this careful observation and comment. We found an error in the analysis program. This oversight is rectified now. The redrawn figure 4 (originally Fig. 2) is now shown in the revised manuscript. The differences in precipitation are only of the order of 0.1 mm/day as opposed to 5 mm/day in the original draft. Accordingly, we have modified the discussion for Fig. 4

3) The objective given at pg 8764 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.

Reply: We agree with the reviewer that the science is more important not the complexity of model or the simulation length. In the revised manuscript, we write “Our objective is to use another model to simulate the vegetation dynamics to understand whether, when and how positive climate-vegetation feedbacks will lead to multiple steady states. We perform a millennial time scale simulation using a comprehensive coupled atmospheric general circulation model and a terrestrial biosphere model to investigate the possibility of multiple states” in the last paragraph of the introduction section.

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.

Reply: Yes, Kleidon et al. 2007 find that if there less number of discrete PFT's the climate conditions do not overlap which can result in the occurrence of multiple equilibria. Vegetation activity strongly modulates the exchange of energy and water between land surface and atmosphere. With an increased number of classes, the difference between the numbers of multiple steady states diminishes, and multiple states disappear completely when vegetation is represented by 8 classes or more in their model. We have now discussed in the introduction (5th paragraph) and discussion (4th paragraph) sections in a much clearer manner.

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 dif-

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ferences 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.

Reply: Many thanks for the suggestion which identifies one possible reason for the absence of multiple equilibriums in our simulations. As suggested by the reviewer, we now show land-mean precipitation and temperature in Fig. 1 instead of the global means. We added a paragraph (sixth) in the results section: “The differences in annual precipitation values between the two runs at year 100 after trees are allowed to grow are low. . . .”

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.

Reply: Thanks for this comment. The revised manuscript has numbers for both global mean and land mean when discussing Fig. 4.

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 re-growth takes such a long time that other processes will be involved. However, I cannot see how the time scale of re-growth 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.

Reply: We have explained the time scale in the revised manuscript using equation (1), Fig.2 and Table 1. Please see the response to comment #2 of reviewer #2.

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.

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Reply: Good point. In the revised manuscript, we write “The low value of Kappa for Tundra is partly related to the smallest area occupied by Tundra in the control experiment (changes are relatively bigger for Tundra) and partly because the vegetation dynamics has not yet reached equilibrium in the high latitudes (Fig. 2d).”

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

Reply: Thanks. Changed IBIS to IBIS2 throughout the manuscript.

10) Typing errors: Change MPIESM into MPI-ESM Reply: It is corrected in the revised manuscript.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/8/C4562/2011/bgd-8-C4562-2011-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 8, 8761, 2011.

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