

## Comment on cp-2021-102

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Referee comment on "Reconstructing Antarctic winter sea-ice extent during Marine Isotope Stage 5e" by Matthew Chadwick et al., Clim. Past Discuss.,  
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The manuscript of Chadwick et al., titled "Reconstructing Antarctic winter sea-ice extent during Marine Isotope Stage 5e", presents a comprehensive study of nine circum-Antarctic sediment records covering the time span of Marine Isotopic Stage 5e (130-116 ka BP) to reveal the temporal and spatial dynamic of Antarctic sea ice under warmer than present climate. The Manuscript (MS) is well written, logically structured, and addresses a major burning question in paleoclimatology at the moment, the behavior of the Southern Ocean environment in a time warmer than present. In addition to their earlier study of circum-Antarctic MIS 5e records based on diatom sea ice and surface temperature reconstructions, comprising previously published cores from locations north of the modern winter sea ice boundary (Chadwick et al., 2020), this new study presents 7 out of 9 diatom records from locations south of the modern sea ice boundary. The authors highlight this fact as a major advantage, as all diatom-based studies before did present only records north of this significant boundary.

Such a study depends mostly on the reliability of the age models for the sediment core sections covering MIS 5e to get an appropriate synchronization of the sequence of events comprising Late Termination II (132-130 ka BP) and MIS 5e (130-116 ka BP) (see e.g. Bianchi and Gersonde, 2002). In the case of the present manuscript (MS), five out of nine chronologies for MIS 5e are based mainly on the correlation of downcore magnetic susceptibility measurements to a dated target curve, like the benthic foraminifera oxygen isotope curve of Lisiecki and Raymo (LR04; 2005). I don't want to talk here about the known timing uncertainties of the benthic isotope stack especially in the Pacific sector of the Southern Ocean of several thousand years, but want to raise a problem concerning the use of magnetic susceptibility as an homolog for glacial-interglacial cyclicity in the circum-Antarctic seasonal ice zone. The theory behind chronologies based on magnetic susceptibility is the fact, that many studies before have shown increased concentrations of magnetizable particles in sediments related to glacial marine conditions, compared to sediments deposited during interglacial times. But most of these records come from open ocean sites and the glacial particle input is related to increased aeolian transport of iron-enriched dust during glacial times. However, for core sites in the seasonal sea ice zone around Antarctica this is different, because in some regions perennial sea ice cover might prevent dust particles from sinking down directly at the location where they hit the air-

water interface and got displaced by sea ice movement. Furthermore, in the circum-Antarctic waters, especially in vicinity of the continent, we get another source of iron-rich particles, so-called ice rafted debris (IRD). IRD is transported mainly northward by ice bergs from the point of the Antarctic coast or shelf ice, where they calve. During glacial times, iceberg calving and thus northward transport of IRD might be reduced, compared to our present warm time or even times warmer than present, like MIS 5e. Thus, one needs to differentiate carefully between the different terrigenous particles in a sediment core, to exclude bias of the magnetic susceptibility due to IRD. The present MS fails to argue in that way, nor does it present a clarifying sedimentological analysis of these particles, and uses magnetic susceptibility-based chronologies without discussion, decreasing the reliability of these age models in my eyes significantly.

