Review #3

Reply: We thank the reviewer for their helpful comments. We are pleased to hear that the manuscript is found to be highly interesting and important in the subject matter. We believe that the issues that the reviewer has raised are generally straightforward to address satisfactorily, and we have substantially revised the manuscript to this effect. Please find below a detailed point-by-point response to all comments (reviewers' comments in black, our replies in blue). N.B Since the reordering and restructuring of the manuscript was substantial, we have written bullet points of our major changes to the manuscript, rather than including a 'track changes' document. Line numbering refers to the revised manuscript, attached as a supplement to the Editor Response.

Major changes:

- Altered the abstract to reflect the new structure of the manuscript.
- Added a clearer 'Aims' section.
- Provided more detail on the potential model.
- Altered the structure, separating the Results and Discussion sections and ensuring a consistent structure in the added sub-sections throughout the manuscript.
- The manuscript now follows a more logical format, with tipping point analysis of the entire speleothem sequence followed by the potential analysis results.
- Reordered the figures and added a paragraph of text to explain each figure sequentially.

In their manuscript, the authors present and discuss the application of two methods for the analysis of time series in nonlinear dynamical systems to palaeorecords of the East Asian Summer Monsoon. The authors hypothesize that there is a bifurcation in the monsoon system and attempt to detect critical slowing down in the time series data as an indicator for such a bifurcation. In addition they apply potential analysis to approximate the system. The authors report that they find critical slowing down for one case of an abrupt monsoon transition, but not for other instances. Overall I found the manuscript highly interesting and important in the subject matter, but not convincing in presentation. The presentation needs major improvements before being acceptable for publication in cp. I recommend major revisions before publication, though a rejection might also be warranted.

Generally the manuscript is lacking clarity, on both the macro and the micro level. On the macro level, it is unclear to me, what the take-home message from the manuscript is. Do the authors want to test the Schewe/Levermann model and confront it with data? This is indicated in the abstract and in sec. 2.1, but is not reflected in the conclusions. Or do the authors just want to test the palaeodata for early warning signals (EWS)? The main conclusion seems to be that the data is not of sufficiently high resolution, a rather weak statement, and more or less trivial. Are there other conclusions than too low a resolution of the data? On the micro level, the methodology is not clearly described in all respects, and most figures are described only superficially, with discussion of some parts of figures completely missing or lacking in depth.

Reply: We have restructured the revised manuscript, and added sub-headings to help guide the reader through the paper. We have clarified the aims and structure of the manuscript, which we hope addresses the concerns raised (e.g. lines 78-83: '*The aim of this study was twofold: (1) to test whether shifts in the EASM during the penultimate glacial cycle (Marine Isotope Stage 6) are consistent with bifurcational tipping points, and (2) if so, is it possible to detect associated early warning signals. To achieve this, we analyse two d^{18}O speleothem records from China, and construct a simple model that we derive directly from this data to test whether we can detect early warning signals of these transitions.'*) We believe that our conclusions go further than simply saying that the resolution of the speleothem data is too low to consistently detected early warning signals. Firstly, our study finds that the system is bistable, with a strong regime and a weak regime, separated by a fold bifurcation. We then show that early warning signals can be detected in this data provided that the noise level and resolution are low enough. Crucially, we observe that the detection of critical slowing down is only detected during termination II. In contrast, during the glacial period no statistically significant critical slow down is detected. We speculate this difference is due to the change in the background state of the climate, causing a different response of the monsoon. Please refer to comments below regarding specific issues with some of the figure legends.

In the abstract, the authors write about bifurcations in the monsoon systems being a hypothesis, but in the text this hypothesis (and thereby the applicability of the Schewe/Levermann model) seems to be taken as a given, though it needs to be evaluated critically. This lack of a critical look at basic assumptions in their approaches is a general shortcoming of the manuscript.

Reply: We have clarified the aims of this study (see above for further details).

Unfortunately the text seems to have been written rather hastily. This is certainly reflected in the ordering and referencing of the figures: Figures 5 and 6 are referenced before figures 3 and 4 (also the numbers are wrong in the text: Page 1318, line 5 references Figs. 4 and 5, though 5 and 6 are meant). In addition, figure 7 is never referenced at all, though the reference on page 1322, line 10, could mean figure 7 and not figure 8, as written in the text.

Reply: We apologise for any confusion. During the restructuring of the manuscript we reordered the figures, and have updated the numbers accordingly.

