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.

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, $\delta^{18}O$ 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 $\delta^{18}O$ 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.

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

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 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.

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

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.