Clim. Past Discuss.,
https://doi.org/10.5194/cp-2018-57-RC2, 2018

Climate of the Past
Discussions

Interactive comment on "Reduced Carbon Cycle Resilience across the Palaeocene-Eocene Thermal Maximum" *by* David I. Armstrong McKay and Timothy M. Lenton

P. Maffre (Referee)

pierre.maffre@get.omp.eu

Received and published: 12 July 2018

First, I have to admit that this study uses statistical methods for time-series analysis which I am not very familiar with. So my remarks may be naive. Nevertheless, care has been taken to explain how to interpret the results given by these methods, as well as the limitations in the eponymous section. This is appreciable for a unfamiliar reader.

As far as I know, this methodology of early warning signals for detecting tipping points is relatively new in paleoclimate studies. It makes this study all the more interesting and the results worth to be broadly communicated. The use of different indicators (distribution moments, autoregressive model, detrended fluctuation analysis, drif-diffusion-jump model) and the sensitivity analysis contribute to strengthen this study. Moreover, this methodology has not been applied to paleorecords earlier than Quaternary according to the cited literature (Dakos et al., 2008, Lenton, 2011, Lenton et al., 2012a, 2012b). Despite the difficulties of using data as far in the past of the Earth âĂŤthat are clearly mentioned in the manuscriptâĂŤ this study constitutes therefore a significant advance. I would suggest that this pioneering application should be highlighted in the main text.

Here follows my comments.

General remarks:

This study deals with the reduction of climate and carbon cycle resilience with very little mention of the processes responsible for this (lack of) resilience. It is acknowledge Page 6 lines 4-6 that "most indicators do not reveal exact information about the nature of the transition itself" and I understand it is not the aim of the study. Nonetheless, for periods of time as extended as the one covered by this study (5 and 8 million years, which is substantially more than the previous EWS studies mentioned), it is commonly hypothesized than silicate weathering is the feedback that stabilize Earth climate (Walker, Hays and Kasting (1981) J Geophys Res 86, 9776; Berner and Caldeira (1997), Geol, 25, 955; François and Goddéris (1998), Chem Geol, 145, 177-212). The authors should explicitly say if they aim at tracking carbon-climate resilience due to silicate weathering feedback (or more generally feedbacks in geological carbon cycle), or only "shorter-term" climate resilience (for instance, the processes mentioned Page 3, lines 21–23), or both. It is of particular importance because of the timescale of considered perturbations of d13C and d18O: It is specified Page 4, lines 16-19 that the data are detrended in order to remove any long-term trends. This is indeed essential to get stationary time-series. But though it is explicitly said that bandwidth "is an important consideration", the only given information is that it is "adjusted heuristically for each dataset". More precision should be given on bandwidth value (is it constant along one given time-serie?) and above all because if it is less than the response time of carbon cycle ($\sim$100–200ky, François and Goddéris, 1998), then the indicators are not (or only partially) measuring the resilience due to its feedbacks. From the timescale of variations of smoothed records shown in Fig. 3, 4 S1 and S2 (red lines), I guess the bandwidth is actually in the order of 100ky. The choice of the timescale of the fluctuations to study and the feedbacks to investigate is up to the authors, but it should be specified, and conclusions can be drawn only for the focused feedbacks.

The section 3.2 (Binned Metrics) and Table 1 show that most of the indicators exhibit significant variations before/after the PETM and the ETM2. This suggest (as mentioned Page 7, lines 26–28) that the hyperthermal events are partly responsible for the loss of resilience of the carbon-climate system, or at least that these events are not simple perturbations followed by a relaxation towards the same "initial state": they induce some permanent or irreversible changes. Even if evidences of tipping points are lacking (as said Page 7, lines 27–28), I think it is an interesting results and should be highlighted in the conclusion, where there is no mention of this fact. I also wonder which component of carbon-climate system can be expected to undergo irreversible changes. If there is any "good candidate", it may be interesting to precise it.

Minor specific remarks:

Page 4, line 15 and line 22: With the definition of autoregressive model as 'x(t+dt) = a*x(t) + e(t)', a constant timestep 'dt' is inherently necessary for the autoregressive coefficient 'a' is directly link to 'dt'. I wonder then how to fit an autoregressive model to the non-interpolated data? Just dividing 'a' by the local timestep is enough? It is not precised in the description of the 'generic_ews' function in R. Perhaps it is a "routine analysis" and is not worth to be precised, I can't really judge it.

Page 6, line 21: The word "divergence" (of standard deviation) may be misleading because the reader would firstly expect to find an "increase", which is in contradiction with the rest of the sentence (and the Figure). Perhaps it is preferable to substitute it for "decrease of standard deviation" or "reducing standard deviation". In addition, this decrease of SD is likely to be due to earlier "extreme" events (between 59.5 and

58.5Ma) that slip out of the rolling window when it reaches 57-56Ma. Indeed, with a 75% rolling window (Fig. S1), SD does not exhibit such a decrease. It does with a 25% rolling window probably because there are "extreme" events up to 57.5Ma. Therefore, this decrease of SD could be not linked to what happen immediately before the PETM, and not contradicting with the increase of AR1.

Page 6, line 29: How to interpret a decrease in kurtosis? Shouldn't we except while approaching a tipping point more frequent extreme deviations, and then a higher kurtosis?

Page 7, lines 10–11: "but that the increase of AR(1) for both d18O and d13C are highly significant (p=0)". Please add "for non-interpolated data" to be explicit.

Page 7, lines 13-15: There is another evidence that the values of d13C during the events (at least for the PETM) are partly responsible for the upward steps in the indicators: with a 25% rolling window (Fig. S2), both AR1 and SD show a downward step at 54Ma, exactly when the PETM leaves the rolling window (and after the ETM2 has come into the rolling window). However, it is true that the binned metrics clearly show steps that are not due to what happen during the events.

Page 8, lines 2-3: Same remark as for the decrease of SD before PETM: the decrease of jump intensity can be due to the "extreme" events between 59.5Ma and 57.5Ma.

Supplementary Figures 1 and 2: How come than skewness is systematically positive? At least during PETM and ETM2, it seems there are more points beneath the red line than above. Moreover, in the binned metrics analysis (Table 1), skewness in systematically negative.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2018-57, 2018.

Printer-friendly version

Discussion paper