Clim. Past Discuss., 9, C597–C620, 2013
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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Holocene climate variations in the western Antarctic Peninsula: evidence for sea ice extent predominantly controlled by insolation and ENSO variability changes" *by* J. Etourneau et al.

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Dear Editor,

We have the pleasure to prepare for Climate of the Past a revised manuscript entitled "Holocene climate variations in the western Antarctic Peninsula: evidence for sea ice extent predominantly controlled by changes in insolation and ENSO variability" by Dr. J. Etourneau, Dr. L.G. Collins, Dr. V. Willmott, Dr. J.-H. Kim, Dr., L. Barbara, Dr. S; Schouten, Dr. J.S. Sinninghe Damsté, A. Bianchini, V. Klein, Dr. X. Crosta, and Dr. G.

Massé (previously referenced as cpd-9-1-2013).

Firstly, we would like to extend our thanks to J. Pike and the anonymous reviewer for their helpful and constructive comments. After careful revision of our arguments and manuscript text, as suggested by the two reviewers, we truly think that our corrections will meet most of the main reviewer's comments and CPD criteria. Corrections in the revised manuscript are highlighted in red. We have added 2 figures that further support the interpretation of our data. We have also included error bars in the temperature reconstructions in Fig. 4 and have clarified which curves correspond to diene and triene concentrations (Fig.5), T. antarctica (warm variety), TEX86L (Fig.7), cholesterol and dinosterol, D/T and T. antarctica (cold variety) (Fig.8).

Please see below the responses to the major points underlined by the reviewers.

We thank you very much for considering our manuscript.

Sincerely Yours, The authors

Reviewer #1:

1. I would like the authors to reconsider their interpretation of weaker UCDW influence on the shelf during the mid-Holocene. This is in contradiction to previous interpretations (many of the publications on ODP Site 1098) that there was persistent presence of UCDW in Palmer Deep through the mid Holocene until 3.6 ka. Etourneau et al. make no reference to this literature for this reason (they do refer to it for other interpretations). Also, Lamy et al. (2010) show that post-deglaciation, the southern westerly wind belt was more southerly through the mid Holocene, retreating northwards _3.5 ka, thus there should be a more consistent influence of UCDW along the WAP shelf during the mid Holocene than during the late Holocene.

Response 1: there are indeed several existing hypotheses regarding the influence of UCDW on the WAP shelf during the Holocene. Shevenell et al. were the first to report at the ODP Site 1098 a quantitative estimate of sea surface temperature changes along

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the WAP, and describe an oceanic cooling trend through the mid-Holocene that cannot be reconciled with a continuous supply of warm UCDW onto the shelf, potentially linked to a poleward shift of the southern westerly wind (SWW) belt. Only the millennial scale 'warming events' can be correlated to UCDW influence and a southward migration of SWW, and not the long-term trend. Our temperature record, which reveals roughly the same pattern as Shevenell et al. (the main difference residing in the amplitude of temperature changes), also reports a cooling trend between ∼7,000 and 3,000 yrs BP and supports these assumptions. This cooling period is further supported by independent diatom proxies, especially by T. antarctica warm variety (see fig. 7), that also yield a cooling trend during the mid-Holocene. The role and migration of the SWW belt during the Holocene is still unclear. Is there a poleward shift/contraction/strengthening of the wind belt? Lamy et al. did not directly document a change in the position of the SWW, but a change in the intensity as their records 'only cover a small latitudinal band in the south'. However, they attempt to report 'by analogy to modern seasonal changes' a southern shift of the SWW belt during the mid-Holocene. However, they also recognized that numerous other studies show different results and interpretation. For instance, Montade et al. (2012) inferred changes in their pollen data from a core located under the influence of the SWW in Chile (close to the Lamy et al. core site location), to show increasing cool and wet conditions during the course of the mid to late Holocene, thus implying a northward migration of the SWW from 7,000 yrs BP to modern. Comparatively, Marcos et al. (2012) demonstrate enhanced SWW over north-eastern Patagonia during the mid-Holocene, which does not support a southward shift of the wind belt. Conversely, the modeling study by Varma et al. (2012) does show that, overall, the SWW were strengthened and shifted south during the mid- to late Holocene. However, they also mention that, during the spring season (when sea ice is melting in the WAP), the wind belt moved equatorward, not poleward, implying a strong influence of seasonality on the changes of the position of the wind belt circulation. Furthermore, they also show a weakening of the mean annual Southern Ocean upwelling within the 50-55°S latitudinal band, and a strengthening within the 55-65°S between

