Reply to anonymous referee 1

We would like to thank our referee for the very helpful comments which will improve our manuscript! If not stated otherwise, our references to pages, figures, etc. are based on the submitted, not the revised manuscript in order to match the references of the reviewers.
We quote the referee's comments one by one, each followed by our reply in italic font.

1. (…) 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 succesive 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 = refered 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.

We understand the reviewer's arguments and have done exactly as suggested.
Sect. 4 is now structured as follows: As before, we introduce system 3 in Sect. 4.1. In Sect. 4.2 we introduce the general framework of hotspot detection which is the same for all cases. In Sect. 4.3 we then introduce the different versions of the method and discuss their advantages and limitations. Both, Sect. 4.2 and 4.3 refer to a simple example which is given in Table 3 and a new figure (now Fig. 7).
All settings are now listed together in Table 4 (previously Table 4 and 5) and the different choices are numbered to be referred to in a simpler and more precise way in the text.
The justification with regard to the choice of EOFs is also included in Sect. 4.3.
We also note that our example in Table 3 has changed as it is more instructive to present three different types of elements.

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 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.

We argue that the absolute size of the system is not essential for our method. It is more important that the different areas have to be distinguishable (very different magnitude of slowing down) to detect an area as a hotspot. If this is not the case, nothing will be detected, which is also a result. To make this clearer we have added at the end of our new Sect. 4.3:
"The detectability of a hotspot, given a specific length of the time series, very much depends on intrinsic system properties like its connectivity and the strength of the destabilising feedback. The more elements contribute to a hotspot, the more difficult it is to detect. n_max should be chosen large in such a case to determine the large extent of the hotspot which slows down the algorithm. More importantly, the stronger the slowing down and the better the elements can be distinguished, the easier the hotspot detection. Our system 3 may already provide a rather demanding case as several 10000-100000 yr long time slices are required for a robust hotspot detection. This time is rather beyond feasibility for climate models of intermediate or high complexity and a hotspot like in system 3 would hardly be detectable. However, if hotspots of a more pronounced structure exist, they could be detected more easily. As an example, consider the most optimistic case of an univariate process where autocorrelation increases substantially over time. This increase would be detectable within the order of some 100 time steps (Ditlevsen and Johnson, 2010). Part II of our two-part paper presents a hotspot detection from climate model time series of hundreds to thousands of years length as another example. It is therefore not possible to provide a general statement on the required length of time series. The required length depends on the nature of the potential hotspot, the exact thing that one aims to infer with the analysis. However, this problem does not impose any restrictions to the applicability of the method, but it implies that a negative result can either be due to the non-existence of slowing down at a hotspot or to too short time series."

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.

We agree and have changed the formulation to make this clearer: "When the system's stable fixed point loses stability when approaching a local bifurcation (e.g. a saddle-node bifurcation), an eigenvalue approaches 0 (if time is continuous)."

- 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.

We now write less concisely: "This problem becomes worse close to the tipping point (for example see Dakos et al., 2012) because the uncertainty of an estimate from one sample of a fixed number of data points increases. In statistical terms, the sampling variances of the estimators of variance and autocorrelation increase with autocorrelation (Priestley, 1981)."

- Fig.1: what is P*(V) ? I guess it should be P(V*)?

With P(V) we mean equilibrium precipitation, which corresponds to Eq. 3 but without noise. In our figure caption we now add P*: "Blue lines: equilibrium precipitation P* as calculated from ...". Furthermore we indicate P* with a bracket under Eq. (3) and in the text we add: "Also following Brovkin et al. (1998) and Wang (2004) we call P*(V) the equilibrium precipitation at a particular location (Fig. 1). P* can be interpreted as precipitation in the noise-free case or as the long-time mean when vegetation cover is fixed at a permanent value."*

- page 648: Pd is not introduced in the text, but only in the Figure caption. I think it would be more relevant to have the opposite: P = Pd + 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). Pd is the control parameter of the bifurcation : it should be introduced properly.

We agree that Eq. 1 is not so essential and could be replaced by a reference to Bathiany et al., 2012 and Fig. 1. However, it must be noted in the article that only the third case (with P1<P<P2) applies in our time series and that we set GDD0 to 10.000 (delta=9100) at all elements. To explain this could be confusing if the according equation is not provided. We therefore argue to keep Eq. 1 in the article. Concerning Pd, we introduce Pd on p. 649, lines 9-12, and explain that in our models, it is not the bifurcation parameter because of the spatial heterogeneity. The bifurcation parameter is B. To clarify this, we add: "Hence, in all systems analysed in this article, B is the bifurcation parameter, determined by one single number."

- p 648: equation (2) is not homogeneous (please introduce time step delta_t = 1).

We agree and have changed this.

p 649: equation (3): why not using superscript t like in equation (2) ? this would help understand the time dependance of parameters.

We agree and have changed this.

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.

We have added this information in the text: "We now explain our method of analysis by applying it to system 3 with additive noise. The analysis is applied to J preferably long stationary simulations for fixed but different forcings Bj (j=1, 2, ..., J) below the Tipping Point. Here we choose time series of vegetation cover for B1=150, B2=90, B3=55, and B4=43 (hence J=4; vertical dashed lines in Fig. 5) with 100000 yrs each."