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Interactive Comment

Interactive comment on "Early warnings and missed alarms for abrupt monsoon transitions" *by* Z. A. Thomas et al.

Anonymous Referee #3

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Review of "Early warnings and missed alarms for abrupt monsoon transitions" by Z. A. Thomas, F. Kwasniok, C. A. Boulton, P. M. Cox, R. T. Jones, T. M. Lenton, and C. S. M. Turney.

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,

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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.

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.

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.

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.

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.

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.

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.

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.

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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.

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?

Results section

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

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 men-

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tions autocorrelation. This needs to be corrected.

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 time-dependent 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 $\sim$200 ka and $\sim$175 ka or insolation minima at $\sim$185 ka and $\sim$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.

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.

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.

With regard to the conclusions, I suggest a complete rewrite after the rest of the paper

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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.

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

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