Further, section 2.2 introduces autocorrelation and variance as EWS, never mentioning AR(1), though later in text and figures, AR(1) and autocorrelation seem to be exchanged randomly. For example Fig. 5c shows the AR(1), also labelled as such in the legend, while the corresponding text on page 1322, line 3 mentions autocorrelation. Figures 7, 8, and 9 then use autocorrelation, while figures 10 and 12 use AR(1). Figure 11 is even more striking, since it shows histogram plots for AR(1) (11b) and autocorrelation (11c). I would suggest the authors clarify whether they discuss ACF or AR(1) and redo all figures to make sure that they are consistent in their usage.

Reply: We apologise for any confusion and have amended the text and the figures to refer only to autocorrelation.

Some more specific points:

Abstract
Page 1314, line 9-10: how do you derive a model simulation from data? I would suggest a reordering of the sentence: . . . and in multiple simulations with a model derived from the data.

Reply: We have reworded this (line 25).

Page 1314, line 10-11: “We find hysteresis behaviour in our model with transitions directly forced by solar insolation.” This is a trivial statement, since the model was constructed to show just this behaviour. Therefore this is not a finding, which is implied by the sentence, and the fact that this was by construction should be mentioned.

Reply: The model is not constructed to necessarily show hysteresis behaviour and directly forced transitions. It also has the possibility of noise-induced transitions with or without stochastic resonance. This is now made clear in the revised manuscript (lines 221-290).

Section 2.1
Page 1318, lines 1-7, starting at “Tipping point analysis...” This section is placed badly: The first part up to line 5 (“is important.”) would fit better in section 2.2, while the last sentence is already a result and should be moved to the results section.
Also, page 1318, line 5 references Figs. 4 and 5, though 5 and 6 are meant.

Reply: We agree that a reordering is beneficial and have reordered the methods section to ensure a more appropriate order, and added sub-headings. We include an introductory sentence to introduce tipping point analysis (lines 86-89). We have moved the parts that are more suited to section 2.2 there (lines 179-197). Although we agree that the last sentence is indeed a result of sorts, it is also highly relevant for the methods section since it is necessary to know this result before proceeding with the rest of the analysis; if this result was different, a different pre-processing would be necessary. Errors in the figure labelling have been rectified.

Section 2.2
Generally, I would suggest a reordering of the section, since the authors mix general things about tipping point analysis and specifics of the analysis they performed. Therefore the part starting on page 1318, line 20 "Autocorrelation and variance..." and ending on page 1319, line 4, should be moved to the beginning of the section. This way general points about the technique and specific application issues are separated.

Reply: We agree that this is a suitable way of rearranging. We have reordered as suggested in the revised manuscript.

I would also like to see more discussion of what constitutes a valid early warning signal, and what doesn't – part of the legend of Fig. 10 (increasing trend, as opposed to absolute value), as well as the sentence on page 1321, lines 10-12 ("Importantly it is ... signals detect."), should be moved to this place, where they make more sense, and be elaborated upon. In addition, this section mentions only autocorrelation and variance as EWS, but both in the text and in the figures the AR(1) also appears, which is never mentioned in section 2.2.

Reply: We have moved the information that was in the Figure 10 legend to Section 2.2 (lines 99-101). We have also added a discussion of false positives and negatives (lines 106-114), as well as a section on '*Assessing significance'* (lines 199-219). As mentioned above, we have amended the revised manuscript to ensure consistent nomenclature.

A further point: Page 1318, line 10: "carried out during each stable period (determined by deviation from the mean)" – I don't understand this. How exactly do the authors determine the stable periods?

Reply: Stable periods were initially identified visually and confirmed using by subsequent analysis using a climate regime detector method described by Rodionov (2004).

Results section

Page 1322, line 10: You reference Fig. 8 here. Do you mean figure 7? That would make more sense.

Reply: Yes, we did mean Figure 7; we have now restructured the manuscript and it is now Figure 5.

Page 1322, lines 25-26, as well as fig. 10: Why is there no clear evidence of critical slowing down? What is clear evidence should have been described in section 2.2, without that description this point is unclear. It is also unclear, why you reject the transition at 200 ka, which shows trends in both ACF and VAR.

Reply: On reflection we agree that our 'clear evidence of critical slowing down' is not explained as fully as necessary. Our selection criteria for whether these trends are significant are based on the relationship to the surrogate time series. If the Kendall tau value falls beyond the 90^{th} percentile of the histogram generated by the surrogate series then we deem this significant (p=0.1). We have updated Figure 8 to include the Kendall tau values for each tipping point analysis of the SB11 speleothem record. This now shows that only the termination at 129 ka BP gives an autocorrelation and variance Kendall tau value beyond the 90^{th} percentile from the surrogate time series. However, in terms of the wider tipping point literature, this is probably the most selective criteria – many papers simply link a simple visual trend as an indicator of critical slowing down. We have added a section to methods (*'2.3.1. Assessing significance'*, lines 199-219) adapted the discussion of our results to reflect this.

