Raitzsch et al present a new planktic boron isotope record across the mid Miocene Climate Transition, and interpret this in terms of changing CO2 and carbon cycle changes. The record enables deeper interrogation of potential climate and carbon cycle feedbacks during this important interval of global cooling. This interval also spans the end of the Monterey Excursion, which contains the well-known "carbon maxima" events. Having more CO2 data across this interval is very exciting, and thus the submission represents a substantial contribution worthy of publication in CP.

However, the narrative of the manuscript could be much improved – e.g. the conclusions are that a particular carbon cycle model is supported by the new records, but this model (or the mechanisms within it) are not referred to in the introduction. Much of the discussion hinges on assumptions of processes that have not been observed but are presented here as fact (e.g., that middle Miocene CO2 variations are caused by changes in shallow water carbonate production). The manuscript also fails to cite some key recent studies. However, I believe that if the narrative of the paper is improved and a few specific scientific questions are addressed then the robust interpretations of this fantastic dataset will be clearer and this will become a really exciting and useful contribution. Below I list the specific scientific questions I would like to see addressed, followed by more minor comments.

1. Is high eccentricity definitely associated with decreasing CO2? Would it be more robust to focus on the clear relationship between the d13C and CO2?

The key finding presented in the abstract of the paper is that "long-term *p*CO2 variations between ~14.3 and 13.2 Ma were paced by 400 k.y. eccentricity cycles, with decreasing *p*CO2 at high eccentricity and vice versa." I struggle to see this relationship in Figure 4 or Figure 5 – i.e. an inverse correlation between the pCO2 record and the thin grey line. I'm not saying the pCO2 record doesn't contain a long eccentricity signal, but the temporal relationship with the eccentricity curve could perhaps be demonstrated more robustly. While I struggle to discern a negative correlation between the authors' pCO2 record and the long eccentricity cycle, the relationship that does seem convincing is the positive relationship between d13C and CO2 across CM6 (Figure 4 and described in section 3.1). Perhaps therefore a clearer and simpler approach would be to set out different mechanisms in the introduction with their respective d13C-CO2 relationships. E.g., Holbourn et al 2007 ascribes the eccentricity signal in the longer d13C record to EITHER increased productivity and burial of Corg (the classic Monterey hypothesis) which would result in a negative correlation between d13C and CO2, OR a monsoon-driven increase in shallow water carbonate deposition, removing alkalinity from the oceans and releasing CO2, producing a positive correlation between d13C and CO2, as observed here. This explanation would be particularly useful because the increase in monsoon intensity variability has different impacts on the carbon cycle, but these are not discussed in the introduction. For example, an increase in nutrient delivery to the oceans and increasing Corg burial tends to increase d13C and decrease CO2. But in the Ma et al 2011 model this is outweighed by the concomitant increase in carbonate burial and associated CO2 release. It would be useful if this current manuscript could comment on the robustness of the relative importance of these processes in the model. However, see also my comment below about the uniqueness of CM6 - what might be expected to happen if for example the climate transition led to a general increase in marine productivity superimposed on these orbital variations? Should we expect the d13C-CO2 relationship for CM6 to hold true for all CM events? This complication should be clearly addressed in the introduction also.

2. What is the evidence for the increased shallow water carbonate deposition?

As far as I know there is no direct evidence for eccentricity paced changes in shallower water carbonate burial. There are dissolution cycles in deep sea carbonates, but I am not aware of any study that rules out the influence of bottom water mass ventilation on these? I believe Holbourn et al 2007 favoured the monsoon hypothesis due to the 50kyr lag between eccentricity and d13C. But this lag is not discussed in this manuscript, and no supporting evidence is given for the statements that shallow water carbonate burial changed at this time.

3. Does the Site 1092 planktic d11B record global pCO2?

This relationship between d13C and CO2 is key, and is independent of age model issues because the d13C has been recorded at each site and is a global signal. So it is interesting therefore that the Malta record shows a decrease in d11B-derived pCO2 associated with the onset of CM6 (increasing d13C) whereas the 1092 d11B-derived pCO2 shown here shows an increase. This raises the important question – is Site 1092 recording global atmospheric pCO2, or a more localised signal? A more local signal showing C storage in the high latitudes of the south Atlantic over the MMCT would also be very interesting of course. The change in the frontal positions is acknowledged in the text (Kuhnert et al 2009) but the change in stratification at this time is not mentioned. Paulsen (2005) (Bremen thesis) shows an increase in stratification at Site 1092 starting at 13.85Ma (using the divergence of surface:deep planktonic foraminiferal d18O). Could this stratification have been associated with an increase in surface water [CO2]? This possibility should be addressed in the manuscript.

