

## ***Interactive comment on “Relative timing of precipitation and ocean circulation changes in the western equatorial Atlantic over the last 45 ky” by Claire Waelbroeck et al.***

### **Anonymous Referee #1**

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General Comments: Waelbroeck et al. present results for 2 cores from the North Brazilian margin, using proxies for AMOC-related ocean circulation changes (Pa/Th, C. wuellerstorfi d13C) and South American precipitation events (Ti/Ca). As the proxy records were generated from the same location/core (more or less) the authors argue that there are no lead/lags related to age model uncertainty and hence this allows to properly assess the phase relationship between AMOC and South American rainfall during the last 45 kyr. Their new data allows to not only focus on the last 4 Heinrich stadials (already presented in Burckel et al., 2015), but also D-O events with shorter frequencies. Based on the careful analysis of their data (using mainly cross-wavelet analyses), they infer that changes in water mass transport in the mid-depth range of the

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western equatorial Atlantic precede precipitation changes in Brazil. This is especially the case at Heinrich-like frequencies, and less so at D-O frequencies, which they relate to a positive feedback mechanism in the ocean/atmosphere system during Heinrich stadials.

The manuscript is well written, well structured, and concerns an important topic that is certainly relevant for Climate of the Past. In essence, this paper is an evolution of the Burckel et al. (2015) paper, but with some extra Pa/Th and d13C data, which makes it possible to better study changes in water mass transport over Dansgaard-Oeschger frequencies. In general, the authors carefully address the possible biases on Pa/Th and other proxy records (influences by marine productivity, differential bioturbation, currents etc.) and deliver quite a good case for the ocean circulation changes and leads/lags to South American precipitation during D-O/Heinrich stadials. I do have some reservations about the age model, as I think there are some details missing in text to properly evaluate the chronology (and uncertainty). Moreover, more details on some of the geochemical analyses are required (citing an “in prep.” paper is in my opinion not enough). If these two main issues are properly addressed, I certainly recommend publication in Climate of the Past.

Specific Comments: p.2 line 18: XRF, do you mean XRF core scanning (as in Jaeschke et al., 2007, done with the CORTEX scanner) or with more conventional XRF done on glass beads/pressed tablets? If it is the former, please change the abbreviation throughout the text, e.g., XRF-core-scanning (XCS).

p. 3, line 9: I miss a paragraph on the geochemical measurements performed to derive the Ti/Ca ratio. The Ti/Ca values were already published in Burckel et al. (2015), but I cannot find the XRF methods in there (I could have overlooked it). The best would be to give details on the used methods here, at least briefly. Note also that if you used the same method as Jaeschke et al. (2007), you probably used a different core scanner (Avaatech? Itrax?).

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p.3, line 22: Log-ratios of Ti/Ca are indeed the way to go, also, because they allow a better statistical modelling of compositional data (see Weltje and Tjallingii, 2008; normal ratios are asymmetric). It would be good to shortly address this too in this sentence.

p.3, lines 9-27 (Chronology): I find the chronological section not yet satisfying. For instance, I miss what software was used to calculate the age model (OxCal?), and more technical details (reservoir age? uncertainties?). I see that in Burckel et al. (2015) the age model is addressed in one of the 16 Supplements of that paper, but I think it is important to at least briefly address the most important parts again. As written now, you might as well have used a simple linear model between the age points, but I cannot find that in the text. As the age model is clearly crucial for the results of this paper, the details should be better outlined (and not simply covered by a reference to an "in prep." paper). For instance, did the authors use the state-of-the-art OxCal Bayesian modeling, and if not, why not? The authors should read the Sections 3 and 4 in the Supplement of Grant et al. (2012), who do a good job of obtaining the chronological uncertainties with a Bayesian deposition model in OxCal (also to calculate lags/leads between proxy records, albeit with a different scope).

p.4, lines 1-16: The details on the  $\delta^{13}\text{C}$  methods should be given here, and not in the Vazquez Riveiros (in prep.) paper.

p.4-5 (New sedimentary Pa/Th data): How was the discrete sampling performed for Pa/Th? This might be important for direct comparison of Pa/Th to Ti/Ca (from XRF-scanning?) during the D-O variability. Are the analyses by both methods exactly performed on the same sediment intervals? Core scanner intervals are often deviating from those that are discretely sampled (e.g., at 1-cm resolution, a measurement at 5 cm is covering the interval from 4.5 to 5.5cm, with an Avaatech core scanner). This might somewhat influence the lead/lag calculations, and may require resampling of the Ti/Ca data (although the impact is probably small).

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p. 5, lines 19-20: Add shortly why the  $^{232}\text{Th}$  is indicative of the vertical terrigenous flux (detrital origin?).

p.10, lines 8-12: The uncertainty of the leads and lags in the cross-wavelets should already be given in the Methods (section "cross-correlation and wavelet analysis"). The error propagation is not entirely clear to me, did the authors use a mean squared error (MSE)?

p. 11, line 23: Is the cross-correlation really imprecise and unreliable, or does it just lump all frequency signals into one and give you an average output, which is basically correct for the time window that was analyzed? The authors could have used different time windows (e.g., 3000 years and 6000 years) and calculate a running correlation across the whole interval (with one of the records shifted towards the other in different time steps). The result from such a running correlation test will/would be probably very similar to the cross-wavelet analyses. Cross-correlation is not imprecise or unreliable, just not the most suitable method to study non-stationary climate signals. I suggest to change this sentence, and also that at p. 11 line 29, more focusing on the fact that these cross-correlations cannot be used to disentangle the leads/lags of variable frequencies in the proxy records.

p. 12, line 26: What is the reason to not just use a  $\ln(\text{K}/\text{Ca})$  ratio, instead of  $\ln(\text{Ti}/\text{Ca})$ , to circumvent these problems? (Other than the reason that previous studies used Ti/Ca, but probably did not consider these bioturbation effects).

p.13, line 10: To me it seems that for HS3 there is also not a clear visible lead of Pa/Th relative to  $\ln(\text{Ti}/\text{Ca})$ . Is this not what you expect considering that the origin of icebergs/IRD seems to be more European orientated for HS3 (Gwiazda et al., 1996; Henry et al., 2016), while the others find their origin mainly from the Laurentide ice sheet? The reduction in overturning seems to be also much less during HS3 compared to the others.

p.13, line 20: Doesn't the  $^{232}\text{Th}$  flux show that the vertical terrigenous flux was largest

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during HS4?

p.14, line 27: Is a 2-4cm downward shift also plausible for differential bioturbation? I suppose there is always bioturbation of both fine and coarse particles.

Technical Comments: p.2 line 8-10 (“In the best. . .into calendar ages”): Sentence does not read well. Rephrase/break up sentence.

p.2 line 26: Add when the core was recovered.

p. 3, line 27: Please write “as defined by Rasmussen et al. (2014)”. This should also be done for the other parts of the text where citations are part of the sentence.

p. 8, line 21: Table S2 considers the opal measurements, which Table needs to be referred to?

Figure 1: I think a larger overview map of South America would have been nice here (e.g., Burckel et al., 2015)

Figure S1: Multiplier for  $\ln(\text{Ti}/\text{Ca})$ , is this really necessary? Is it not sufficient to change the range on the y-axis?

Figure S1: The unit for the sedimentation rate is missing partially on the y-axis.

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