(Reviewer comments : Our reply to the reviewer)

The authors present a set of climate simulations forced with several orbital configurations, including a precessional cycle and extreme cases of eccentricity. The analysis of the model output focuses on the direct influence of the eccentricity parameter on seasonal cycles of surface temperature and productivity of the tropical oceans. The authors find that a highly eccentric orbit largely amplifies annual cycles of marine surface temperature and productivity in the tropics, and that the seasons timing in these annual cycles shifts as a function of precession. To distinguish such eccentricity-enabled (or -enhanced) and precession-phased warm/cold seasons in the tropics -- different from typical axial tilt-related boreal/austral seasons -- the authors propose the name "eccentriseasons".

The study fits well within the scope of CP and is clearly motivated. It attempts to generalize previous findings for specific regions within the tropics and takes advantage of a biogeochemistry model component to discuss patterns in palaeo-productivity proxies, having key implications for understanding of low-latitude climate variability. The manuscript seems well outlined, although I also think it would be most helpful to break up some text blocks into paragraphs. I agree with another reviewer that parts of the text should make more clear some steps in the methods, as well as the spatial and temporal extent of reported statistics. Below I list some specific comments and technical details that I think should be addressed and could be helpful.

We thank the reviewer for this general comments. We will re-organize the paragraphs, so the manuscript read more clearly, and describe methods more extensively.

Specific comments

1. I think it is relevant to the discussion to consider the limitations of the simulated response to orbital forcing, when pre-industrial settings of greenhouse gases and ice volume are fixed. The authors assert with confidence a seasonal increase of about 2 K in a highly

eccentric orbit, but it is important to discuss that such change could be modified (amplified or dampened) by a reorganization of atmospheric and oceanic flows in response also to the high-latitude glaciation cycles. Would such additional concurrent changes in boundary conditions change amplitude or phasing in the results? Although this is probably difficult to know without additional experiments (which I do not expect authors to run), I think it benefits the discussion to address to some extent such considerations.

The aim of this study was not to test the effect of glacial cycles on the response of precession. We however agree with the reviewer that we have to state that those have an effect on the temperature and productivity. We will address the limitation of the present simulations and discuss the glacial interglacial cycles in a new discussion paragraph.

2. The authors find surprising a lack of a marked precession signal in the mean annual primary production. I wonder if this could also be related to model constraints I think the discussion would also benefit from briefly mentioning previous palaeo-applications of the biogeochemistry model. In this case the model is being used to understand patterns in proxy data, but has palaeo-data been used to assess model performance? The comparison to modern data in the manuscript is a useful reference, but I think it would also be relevant in case there is a previous application of the biogeochemistry model to, for instance, mid-Holocene or Last Glacial Maximum conditions.

The small response of annual PP to precession was not necessarily expected (paleodata show a responsive PP), but it was not surprising since the signal is averaging. We do not think that the modest change in annual PP is due to model constraints, but more by the use of a tropical average (hint: a regional increase somewhere can be canceled out by a regional decrease somewhere else). We will add one paragraph discussing this in more detail.

This model has been used the look at productivity pattern during LGM, but we recognize that this type of framework has its own limitation because to simulate accurately the paleo-productivity you would need to be able to consider changes such as river nutrient supply or dust as

boundary conditions. This is usually not done when running orbital simulations. We therefore acknowledge some limitations in the assessment of PP and could only state that the signal we simulate is the effect of changing the oceanic circulation alone. We will introduce a limitation paragraph in the discussion.

Technical comments

We will correct all technical comments accordingly to the reviewer's suggestion.