

## ***Interactive comment on “Detecting hotspots of atmosphere-vegetation interaction via slowing down – Part 1: A stochastic approach” by S. Bathiany et al.***

### **Anonymous Referee #1**

Received and published: 29 August 2012

In this manuscript, the authors are presenting a method for detecting which part of a physical system is responsible for a loss of system stability. This work could therefore be a very useful contribution in the climate modeling community, where it is often difficult to disentangle the causal relationships in strongly coupled systems. This is quite a technical paper that describes in detail the new methodology designed by the author. The manuscript is well written, though some clarifications or some additions would sometimes be useful in order to improve readability. I am therefore asking for some minor revisions before publication in ESD.

General comments.

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1 - Readability of the manuscript. The paper appears very technical and is sometimes difficult to follow. Though this kind of paper is of course necessary, some additional efforts would be appreciated in order to help the reader. The authors have done already quite a good job at describing the algorithm in Fig.6 and section 4.2. But one difficulty is to present afterwards the alternative versions of the method : These options are not always identified, introduced or justified in an appropriate fashion. This is partly due to section 4.2 being used on a specific example (additive noise ...), while successive changes are introduced afterwards. This generates confusion. For instance: - section 4.3, line 5: there is a list of 4 modifications (steps B2, B3, B6, C in fig.6). But it appears later on that this corresponds to 2 optional settings only (B2 = computation of EOF; B3+B6+C = referred after as elimination rule). A new acronym ER 1 or ER 2 is used afterwards (line 21 page 660) but is introduced only in the Table caption 4 (which was difficult to find...). In this section, there is no justification for using covariance instead of correlation for calculating the EOF (step B2): This change appears therefore quite arbitrary. - section 4.4: a new possibility emerges here in the performance analysis: using variance increase instead of lag-1 autocorrelation as EWS. Again this introduces confusion: it is likely that results presented before (eg. Fig. 7 and Fig. 9) are using the first EWS (lag-1 AC) but it is not clear at all for eg. Fig.10 (I can't find which EWS was used here). I would suggest introducing the 3 different flavors of the method (EOF, ER, EWS) from the beginning (section 4.2) with its corresponding justification, and a clear identification afterwards of which option is used. In this spirit, Table 4 and 5 could be merged and the experiments discussed in the text and figures could be highlighted in this table. This would summarize options and parameter settings in a single location for easy reference.

2 - Relevance and applicability. The method is applied here on very simple examples (25 boxes) but it is designed to be applicable on more realistic, larger systems, in particular climate model results. Though I understand that this will be part of a second paper (= Part 2), it would be useful in this first paper to have a feeling of numerical constraints for this new methodology. Will the method still work with 100 or 1000 or more

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boxes ? The hotspot detection has been performed for a 500-members ensemble of 2000-yr long simulations. Would this still be the case for a climate model ? How would these numbers scale with the size of the system ? I think these additional informations would be useful in a generic context and should be presented in this manuscript, without a specific application in mind like the Holocene vegetation transition using an EMIC model.

Specific comments.

- page 645 line 11: "the saddle-node bifurcation where an eigenvalue approaches 0". The fact that an eigenvalue approaches 0 is not specific to the saddle-node bifurcation.

- page 646 lines3-6: I don't understand what the authors mean by "the sampling error of EWS". I guess this will only be understood by the specialists ... Please explain.

- Fig.1: what is  $P^*(V)$  ? I guess it should be  $P(V^*)$  ? - page 648:  $P_d$  is not introduced in the text, but only in the Figure caption. I think it would be more relevant to have the opposite:  $P = P_d + k V$  in the text (as equation 1), and the precise shape of the sigmoidal  $V^*$  function in the figure caption (which is not necessary to understand the following model description).  $P_d$  is the control parameter of the bifurcation : it should be introduced properly.

- p 648: equation (2) is not homogeneous (please introduce time step  $\Delta t = 1$ ). - p 649: equation (3): why not using superscript  $t$  like in equation (2) ? this would help understand the time dependance of parameters.

p 653 line 24: Better introduce  $J$ . For instance, add here " $J = 4$ ". This seems obvious, but  $J$  is used afterwards in the method description, which requires a second reading to catch its definition.

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Interactive comment on Earth Syst. Dynam. Discuss., 3, 643, 2012.