Author: Below, we have copied the review by the referee, and have added our responses in blue and between square brackets.

R2 - Chris Hollis (Referee)

General comments

This is an interesting and important study, comparing and contrasting dinocyst assemblage changes between ODP sites 1170 and 1172, one within the Australo-Antarctic Gulf and one in the SW Tasman Sea, during a time of major climate change in the middle Eocene. The study uses evidence from the assemblages to unravel the interplay of changes in ocean circulation due to tectonics and climate changes. Additional sites and data are used to build the case for a significant regional response to the middle Eocene climatic optimum (MECO) – in terms of changes in plankton communities, terrestrial vegetation and sea level. The interpretations are reasonable in most cases but there are a few areas where the argument is weakened by over-interpretation of what the authors admit are ambiguous data.

[AR: We thank the referee, Chris Hollis, for his positive evaluation of our manuscript, and constructive criticism and comments. We hope we adequately respond to these below.]

The key areas are: the definition of the MECO at Site 1170 based on the TEX86 record, which is clearly open to interpretation;

[AR: While the definition of the MECO at Site 1170 is indeed open to interpretation, we prefer the correlation presented in the current manuscript. We are however open to additionally presenting an alternative interpretation in the revised manuscript. We elaborate on this in response to the comment below regarding p11, line 8.]

the lumping together of cosmopolitan and low/mid latitude taxa, when the latter group is the one that is best able to signal the influence of the EAC and PLC;

[AR: We agree this will be a good addition and will separate these groups in a revised version of Figure 3. This further distinction will however not change our main results or conclusions.]

the lack of convincing evidence for the presence of the MECO in the Latrobe-1 borehole;

[AR: As noted below in the AR to the comment on p12, line 17, presence of the dinocyst species *Dracodinium rhomboideum* in two samples from the Latrobe-1 borehole tightly constrains this interval to the MECO. However, also in light of comments by Referee #3 on this topic, in the revised manuscript, we will refrain from separating the four studied Latrobe-1 samples into pre-/post-MECO and

MECO samples, and will present these data together without describing trends through time.]

and the very tenuous correlation of middle Eocene transgression to a purported MECO-related glacioeustatic event.

[AR: In the current version of the manuscript, we tried to convey that this correlation is tentative. We propose to elaborate on this in the revised version of the manuscript, as outlined in the AR to the comment on p16, line 28.]

I have made numerous comments on these and other issues at the places they occur in the text.

However, there is a hidden gem in this dataset that I'm disappointed the authors appear to have overlooked. In our warming world, we are increasingly concerned about the ways ecosystems will be adversely affected by warmer oceans and changes in ocean circulation. For dinoflagellates there is the further concern of how toxic blooms may impact coastal fisheries. The authors provide a dataset that clearly shows the MECO in this region is linked to dramatic increases in the abundance of single species, analogous to present day blooms. And intriguingly, a species of one genus dominates at Site 1170 whereas another species of the same genus dominates at 1172. Even more intriguing, both species have short-lived blooms leading up to the MECO at 1172. Much of the paper simply combines the data for these two species with their respective biogeographic groups (cosmopolitan and endemic) but these two taxa clearly dominate these groups (as shown by DCA and NMDS) and it is certainly worth considering that the rise and fall of these two species is more directly related to local watermass conditions than to current transport. I'd like to know if there is any indication of EAC or PLC influence with E. multicornuta removed. And I'd like to see more discussion on the watermass conditions that might lead to monospecific blooms of these two species.

[AR: We thank the referee for his interest in, and suggestions on, this specific part of our results. Although it would be very useful to be able to reconstruct harmful dinoflagellate blooms in the past, we unfortunately do not possess enough information to be able to make such assertions here. While we record acmes of fossils in the sediment, we cannot know what kind of paleo-concentrations of plankton in seawater on what timescales (short seasonal blooms? dominance of species throughout the year?) are actually represented by the data. Furthermore, it is not known if dinocyst species within the genus *Enneadocysta* have a blooming-type ecology, as they are not represented by extant dinoflagellate species with a known ecology. From the fossil record, they typically seem to be mid-shelfal species rather than near-coastal. We are therefore hesitant to claim more than a possibility of paleo-blooms, which we suggest in the present manuscript paragraph 5.3, e.g. p15 line 18–20: "The relatively low diversity of the dinocyst assemblages in combination with the high dominance of a single taxon (*Enneadocysta dictyostila* in the MECO interval) suggests a generally eutrophic setting that could have been characterised by seasonal plankton blooms."