Furthermore, the authors of the MS failed to explain the causes leading to a significant lack of MIS 5e-covering sediment records south of the present winter sea ice extent. Interpretation of diatom records and the estimation of sea surface temperatures and sea ice concentration from diatom census via transfer functions are highly dependent of the preservation stage of diatom assemblages (Zielinski et al., 1998; Esper et al, 2010; Esper and Gersonde, 2014a,b). Significant dissolution biased the composition of assemblages from oceanic sites located in areas with about 75% or more WSI occurrence probability (Zielinski and Gersonde, 1997; Esper and Gersonde, 2014a). In general, these areas are characterized by low biogenic opal deposition (Geibert et al., 2005). As this is true for modern diatom assemblages, this might be even worse in sediment samples from glacial times, with an opal belt moving northward circumpolar and opal concentrations decreasing in the core sediments. Dissolution-biased downcore records of diatoms treated with Modern Analogue Technique-based transfer functions (TF) with preservation-adjusted reference data sets would lead than to a kind of non-analog situation in the analog sample selection sequence of the TF. Esper et al. (2010) have shown, that especially weakly silicified diatoms, like the sea ice diatoms *Fragilariopsis curta* and *F. cylindrus* are prone to selective dissolution, altering diatom assemblages in vicinity of sea ice to assemblages dominated by heavily silicified diatoms of intermediate temperature affinity and no sea ice relation (e.g. *Fragilariopsis kerguelensis* and *Thalassiosira lentiginose*). TF treatment of such altered diatom assemblages leads to temperature overestimation and sea ice concentration underestimation (Esper and Gersonde, 2014a,b). This becomes obvious in the presented sea surface summer temperatures (Fig. 4), with maximum values of about 6°C in the late Termination II and >4°C during MIS 5e in most of the cores from the Atlantic and the Indian sectors, where sea surface summer temperatures of 0° to 1°C prevail today. Thus, the problem of selective diatom preservation might have led to the exclusion of many diatom records located south of the winter sea ice boundary from previous MIS 5e studies. However, in the MS of Chadwick et al., this issue is not addressed nor did the authors present any clue for the preservation stage of their nine diatom records. Neither did the authors present measurements of biogenic opal to proof the quality of the diatom assemblage concerning preservation.

To conclude, I want to highlight the scientific significance of the study presented by Chadwick et al. Addressing sea ice variability and temperature field changes in an environmental setting warmer than present day is very important for answering questions on the current climate change. However, the scientific approach chosen by Chadwick et al. needs a careful revision, corrections and a detailed consideration of diatoms as sea ice and temperature proxy in the seasonal sea ice zone around Antarctica. Besides the significant uncertainties I raised concerning the age models and the sea ice and temperature estimates based on diatom assemblages, the general form of presenting this MS is clear, concise and well-structured. Before getting this MS published, I highly recommend a

detailed estimation of the preservation stage of each record to proof, that the quality of the assemblages is good enough for a transfer function treatment. Such a quality appraisal could for example be done following the approach of Benz et al. (2016), who presented different levels for diatom preservation quality, TF estimate quality, and age model quality. Concerning age model construction, the authors may find some clues in the recent publication from Xiao et al., 2016, dealing with dating obstacles in the Atlantic sector of the Southern Ocean. In the current state I recommend a rejection of the MS until the authors have proven the applicability of diatoms as reliable environmental proxies in the seasonal sea ice zone and the reliability of the applied age models.

## **General comments**

### **Abstract**

1.) There is no information included in the Abstract, on which proxies (e.g. marine diatoms and TF-derived environmental conditions) the study is based. Nor is there any detail on methodology for sea ice and sea surface temperature reconstructions mentioned. However, both information would be of great value for getting the main idea behind the study immediately.

2.) There is no proxy for meltwater flux or ACC flow band shifts mentioned in the Abstract.

### **Introduction**

3.) Line 057: Missing reference for a definition of the timing and length of MIS 5e – e.g. Fischer et al. (2018) define the Last Interglacial (LIG) (129-116 ka BP).

4.) Line 075: Chadwick et al., 2020 previously presented a circum-Antarctic reconstruction of winter sea ice extent and sea surface temperatures. So what is the difference or gain of the new study?

5.) Line 079: The main approach of this study, to transform qualitative sea ice extent and

temperature variation estimates based on diatom assemblages into qualitative values of sea ice concentrations and sea surface temperatures is not described or referenced, concerning reliability and applicability in the working area.

## **Material and Methods**

6.) Line 091: The question arises, why no diatom records from MIS 5e south of the modern winter sea ice extent have been published before. Are there factors hampering previous methods? How will these potential obstacles be overcome?

7.) It is not mentioned how the effect of selective preservation of diatoms, especially south of the winter sea ice extent has been addressed! Bad preservation is a main factor negatively influencing transfer function results of diatom assemblages!

8.) Line 110: The reference of Crosta et al (2020) does not describe the application of MAT for sea ice reconstructions nor does it describe MAT in detail or deal with the mentioned D-257-33 configuration of the TF applied in the MS!

9.) Schweitzer (1995) is a bit old-fashioned for a sea ice reference data set (resolution only 2x2 deg) -) see Esper and Gersonde (2014)

10.) Line 123: A relatively high uncertainty for sea ice concentration estimates!

