**Earth System
Dynamics
Discussions**

Interactive comment on "Detecting hotspots of atmosphere-vegetation interaction via slowing down – Part 2: Application to a global climate model" *by* S. Bathiany et al.

Anonymous Referee #1

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In this second part of a two-part paper a detection method for local bifurcations in an extended system is applied to a coupled climate-vegetation model. The method for "hot spot detection" was presented in the separate Part 1 paper.

The focus of the paper is on the possibility of jumping between two (local) states of wet-vegetated or dry-deserted climates. The method of detecting hot spots cannot be directly applied to the full climate model, so a simpler low dimensional model based on parameter fitting via linear regression from the climate model output is introduced. This method of hot spot detection can be applied to the simpler model, and the results subsequently interpreted using the (more realistic) climate model.

Since climate models are highly complex and outputs often as difficult to interpret as observations, the approach of introducing simpler models in order to better understand the behavior of the climate models –and hopefully the real climate- is most welcomed. The paper can thus be published after considering the points below:

The model contains a set of discrete coupled grid cells. A discussion of the continuum limit (increasing resolution of climate model) of the hot-spot idea would be relevant. This might also relate to the fundamental problem with climate models apparently incapable of showing multiple states.

In the manuscript this is discussed in connection with the fact that the hotspot detection scheme cannot be applied directly to the climate model. Especially the sentence in p 688, bottom seems to me to be central. The "claim" is that there might be multiple states, but internal variability is too large for the system to settle in any of these states. On which grounds can one argue that they are there?

The statement "Insofar, the prerequisit for an application of EWS-based analysis are in conflict with the case of PlaSim-VECODE-tr." and applying the method to the regression model as "an intermediate step" is unclear. As I understand it will never be possible to apply the method to the climate model, thus applying it to the regression model is the final step, where the results are then interpreted using the climate model. I might be mistaken, please clarify.

Building simple effective models from GCMs is an important task. The RM1 and RM2 are interesting. It would, however, be nice to be able to judge their quality in equation (2) from scatter plots of P vs. V from which s_i and k_{ij} are obtained. Minor points:

I do not particularly like the term "toy model" (or "surrogate" for that matter) to characterize the regression model. Consider "simple model", "low order model" or just "regression model".

Reference to a figure in another paper (not Part 1) is annoying. It would be nice with a

P(recipitation)-V*(egetation) plot here. In this case perhaps with the modified VECODE shape of V*(P) (and thus omitting eq (1) which is not in use here (?)

A discussion of the difference in running the model in the transient and the equilibrium modes would be nice. Especially what is the physics behind the different time scales of the vegetation.

Fig 1: "light red" is referred to as "orange" in text. "purple" is blue on my printer, consider helping readability by using less specific color nuances.

Fig 2: Would you dare indicate curves for the supposed unstable steady state? (in panel 1 beginning at 0.3, merging with the stable equilibrium around 6K?)

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