Especially at Site 1172, the dinocyst assemblage as a whole is characterized by alternating dominance of different taxa (*Enneadocysta*, *Deflandrea*, *Spinidinium*/*Vozzhennikovia* and *Phthanoperidinium*). We interpret this succession of dominance of different species as changing conditions rather than as a succession of blooms. Therefore, while we certainly agree with the referee on the relevance and appeal of this topic, we are hesitant to include more speculation on the possibility of (harmful) plankton blooms.]

Specific comments/Corrections by page, line:

1, 20: I see the term "Tasman Gateway" or "Tasman Seaway" has been used in the literature but it's incorrect. The proper term is "Tasmanian Gateway", being the gateway between Tasmania and Antarctica (see any Leg 189 publication).
[AR: we will correct to "Tasmanian Gateway" throughout the paper]

1, 22: ", including the organic walled cysts of dinoflagellates (dinocysts). I'd like to see a distinction made between dinoflagellates (plankton) and dinoflagellate cysts or dinocysts (fossil remains of the plankton)
[AR: we will clarify this distinction where appropriate in the manuscript]

1, 23: prefer "geographic" to "spatiotemporal" (here and elsewhere)
[AR: we respectfully feel this is a matter of preference and prefer to retain "spatiotemporal", also because the "temporal" aspect is less clearly represented in the word "geographic"]

1, 24: "geographic" here is superfluous. And is it primarily controlled by tectonism? What about the rotation of the Earth? I wonder if this simplistic separation of tectonic and climatic controls is warranted or needed in an abstract? Sentence is awkward, so how about rephrasing: "The extent to which the climatic and tectonic controls on the distribution and composition of surface currents have influence the composition of fossil assemblages ...".
1, 26: This sentence is also a little awkward. "Indeed, the extent to which climate change affects oceanographic processes is still poorly understood"?
1, 29: Also, an awkward sentence. "trend, the Middle Eocene Climatic Optimum (MECO, 40 Ma). This 500 kyr-long episode of global warming is unrelated to ..."
1, 31: "ocean's"; replace "only" with "alone"
[AR: we will change wording in the abstract according to above suggestions]

2, 1: "our new results...", no hyphen between surface and ocean
[AR: we will change the text accordingly]

2, 2: replace "southward" with "south"
[AR: we will change the text accordingly]

2, 3: Explain how "warm temperate with paratropical elements" MECO assemblage differs from the general middle Eocene pollen assemblage?
[AR: We elaborate on this in the relevant discussion section on the terrestrial palynology of the Latrobe-1 core (5.4). Here we note that this warm flora overlaps

with the MECO interval, based on the dinocyst species that are present in the samples, but, p 16 line 15–17: "Future regional pollen studies focussing on the Nirranda group might therefore elucidate whether the relatively warm-loving flora described here was restricted to the MECO interval, or to a broader interval of middle-late Eocene "background" conditions."]

2, 8: change "into" to "to"
[AR: we will change the text accordingly]

2, 13: does "intermediate-deep" mean somewhere between upper and lower deep water or is it shorthand for "intermediate and deep", in which case this formulation is less ambiguous.
[AR: we will change the text accordingly]

2, 15: None of these sites are close enough to the Antarctic margin to be sources of deep water and are all north of the 60S demarcation for the SO, using pmag reference frame (although noting the uncertainty).
[AR: In these lines, we did not have the intention of suggesting that these sites precisely represent the locations of Eocene deep water formation, but instead, that model simulations suggest that these sites lie close to the region of intermediate-deep water formation. In this we mean to distinguish and refer to intermediate-deep water formation. According to model simulations, while bottom waters formed on the Antarctic continental shelf, intermediate-deep waters formed at southern high latitudes, not necessarily only on the Antarctic margin. We will better clarify the above in a revised version of the text.]