4. Is it appropriate to compare the records across CM6 with those of a model that does not consider the impacts of the MMCT itself?

CM6 immediately follows the ice growth of the MMCT and is the largest by far of the CM events, making its interpretation more complex than the other CM events. It follows a major sea level fall, which would have affected the shelf:basin burial of carbonate, d13C and CO2 (e.g., Mckay et al 2016, Ma et al 2018). Further, the ice advance was likely associated with changes in ocean circulation and ventilation of bottom water masses (with implications for both d13C and CO2). Is it therefore appropriate to make a straightforward comparison of the d13C and CO2 records to the Ma et al 2011 model without any consideration of these other processes? The Ma et al 2011 model was constructed to explain the long eccentricity signal in the d13C record throughout the Miocene Climatic Optimum, rather than examine CM6 specifically. It is solely forced by ETP, and does not include processes triggered by cryospheric thresholds in the climate system and resulting impacts. On the other hand, the Ma et al (2018) model suggests that a significant cause of the d13C increase at CM6 was the increased weathering resulting from the sea level fall. By saying the results here support the Ma et al 2011 model for CM6, where does that leave our understanding of the importance of shelf-basin carbonate deposition on global d13C signals?

5. How does the pCO2 record compare with the high-resolution B/Ca record of Sosdian et al 2020?

It is odd that the manuscript does not compare the d11B-CO2 record with the high resolution B/Ca records published by Sosdian et al 2020. Those authors suggest that the increasing d13C of the CM events is associated with decreasing surface water DIC at Site 761. This is consistent with an increase in Corgburial:Carbonate burial which would predict a negative relationship between d13C and global CO2 across CM events, rather than the positive relationship observed across CM6 at Site 1092 here. Although, before a direct comparison can be made the origin of the d11B signal at Site 1092 needs to be thoroughly addressed.

6. What is the significance of the change in the Malta age model for the GSSP?

The GSSP for the Langhian-Serravallian boundary is placed in the Malta section. There should be some comment about this in the age model section as the authors have changed the Malta age model.

More minor comments:

Line 45: *"However, most proxy records for the history of pCO2 across the MMCT are incomplete or at low resolution, thus prohibiting resolution of the CM events (Pagani et al., 1999; Kürschner et al., 2008; Foster et al., 2012; Ji et al., 2018; Sosdian et al., 2018; Super et al., 2018) and making it difficult to identify the mechanism responsible for the major step into the "icehouse" world."*

This statement is a bit disingenuous, these published records clearly show a significant pCO2 decrease just prior to the MMCT (see also Sosdian et al 2020). What is interesting of course is the higher resolution CO2 changes, and questions such as: what caused the CM events? how do the CM events depend on background climate state? Why is CM6 (immediately following the major ice growth) the largest of the CM events?

Line 35: *"In a recent study, it was proposed that a more sluggish meridional Pacific Ocean overturning circulation, due to reduced deep-water formation in the Southern Ocean, enhanced the weathering of ^{13}C-enriched shelf carbonates".*

This is incorrect, the enhanced weathering in this model was ascribed to the sea level fall. This model (Ma et al 2018) is very different to the Ma et al 2011 model. The Ma et al 2018 model treats the MMCT as an "event" whereas the Ma et al 2011 uses continually evolving orbital parameters as forcing. This important point is not explained in the current manuscript.

I find it odd that pH and pCO2 are plotted as the same curve with the same uncertainty envelope in Figure 4. pCO2 clearly has additional uncertainties (e.g. estimate of TA, d11Bsw), and I think these should be better represented in Figures 4 and 5.

Line 31: Need to also cite Foster et al 2012 as demonstrating climate-CO2 relationship across MMCT.

Careful with all references – e.g. Fig 3 caption "Badger et al 2015" should be "Badger et al 2013".

Line 168 – Fig 5 should be Fig 4.