

## ***Interactive comment on “Multiple steady-states in the terrestrial atmosphere-biosphere system: a result of a discrete vegetation classification?” by A. Kleidon et al.***

### **Anonymous Referee #3**

Received and published: 15 April 2007

The manuscript by Kleidon et al. addresses an important question of multiple states in the climate system. The authors present a conceptual model of multiple steady states in the vegetation-atmosphere system which resembles conceptual model by Brovkin et al. (1998). This model is reformulated in discrete form and applied for interpretation of coupled Planet Simulator - SIMBA simulations with different numbers of vegetation classes. Their conclusion that using discrete vegetation classes instead of continuous parameterization can lead to artificial steady states is a bit trivial, it can be easily demonstrated without using such a complex model. Kleidon et al. can enrich the paper substantially by providing examples of quantification of their conceptual model for areas where atmosphere-land interaction is especially strong, e.g. for semiarid or bo-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

real regions. They also need to discuss limitations of climate model parameterizations because a presence of multiple steady states in coupled system may depend on land surface parameters such as bare ground albedo (Claussen, 1994).

### Specific comments

Page 3.

“Woody cover depends on vegetation productivity” - does it mean that all vegetation is woody?

“In semiarid regions it is limited primarily by the availability of water, and is therefore a function of precipitation P.” What is limited here: woody cover or productivity? In reality, the woody (tree/shrub) cover in semiarid regions is controlled not only by precipitation, but also by disturbances (fire, grazing) and soil feedbacks (shrub island effect). Justify your statement on woody cover dependence on precip or correct it (see comments below).

Presentation of the conceptual model is very similar to the model description by Brovkin et al. (1998, pp. 31,615-31,616). Please give proper credits to the previous work and explain what is new here, e.g. a discretization of vegetation parameterization.

Page 4.

Why the Planet Simulator has been used without interactive SST/sea ice? Prescribed SSTs cut-off many important feedbacks between climate and land cover, such as sea ice-albedo feedback or water vapor feedback. The latter may change a sign of climatic impact of tropical deforestation from warming with prescribed SSTs to cooling with interactive SSTs (Ganopolski et al., 2001).

It is not entirely clear whether SIMBA is a model for woody vegetation only or not (see also comment above). I could not find a value of biomass time scale,  $\tau_{veg}$ , in the reference paper (Kleidon, 2006a). If biomass turnover time is about decade, could it be interpreted as non-woody vegetation? I do not think so. Explain the difference between

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

terms “vegetation” and “woody vegetation” (cover) in your model and do not mix them up.

What is “leaf cover” (Fig. 2) - is it leaf area index scaled between 0 and 1? Why leaf cover is too low in tropical regions?

Page 5.

From the statement "... mean vegetation biomass is proportional to mean productivity in our model" and Eq.1 which parameterizes woody fraction a function of vegetation biomass (all or woody only?), it follows that the woody fraction in the model is a function of productivity. Consequently, woody fraction dependence on precipitation on Fig 1 is, in fact, a dependence of productivity on precipitation. This is correct for drylands and gives enough justification to the conceptual model described on page 3 assuming that the woody fraction is a function of productivity (although this is not always correct, see comments above).

Intermediate vegetation steady states could exist because feedbacks on local level absent in the coupled model. The point that model does not capture them doesn't mean these states do not exist in reality (von Hardenberg et al., 2001, Dekker et al., 2007).

Page 6. Results section.

There is no explicit use of the conceptual model in the discussion of the model results hereafter. Why? It would be very interesting to see results of the coupled model plotted in terms of conceptual model (woody fraction against precip or temperature), at least for drylands and high-latitude regions.

Page 7. Conclusions.