2, 18: change "marine-based" to "sea" and, no, they are not supported by estimates for land temperatures from NLR approaches, which are in general closer to the modelled temperatures (add Pancost et al. 2013), so SST estimates are 5-10C warmer than models and LAT estimates.
[AR: we will add the land temperatures from NLR approaches to this section of the introduction, citing Pancost et al. 2013]

2, 21: add comma after processes
[AR: we will change the text accordingly]

2, 22: remove parentheses around global
[AR: we will change the text to "regional and global"]

2, 31: plural "changes". Lord Howe Rise is part of Zealandia so rephrase: "submerged parts of NW Zealandia..."
[AR: we will change the text accordingly]

3, 1: that's a lot of potential effects but rather speculative. Suggest you keep it simple. "... should have affected ocean circulation in the region with likely impacts for global heat transport and climate."
[AR: we will change the text accordingly]

3, 4: change “of” to “from”
[AR: we will change the text accordingly]

3, 5: Change “Southern Ocean” to “SO”.
[AR: we will change the text accordingly, and verify use of abbreviations after first definition is consistent throughout]

3, 6: Rephrase: “... endemism are characteristic of a diverse range of fossil groups ...” (circum-Antarctic is tautological when you’ve already said Southern Ocean)
[AR: we will change the text accordingly]

3, 9: here is where I’d prefer you to use “dinoflagellates”. If you use cysts here, you really also need to use frustules for diatoms and tests for forams and rads. Personally, I don’t think you need to use “dinocyst” at all, but certainly should not be used when you are talking about plankton as opposed to assemblages in sediment.
[AR: we respectfully disagree on this point, and prefer to restrict the discussion to dinocysts, not dinoflagellates. In contrast to diatom frustules and foraminifera and radiolaria tests, dinoflagellate cysts, being vegetative resting cysts, do not have a 1:1 relationship to the living organism. (The body of the motile dinoflagellate is composed of labile organic material and in general does not preserve in the sediment.) While the abovementioned frustules and tests of other microfossils are truly body fossils of the living organism, the dinocyst is a resting cyst produced during the life cycle of the dinoflagellate. Since we know that not all dinoflagellates produce resting cysts (Head 1996), a single species of cyst-producing dinoflagellate can produce multiple species of dinocyst (Rochon et al. 2009) and dinoflagellate taxonomy is distinct from dinocyst taxonomy, there is a big discrepancy between the dinocyst assemblage and the dinoflagellate assemblage. For the Paleogene, relationships between this plankton group and its biogeography and environmental preferences are all based on dinocyst species and groups, typically without knowledge of which dinoflagellate produced them, certainly as most Paleogene cyst types are extinct. Therefore, we should refrain from extrapolating to dinoflagellates and restrict the discussion to dinocysts. Since, in p3 line 9, we are enumerating types of fossil assemblages, we are referring to dinocysts, not dinoflagellates.]

[Author comment: below, we have grouped a few of the referees comments on the topic of biogeographic terminology, to answer these collectively.]

3, 12: Query use of “cosmopolitan”. This is unconventional usage. Cosmopolitan means found everywhere, so hard to see why this group signals the influence of the PLC or EAC.

4, 2: low-latitude and cosmopolitan are not the same thing.

7, 14, 16: Key problem issue for this paper. Definition of “cosmopolitan” is ambiguous and not in line with convention: cosmopolitan = found everywhere. I

recommend you use only low and mid-latitude taxa as your guide to PLC and EAC influence.
10, 12: Differentiate cosmopolitan from low/mid latitude.

12, 24: Differentiate cosmopolitan from low/mid latitude taxa.

14, 7: This statement further serves to highlight why it would be helpful to differentiate cosmopolitan from low/mid latitude taxa

[AR: In our dinocyst grouping, we consider species that occur everywhere with respect to latitude, and do not have a specific latitudinal affinity, as "cosmopolitan" species. However, it is definitely likely that within this group of cosmopolitan species there are different habitat preferences, even though these species are principally able to occur at all latitudes (as seems indicated by the ordination analysis and statement on p 14, line 7). Regarding surface currents, the EAC and PLC are expected to bring an assemblage consisting of both cosmopolitan and low/mid latitude taxa, and no Southern Ocean endemic taxa, whereas the TC primarily transports an SO endemic assemblage. During MECO dominance of *Enneadocysta multicornuta* (cosmopolitan ecogroup) is recorded at Site 1172, at the expense of Antarctic endemic species. Although presence of a cosmopolitan assemblage by itself might not be very informative, this change from dominantly Antarctic endemic species to cosmopolitan species during MECO provides information, signalling a change in surface currents. We propose to better explain the above reasoning in paragraph 3.1.2 on "Dinocyst biostratigraphy and palaeogeographic affinity". Furthermore, we propose to better differentiate the cosmopolitan from low/mid latitude group where the referee is asking for a distinction.]