11.) Line 124: As we have no information on the circum-Antarctic distribution of the TF reference samples, regional lacks for e.g. the Pacific sector cannot be addressed! It would therefore be of great benefit to see the spatial distribution of the training data set of the TF to avoid regional biases.

## **Age Models**

12.) Five out of nine sediment core chronologies for MIS 5e rely mostly on the comparison between magnetic susceptibility and the benthic foraminifera isotope stack of Lisiecki and Raymo (2005) (Table 2). This is problematic, because magnetic susceptibility records in the seasonal sea ice zone might be biased by ice rafted debris and seasonal ice cover, at the end not reflecting glacial-interglacial cyclicity. This becomes especially than problematic, if no other age source could be used in addition or comparison, like in cores TPC287, NBP9802-04, and ANTA91-8. Totally questionable is the dating method for core PC509, using wet bulk density as a proxy for biogenic opal, which is a proxy for glacial-interglacial productivity changes. Thus, this parameter is prone to several alteration processes, starting with changing downcore sedimentation rates leading to different compaction rates and not ending with selective diatom preservation altering the opal content. Thus measurements of magnetic susceptibility or biogenic opal are good proxies for a quick and dirty age model, especially onboard a research vessel, but lack the reliability needed for timing the climatic events related to MIS 5e to be presented in a research paper.

13.) I wonder, why the reliable diatom stratigraphic marker *Rouxia leventerae* (Zielinski et al., 2002) has not be applied, as detailed diatom assemblage should be available for this study. I also wonder, why the diatom stratigraphic marker *Hemidiscus karstenii* has been used, although this diatom got extinct end of MIS 7 (about 191 ka BP according to Zielinski and Gersonde, 2002). The biostratigraphic approach needs to be improved.

14.) The age uncertainties of all cores are >2.5 ka, projecting discussions of leads and lags of sea ice processes compared in different Antarctic sectors into the error range! Tuning only one proxy record (e.g. magnetic susceptibility) to a target curve (e.g. oxygen isotopes) for each core is a bit weak. It would be good to have at least one biostratigraphic datum for each core to get a starting point for the tuning correlations.

15.) Line 162-166 : The discussion on the use of diatom of the genus *Rouxia* lacks necessary details. First, one should use the presence or absence of the defined species *Rouxia leventerae* only, as e.g. *Rouxia constricta* got extinct end of MIS 8 (about 280 ka ago) and its presence would point to significant reworking. Second, an abundance of *R. leventerae* > 1% for MIS 6 diatom assemblages is reported from core location north of the present winter sea ice edge only (Zielinski and Gersonde, 2002), not neglecting possible influence of selective preservation on the record ANTA91-8 far south of this boundary and therefore altering the maximum abundance of *R. leventerae*!. Thus, without a detailed examination of the diatom preservation, *Rouxia* sp. does not corroborate anything.

## Results

16.) First of all, it is important to remark, that not all cores exhibit the same chronological resolution. Core U1361A for example, has only 6 samples within 12ka, leading to a

resolution of one sample per 2k years. Other cores, like TPC287 and ANTA91-8 have a better resolution of one sample per 800 years. Only the latter cores are then appropriate to indicate short variations in sea ice cover and surface temperatures. Low res cores are prone to signal distortion due to uncertainty changes.

17.) MAT sea ice concentration estimates and FCC cumulative abundances indicate for the Atlantic sector low to intermediate sea ice cover during the late glacial stage 6, increased sea ice values during Termination II and relatively high sea ice cover during MIS 5e (Figure 3). Sea ice records for the Indian and Pacific sectors indicate low, but constant sea ice cover over the whole analyzed interval (132-120 ka). Taking into account, that only the two Atlantic cores TPC290 and TPC288 are located north of the modern winter sea ice edge, the WSIc values at least for the glacial Termination are far too low, indicating a sea ice retraction from the modern winter extension at most of the core locations during the glacial and sea ice expansion during MIS5e (especially at core position TPC287).

18.) Line 196: Unfortunately, the reference of Armand et al., 2005, assuming *F. separanda* to be related with sea ice is not state of the art any more. Esper et al. (2010) indicate a wide temperature range of -0.5°C to 4°C for this species in marine sediments. Surface water studies report a temperature range of 1.2°C to 8.7°C, which makes it unlikely that *F. separanda* is a typical sea ice related taxon (Esper et al., 2010). Esper and Gersonde (2014b) show a temperature range of *F. separande* in 336 surface sediments between 0° to 8°C with a maximum abundance occurring around 2°C. Thus, the presented explanation of *F. separanda* biasing the TF to colder temperature values and higher sea ice concentrations is not likely.