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7,000 and 250 yrs BP. This was linked to the annual-mean poleward shift of the SWW. However, there is unfortunately no comparison between the different seasons which pose fundamental questions regarding the seasonal variations in the upwelling intensity in the coastal regions during the winter, spring and summer. Here, our temperature record indicates that increasing warm temperatures were not a feature of the WAP during the mid-Holocene, compared to the early or late Holocene. We strongly believe that changes in the seasonality are the primary factor controlling climate variability in the WAP. However, clearly, more work is needed to unravel the varying modes of seasonality associated with the WAP. In the revised manuscript, additional text and discussion has been added regarding the reconstruction of the southern westerly wind belt during the mid- and late Holocene (l.457-467 and l.562-574).

In Figure 7 (in the revised version of the manuscript) is shown the comparison between the TEX86-L temperature (°C) record and T. antarctica warm variety (%). The diatom record has not been shown in Fig.5 of the manuscript and will be included in its revised version. These two independent records depicts the same profile, a warm Holocene followed by a cooling period before increasing temperatures. This clearly supports our assumptions that the mid-Holocene was a cooling interval and not a warming phase.

2. In terms of La Niña influence along the Antarctic coast, Etourneau et al. are correct in that mostly La Niña brings cooler conditions, however, this is not the case for the WAP. Here, La Niña brings a flow of warm, northerly air across the region. This is in contrast to cooler conditions across the eastern Antarctic Peninsula (i.e. the site of the James Ross Island ice core – Mulvaney et al., 2012), due to the Antarctic Dipole. Hence, I would like the authors to reconsider their interpretations, particularly during the late Holocene, with respect to ENSO.

Response 2: Although it might not be clear in the manuscript, we perfectly agree with the reviewer. This is now clarified in the revised version. ENSO and more specifically La Niña cool the Pacific sectors of the Southern Ocean, but warm the WAP and the eastern side of the Antarctic Peninsula (Stammerjohn et al., 2008; Yuan et al., 2004).

We wrote in the revised manuscript (l. 516-527): 'ïĄŻ. . .ïĄİStrength and frequency of ENSO vary in line with changes in east-west temperature and pressure gradients along the equatorial Pacific. For the Antarctic Peninsula, El Niño events result in high pressures over the Amundsen Sea advecting cold air from the south and promoting a cooling of sea surface temperatures and heavier sea ice conditions. Conversely, la Niña events promote a deeper Amundsen Sea Low generating a flow of warm and northerly air across the WAP (Yuan, 2004; Stammerjohn et al., 2008; Russell and McGregor, 2010). Temperatures and sea ice in the Antarctic Peninsula are more sensitive to La Niña than El Niño events and respond with greater amplitude to the former (Yuan and Martinson, 2001). These two climatic states also induce a shift in the Westerlies, with the circumpolar wind belt transposed further south during La Niña events, which would in turn cause an increase in the upwelling of warm UCDW. Thereafter, the El Niño and La Niña signal would propagate into the warming WAP surface waters (Martinson et al., 2008; Willmott et al., 2010; Steig et al., 2012).'. We then postulate that the increasing surface temperatures during the late Holocene is likely induced by stronger La Niña events, and that the enhanced frequency and amplitude of ENSO events (El Niño vs La Niña) increased the seasonal variability in the WAP, explaining why sea ice cover is paradoxically also very high. (see. 5.2.4)

3. The HBI index D/T has been presented as an indicator of sea ice vs open water. This is not strictly the case as shown by Collins et al. (in review, Quaternary Science Reviews). Those authors show that D/T in the Scotia Sea is more of an indicator of sea ice vs. marginal ice zone conditions, i.e. not true open water, but sea ice vs. icy/slushy/partial ice-free etc. I would not normally ask authors to consider papers that are in revision, however, given that some of the authors are the same for both papers (and I also admit to being a co-author), I think it is reasonable to ask for consideration for issues of future consistency.