3, 13: NZ is not in the Tasman Sea. It is east of it.
[AR: we will change the text accordingly]

3, 26: change "biogeographical patterns" to "biogeography"
[AR: we will change the text accordingly]

3, 27: why the "cf."?
[AR: upon reflection, we think the "cf." in front of the reference is redundant and will remove it]

3, 28: Why is "orbital scale" mentioned? Is it relevant? Why the "cf."?
[AR: upon reflection, we think the "cf." in front of the reference is redundant and will remove it. We would like to mention "orbital scale" variability here as an indication for the timescale on which these assemblage changes can occur.]

3, 32: Why is deep ocean warming described as "transient" and surface-water warming described as "widespread"
[AR: "transient" is meant to describe both deep- and surface-water warming, whereas "widespread" is meant to describe surface-water warming. We will change wording to clarify.]

3, 34: be a little more specific than “global perturbations”
[AR: we will change the text to more specifically describe oceanographic and environmental changes during the MECO at the sites studied in the cited papers]

4, 3: change “outstanding” to “unresolved”
[AR: we will change the text accordingly]

4,5: Sentences in this paragraph from “In addition ...” to end of paragraph should come before the description of the dinocyst assemblages. These sentences are part of the general description of the MECO.
[AR: agree and we will change the text accordingly]

4, 8: The two factors mentioned do not “imply” a volcanic explanation. Revise this sentence and provide a reference for the volcanic carbon hypothesis.
[AR: we omitted to mention carbon isotope trends over the MECO here. As $d^{13}C$ of DIC does not show a negative trend over the MECO, this rules out a depleted source of carbon. Together with the cited reconstructions of carbon cycling during MECO, a more heavy source of carbon, such as volcanic carbon, thus was the more likely cause. We will add these additional constraints in the revised text to clarify this sentence.]

4, 11: Last sentence of paragraph is poorly worded. Revise.
[AR: we propose to simplify this sentence to “, constraints on global sea level change during the MECO are lacking”]

4, 25: Revise: “in the 2–3 km-deep and 50 km-wide Ninene Basin”.
[AR: here we meant to describe that Site 1170 is located in one of the grabens within the Ninene Basin, and that this certain graben is 2-3 km deep and 50 km wide. We will revise the text to clarify.]

5, 18: Delete “interval”; no hyphen between shallow and marine, as for 5, 21.
[AR: we will change the text accordingly]

5, 31: Sentence doesn’t make sense. What covers the unconformity and overlies basal Nirranda Group?
5, 32: “Latrobe-1 borehole”
[AR: we will combine this sentence with the next to “The Latrobe-1 borehole (38.693009° S, 143.149995° E) was drilled in 1963–1964 near the Port Campbell Embayment depocenter, reaching a total depth of 620 metres.” The information on stratigraphy then follows in the following lines.]

6, 2: change “overlying” to “underlying”; What’s the age of the Dilwyn Fm?
[AR: we will change wording here to clarify that the middle Eocene Narrawaturk Fm overlies the early Eocene Dilwyn Fm.]

6, 11: Elsewhere in text it is referred to as Hampden section. Be consistent. Why no

mention of the work on the rest of the Eocene / Paleogene section (e.g. Morgans, 2009; Hollis, et al., 2012; Inglis et al., 2015)
[AR: to conform to the other location descriptions, we will add more background information on this section, including these appropriate references for which we thank the reviewer.]

