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Biogeosciences Discussions

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 #2

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This manuscript documents evidences from both conceptual and numerical modeling to support the argument that multiple steady states in a terrestrial atmosphere-biosphere model can potentially result from discrete vegetation classification. The authors first made use of a conceptual diagram (Fig.1), an approach that has been used in several previous studies, to show conceptually how a discrete representation of vegetation can potentially lead to multiple steady states; the authors then altered the estimation of vegetation fractional cover in a simple vegetation dynamics model to map biomass in the model to a finite number N categories, and showed that when applied to the Planet Simulator, such discrete representation leads to spurious model sensitivity to initial conditions when N is small (which gives rise to multiple steady states). When

N is larger than 7 or so, such sensitivity to initial conditions disappears. Note that the control version of this specific model (which is the version with continuous vegetation fraction formulation) has only one steady state. The potential existence of multiple steady states and its model dependence is a very important topic with significant implications for multiple disciplines. The authors pointed out one possibility (out of many) where multiple steady states occur spuriously. This will provide important insight and food for thought to many researchers interested in this topic. However, there are major problems in this manuscript that have to be addressed before it can be published.

Major Issues:

1) Page 3, 2nd and 3rd paragraphs, and Figure 1 together: The approach of using the two curves/lines in locating the steady states of a coupled biosphere-atmosphere system, as described in these two paragraphs and illustrated in Figure 1, has been used in the past by other researchers, specifically by Brovkin et al. (1998) and Wang (2004). However, the authors did not indicate this in the presentation and readers can be misled to think this is the authors' new contribution. Given how closely related the works of Brovkin et al (1998) and Wang (2004) are, the authors should describe/introduce the relevant results in these two previous studies.

However, the use of a step-wise relationship between vegetation and precipitation (as a result of discrete representation of vegetation) is the authors' new contribution. This should be made clear to the readers, by including something like "Previous conceptual modeling studies assumed a continuous W=f(P*) relationship (e.g., Figure 1 b). However, with discrete vegetation representation, this relationship becomes a step-wise function (Figure 1a)".

2) Pages 7-8, last paragraph of this paper: The statement about discrete representation of vegetation in dynamic global vegetation models (in terms of plant functional types) is wrong. The authors' suggestion that MSS in models using dynamic vegetation models that represent vegetation in terms of PFTs is therefore wrong and unfair.

BGD

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Interactive Comment

In all dynamic global vegetation models I used or knew of (e.g., LPJ, IBIS, CLM-DGVM, ED), vegetation at any specific location is a combination of different PFTs (with different spatial coverage), and the combination results from the competition of different PFTs under the same climate forcing. This representation of vegetation is therefore not discrete, and the corresponding W(P*) relationship is therefore a continuous line. In fact, on Page 3, the authors indicated that "A continuous parameterization for W=f(P*), which would account for a mixture of vegetation types and its diversity within a region, would likely lead to a smooth line". For example, the multiple steady states in Wang and Eltahir (2000a,b) are from a coupled biosphere-atmosphere model with IBIS being the dynamic vegetation model. Such MSS are therefore not due to model artifacts as the authors suggested here.

3) Given 2), the question then becomes: Under what conditions will the authors' finding apply? Or in what type of models are the authors' findings relevant? To prove that their work is relevant to the numerical modeling community, the authors need to identify at least one vegetation model that is discrete in representation.

Other comments:

Page 3, bottom paragraph: First of all, there is nothing wrong in this manuscript. However, the authors can be easily more informative and serve the readers better by indicating that, based on existing literature (Brovkin et al 1998 and Wang 2004) and the author's Figure 1, one can get MSS with a smooth W=f(P*) relationship (with a steeper P=g(W*) relationship) (as was in Brovkin et al. and Wang); one can still get MSS even in absence of a steep P=g(W*) relationship by using a step-wise function for W=f(P*).

Minor:

Page 2, "Multiple steady states in this transition region Ě" Which transition region? I guess the authors meant "West Africa" as the cited references are about that region.

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