Response 3: It is true that during the submission and revision of Etourneau et al., Collins et al have revised the interpretation of the HBI triene, albeit in a very different

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environmental setting: the open ocean Scotia Sea, during the glacial. In this environment oceanic zones are more demarcated than along the Antarctic margins with a clear transition from seasonal sea ice to the marginal ice zone to the permanently open ocean. One of the suggestions, put forward by Collins et al., regarding triene, relates to the ambiguity of the term ‘open waters’, an important consideration in such an environment. Collins et al. propose that triene is primarily biosynthesized in stratified, nutrient-rich open waters. In the open ocean glacial environment such a setting is primarily associated with the sea-ice edge or marginal ice zone, rather than the comparatively less productive open ocean. In addition, the records provided by Collins et al. are under the influence of oceanic currents arising from the Weddell gyre, and therefore have a different influence than those regulating the WAP coastal regions. Stratified nutrient-rich open waters along the Antarctic margins can be generated via means other than sea-ice melt (i.e. glacial ice melt, UCDW upwelling). Therefore, while we appreciate the interpretations of Collins et al. regarding HBI in the Scotia Sea, in Etourneau et al., we use the HBI proxy in a more conventional way, as previously demonstrated by other coastal studies (Barbara et al., 2010; Denis et al., 2010; Massé et al., 2011). Each of these studies demonstrate a good correlation between the ratio D/T (diene/triene) and sea ice/open water diatom groups during the Holocene, and thus propose that the D/T ratio can be used as a reliable proxy for monitoring past changes in sea ice/open water, along the Antarctic margins. However, this new and exciting proxy needs further calibration work in order to improve its use, particularly its interpretation regarding coastal versus oceanic areas where seasonal sea ice cover is much less extensive in open water regions, and might distinctly affect the triene synthesis compared to margin zones.

4. I would like to ask the authors to consider their interpretations of the late Holocene ice environment in light of the recently published Pike et al. (2013) paper. I realise that their manuscript was submitted prior to the publication of this paper, however, given that the two papers are from the same location (Palmer Deep) I think it is appropriate that they should at least consider it. This is because I believe it will help to sort out their

apparent inconsistencies in the late Holocene. Etourneau et al. focus strictly on sea ice, whereas Pike et al. (2013) consider glacial ice vs. sea ice, arguing that the two represent different processes. Glacial ice is derived from glaciers that terminate in the ocean and floating ice shelves, etc. and can be driven by atmospheric forcing. Glacial ice (slush/brash ice/bergie bits etc.) can occupy the summer ocean following sea ice melt and inhibit the growth of diatoms would thrive in a summer sea ice melt-induced environment. Hence, Pike at al. (2013) hypothesise that it is possible to have early sea ice melt, but also cool SSTs due to the glacial ice input through the summer. I would like the authors to consider whether the organisms that produce the molecules analysed for D/T and TEX86L are influenced strictly by sea ice, or could also be influenced by environments dominated by ice of any source.

