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

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Thank you Pierre for a thoughtful and clear review of our paper. Here we will respond in brief to your comments and describe how we will subsequently revise the paper.

You are right to say that it is important to be clearer about which long-term carbon cycle or climate processes may be implicated in our analysis, and the silicate weathering feedback is indeed the most important of these for the geological carbon cycle (along with long-term changes in the burial rates of organic carbon and ocean carbonate). As mentioned in page 3 lines 20-26, due to the temporal resolution of our data we cannot resolve short-term processes (i.e. that take place over less then $\sim$10 kyr), but in our

revised manuscript we will make this section clearer and be more explicit about which processes of the geological carbon cycle and long-term climate system we expect to play a role.

You are also right that bandwidth choice is important in the context of process timescales as well, and that the original manuscript failed to mention the bandwidth choices for the main rolling window metrics – we found that the optimal Gaussian kernel bandwidth was 0.1 and we will make this clear in the revised manuscript. This does not directly translate to a frequency limit, but as shown by the red line in Figures 2 and 3 this removes all of the secular trends and the long-term orbital-scale ($\sim$100+ kyr) variability. While this at first inspection would seem like we're filtering out what we're interested in (the long-term carbon cycle / climate system), with this methodology it is in fact the short-term noise that we are interested in as this short-term noise reveals the resilience of the longer-term processes. But as mentioned above, the data's temporal resolution places a lower limit on what timescale processes this method can reveal (in this case anything shorter than 10s of kyr). As a result, although we filter out the direct signal of longer-term processes, with this method this does not exclude these processes from affecting the results and interpretation.

Our results do indeed clearly show increased and persistent long-term destabilisation following both hyperthermal events in our record, and this has implications for the early Eocene (as a destabilised carbon-climate system may have played a role in the subsequent repeated hyperthermals). In our original manuscript we mostly focused on the implications of destabilisation for the presence of tipping points preceding the two hyperthermal events in our record, but you are right to point out that the subsequent persistence of the loss of resilience is important as well and in the revised manuscript we will make this clearer (as well as the processes likely to be implicated in this persistent loss of resilience, such as a dampened silicate weathering feedback).

Regarding the minor remarks, we will provide clarifications in the revised manuscript where requested. In brief, for the AR model time-step the R function simply assumes a constant time-step throughout the time-series (i.e. it takes the time-series length and divides by N to get the constant dt). Of course without interpolation the actual data points will slightly differ from this dt which will introduce some error (and is why the interpolated run is considered the default, despite the alternative problem discussed in the text that this introduces instead), but there is no systematic bias in the distribution real data time-steps which limits any systematic error as a result. We will clarify that d18O SD declines just before the PETM and discuss why.

We also agree that one would expect kurtosis to increase prior to a tipping point as extreme data values become more common, but this is not considered to be as strong an indicator of resilience as other measures like variance or AR1. In this case it seems that the section in the d13C record between ∼58-59.5 Ma seems to have especially high kurtosis (as shown in supplementary fig 1 especially by the 25% window), and kurtosis gradually falls after this as this period leaves the rolling window, potentially masking any pre-PETM signal. The within-event data in the whole-dataset rolling window analysis does affect the results, which is why the excluding-event bins help to check the increases are there – we can add extra discussion of this in the revised manuscript. This isn't the case for the npDDJ model though, as this does not use a rolling window.

Lastly, for d13C skewness, there is indeed a mismatch between the skewness as calculated on a rolling window (>∼0 at all times and gradually increasing) and in the bins (<0 and decreasing for every bin). The events themselves should skew negative (as shown by the initial small drops in each in the rolling window analysis), yet the analysis including them is systematically positive while excluding them results in negative skewness. This is a consistent output despite using the same data, so it's not entirely clear why this is the case. We will further explore and then discuss the reasons behind this mismatch in the revised manuscript.

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