Related to that: Figure 10, legend (p. 1339), lines 4-5: this sentence doesn't make sense to me: You claim that no values are annotated, though there are values on the y-axis of the figures. Also the second part of the sentence, where the authors clarify that it's the trend and not the absolute values that indicate the critical slowing down, is important enough that it should appear in section 2.2 and not just in the figure legend.
Figure 10, legend (p. 1339), lines 5-7: This sentence doesn't make sense, either. There is no colour legend in the figure!
Furthermore, the axis label refers to an "AR(1) indicator", while the figure legend mentions autocorrelation. This needs to be corrected.

Reply: We apologise for the confusing figure legend here; we have amended to remove the reference to the colour legend, and correct the annotation statement (now Figure 4, line 364). As also suggested by Reviewer #1, we have moved the statement about the trend versus the absolute values in the main text rather than the legend (line 100). Again, we have corrected the AR(1)/autocorrelation inconsistency.

Potential model (discussion on pages 1322-1323)
The authors construct a model of the monsoon transitions from the Langevin equation and a potential function they derived from the proxy-data in combination with a timedependent term, which is a function of solar insolation. Looking at Fig. 11a, this model seems less than convincing a model for the proxy time series. For one I don't see the "high degree of synchronicity between transitions and solar forcing" the authors claim (page 1322, line 29 to page 1323, line 1), if I compare insolation and proxy data. There seem to be time lags of variable length between forcing and transitions (compare insolation maxima at ~200 ka and ~175 ka or insolation minima at ~185 ka and ~140 ka), making the claimed synchronicity highly questionable. Also, the proxy timeseries seems to be at the very edge of the range of model simulations around 175 ka and 130 ka. To me this implies that the model may not be applicable to the entire proxy series (if it is applicable at all). For the paper to be more convincing, this discrepancy needs to be discussed. The authors seem to touch upon this point on page 1323, lines 9-11, where the authors mention that the model doesn't seem to fit at 129 ka, but some elaboration really is required here. For example, the applicability of the model might be questioned due to this discrepancy, either just for this time, or entirely.

Reply: It is certainly true that the non-dimensional Langevin equation is an extremely simple skeleton model. The palaeoclimatic record results from a multitude of complex processes and cannot be expected to exactly be a realisation of such a simple model. Nevertheless, the potential model allows to test basic mechanisms such as directly forced versus noise-induced transitions. As noted by the reviewer, we do touch upon the fact that the model simulations follow the speleothem data for all bar the transition at 129 ka, and further on in the manuscript we discuss possible reasons for this (see discussion regarding the weak monsoon interval at termination 2). More detail on the potential model has been added to the revised manuscript (lines 221-290).

Furthermore the search for early-warning-signals (EWS) in the model realisations discussed on page 1323, lines 12-14, is not convincingly explained. How were the histograms in Fig. 11 b-d actually derived? How does one interpret these? This does not become clear from either text or figure.

Reply: The methods used here are explained in the methods section, however we agree it would be useful to recap here and briefly explain this again. The explanation of how the histograms are derived is found in the methods section as well (p1319 lines 8-17). The histograms are derived from tipping point analysis of surrogate time series generated from 1000 randomisations of the original data. The Kendall tau values for autocorrelation and variance from these 1000 randomisations are plotted as histograms.

Page 1324, line 18: A new paragraph might be justified here. The discussion of alternate forcing mechanisms is not sufficiently motivated and the connection to the next paragraph. The WMI may play a role here, but the discussion in its present form doesn't make much sense.

Reply: As discussed above, the fact that the model simulations do not seem to fit the palaeoclimate data at 129 ka BP motivates the discussion of alternate forcing mechanisms. We have elaborated further here on possible mechanisms for why this is the case (and, inherently, why critical slowing down may be detected at this time) (lines 501-523).

With regard to the conclusions, I suggest a complete rewrite after the rest of the paper has been redone. The Schewe/Levermann model needs to be touched upon, after having been introduced in the abstract, and clear take-home messages need to be communicated.

Reply: We have substantially revised the manuscript and as a result the Schewe/Levermann model is not now explicitly introduced in the abstract. We have added a section in the introduction to introduce the Schewe/Levermann model (lines 62-76). We have also re-written the conclusions, and believe that a clearer take home message is now explicitly stated in the conclusions, with the structure of the conclusions now mirroring both the abstract and the results/discussion.

Is there a reference for Figure 2? It seems unlikely that the authors drew the map themselves. There might also be copyright issues involved...

Reply: Figure 2 was drawn by the Exeter University drawing office so there are no copyright issues. We have included this information in the acknowledgements (lines 706-707).