6, 12: missing comma after "...E)"
[AR: we will change the text accordingly]

6, 13: "end-member" is not the right word. How about "analysed to identify influences from the TC or EAC in the middle Eocene prior to the MECO".
[AR: we will change the text accordingly]

6, 28: lower case "s" for section.
[AR: we will change the text accordingly]

7, 2: 50 and 90 are normally seen as too few for robust statistical analysis.
[AR: we agree and will add discussion on this to paragraph 3.3 on statistical analyses]

7, 5: and identified to what taxonomic level?
[AR: typically to the level of genus - we will add this to the text]

7, 27: Again, ambiguous terminology. Your example is not of a taxon with unknown biogeographic affinities, but with conflicting biogeographic affinities.
[AR: We do mean to use the term "unknown" here. Regarding the specific *Deflandrea* example - different species within the genus *Deflandrea* have different geographic ranges. For example, *Deflandrea antarctica* is endemic to the Southern Ocean, whereas *Deflandrea phosphoritica* occurs globally. In our samples, we encountered specimens of *Deflandrea* that we could only bring to the genus level, because of poor preservation. For those specimens of *Deflandrea* spp., no biogeographic grouping could therefore be made, and they are categorized as "unknown".]

9, 7: What is meant by "spatial"? Lateral? Geographic might be a better term.
[AR: we will change the text accordingly]

9, 24: U is not a direct proxy for TOC.
[AR: we agree and will remove the part in between brackets here]

9, 26: Change "like" to "As with".
[AR: we will change the text accordingly]

10, 2: Change "for" to "of".
[AR: we will change the text accordingly]

10, 5: Change "dinocysts" to "assemblage"
[AR: we will change the text accordingly]

10, 7: Can low salinity be consistent with low BIT?
[AR: since the BIT index is the relative proportion between a (chiefly) terrestrially- and a (chiefly) marine-produced set of components, changing either terrestrial input or marine production can change the BIT index. In terms of BIT index, an increase in influx of terrestrially produced components can thus be offset by an increase in the accumulation of marine components. Therefore, in some settings, low salinity can indeed be consistent with low BIT indices, if marine GDGT production is relatively high.]

10, 9: Change "most dominant" to "most abundant".
[AR: we will change the text accordingly]

10, 13: What does "a.o." mean?
[AR: we will change this to "i.a.", inter alia]

10, 20: delete "at this site"; redundant.
[AR: we will change the text accordingly]

10, 23: Provide error values for SST estimates and show on Fig. 3.
[AR: we will add error bars to Fig. 3, incorporating calibration and analytical uncertainty]

11, 8: "Precarious" is the wrong word, but a good choice nevertheless, because the whole interpretation of this section is precarious due to the subjective way the SST record has been interpreted. This is only one possible interpretation. Another is that the warming at 670 m precedes the MECO and perhaps can be correlated with the broad peak around 440 m at 1172. Thus, the MECO is the interval between 5570 and 600 m at 1170. This shorter duration is consistent with the biostrat and would mean that the cyst accumulation rate is not so untenably high. Both options should be considered.
[AR: As we agree the age constraints for Site 1170 are not conclusive, we present the data for Site 1170 in the depth domain. Our dinocyst age constraints indicate the oldest layers studied (around 770 mbsf) contain *Impagidinium parvireticulatum* (FO 44 Ma), implying they are younger than 44 Ma. Therefore, it is highly unlikely that the temperature optimum around 675 mbsf represents the EECO. While we interpret this temperature optimum to represent peak MECO conditions, we cannot exclude the possibility that a "pre-MECO warming phase" occurred at Site 1170. Although we note that such a warming, to temperatures above peak MECO, would likely be regional in nature, as it does not seem to occur in MECO SST records from other sites (e.g. Bijl et al. 2010; Boscolo-Galazzo et al. 2014; Cramwinckel et al. 2018) or the global deep ocean (e.g. Zachos et al. 2008). Furthermore, we do note that high sedimentation rates are plausible given the seismic interpretation, as shown in Figure 2B. However, we agree that both temperature correlations cannot be excluded and propose to present the alternative explanation in the revised manuscript. We will then revise our (very rough) estimate of cyst accumulation rates to consider both options. We note however, that tenfold lower dinocyst accumulation rates would still be very high,

and dinocyst concentrations are very high regardless. All of our other analyses and conclusions are neutral to which interpretation is chosen, as the dinocyst assemblages are highly similar over the interval ~575-680 mbsf at Site 1170.]

