I read the author's responses to both my comments and that of another reviewer who has considerably more expertise in climate modeling than I do. The authors clearly evaluate the concerns raised and to the most part address them in this revision.

While the authors choose to revise their title, I find it awkward, specifically the construction of a descriptive component and an active one that is vague: "large scale climate features and constraining sensitivity". What kind of sensitivity is left unanswered, although I imagine most people will infer it as ESS to CO2.

That said, I find the current revision clearly superior to the initial submission and full of insightful information. I raised questions in my previous review as to the quality of the Foley/Dowsett proxy SST data set but on re-examination, and based on the author's response, I don't think this is a first-order limitation to their results.

SPECIFICS:

I am not sure if the response to my earlier query is satisfied:

[TDH6]: I would have thought that reduction in winter sea ice and/or lower land surface albedo would have generated a larger winter warming relative to the mean anomaly. [lines 230-240].
The amount of winter sea ice has very little effect on temperature. This is because there is no sunlight over the winter pole, and so the value of the surface albedo is irrelevant.
The reduction in winter snow cover away from the pole will affect only be a small proportion of the northern hemisphere surface. Therefore, it can only have a limited effect on hemisphere averaged temperatures

My understanding is that sea ice has an important effect not through albedo, but by capping off the surface ocean which has a large capacity to buffer atmospheric cooling in the winter. My query comes from studies that suggest that extensive sea ice during the younger Dryas, for example, led to very pronounced winter temperature anomalies. Likewise, I based the land albedo comment on reading that winter anomalies are reduced when tundra is replaced by denser vegetation. Can the authors comment?

The revised manuscript has this sentence in the introduction:

"Modelled sea-ice responses were studied by Howell et al. (2016), who demonstrated a significant decline in Artic sea-ice extent, with some models simulating a seasonally sea-ice free Arctic Ocean driving polar amplification of the warming. "

I would note that the revised manuscript has more discussion of seasonality and the timing of seasonal temperature maxima (p 19-20) that are very interesting and useful to a proxy person.

Likewise, I'm not sure the response to this comment hits the nail on the head

[TDH9]: To me, the pattern of data anomalies exceeding model anomalies near the gyre boundaries is quite robust (see MyClymont or example) and is a general feature of "warm climate" reconstructions- see Brierley for an earlier Pliocene time slice, and many others for the Miocene and Eocene. I think there is a fundamental model deficiency here. [line 401-406]
We agree that there could be a fundamental model deficiency in these regions and have stated near line 520 "The simulation of upwelling systems is particularly challenging for global numerical climate models due to the spatial scale of the physical processes involved, and the capability of models to represent changes in the structure of the water column (thermocline depth) as well as cloud/surface temperature feedbacks". However, the interpretation of data in upwelling regions is also not trivial and we also discuss this in our paper.

The movement of SST gradients well poleward from the present gyre boundaries is a robust feature of warm climate SST reconstructions. Whether this actually represents extending the gyres is another question implied by the SST data but not proven. But I do not consider gyre boundaries to be "upwelling zones"- perhaps the authors misunderstood my comment. The gyre boundaries today represent major areas of thermocline ventilation and generally where mode waters form- very distinct processes from my understanding of "upwelling". I think the problem of the models is much larger than a failure to simulate upwelling , which of course depends a lot on model resolution, coastline resolution etc.

I would also like the authors to extend their evaluation of upwelling biases in alkenone SST: Benguela is not the only instance where there is no detectable upwelling bias. We wrote a paper on the California margin (Herbert et al.,) and one can also look at the Arabian Sea and Peru-Chile margin in vain for large upwelling-related anomalies.

Herbert, T.D., J.D. Schuffert, D. Thomas, K. Lange, A. Weinheimer, and J.-C. Herguera, 1998, Depth and seasonality of alkenone production along the California margin inferred from a core-top transect, *Paleoceanography*, **13**: 263-271.