Anonymous Referee 1

This is the 2nd round of review, therefore my main focus was on implemented changes, but I also checked the final draft again.

I can accept most replies and changes made by the authors to the draft. However, I have still a small list of correction, which you find below and one more general point, which I forgot to elaborate on in the 1st round of reviews which is the effect of shifted southern hemisphere westerly winds on CO2, as noted in Toggweiler et al. (2006). The Toggweiler paper is quite controversial discussed in the modelling community for several reasons. One is, that the argument is made in Toggweiler that latitudial shifts in winds might influence CO2, but in the paper only changes in wind strength (no shifts in position) have been investigated with a rather unrealistic model. This and the fact that the model response of CO2 to shifting winds is quite model dependent (even in the sign) are reasons why the Toggweiler et al 2006 should not be cited without further refinements as support for the idea how shifting westerlies might influence CO2. A review of published modelling studies (Gottschalk et al. 2019, doi: https://doi.org/10.1016/j.quascirev.2019.05.013, Fig 14b), found that most studies found a RISE in CO2 for northward shifted winds (and a decrease for southward shift), the opposite of the Toggweiler paper. The effect is small, model-dependent (and different for different climate background conditions) and includes not only a change in the upwelling areas, but subsequently also impacts via changes in nutrients the marine biological carbon pump. For example, Völker & Köhler (2013, doi:10.1002/2013PA002556) analysed the effects of shifting winds in more detail, finding higher CO2 for both southward and northward shifted winds, but for different reasons (how changes in ocean physics and marine biology finally add up to a net effect).

Furthermore, it is only one of many processes which play a crucial role in the carbon cycle. I am aware that here we have a data paper, however the model-based suggestions of Toggweiler should be looked at more critically since different models and groups have not been able to confirm it. Please add some sentences discussing this issue and/or shift from citing mainly the Toggweiler paper to others (e.g. the Gottschalk review paper which is long and difficult to grasp in detail, but looking only at the sections with changes in SHW winds should help, eg section 3.3. there). Or the authors might use this review paper to discuss in more detail other effects related to the paper here, eg Antarctic sea ice (Gottschalk section 3.4).

Reply: We are very grateful that the reviewer explains the debates in modelling community which helps us better understand the hypothesis and improve the manuscript. We will elaborate our discussion by referring to more modelling paper. Wind/front migration is indeed one of the many process, and other processes have already been discussed in the manuscript.

Proposed changes: "Our data is compatible with the hypothesis proposed by Toggweiler et al. (2006), however, other modelling studies do show opposite results (Gottschalk et al., 2019 and references therein). It should be noted that some feedback mechanisms associated with westerlies/fronts shifts are incompletely represented in models, for instance, Antarctic sea ice cover and ice sheet calving (Gottschalk et al., 2019) and these can seriously impact the outputs. Noteworthily, the consistency of our results with that of Toggweiler et al. (2006) adds to the debate on how oceanography and atmospheric CO2 interact."

Minor comments (line count in the draft with annotated changes):

- line 8: „temperature was ~3°C higher“ THAN PREINDUSTRIAL. Without these extra words, it would imply higher than today, which is also about 1.5°C higher than preindustrial, which would add up to 4.5°C higher than preindustrial which I believe is wrong.

Proposed changes: corrected as suggested

- line 18: „efficiency of SO carbon outgassing: 2 points: 1) only CO2 can outgass, not carbon; 2) efficiency is in my view a poor choice of words (already mentioned for a different place in 1st round of reviews). I think what is meant here is the „Southern Ocean net CO2 uptake or release“

Proposed changes: corrected as suggested

- line 127: I think „Plate S1“ should be named „Figure S2“

Proposed changes: corrected as suggested

- line 175: „for“ missing in „0.04permil d13C“

Proposed changes: corrected as suggested

- line 181: 50m PER 1 million years

Proposed changes: corrected as suggested

- line 183: „Hole 1158A“ -> Hole A of site 1168

Proposed changes: corrected as suggested

- line 184: space missing in „HoleA“

Proposed changes: corrected as suggested

- lines 200, 203: Still LR04 is mentioned here, but is was argued in the rebuttal that this is not used, please delete, aslo already done on line 186.