Response 4: indeed, Pike et al. (2013) has been published after the submission of this manuscript in CP. We of course cite this work and refer to it in the revised version of the article, and include further detailed explanations (e.g. l.224-234). The HBI diene is mostly produced within the sea ice matrix as it is colonized by algae during the warming spring and summer months. Comparatively, glacial ice does not support an internal algal community and therefore is unlikely to directly influence this compound. Regarding indirect influences, the calving of large icebergs has been shown to encourage pack ice growth (Arrigo et al 2002), which would result in greater diene production, and thereby lead to a higher D/T ratio, which could be consistent with the reconstructed diene concentration for the late Holocene (see fig.5 below). We cannot therefore totally exclude that the melting of glacial ice could indirectly impact the diene concentration during this period. The triene concentration was elevated, parallel to the diene concentration, over the last 2,000 years (see Fig.5). The HBI triene can be affected by broken ice shelves that may inhibit algal growth, especially the Rhizosolenoid species, the main triene synthesizors, by shortening the growing season, thus leading to lower triene synthesis. This could be even more relevant under a warmer summer scenario, where upwelled warm UCDW waters would accelerate the ice shelf melting, but this is not consistent with our reconstructed triene record. It is also possible that a longer sea ice season (influenced by iceberg presence (Arrigo et al., 2002)) could result in a more intense summer growth period due to greater sea ice melt and induced stratification in a high-light environment, thus resulting in higher concentrations of triene in the summer, and thus tending the D/T ratio to lower values (Stammerjohn et al., 2008; Collins et al., 2013). However, the decline of specific diatom species (e.g. CRS) indicative of stratified conditions does not support such a scenario. As mentioned earlier, the HBI proxy is used here in a more conventional fashion, so that the diene concentration reflects sea ice conditions, while triene concentration is coupled to open water conditions. In addition, our interpretation is based on the HBI record combined with other records (diatom assemblages and TEX86L-derived sea surface temperatures), which appear to support our interpretation. The GDGTs are mainly produced by Thaumarchaeota during the winter and early spring, prior to or during the early sea ice melting season, and to a much lesser extent during the summer (Kalanetra et al., 2009; Murray et al., 1998). Their period of occurrence might be related to the competition with other microbes growing during the summer season (Murray et al., 1998). These authors recognized that there are still many remaining questions regarding the physiological lifestyles of the archaea, and, again, more work is required. However, while we cannot testify that other types of ice could not directly influence the GDGTs synthesis, because, as mentioned above, icebergs can promote a favorable sea ice environment for archaea growth, changes in seasonality could strongly affect the local ecosystem living in the surface waters, including the direct competitors of archaea. We have to admit to our limited knowledge regarding the influence of the different kinds of ice (slush/brash ice) on the production of select organic compounds. Ongoing studies are presently investigating the synthesis of HBIs in a variety of polar surface waters and we agree that more work is absolutely needed. This should be one of the key issues to address for future studies using these proxies. However, we believe that these issues are not relevant to the scope of this paper and should be addressed in a separate and independent article. According to our results, we think the warming SST and increasing sea ice during the late Holocene do not represent an inconsistency. We propose that this can be explained by changes in seasonality, an increasing contrast between warm summers and cold winters driven by increasing ENSO frequency and amplitude during a low in annual and spring insolation, and a high in summer insolation; an alternative explanation that has never been proposed for the WAP before. As mentioned by the reviewer, sea ice and glacial ice are subject to two different processes (oceanic vs. atmospheric temperatures). If indeed, glacial ice, induced by strong glacier melting due to warming atmosphere and tropical/extratropical teleconnections, occupied the summer ocean during the late Holocene, this would have cooled the surface ocean - as recognized by Pike et al. - and we should not observe a +2°C warming surface temperatures (Fig.3). Strong upwelling conditions are therefore required to increase the supply of warm UCDW into the surface waters that may have, in turn, accelerated the melting of an extensive sea ice cover developed during the cold winter and spring seasons. In addition, the decrease in total diatom productivity, and more specifically in CRS production, does not indicate a prolonged sea ice melting season during the last 3,000 yrs BP, but instead indicates a rapid melting season compared with the mid-Holocene, which is in good agreement with the Pike et al. data. We therefore argue that changes in insolation, combined with equatorial Pacific climate, governed the climate variability in the WAP during the Holocene, which is in line with the reviewer's and other author's hypotheses.

In Figure 5 (in the revised version of the manuscript) is represented the Holocene diene and triene concentrations records at the JPC-10 core site. The diene concentration progressively increases from 7,000 years BP to modern, with a sharp increase over the last 2,000 years BP. Comparatively, the triene concentration drastically decreases from 9,000 to 3,800 years BP before experiencing a stepwise increase during the late Holocene, with peaks at 3,000, 2,000 and 500 years BP. The two records suggest drastic changes in seasonal sea ice presence in the WAP, in agreement with the other presented records: (i) a warm early Holocene dominated by open water conditions, melting short sea ice season and high supply of warm UCDW, likely driven by high annual and spring insolation, (ii) a mid-Holocene characterized by an extended

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sea ice season and reduced warm waters upwelling in response to annual and spring insolation decline, and (iii) a late Holocene governed by both enhanced sea ice and intense open waters conditions, likely responding to an increasing contrast between very cold winter and warm summer seasons as a result of both low annual/spring insolation, high summer insolation and increasing ENSO variability. This figure, not included in the original manuscript, will be included in a revised version.