11, 16: Poorly worded. "sufficient numbers of dinocysts were encountered for counts of 50-100 specimens to be undertaken. Other marine palynomorphs such as prasinophytes and acritarchs, were rare/common(?)"
[AR: we will change the text accordingly (using "rare")]

11, 31: Revise sentence beginning "Furthermore..." to "Cycadopites ... are also present but rare.
[AR: we will change the text accordingly]

12, 1. Simultaneously is the wrong word. Delete. The abundance of Dilwynites, Protea... also decrease towards the top of the borehole.
[AR: in light of comments by Referee 3, we will remove the description of trends in these four samples.]

12, 17. Very poorly worded but crucial sentence. The FO of this species is said to be at 40 Ma. When is the LO? It can only be used to define the MECO if it's restricted to the MECO. I conclude from the biostrat presented that the interval may include the MECO but equally may be younger (anywhere between 40 to 35.95 Ma).
[AR: In fact, the stratigraphic range of *Dracodinium rhomboideum* in the South Pacific Dinocyst Zonation of Bijl et al. 2013 is very restricted, as *D. rhomboideum* was only recorded in one sample at Site 1172 with an age of 40 ± 0.1 Ma, within Chron 18n.2n. This corresponds to peak MECO in a compilation of deep sea stable isotope records (Bohaty et al. 2009) as well as coinciding with peak SST based on TEX_{86} at Site 1172 itself. Notably, the range of *D. rhomboideum* in the North Atlantic Ocean (Eldrett et al. 2004 Marine Geology) is also restricted to the MECO interval (from C18n.2n 0% to C18n.1r 50%, or from 40.14 Ma to 39.66 Ma in GTS2012). Therefore, even a few specimens (1 in sample L85, 3 in sample L86 in this case - we will add the counts in addition to the relative proportions in the supplementary datafile) of this dinocyst species firmly correlate this interval to the MECO.]

12, 29: Which species help to constrain the age? And revise to "this 4 m-thick interval within the section".
[AR: we will change to: "This dinocyst assemblage is in agreement with the age of c. 41.7 Ma as previously assigned to this 4 m-thick interval within the section]

13, 6: Use of "records" implies plural, meaning more than just the Hampden section. Are there data from other NZ sections?
[AR: In addition to the here presented data from the Hampden section there are also a few records from other NZ sections as presented in Bijl et al. 2011 Paleoceanography Figure 2f. We will add a citation here.]

13, 10: What is meant by "60degS front"? Do you mean the polar front? What evidence is presented for it lying north of the gateway?

[AR: Here we are indeed referring to the polar front. As further elaborated in the response to Referee 3, the fossil plankton evidence is suggestive of a Tasmanian Gateway that is influenced by a westward surface circulation, i.e. the polar front separating the polar easterlies from the westerlies to the north.]

13, 12: This SST range excludes the high SSTs in the MECO and possible MECO intervals. Why?
[AR: we could indeed expand this range to also include MECO SSTs, and not just "background" SSTs and will do so.]

13,14: Surely we are not interested in mantle-based paleolatitudes, which are not linked to the Earth's spin axis. Restrict discussion to the uncertainty on the pmag reconstruction.
[AR: Since, as far as we can judge, there is still discussion within the community over which reference frame to use, we prefer to be inclusive and shortly mention both. This is also relevant in model-proxy comparison, as several GCMs use mantle-based absolute reference frames.]

13, 19. This is a key part of the argument, so needs a stronger word than "may". How about "is more likely to"
[AR: In response to comments by Referee 3 we will adapt the section including this line in two ways. First, we will support the possibility of further southward extent of the EAC during MECO by citing literature on model simulations and modern observations illustrating wind-driven intensification of the EAC under conditions of enhanced global warmth. Next to this, we will also discuss the suggestion of weak eastward surface transport through the northern part of the Tasmanian Gateway (see response to Referee 3 for more details).]

13, 20. This is an observation, so replace "suggest" with "find", but I suggest you drop the word "transported", which is interpretation.
[AR: we will change the text accordingly]

13, 21: "transported" is similarly redundant here - "southward reach of the warm EAC..."
[AR: we will change the text accordingly]

13, 24: "Additionally" is not needed.
[AR: we will change the text accordingly]

14, 1: This is an interesting finding, and should be investigated further (see general comments)
[AR: see AR above under the general comments]

14, 15: You don't explain how this species responded and consequently miss the opportunity of expanding on a major discovery: mono-specific blooms of different species of Enneadocysta during the MECO at Sites 1170 and 1172 warrants more discussion.

