

## ***Interactive comment on “Millennial changes in North Atlantic oxygen concentrations” by B. A. A. Hoogakker et al.***

**Anonymous Referee #2**

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Babette Hoogakker and colleagues present new Dd13C measurements – a proxy allowing quantitative changes in dissolved O<sub>2</sub> concentration to be reconstructed - from intermediate-depth sediment core ODP 1055. The time interval of interest encompasses the last glacial inception as well as several transient cooling events associated with ice rafting and meltwater release to the North Atlantic. The authors compare their newly generated reconstructions with previously published observations from the Iberian Margin.

They show – somewhat unexpectedly – that changes in oxygenation in intermediate- and deep water masses followed a similar pattern during the interval 60-80 kyrs. This is seemingly at odds with the notion that NADW formation was reduced during stadials - allowing nutrient-rich, oxygen depleted waters of southern origin to propagate north-

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ward - while intermediate water masses were better ventilated. The authors reconcile this apparent discrepancy by invoking (local?) changes in respiration rates, possibly overprinting the ventilation signal.

Moreover, Hoogakker and colleagues show that their data are in good agreement with climate model outputs, which predict a general decrease in oxygenation in the North Atlantic during cold intervals.

I find the paper interesting in that it highlights the potential of the proxy used to quantitatively reconstruct past changes in oxygenation. Hoogakker has done a great job in refining the proxy, which will undoubtedly prove to be very helpful to deepen our understanding of past changes in the carbon cycle. This being said, I find the paper largely reworks arguments that were developed in a previous manuscript (Hoogakker et al., 2015, Nature Geoscience), thereby somewhat limiting its impact. Nonetheless, given the potential application of the proxy and its usefulness for the scientific debate, I would support publication of the present study, provided the rather minor comments outlined below can be addressed.

General comments -

I would urge the authors to better describe the parameters influencing local oxygenation in their introduction. In particular, the authors need to better explain the often interwoven influence of physical vs. biological mechanisms on oxygen levels. (A useful approach, I find, is provided by Schmittner et al., 2007, Paleocyanography or Jaccard et al., 2014, Oceanography).

I also find the discussion related to the ODP 1055 record (p. 12959, l. 5-27) clumsy and confusing (see below).

Detailed comments -

p. 12947 l. 4 - affiliation – University of Oxford p. 12948 l. 11 – ... at intermediate-depth core. . . l. 20 – export production instead of productivity l. 24 – ... can be linked

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to increased export of organic material from the surface ocean and its subsequent remineralisation in the water column and the sediment. p. 12949 l. 1 – 231Pa/230Th l. 16 – overturning p. 12952 l. 8 - ... oxygen and carbon are stoichiometrically linked. . . p. 12953 l. 1 – please either remove etc or be more specific. l. 2- please delete quantitative p. 12954 l. 4-7. This sentence seems out of place here. p. 12955 l. 16 – remove etc l. 18-23. Can you provide some hypothesis, as to why there is so much discrepancy in the downcore  $\delta^{13}\text{C}$  signal derived from intermediate-depth sediments? p. 12957 l. 9 – is the  $\text{O}_2$  concentration difference inferred for the LGM (i.e.  $200 \pm 17 \text{ umol*kg}^{-1}$ ) and MIS6 (i.e.  $180 \pm 17 \text{ umol*kg}^{-1}$ ) statistically significant? l. 12 – please add adequate references. l. 15. Böhm et al., 15, Nature seems a more adequate reference here. l. 13. McManus et al., 2004 l. 15. Heinrich Stadial 1 p. 12957 l. 15 – this observation is also valid for shallower records (Praetorius et al., 2008, Nature Geoscience) p. 12959 l. 1 – please add reference l. 25 – please add reference l. 26-28. This argumentation is confusing and needs to be rephrased. While I agree that there could have been more organic carbon exported during Heinrich stadials in the Atlantic, I do not agree that this implies a strengthening of the biological pump. Evidence suggests that the global efficiency of the biological pump was reduced during HS1, allowing carbon to be released from the ocean interior (e.g. Galbraith Jaccard, 2015, QSR). I guess the argument here, was that enhanced local export production lead to enhanced oxygen removal during Heinrich stadials at intermediate depths. One should keep in mind that under colder temperatures, the remineralization length scale increases (e.g. Matsumoto, 2007, GRL), which would have shifted the OM remineralization maximum to the core of NADW (Kwon et al., 2009, Nature Geoscience), consistent with enhanced  $\text{O}_2$  depletion in the deep Atlantic, but somewhat at odds with the observations presented for core ODP 1055. p. 12960 l. 1 – Again, I don't think there is any evidence supporting a strengthening of the biological pump during North Atlantic stadials. A reduction of the relative contribution of northern sourced waters in ventilating the global deep ocean during North Atlantic stadials would contribute to weaken the biological pump globally (e.g. Sigman et al., 2010, Nature). Increased export does not neces-

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sarily imply a strengthening of the biological pump. l. 9-12 l. 24-25. I agree that the data compares well with the UVic model outputs – and this is great. However, the UVic model also predicts decreased export production in the North Atlantic during periods of weak NADW (see Schmittner, 2005, Nature, Fig. 1 g/h) resulting from a shoaling of the winter mixed layers, which is at odds with the argument put forth by the authors (p. 12959). One cannot pick the argument that fits the main hypothesis and not mention the model outputs that don't fit the observations.

Fig. 5 – error bars are not visible for all the measurements. Does this imply that the error bars are smaller than the symbol size?

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