Reviewer #2:

The manuscript by Etourneau and colleagues seeks to reconstruct sea surface temperatures and sea ice conditions in the Palmer Deep, as evidence for changing climate in the West Antarctic Peninsula over the last 9,000 years. This is a critical region for understanding climate change, since it is warming faster than many other locations on Earth and it is important to understand both the drivers of the recent trends as well as understanding the main processes and feedbacks which control ocean circulation and thus controlled climate change through the Holocene. The authors present data from several proxies, focusing on two biomarkers (TEX86-L and the diene/triene ratio from diatoms) and diatom assemblages. In general, these indicators produce consistent and reinforcing patterns of oceanographic change, detailing a cooling and expansion of sea ice from the early Holocene to the mid Holocene. In the later Holocene the story becomes more complex and warming as well as sea ice expansion is observed. The authors present a well-written and considered account of the trends which they observe, and it is certainly suitable for publication in Climate of the Past.

1. The manuscript raises important new questions about the connections between Antarctic Peninsula climate and insolation, ocean circulation and tropical Pacific teleconnections. But, there is a challenge here because there are several times where trends are accelerated despite gradual external forcing from e.g. insolation. Perhaps this reflects the sensitivity of this site to the movement of the sea ice boundary, which may be migrating gradually but promotes a threshold response in temperature or diatom species when the sea ice reaches a certain distance from the core site. Although

they show the comparison to Shevenell (2012), they cite other records from the region (e.g. page 18) which could have been used to demonstrate whether or not these changes were rapid at a regional scale or affected just this one core in this way. Instead, they choose to focus on evidence of tropical Pacific climate change, but I feel that this misses an assessment of whether their record is reflective of the region rather than an isolated site.

Response 5: we cannot exclude that some small variations in our records might reflect a regional response to external forcing and not strictly correspond to more widespread changes along the WAP. As explained in the discussion of the manuscript, we recognize that there are some convergences with other records from the Marguerite Bay or Lallemand fjord for instance, but also some discrepancies. The main differences with those records may be due to local changes that affect records in the other WAP regions due to their respective locations, bays or fjords, where the local configuration plays an important role in seasonal sea ice development and oceanic temperature changes. Here, our study sites (JPC-10 and ODP Site 1098) are located on the continental open shelf which implies that our records are particularly sensitive to atmospheric and oceanic changes along the WAP. In addition, the good correlation between recent atmospheric records (Mulvaney et al. (2012)) and our marine records strongly suggest that our records mainly mirror climatic changes along the WAP and make us confident that they are unlikely significantly affected by local effects.

2. The authors use the D/T ratio to give an indication of sea ice extent (D/T is an expression of the relative abundance of two isoprenoid lipids synthesized by diatoms). However, the diene is not specific to diatoms only found in sea ice. I would like the authors to explain more about this proxy e.g. could a general increase in productivity by diatoms lead to enhanced diene production, rather than just a sea ice signal? Is this why the authors choose to use D/T to account for this? How precise is this ratio? For example, in the mid-Holocene (page 19) could the enhanced primary productivity driven by enhanced stratification in response to later melting sea ice not lead to more

production of the triene by open-water diatoms? The rationale for the interpretation of D/T needs to be strengthened either in the early stages of the manuscript or during the discussion. There should also be a note to the more specific (but undetected here) monoene IP25, which has been successfully applied in Arctic sediments. There is a chance that a reader unfamiliar with the subtleties of the differences in compound may assume that these are the same isoprenoids; I think it would be useful to confirm that they are not.

Response 6: see detailed explanations in Response 3 and 4, and the revised version of the manuscript. In addition, we will include short sentences in the manuscript explaining the difference between the IP25 used in the Arctic Ocean and the D/T ratio applied in the Antarctic Peninsula sediments (l.213-221). Contrary to the Arctic Ocean, the monoene IP25 is not present in the Antarctic coastal regions. In contrast, diene and triene are relatively abundant. In the first version of the manuscript, we only used the ratio between the two compounds. In the revised version, we now show the relative concentration of each compound (Fig.5) that gives an indication of sea ice vs open waters conditions, thus supporting our assumptions. Furthermore, in the Antarctic, the diene is strictly synthesized by diatoms specific to sea ice as recently shown by the carbon isotopic composition of this compound (Massé et al., 2011).