"Multiple steady states in the vegetation-atmosphere system may simply be model artefacts that disappear if the full complexity and heterogeneity in vegetation form and functioning is represented in the model." The first part of this statement - “multiple steady

states in the vegetation-atmosphere system may simply be model artefacts” - is too obvious. A quick look at Fig. 1 is enough to understand it. The second part - “... that disappear if the full complexity and heterogeneity in vegetation form and functioning is represented in the model” - is not convincingly proved in the paper. The vegetation model presented here does not include explicit presentation of plant functional types and cannot pretend to reflect “the full complexity and heterogeneity in vegetation form and functioning”. Treatment of woody cover as a diagnostic function of productivity is a serious SIMBA limitation, and its consequences should be discussed in the paper. The authors should also discuss limitations of their climate model setup and effects of land surface parameterizations on existence of multiple states in the coupled model. For example, could higher values of land surface albedo in Sahara lead to multiple states there, like in the ECHAM-BIOME experiments by Claussen (1994, 1998)?

“Second, a discrete representation of vegetation as is typically done in dynamic global vegetation models in terms of plant functional types ...” - this is a wrong interpretation of DGVM concept as presented in the paper by Cramer et al. (2001) which is not cited here. DGVMs were developed as a next step from discrete vegetation models (biome-type models) to models with fractional mixture of different plant functional types (PFTs) as needed for coupling with climate models. In addition to equations for carbon cycle dynamics, DGVMs include equations for dynamics of PFT individuals or PFT fractional areas. These equations are dynamic (prognostic) and not diagnostic as Eq. 1 for woody fraction used in SIMBA.

“Second, a discrete representation of vegetation ... seems to result in a general under-estimation of terrestrial productivity”. There is no clear explanation why using discrete classes should decrease and not increase the productivity. I think that this result depends on the truncation scheme (Eq. 2) used in the paper. In accordance with this equation, woody classes are truncated depending on land productivity, which is unevenly distributed across the globe. It might be simply that in the “n=2 simulation” most of land is prescribed as non-woody type. This may include areas with strong bio-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

physical feedbacks, i.e. semiarid regions and treeline boundaries. “Deforesting” these low-productivity regions in the “n=2 simulation” results in positive climate feedback and further productivity decline. This is just a speculation, but since the authors call this conclusion “an important implication”, they should shed more light on the mechanisms behind it and prove that this is not an artefact of their discretization approach. For example, they can plot maps of woody classes in experiments n=2 and n=8 so that readers can immediately see implications of truncation method for woody vegetation cover.

Fig 3. “Climate sensitivity of annual mean land averages” - climate sensitivity is a term reserved for temperature change in response to some forcing, such as CO<sub>2</sub> or insolation. What is presented here is not climate sensitivity but fluxes in different model simulations.

Fig 4. “Climatic differences in vegetation biomass for (a) ”full vegetation” - ”bare ground” - this term is very misleading. These are just differences in biomass due to climatic change.

Figs. 4-6. Using gray scales for both negative and positive values makes these figures almost useless. Why not to use colors for these figures?

#### References

Cramer W., A. Bondeau, F.I. Woodward, I.C. Prentice, R.A. Betts, V. Brovkin, P.M. Cox, V. Fisher, J. Foley, A.D. Friend, C. Kucharik, M.R. Lomas, N. Ramankutty, S. Sitch, B. Smith, A. White, C. Young-Molling, 2001. Dynamic responses of global terrestrial ecosystems to changes in CO<sub>2</sub> and climate. *Global Change Biology*, 7, 357-373.

Dekker, S.C., Rietkerk, M., and Bierkens, M.F.P., 2007. Coupling microscale vegetation-soil water and macroscale vegetation-precipitation feedbacks in semiarid ecosystems, *Global Change Biology*, 13, 1-8, doi: 10.1111/j.1365-2486.2006.01327.x.

Ganopolski A., Petoukhov V., Rahmstorf S., Brovkin V., Claussen M., Eliseev A., Ku-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

batzki C., 2001. CLIMBER-2: a climate system model of intermediate complexity, Part II: model sensitivity, *Climate Dynamics*, 17, 735-751.

von Hardenberg, J., Meron, E., Shachak, M., Zarmi, Y., 2001. Diversity of vegetation patterns and desertification. *Physical Review Letters*, 8719, doi: 10.1103/PhysRevLett.87.198101.

---

Interactive comment on *Biogeosciences Discuss.*, 4, 687, 2007.

**BGD**

4, S311–S316, 2007

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper