Clim. Past Discuss., 9, C67–C71, 2013
www.clim-past-discuss.net/9/C67/2013/

Climate of the Past Discussions

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.

Anonymous Referee #2

Received and published: 25 February 2013

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, focussing 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 expension 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.

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.

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 synthesised 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.

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 labelling their axes 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.

Minor comments:

How many samples were analysed for each proxy, and so what is the temporal reso-

lution 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.

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?)

Were any of the exact same samples analysed 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.

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).

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.

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).

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

Interactive comment on Clim. Past Discuss., 9, 1, 2013.