I have carefully read the new version of the ms submitted by Cramwinckel and co-authors to Climate of the Past. The authors modified most of the points highlighted by the three referees and presented arguments to support some of the concepts and hypothesis they decided to maintain. As I mentioned in my first review, the most critical point was the lack of physical arguments to explain the proposed change in the Southern Ocean's surface circulation through the MECO, which is the crucial aspect of this work. I regret to say that despite the information added to the discussion (point 5.1), the work still lacks physical foundations for the proposed hypothesis. I will refer in detail to the authors´s answers to my concerns (AR in blue italics) and part of the revised version (RV in black italics). I also have comments and serious worries about the new Fig. 1 and Fig.3.

AR. We explore several mechanisms and identify the one we consider most likely (southward extent of the EAC). Indeed, such changes in surface ocean circulation would follow changes in the wind pattern - given bathymetric and geographic constraints. We would like to emphasize that the bathymetric/paleogeographic constraints are just as important as the wind patterns, and both are much less well-constrained than the existing model simulations seem to suggest.

Given the dinoflagellate cyst distribution the authors pick-up a possible explanation (a southward extention of the EAC). The problem is that to sustain this explanation the current should be more than 15º south of its present configuration. Without a proper physical explanation of how the current can attain the studied area this hypothesis is as weak as the one proposed be Kennet during the 70´s (See Huber et al., 2004).

AR: While we agree that it would be insightful to draw in the prevailing wind directions in the Eocene, unfortunately these reconstructions do not reliably exist, so we respectfully refrain from drawing them. The middle Eocene, as this additional step would introduce a lot of uncertainty. Alternatively, drawing wind circulation patterns as derived from model simulations does not provide a solution either. Atmospheric simulations as derived from fully-coupled coarse resolution GCMs (that are tuned to reproduce modern conditions), are still limited by the poorly-constrained Eocene boundary conditions. Detailed model output is too dependent on these poorly resolved boundary conditions in order to be leading in drawing atmospheric reconstructions.

On one side the authors have the dinocyt assemblage distribution, or the fossil plankton biogeography (THE DATA). On the other side, they are suggesting a paleoceanographic model based on the dinocyst distribution (THE INTERPRETATION). Any surface ocean circulation hypothesis at this scale has to be consistent with an atmospheric circulation pattern at the same scale. So, the authors cannot *choose to draw ocean circulation patterns based on fossil plankton biogeography, but prefer not to infer wind circulation patterns from this.* If they are not confident about the prevaling wind direction they cannot be confident on the EAC extension either. Again, the main hypothesis of this manuscript is weakly supported by ocean-atmposphere physics. If the authors cannot explain (physically) the huge southward shift of the EAC the hypothesis should be carefully revised and discussed and probably disregarded.

RV, page 14 line 15: *As the second option, southward extension and/or intensification of the EAC could have sustained cosmopolitan assemblages at Site 1172 (Figure 1c). Increased southward reach of the relatively warm EAC has been suggested before as a mechanism to warm the SWP throughout the hot early Eocene (Hollis et al., 2012; Hines et al., 2017).*

Hollis et al. (2012) show model simulations for the middle Eocene and EECO. Their Fig. 7 shows a nortward flowing western boundary current in all scenarios (presumably, because the caption does not indicate what the arrows are). I do not understand how these simulations could help the authors to sustain their hypothesis of a southward flowing EAC reaching 60°S.

In their conclusions Hines et al. (2017) state: "Intensification of a proto-East Australian Current (EAC) during the EECO provides an efficient means of oceanic heat distribution, subsequently resulting in the decreased thermal gradient from the equator to poles suggested by Southwest Pacific proxy records". This, like the one proposed in the revised manuscript, is a very surprising conclusion, because there is not a single paragraph in Hines et al (2017) explaining how and why such a proto EAC could be generated. What is the (physical) forcing mechanism that induces "an intensification of a proto-East Australian Current (EAC) and corresponding weakening of the Tasman Current (TC)" (Hines et al, 2017). It seems like the physics of climate is again underestimated.

RV, page 14 line 18: *Model simulations (using modern boundary conditions) indicate that a wind-driven strengthening and further southward extent of the EAC is expected under conditions of enhanced global warmth, as part of intensification of the southern midlatitude circulation (Cai et al., 2005). Indeed, observational data indicate a strengthening of the South Pacific Gyre over the past six decades, including a southward extent of the EAC at the expense of the Tasman Front (Hill et al., 2008, 2011). Similarly, SST anomaly reconstructions over the peak interglacial Marine Isotope Stage 5e (~125 ka) indicate intensification of the EAC to offshore Tasmania (Cortese et al., 2013). Possibly a similar atmospheric and oceanographic response to global warming occurred during MECO.*

Indeed, all the papers about the present EAC extension are related to changes of the wind stress curl (See Hill et al., 2011, 3rd Paragraph:"The pattern of wind stress curl determines the strength and spatial pattern of the gyre [Munk, 1950]. Hence variations in the basin-scale wind field will drive variability in the strength of the western boundary current"). So, how many latitudinal degrees outspreads today the southward extension of the EAC? According to Ridgway and Hill (2009) it corresponds to a poleward extension of some 350 km (~ 3 degrees). How this scenario can be used to explain the EAC extension reaching 60°S?

Cai et al., 2005 indicates an intensification of wind stress curl and of the EAC transport but it seems that the zero of the wind stress curl does not shift southward very much. Therefore it is still an open question how the EAC can flow until 60ºS as proposed by the authors. Remember that to support the proposed hypothesis we are talking of a 15 degree southward extension of the EAC. This would require a major change of the global wind stress distribution whose drivers are not explained in the revised manuscript.

Cortese et al., 2013 indicate an intensification of the EAC until ~45S. If a similar atmospheric response to global warming occured during MECO then the current should reach 45ºS and not 60ºS. Note again that the authors must change the latitude of zero wind stress curl to push the EAC farther south (page 1, 3rd paragraph of Hill et al, JGR, 2011). The large (proposed) southward extension of the EAC should be correlated with a corresponding change in the spatial wind pattern during the MECO. Unless there is a climatological (physical) explanation of how (and why) these changes are produced the hypothesis is flawed.

Considering an alternative hypothesis the authors indicate (page 14 line 8): *Two possible oceanographic features could have resulted in a dominantly cosmopolitan dinocyst assemblage at Site 1172 and not at Site 1170. First, weak eastward flow could have occurred through Bass Strait and/or the northern portion of the Tasmanian Gateway from the AAG (Figure 1c). The uncertainty on paleolatitude in principle allows for weak continuous eastward flow (or discontinuous eddy transport) under influence of the westerlies through the northern part of the TG. While this remains a possible scenario, we consider it unlikely that such a nearby current would not be reflected in the plankton assemblages at the depocenter of Site 1170, particularly since the widest opening in the TG would be located south of the South Tasman Rise (Bijl et al., 2013b), close to Site 1170. In addition, the Bass Strait, or Bass Basin, to the north of Tasmania was likely too restricted at its eastern end for throughflow (Cande and Stock, 2004).*

According to Fig 3b *Enneadocysta multicornuta* (cosmopolitan) along with other cosmopolitan dinocysts and low-mid latitud dinocyts were already well represented at the Site 1172 from the bottom of the core and even since 480 m upwards (see supplementary data). Those levels are considered to be about 44 Ma in age. It is clear that those species were close to the 1172 Site at ~4Ma before the MECO. Perhaps a weak eastward flow would reach the ETP (Site 1172) but not the STR (Site 1170), dominated by the TC and a proto-ACC. To understand the increase of different taxa during the MECO we can use a good explanation settle by the authors (From 5.2, page 15, line 15)*: Taken together, these results confirm previous evidence that once a surface-oceanography-tracking plankton community has become established, relative abundance changes within the community correspond closely with changes in SST (Bijl et al., 2011). T*he surface temperature rise during the MECO would have resulted in increased production of the cosmopolitan Enneadocysta multicornuta and other cosmopolitan taxa on the ETP but not on the STR, where the dominant species is

Enneadocysta dictyostila. This species is the member of the Antarctic endemic assemblage most tolerant to warm surface waters.

Thus, the authors should look for a source of waters bringing cosmopolitan taxa to the Site 1172 area since at least 44 Ma. Perhaps this interesting dataset needs both tectonic and climate mechanisms to find an acceptable hypothesis, which certainly in not an EAC extention reaching 60°S during the MECO.

Comments on Fig. 1. This new figure instead of adding some light to this work presents serious theoretical mistakes and is really confusing:

Fig 1 b represents both the pre and post MECO scenarios and Fig 1c the paleoceanographic situation during the MECO. I cannot understand why the shallow connections are resticted to the MECO interval (Fig 1c). Why the increase in SST produces an incipient eastward flow through the northern part of the TG from the AGG and then, when the temperatures decrease to normal conditions (at about 39Ma), the connection is blocked again? From the tectonic point of view this sequence is really strange. Fig.1 needs revision and / or a suitable explanation.