3. Furthermore, in the comparison with ENSO (Figure 6) the D/T ratio is used to indicate the sea ice extent. Yet, the diatom assemblages seem to give much more detail (and better constrained information) about surface ocean conditions than D/T. I would have preferred that the comparison was made with selected diatom abundances e.g. Chaetoceros (since it prefers conditions which are more stratified) and T. Antarctica (cold). I think that the arguments and interpretations would be much stronger if the authors compared their diatom data with the ENSO proxies. Page 26 contains the confusing sentence “the applicability of this proxy as an indicator of sea ice... ” but all of the text which precedes this has shown the increased level of detail which can be obtained from the diatoms. In the figure (6) the authors focus on labeling their axes

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according to more or less El Nino events, and yet in the text (page 22) they make clear that the WAP is more sensitive to La Nina not El Nino. So what do the authors mean by “greater ENSO activity” and how does this relate to the WAP? Are they indicating that there is more variability between El Nino / La Nina conditions (higher frequency or higher amplitude fluctuations?) and it is this variability which is translated into WAP sea ice conditions? The increase in La Nina events does not show a convincing relationship to D/T ratio: but might it with the diatom species? Figure 6 should also make clear where exactly these ENSO records come from and what they show. “dinosterol and cholesterol abundances” without any meta-data about the site, it’s location and influence of ENSO, how the interpretation was reached etc is difficult to assess.

Response 7: Indeed, the diatom assemblages give important information for past surface ocean conditions. We also believe that the reconstructed concentrations of both diene and triene combined with the ratio of the two compounds, which vary in line with the diatom records, also provide major insight that can be directly compared to ENSO records. In the revised version of the manuscript, the T. antarctica (cold variety) record is included in addition to the HBI record and compared with past changes in ENSO activity (Fig.8). By increasing ENSO activity, we mean higher frequency and amplitude/intensity. This is now clarified in the revised version of the manuscript (e.g. l. 528, 536). We also specify the location of each ENSO record in the figure 8 caption, although it was already mentioned in the discussion paragraph in the previous version (Lines 24-27, p.22). As also mentioned in the text, the peak to peak correlation is not clear between diatom/HBI/TEX86-L and El Niño/La Niña records (l. 530). This might be due to several factors including the role played by other climatic forcings (e.g. SAM) and the ENSO records themselves, which show significant differences depending on their location, proxies used.... On the other hand, there is a good correlation between the increasing frequency and amplitude fluctuations of El Niño/La Niña and increased seasonality in the WAP on the long-term trend, implying a close link between low- to high-latitude regions.

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Minor comments: 4. How many samples were analyzed for each proxy, and so what is the temporal resolution of each? The authors describe centennial-scale data being achievable, but this only looks to have been possible for the magnetic susceptibility and diatom measurements, and not with the biomarkers.

Response 8: in the revised version of the manuscript, we precisely state that 326, 86 and 81 samples have been analyzed in HBI, TEX86-L and diatoms, respectively, which correspond to a resolution of ∼25, 100 and 110 years (l.122-124).

5. The authors note (page 9) that Thaumarchaeota are also found in sea ice, although the sentence in which this is presented is confusing. Could this have had an impact on their data, or are they introducing this information here to confirm that they do not think that there was any impact (e.g. because the abundance was too low?)

Response 9: See Response 4.

6. Were any of the exact same samples analyzed as Shevenell? It is difficult from Figure 4 to know whether the differences in the absolute values reflect the samples or the technique. Shevenell presented a much higher resolution and more variable record, which makes it difficult to assess the different controls on the two records if the samples were not exactly the same.

Response 10: A short sentence has been included (l.186-188) explaining that we selected 19 samples taken exactly at the same depth as Shevenell et al. in order to obtain a direct comparison of both records.

7. I don't find the use of the word "step" to describe the cooling trends an appropriate term. There are times of accelerated cooling but not as abrupt or large as suggested by the term "step" (e.g. page 13). Likewise, on page 14 the authors describe a "two-step increase", but actually the data show two intervals where the D/T ratio is high. A two-step increase implies that first there was a rapid rise, followed by a second rise much later (and a final position well above the starting point).

Response 11: the word ‘step’ has been replaced by ‘interval’ of cooling or warming.

8. Given the dominance of the Chaetoceros species in the sediment core, section 3.2.4 surprisingly gives little information as to what the habitat or ecological preferences of these species might be. It may be that they are widespread and not indicative of a particular environment (although the subsequent text suggests not), but that should still be stated.