Proposed changes: corrected as suggested

- lines 200-205: nothing is said on tie point #8

Proposed changes: A maximum in $\delta^{18}O_{bulk}$ at 30 mbsf is tuned to MIS G20 (#2) **and a minimum at 37 mbsf is tuned to MIS MG3 (#8).**

- Fig 3: The two right y-axes labels are aligned in different directions, please switch one, at best obliquity to have all y axes labels shwon in the same way

Proposed changes: corrected as suggested

- Fig 3 Header: space missing in „HoleA“ -> „Hole A“

Proposed changes: corrected as suggested

- Fig 3 Caption: „Age tuning of Pliocene Site 1168A“ -> „Age tuning for the Pliocene of ODP Site 1168 Hole A“

Proposed changes: corrected as suggested

- line 336: Again efficiency in „ocean uptake efficiency of atmospheric carbon“. I suggest to rewrite to „oceanic net uptake of atmospheric CO2“

Proposed changes: corrected as suggested

- line 339: Typo extant -> extent

Proposed changes: corrected as suggested

Referee #2: Jan Hennissen

Proposed changes: "the surface ocean **might have become** ~1.5 psu fresher during MIS M2 deglaciation **comparing to pre-M2, according to their modern affinities**."

I believe it would be more prudent to drop any reference to any form of salinity units. The problem is that a psu is equal to one gram of salt per 1000 grams of water. As mentioned in my review, we have currently no hard data confirming the exact composition of Pliocene seawater at the psu resolution and most records give relative rather than absolute values.

I believe you make your inferences based on the modern distribution of dinoflagellate cysts in the Southern Hemisphere (Thöle et al., 2023). If the units are to be retained, I would recommend specifically mentioning this caveat at this point and cite the source where modern distribution patterns are linked to extant salinity measurements.

Reply: We appreciate that the reviewer raised this concern and elaborated once more in the second round.

Proposed changes: we removed "1.5psu" and kept the phrasing qualitative.

Proposed changes: We will update the age model of Pliocene Site 1168 using slope-based tie-points where possible and present with error bars.

The authors compare their SST, δ18O and dinoflagellate cyst assemblage composition records from Site 1168 with the pCO2 record from Site 999 and conclude (line 253): "…frontal shifts and *p*CO2 lag SST and benthic δ18O across M2."

It is true that the dinoflagellate cyst assemblage composition converted to an "STFindex" (line 110) used as a frontal shift indicator lags the benthic isotope composition and that this can be established independent from the age model as they were generated on the core. However, the statement from line 253 above, links it to the pCO2 record from Site 999 and generalises it for MIS M2 and I think it does hinge on a correct age model for the current study.

Equally, the graphic presentation of the data in Figure 4 does, in my opinion, rely on an accurate age model for Site 1168.

I am still not entirely convinced by the age model in its current state (especially from MIS M1 to KM2), but I do agree with the authors that it is very likely that they captured MIS M2, which forms the focus of the study.

Reply: We appreciate that the reviewer agrees that M2 is captured in the record and the lag between dinocyst and SST is age model independent. The current tie-points yield a robust linear age model and the those associated with MIS M1 and KM2 fall well in line with other tie-points.

There could be some wiggle room in how long the lag is, which is dependant on the interpolation method. But the same problem also holds for the pCO2 record of Site 999. Furthermore, Fig. 5d presents the d18O of Site 999 and illustrates the stratigraphic correlation very confidently. There are also more and more evidences that other parts of the earth system, such as deep ocean temperature (Braaten et al., 2022), pCO2 (Kirby et al., 2020) and fronts in the Tasmanian sector (this study) lag d18O.
Proposed changes: no changes made.

