Review #2

Reply: We thank the reviewer for their constructive review of our manuscript. We have taken the comments on board to improve and clarify the manuscript. 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.

Review of Early warnings and missed alarms for abrupt monsoon transitions by Thomas et al. This review is influenced by the review of anonymous reviewer 1. I basically agree with his/her comments. The paper reflects a major technical effort, addresses an interesting topic and produces valuable results. What I miss is an overall critical attitude towards results and methods used from the authors. For instance, fig. 3a shows a non-Gaussian distribution. I doubt whether it is really bimodal and not just skewed. The authors could have used the Dip-test of Unimodality (or another suited test) to check whether this is really true. I am not a fan of potential well analysis. For instance, if there is just one equilibrium, but the system is subject to a large excursion from this equilibrium (due to enhanced noise or a transient perturbation in the forcing), the potential well analysis will identify two stable states. It assumes that any form of multimodality is associated with multiple equilibria, which is not necessarily the case. What if sigma changes or alpha (Eqs on line 8 and 12 of page 1320)? The ESW for this potential switch at 129 ka looks convincing. But the discussion on missed alarms seems somewhat biased. Could it also be that the EWS at 129 ka is a false alarm iso the absence of EWS at other events being missed alarms? In summary, the ms is unbalanced. Section 2.1 starts with "To test the proposed conceptual model of Schewe et al." The ms reads too much as an attempt to prove the model is right and not really investigates the alternative of it being wrong. I recommend a rewrite towards a more balanced interpretation of the proxy record.

There may have been some confusion in the previous draft of the manuscript regarding the aim of this study. We have now revised the text to read: '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.'

The issues raised by Reviewer #1 that Reviewer #2 refers to are addressed to the author response to Reviewer #1. Further comments are made below.

The histogram in Figure 3a is used as a first pass justification of why we believe that the EASM may be bimodal. We then apply more sophisticated techniques such as our potential model (as shown in Figure 3b and 3c). However, the Dip-test of Unimodality is a sensible test to use in this case; we have applied this test and indeed find that the null hypothesis that the data is unimodal is rejected, and thus that the data is at least bimodal (dip statistic D=0.018, p=0.0063). We thank the reviewer for this suggestion and have added this analysis to the revised manuscript (lines 170-173, lines 294-296).

There is a sound theoretical basis for potential well analysis, as discussed in some of the papers we reference such as Livina et al. (2010). The potential model is not built in a way that it necessarily exhibits bifurcations and hysteresis behaviour. It also has the possibility of two states always being available and the transitions being noise-induced with or without stochastic resonance. The parameter estimation reveals which mechanism is better supported by the data; the parameters of the potential model are estimated according to maximum likelihood. 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. The procedure is now described in more detail in the revised manuscript (lines 221-290).

A new sub-section entitled 'Assessing significance' (lines 199-219) has been added, describing our thorough significance analysis. This technique uses surrogate data to determine whether the results we obtain could be due to chance and are likely false positives. We also discuss the possibility of type 1 and type 2 errors in the introduction (lines 106-114), however, to ensure a non-biased assessment we have added to the discussion the possibility that the critical slowing down signal at termination II is the result of a false positive, though note that: *'Type I errors are potentially easier to guard against by employing strict protocols by which to reject a null hypothesis'* (line 113-114).