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Climate of the Past
Discussions

Interactive comment on "Millennial-scale variations of sedimentary oxygenation in the western subtropical North Pacific and its links to the North Atlantic climate" *by* Jianjun Zou et al.

Anonymous Referee #1

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Review Zou et al 'Millennial-scale variations of sedimentary oxygenation in the western subtropical North Pacific and its link to North Atlantic climate.

Zou et al. study sedimentary redox conditions in the Okinawa Trough, and use this as a proxy to infer bottom water oxygen concentrations (I think, it is not always very clear from the manuscript). The title promises more than the paper delivers; the link with North Atlantic climate is only mentioned briefly and explanation is sometimes unclear.

The authors present interesting data, but the paper itself needs work. There is so much information (several times incorrectly referenced), and some information seems irrelevant. There is also a lot of internal discussion within the paper without reaching

firm conclusions. While the authors are critical about their own proxy, they are less so about others and this needs to be improved.

Comments: Authors should check that their references are appropriate. Several references are not put in the right context.

There are several other studies that deal with the North Pacific and NPIW, which are not referenced here; this includes work by Rippert et al. (2017), Max et al. (2017)

Abstract: Lines 44-50: these sentences go around the bushes. Really what you want to say is that sedimentary oxygenation conditions at mid-depth in the subtropical western North Pacific were more or less similar over the last 50,000 years, apart from the Bolling-Allerod and Pre-boreal. However, it may not be possible to compare with Holocene data, as this may be compromised by ash. This is not made very clear in the manuscript.

Lines 59-61: how does it seem to be driven? Is this not something you are proposing? Then it is not seem.

The authors mix up NADW and the Atlantic Meridional Overturning Circulation. For a good description of AMOC see recent paper by Frajke-Williams et al. (2019).

Introduction: Lines 70-73: not sure how to interpret this. Where is the respired carbon stored? At the sediment-seawater interface, in sedimentary pore-waters, or in seawater? The study of Lu et al. (2016) deals with I/Ca in planktonic foraminifer in the Pacific Sector of the Southern Ocean, to reconstruct upper ocean oxygenation, the part of respired carbon in their paper refers to a different study (Hoogakker et al., 2015).

Lines 76-83: the study of Cartapanis is from the northeastern Pacific, but not high latitude or subarctic.

Line 92: explain what cabbeling is, not everyone will have heard of the term. Lines 95-97: do the data really show this? The one core at $\sim$ 1km is about 0.04 per mil lighter (and within error), but crucially there is no Holocene equivalent for the 0.7 km core.

Line2 149-152: do you mean 'is governed by' instead of 'is the balance between'.

Figure 1. O2 map, and locations of cores. Are all the cores discussed in the paper? Is it worrying that the main core CSH1 is from just south of Japan and perhaps should not be considered an open ocean core? What do the letters A to E stand for?

Setting: Do details of discharge and SSS add anything to this study?

Material and methods: What causes the high accumulation rates in this core? As the accumulation rates vary significantly, how do these influence the patterns in redox elements etc?

Lines 339-340: preservation of TOC and CaCO3 are influenced by many factors and not a widely used paleo-export proxy.

Line 354-357: have you checked that it is an extant biological component that makes up the high CaCO3 going from B/A to $\sim$ 8000 years? Can you explain the differences in the LSR figures between Figures 3 and 4? For examples, in Figure 3 highest rates occur centred around 22 kyrs as part of a large interval of high LSR (from 30 to 20), whereas in Figure 4 this occurs earlier (33 to 24 kyrs).

Lines 365-266: if U concentrations are affected by volcanic material over the last 8.5 yrs, then surely so are other sedimentary properties? I would like to see an argument in the main text discussing why certain proxy methods are deemed not to be influenced by this volcanic material, whilst others are. If it turns out that interval should not be used than this creates the complication of not being able to compare the down core data with more modern.

Line 370: change 'seems' to 'may'.

Lines 372-377: are there any other studies that use Mo/Mn ratios as a sedimentary oxygenation proxy, to support your interpretation?

Line 380: define oxygen deficient.

Discussion: Lines 387-392: I would recommend the authors to use more appropriate scheme that is used for sea-water that includes hypoxic, for example as defined by Bianchi et al. (2012). You will also found that suboxic is classified as < 2-10 μmol/l.

Lines 403-405: I do not understand this reasoning. You have not linked weakly restricted basin settings with euxinia? It is confusing talking about ppm in the main text, whilst Figure 4 gives concentrations in μg/g.

Lines 406-412: Mo/U ratios are not shown in the manuscript. This is out of the blue.

Lines 413-425: two studies. More importantly though, Figure 4 shows no benthic foraminifera data, and it is therefore impossible to confirm this claim of ventilation pattern from benthic foraminiferal assemblages to be similar to that of the RSEs. It would also be good to see a more critical discussion about this proxy.

Lines 425-428: No. There is at least 800 meter water depth difference between your core and the others. The core of the current study is situated just above the low oxygen zone, whereas those of the other two studies are in /below the low oxygen zone.

Lines 439-448: why are you looking at one NE Pacific to find out what is happening at your core site? There are several studies from across the Pacific that show something happening around the same time period (for example see Moffit et al., 2015), and Galbriath and Jaccard (2015), so rather than repeating the same discussion for a very small area, it would be easier to build up on those results.

Lines 454-457: no, at those high temperatures you would only get a reduction in O2 of $\sim$ 3 for one degree warming, and 15 for a four degree warming (assuming no large salinity changes). Higher glacial salinity would cause less reduction in O2.

Lines 457-458: sentence does not make sense.

Lines 848-846: does not make sense. How does subsurface water oxygen consumption lead to lower oxygen concentrations in deeper waters?

Lines 491-494: again not taking into account other factors that influence CaCO3 accumulation and preservation in sediments. For discussion on Kuroshio Current: see Lim et al. (2017).

Line 544-550: coined? Matsumoto et al. (2002) discuss one radiocarbon age from the Santa Barbara basin in relation to oxygen content, but at no point do they propose that GNPIW was stronger oxygenated. Cartapanis et al. (2011) and Ohkushi et al. (2013) discuss that the NE OMZ Pacific strengthened and weakened at millennial time scales, not glacial interglacial timescales. Also around the equatorial Pacific it is suggested that there was no difference in intermediate water oxygenation between the last glacial and Holocene (Hoogakker et al., 2018). Further down in the South Pacific Lu et al. (2016) suggest that upper waters were depleted in oxygen during the last glacial.

Lines 550-556: generalised comment, what about brine (aka Kim et al. 2011).

Lines 556-559: what is intensified GNPIW?

Discussion lines 560-571: needs tightening, it is unclear where this goes and how it relates to this study?

Line 629: what is ocean ventilation seesaw? There is hardly any explanation for this in the main text, and Figure 7 shows strength of AMOC in the Atlantic and compares this with the current study.

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