Proposed changes: We will discuss about the potential changes in dinocyst affinities and acknowledge to the suggested literatures in section 4.1 as follows. "**Our interpretation on dinocyst assemblage is mainly based on its modern distribution (Thöle et al., 2023). An evolutionary affinity of dinocyst assemblage/cluster can potential hamper an absolute quantitative estimation of paleo-oceanic conditions. For example, *Impagidinium pallidum* is restricted to polar regions in modern ocean (Zonneveld et al., 2013), however, it thrived in lower latitudes in the Neogene and associated with higher SSTs (De Schepper et al., 2011; Hennissen et al., 2017). Given the dinocyst assemblage record found at Site 1168, an alternation from warm (*I. aculeatum* and *O. centrocarpum*) to cool (*N. labyrinthus*) assemblage is distinctive, which was similarly discovered in the Pliocene North Atlantic (De Schepper et al., 2009, 2011)**."
My comment about *I. pallidum* was mainly to serve as an illustration of what could happen if modern analogues are used indiscriminately to interpret palaeontological records. My intention was for this specific example to be included in the current paper, however, I wanted to draw the authors' attention to this assumed ecological uniformitarianism and I believe a broader discussion of the caveats (modern analogues, sharing of ecological niches of the biotic carriers for your SST interpretation and dinoflagellate cysts etc.) is required. This will also address some of the concerns I expressed in Major Comment #1. I think such a paragraph on caveats could (and probably should) be included in the methodology section or precede the discussion
Proposed changes: We elaborate the discussion as follows: "...An evolutionary shift in ecological affinity of dinocyst assemblage/cluster can influence an absolute quantitative estimation of paleo-oceanic conditions**. In light of that, modern analogues of dinocyst distribution should be applied with some degree of caution.** For example, *Impagidinium pallidum* is restricted to polar regions in modern ocean (Zonneveld et al., 2013), however, it thrived in lower latitudes in the Neogene and associated with higher SSTs (De Schepper et al., 2011; Hennissen et al., 2017). **However, the most abundant extant species such as *O. centrocarpum* and *N. labyrinthus* are shown to have comparable SST ranges in the past, by referring to geochemical proxies (De Schepper et al., 2011; Hoem et al., 2021, 2022; Hou et al., 2023b; Sangiorgi et al., 2018), and today. Besides temperature affinities, dinocyst distributions can also indicate salinity in the modern ocean However, quantitative salinity reconstructions remain scarce, and as a result the absolute range of salinities for the Pliocene are unknown. Thus, we can only postulate relative surface salinity change across MIS M2.** Given the dinocyst assemblage...**"**

Reply: Indeed, the concentration/flux was not presented. Total concentration of dinocysts remains relatively stable throughput the record, except a substantial increase at 34.05mbsf (~3240 ka).
Proposed changes: We will update the supplementary data with sample weight, dinocyst concentration and flux. We will incorporate the concentration/flux information into supplementary file and results.

If not included in Figure 4, please do indeed supply it as supplementary data. At 3240 you seem to have your maximum for *Operculodinium centrocarpum* (high-ocean cluster in Thole et al 2023). May be interesting to explore this in the future, but I appreciate this may not be the main focus of the current study.

Reply: We will update the data in zenodo.

Reply: We have carefully read these literatures during our study. As the reviewer mentioned, those studies are focusing on the other side of the earth, thus they were not cited in the first submission. Regarding the forcing of MIS M2, it is still mysterious and requires more investigation. Although De Schepper et al. (2013) has proposed a shallow open Central American Seaway hypothesis, modelling outputs do not support that (Tan et al., 2017; epsl).

Proposed changes: We will cite the suggested literatures, please refer to point 3 above.

I agree that the actual records from De Schepper et al. (2009) and (2013) are indeed from the Northern Hemisphere but the mechanisms that these authors propose will have implications for records in the Southern Hemisphere. This is emphasized in De Schepper et al. (2014) where several paragraphs are dedicated to the Antarctic domain.

Reply: we will make a northern-southern hemisphere comparison in section 4.2.

Proposed changes: "...reconstructed. **Previous similar combined dinocyst and SST records across MIS M2 were generated along the path of Atlantic Meridional Overturning Circulation (AMOC; e.g., De Schepper et al., 2009a, 2013, 2014). In those records, no obvious lead-lags can be observed between dinocyst assemblage, SST and $\delta^{18}O_{bf}$. Such a spatial difference may be accounted for different forcing processes.** Thus, the mechanism we propose involves the ocean as source and sink of atmospheric CO_2 (Kirby et al., 2020) and the shifting fronts and Antarctic ice extent (Toggweiler et al., 2006) **due to the hysteresis of East Antarctic ice sheet. Our data shows that the two subpolar zones behaved fundamentally differently during the M2 deglaciation phase."**

I agree with the proposed changes for the Minor Comments I raised in the original review.

Reply: Thank you!

Thöle, L.M., et al., 2023. An expanded database of Southern Hemisphere surface sediment dinoflagellate cyst assemblages and their oceanographic affinities. J. Micropalaeontol. 42, 35- 56 10.5194/jm-42-35-2023.