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Climate of the Past Discussions

Interactive comment on "Persistent millennial-scale link between Greenland climate and northern Pacific Oxygen Minimum Zone under interglacial conditions" *by* O. Cartapanis et al.

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The manuscript describes a new set of data from Baja California recording changes on millennial timescales in ocean productivity and dissolved oxygen in subsurface waters and their relation to Greenland climate. As a climate modeler I cannot assess the methodological details of the proxies used. However, I have a few comments related to the mechanisms proposed that I think should be considered in a revision.

The authors argue that Greenland climate and the ocean processes off Baja Californina are linked by atmospheric processes. However, none of the arguments presented in subsection 5.2 are quantitative. To my knowledge no model simulation exists that

would support quantitatively these inferences. However, quantitative model based evidence does exist to support an oceanic mechanism. I have shown in Schmittner (2005, Nature) that changes in the Atlantic Meridional Overturning Circulation (AMOC), which have been previously linked to millennial climate change in Greenland, impact global ocean productivity particularly at low latitudes in the Pacific and Indian oceans. As AMOC is reduced, upwelling of nutrients is also reduced and productivity in the Pacific (and Indian ocean) decreases. Consequences of these productivity variations on subsurface dissolved oxygen have also been quantified in our Schmittner et al. 2007 paper in Paleoceanography (cited by the authors). However, the authors cite this paper only in relation to NPIW production, whereas, I think, its quantitative oxygen simulations is much more relevant to the currentl study. It shows that subsurface dissolved oxygen strongly increases during phases of AMOC reduction. I think it is fair to say that in contrast to the atmospheric mechanism favored bu the authors this mechanism has been qualitatively tested in a model. It also fits the observations of productivity and oxygenation. So I think it deserves at least discussion. I suggest to discuss this mechanism already in the introduction.

Minor issues:

Page 3922 line 13: remove "s" in "represents"

Fig. 2: are the green symbols the opal measurements? If so, please state that in the caption.

Page 3927 line 22: "lower opal content" I don't see this in the figure. I see two opal peaks that seem to coincide with DO 19 and 20. Please clarify.

Page 3929 line 1: d18O data are difficult to interpret. A model is needed (d18O enabled models exist, e.g. NASA GISS's model by G. Schmidt and A. Legrande).

Page 3929 line 2: Fig. 3 does not show d18O of Lake Owens.

I didn't find a discussion of Fig. 3 at all. So, I would suggest to remove it. Page

3930 lines 10-13: This is speculation. Figs. 6 and 7 in Schmittner et al. (2007) show quantitatively how changes in NPIW ventilation affect subsurface oxygen. (This also applies to the statement on page 3934 lines 23-25.)

Interactive comment on Clim. Past Discuss., 9, 3919, 2013.