[AR: as outlined above, correlating fossil acmes to ecological blooms warrants caution and we are therefore hesitant to call these acmes "blooms".]

14, 26: This section is based on the so-called "precarious" use of the SST record to define the EECO at 1170. The alternative correlation noted above also needs to be considered.
[AR: In this section we primarily discuss dinocyst concentrations, not accumulation rates. These concentrations are very high, regardless of age model. With our preferred MECO correlation, cyst accumulation rates, although with large error, would also have been extremely high. We will add the alternative correlation to generate a low-end estimate on accumulation rates.]

Note too that the MECO has not been identified for sure on the Otway Basin and is not described at Hampden.

15, 18: Again, a stronger word than "might" is needed here: "most likely"?
[AR: we prefer the word "might" here, since more positive evidence would be necessary to improve certainty]

15, 25: "production OF dinoflagellate prey ..."
[AR: we will change the text accordingly]

16, 3: Again "seem" is too weak a word. If there is evidence, specify it.
[AR: we will change to "but also include a small proportion of meso- and megathermal components"]

16, 4: Repetition. Replace "sporomorph record at" with "assemblages in"
[AR: we will change the text accordingly]

16, 10: Numerous terms introduced here, either for the first time or with limited context: Wilson Bluff, Latrobe unconformity, Lutetian gap, Khirthar transgression. Consider which ones are actually needed for the argument and explain them more fully.
[AR: we agree and will make this more concise, focussing on the sequence of early Eocene sedimentation followed by erosion (Latrobe unconformity, Lutetian gap) followed by middle Eocene sedimentation (Wilson Bluff transgression, Khirthar transgression).]

16, 28. Highly tenuous to suggest a short-lived event like the MECO could be linked to such a large- scale change in base level, accommodation space. A more fruitful approach may be to consider the longer-term climate shift from EECO to MECO, where significant cooling is inferred for early middle Eocene and the MECO is seen in the context of generally warmer conditions in the later middle Eocene (e.g. Pekar et al. 2005)
[AR: In a largely ice-free world such as the middle Eocene, accommodation space on the continental shelf (on time scales of 10^6-10^7 years) would have been determined indeed not only by thermal expansion, but also by sediment supply and basin subsidence. The renewed drowning of the continental shelf, as reflected

in the Wilson Bluff transgression, seems unlikely to be related to slow and continuous basin subsidence. Instead, ocean warming during the MECO would have caused thermal expansion of seawater, and climate and environmental change could have altered sediment supply. As we note in the Conclusion section, the current age control on these sections is not nearly sufficient to be able to correlate these transgressive surfaces to the MECO with certainty. We are merely noting the curious coincidence in timing, which we feel is worthy of further investigation. We shall express this point more clearly in the revised text.]

17, 15 and 18: STR and ETP are areas of ocean floor not localities, so the plankton communities are found "on" them not "at" them.
[AR: we will change the text accordingly]

17, 20: Difficult to reconcile, but you suggest it may be related to the nature of preexisting assemblages. Something on this idea needs to be added to the conclusions.
[AR: With this statement, we meant to indicate that similar sea surface temperatures above the STR and ETP are not expected, if indeed an extension of the (warmer) EAC reaches the ETP, while the (colder) proto-ACC influences the STR. We will rephrase to clarify.]

17, 21: This conclusion is contingent upon age model assumptions.
[AR: We will add the accumulation rate estimates based on alternative age constraints, but note that absolute concentrations of dinocyst are very high given the setting, independent of accumulation rates.]

17, 25: Correlation with the MECO is uncertain.
[AR: As noted above, we will better illustrate the stratigraphic usability of *Dracodinium rhomboideum* to strengthen this correlation.]

17, 26: SLR link to MECO is too speculative. Is there evidence for SLF after the MECO?
[AR: We agree the SLR link to the MECO is speculative. Higher resolution age control combined with a more detailed paleoenvironmental and/or sedimentological study could better resolve the timing of SLR and SLF. However, as noted above, the stratigraphic range of *Dracodinium rhomboideum* is very short and strongly tied to the MECO. We would like to include this curious timing of the regional transgression in a final discussion paragraph, to be able to present this as a promising direction for further investigation to the paleoceanography community.]

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