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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.

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This manuscript presents a detailed, carefully analysed, well-considered and well-argued multiproxy study from Palmer Deep on the west Antarctic Peninsula (WAP). It is well-written and presented and is within the scope of Climate of the Past. It represents the first application of the HBI sea ice proxy, as well as the first application of the TEX86L proxy, to the Holocene WAP. The combination of diatom assemblages, HBI and TEX86L analyses is a powerful one and allows significant insight into the processes forcing marine variability in this region. The authors are careful with their

interpretations and I commend the careful comparison with the previously published TEX86 record (Shevenell et al. 2011). The comparison mostly shows that the pattern of SST change is consistent between two records, but the newer calibration produces more realistic absolute values of SST.

In terms of interpretation I would like the authors to consider these four points: 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 $\sim$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.

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.

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.

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.

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