19.) Regarding the reliability of sea ice TF results, one should not rely on the dissimilarity threshold only, but should have a look to the origin of the 5 analogs itself. As a quality measure, one can say, that the closer the analogs were selected regarding the core location, the more likely the relationship between assemblage composition and target environmental variable is. Esper and Gersonde (2014) showed significant differences of the diatom assemblage composition in the three Antarctic sectors regarding locations in sea ice vicinity. Thus, it could be important for a reliable sea ice concentration estimate, that the MAT analogs are selected as close as possible to the core location, or at least coming from the same Antarctic sector.

20.) Generally, the authors seem to avoid to describe their signal in full chronological length, starting at the end of stage 6. Otherwise, I am not able to explain, why they do not question the low sea ice values during the glacial Termination II. For example, Atlantic core TPC287 indicates low sea ice values before 130 ka, highest sea ice concentration at the Antarctic Ice Core Thermal Optimum around 129 ka and higher than Termination II sea ice values across MIS 5e. A similar pattern can be found in the Pacific sector. This is in direct contrast to the results published by Bianchi & Gersonde (2002) for the area directly north of the modern sea ice edge in the Atlantic sector.

21.) In the Atlantic sector, cores TPC288 and TPC287 show highest sea surface temperature during glacial Termination II, a significant drop in temperature during the

Antarctic Ice Core Thermal Optimum and lower than glacial temperature during the Last Interglacial!?! For example, core TPC287, today located in the seasonal sea ice zone with modern summer temperatures around 0°C (Fig. 1) shows post-glacial (!) temperatures of 6°C and interglacial values below 1°C. Core TPC288 slightly north positioned from that shows also 6°C post-glacial and up to 4°C interglacial temperatures, but in the Antarctic Ice Core Thermal Optimum interval, temperatures drop to nearly 0°C. What's going on there?

22.) The authors support their MAT-based temperatures with so-called "subtropical diatom" abundances, which have not been defined as a group or even mentioned in the Material & Methods section. Neither do they present counts or graphs of single diatom species, which could help to identify the nebulous "subtropical" species. The subtropical species within the Romero et al. (2005) reference have their habitat indeed in the subtropical zone, about 20° in latitude to the north of the core locations, with water temperatures of >10°. It is obvious, that those species are rather unlikely to be endemic in the Antarctic Zone, thus their sporadic appearance might be addressed to lateral transport. On the other hand, the term "subtropical" might be misleading. I recommend a table of those species belonging to the group. Assuming truly "subtropical" species to be present in the core, this would also bias the TF estimates by shifting the "collection area" of the 5 analogs too far to the north, leading to higher surface temperature averages and low sea ice concentrations.

## Discussion

23.) Generally, it makes no sense to comment on the **Discussion** section in detail at this point, as changes in the environmental variables to be discussed may change significantly, if all the questions raised concerning diatom assemblage reliability and age model reliability have been addressed. In the following, I will comment therefore only on issues independent from age and sea ice/temperature estimates.

24.) Line 272-272: First of all, according to Crosta et al. 1997, increased *Chaetoceros* Resting Spore (CRS) abundance might point to higher meltwater discharge from Antarctica. Such a CRS peak occurs in many known diatom records around Antarctica within glacial terminations II and I (e.g. Bianchi and Gersonde, 2002, 2004; Benz et al, 2016). Second, I wonder why CRS are included in the TF. According to multivariate statistical analyses (e.g. Esper et al., 2010; Esper and Gersonde, 2014a,b) CRS variance is neither related to sea surface temperature variability nor sea ice cover variability. Esper and Gersonde (2014) discuss the unnecessary integration of reference species not related to the variance of the target variables (temperature, sea ice). I suggest to adjust the TF reference data set to avoid such phenomena.

## Conclusions

25.) In general, the **Conclusions** section continues the **Discussion** section by adding more comparisons between own results and the literature. In my opinion, the **Conclusions** section should be reduced to a short recapitulation of the major results and significant statements on the implications. The remaining text could be integrated into the **Discussion** section.

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