Response 12: we described the environment and preferred location along the Antarctic Peninsula in which CRS mainly develop (Lines 10-18, p.12). Here, we add further explanations regarding their habitat and ecological preferences: ‘CRS mostly develop in the Antarctic Peninsula region and the Embayment systems such as in the Ross and Weddell Seas (Crosta et al., 1997). Along the WAP, its preference for growing is in the northern part where the area has a longer ice-free season (Buffen et al., 2007; Pike et al., 2008). This favors blooming during periods of most intense water column stratification, thus exhausting surface nutrients (Crosta et al., 2007, 2008; Denis et al., 2006; Leventer et al., 1996). CRS are usually associated with high productivity events at the receding sea ice edge (Crosta et al., 2007; 2008; Denis et al., 2006; Leventer et al., 1996, 2002) and constitute most of the preserved diatom species found in the JPC-10 sediments accounting for more than 90% of the total diatom species.’ (l.271-278).

9. Page 23 line 27: needs clarification here, since I assume the authors are trying to say that sea ice persisted longer (D/T ratio) and upper ocean temperatures became higher (Tex86).

Response 13: a ‘and’ was missing. It has been added in the revised version of the manuscript.

10. Page 25 line 11: “during the LATE Holocene we suggest the role of low-latitude forcing...”

Response 14: we replaced ‘during the Holocene’ by ‘during the mid to late Holocene’ as the mid-Holocene marked the onset of the low-latitude climate imprint in the WAP.

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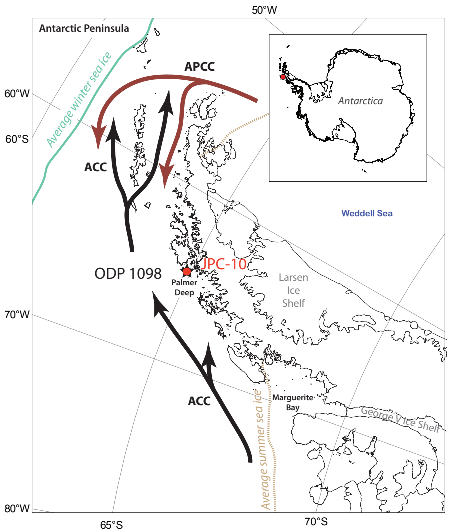

Fig. 1. Figure 1

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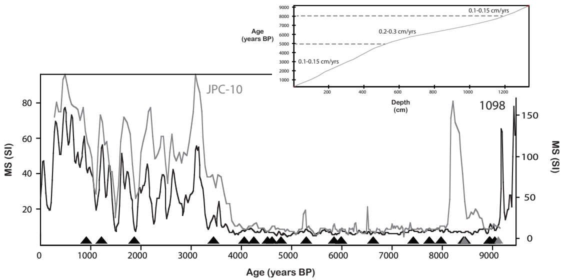

Fig. 2. Figure 2

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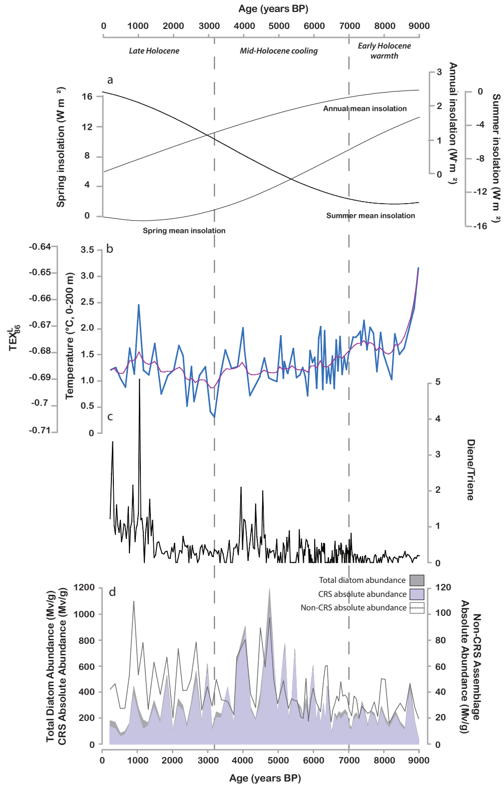

Fig. 3. Figure 3

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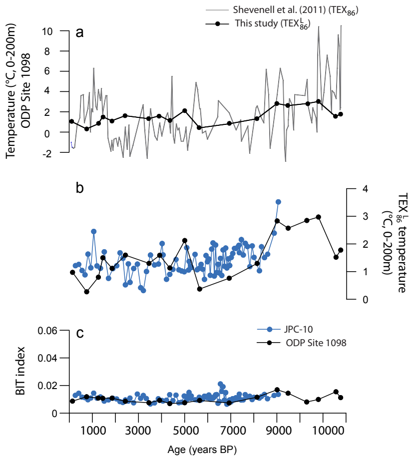

Fig. 4. Figure 4

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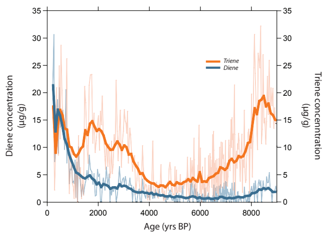

Fig. 5. Figure 5

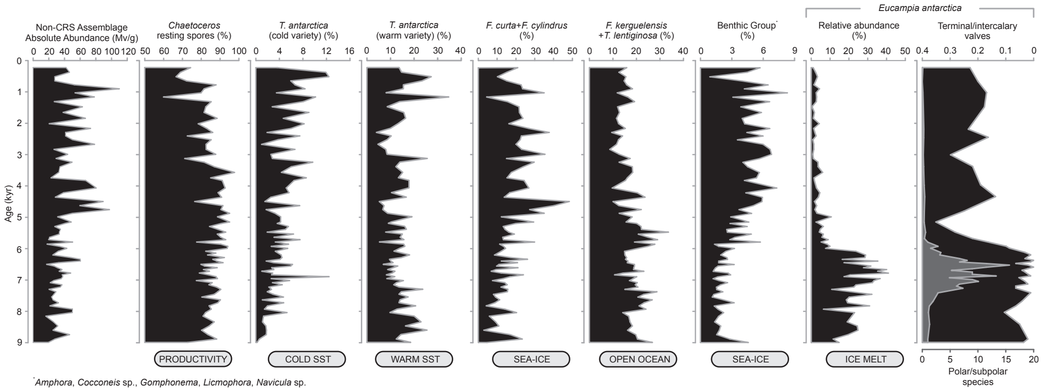

Fig. 6. Figure 6

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9, C597–C620, 2013

Interactive Comment

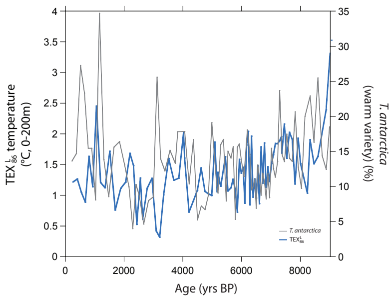

Fig. 7. Figure 7

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Interactive Comment

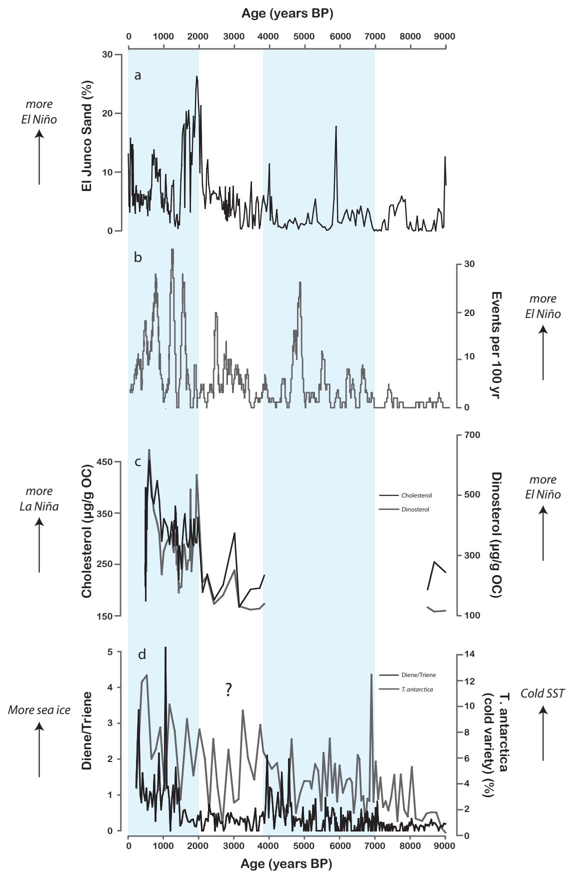

Fig. 8. Figure 8

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