

Interactive comment on “Evolution of ^{231}Pa and ^{230}Th in overflow waters of the North Atlantic” by Feifei Deng et al.

R. Francois (Referee)

rfrancois@eos.ubc.ca

Received and published: 13 May 2018

This paper is an important contribution to the growing database describing the distribution of Th-230 and Pa-231 in the world ocean. I recommend publication after considering the comments and questions listed below:

Abstract; line 19-20: The reason for a weaker correlation between Pa-231 and water mass age is likely due to the much lower concentrations and rate of ingrowth of Pa compared to Th. It may be worth adding error bars to Fig. 8(a),(c),(e) to highlight this point.

P 3; line 13: why was a ^{236}U spike added to the sample?

P 4; Line 20 – 23; Fig. 3: If the purpose of this figure is to show that the concentrations
C1

measured on the GEOVIDE line are much lower than measured further “downstream”, I don’t think that the data from 40°S are useful because they raise questions that are likely beyond the scope of this paper. The wording used in line 20 – 23 is incorrect since there is an apparent southward decrease instead of an increase between the North Atlantic and 40°S . Hayes et al.’s data show a strong longitudinal gradient for both Th-230 and Pa-231. Were the averages calculated over the entire longitudinal GA03 section? Were the averages calculated for 40°S comparable?

Fig. 5: ISOW is not indicated on this figure, even though it is one of the most prominent water mass. I would suggest replacing the salinity contour lines by the contour of the main water masses reported on Fig. 4, since the purpose of this figure is to relate these water masses to the distribution of the radionuclides

Line 31; p5 and Fig. 6: It is not entirely clear to me how the CFC ages were determined for the GEOVIDE section. Water masses and CFC ages were determined in 2012 of the eastern side of the section. From these data, the CFC ages of the different water masses could be determined for 2012 and these ages were used to determine the CFC ages of all the samples collected in 2014 from their water mass constituents.

My first problem is how you could estimate the CFC age of the water masses west of Greenland. Surely, the age of ISOW and DSOW in this section of the transect must be older than in the eastern section since these water masses are farther removed from their site of formation.

My second problem is how can you distinguish ages between 100 and 800 years with CFCs considering that CFC manufacture only started 80 years ago?

Finally, and maybe more fundamentally, I question whether you can apply CFC-age to Th-230 and Pa-231. I am not an expert on this question but here are my concerns:

Each (most?) of your samples consists of a mixture of different water masses. Even the red zones on Fig. 4, indicate up to 20% mixing. If you mix 2 water masses of different

ages, the CFC age of the mixed sample will be biased toward the younger age because there is more CFCs in the younger end member (although CFC concentrations have quasi plateaued since 1990). On the other hand, for the same mixed sample, the Th-230 age will be biased towards the older end-member because it contains more Th-230. So, in a mixed sample, Th-230 had more time for in-growth than indicated by the CFC age. Because Pa-231 grows in slower than Th-230, this effect is less pronounced. Therefore, I believe that the ingrown Th and Pa calculated with CFC ages using equations 4.1 and 4.2 must be taken as minima and more so for Th than for Pa.

Fig. 6: Adding water mass contour lines would be helpful

Line 16; p6 “Preformed component” “..in the absence of measurement at the exact location of deep water formation. . .”.

While this statement is correct for the formation of DSOW and ISOW, I would say that station 69 is essentially at the location of LSW formation

“..we set the preformed value as the average of concentrations measured in waters < 100m depth for this section..”

I think the authors need to elaborate on their rationale for doing this. It is not like deep water is formed uniquely from water sinking from a depth < 100m. Deep water convection homogenizes the water column in the Labrador and Nordic seas, which then spreads laterally at depth. Therefore, the homogenized water column at the sites of deep water formation could be the starting point providing preformed concentrations. If so, data from station 69, as well as earlier data from Moran et al indicate that preformed Th-230 should be about 0.3 dpm/1000l both for the Labrador and Nordic seas, instead of 0.1 dpm/1000l. Such value for the Nordic Sea (based on one profile from Moran et al., 1995) is of course problematic since it is higher than measured here in the IC basin. In addition, Moran's data from the Labrador Sea shows substantial inter-annual variability. I think the authors should be more nuanced in their choice of preformed values and consider how the uncertainties on this number could affect their conclusions.

C3

Alternatively the reasoning followed by the authors may be that cooling happens at the surface, and therefore that must be the starting point, and the higher Th concentrations found in the water below at the sites of deep water formation reflects the residence time of this surface water in the convection cell. If this is the case, that should be more explicit in the paper. I would also use the available data for surface water in the Labrador and Nordic Seas only, which are available from this study and Moran's.

Line 30; p6: as per discussion above, calculated potential total concentration should be viewed as minima (because CFC ages underestimate the Th and Pa “ages”)

Line 4; p7: “..is the net of nuclide added from above by [add: DESORPTION FROM] settling particles. . .”

Line 8-9; p7: Considering that the CFC ages are younger than the Th ages, potential and scavenged Th-230 must be viewed as minima. Also, the underestimation of fraction scavenged is larger for Th than Pa.

Line 14; p7: I guess the authors deduce that about 75% of the Th-230 produced is scavenged because the slope of slope of observed Th-230 vs time is 25% of the rate of production, right? If so, it would be helpful to be explicit and to report the value of the slope in Fig. 8a. Wouldn't the intercept also provide an independent estimate of preformed? I am not sure what the intercept on scavenged Th-230 figure means. I would also reiterate here that 75% scavenging is a minimum, and it could be that scavenging is more underestimated in older water than younger ones, producing this apparent intercept. It is not clear to me why the same can't be attempted for Pa, at least for the observed values.

Line 15-14; p7: “This ratio is consistent with the average 230Th for these waters, which requires that about three times more 230Th has been removed by scavenging”

Line 18; p7: “...the average [Pa] values indicate that about half of the 231Pa remains in the water”

C4

I can't follow the line of reasoning here. I think the authors need to be more explicit.

P2; line 23: “.. there is no simple relationship between increasing Pa/Th and age, as would be expected”

P7; Line 21; p7: “..The hypothesis that Pa/Th ratios increase as water mass ages..”

We should not expect seawater Pa/Th to simply increase with water mass age. If dissolved Th and Pa activities are initially low, Pa/Th in the water column (and underlying sediments) should initially decrease with age of water mass because Th grows in faster than Pa. It's only once Th has regained levels closer to equilibrium with scavenging (and therefore grows more slowly) that Pa starts growing faster, resulting in increasing Pa/Th. This effect is clearly illustrated in the paper by Luo et al. (2010) (see Fig. 14 in the paper). Fig. 8e,f may show a hint of this initial decreasing trend.

(Luo et al also argued “Clearly, it is impossible to constrain the history of changes in the AMOC from the evolution of $^{231}\text{Pa}/^{230}\text{Th}$ at one site, as was attempted by McManus et al.; p 395; last sentence of first paragraph)

Section 4.3: I am puzzled by Fig 9. The intercepts don't seem to match with the preformed values. The fit with the data is terrible. I am not sure what this section is telling us.

Line 28; p7: wouldn't it be better to follow density rather than depth?

Line 4; p8: why did you choose station 1 and 13? Is it because they are the least ventilated and therefore these parameters could be estimated from a linear fit? If this is the case, that should be indicated. However, is it reasonable to apply scavenging parameters from a margin to open ocean stations?

Fig. 9: How can Pa/Th (zero preformed) increase with age (c) if Th (a) grows faster than Pa (b)?

Section 4.4:

C5

This discussion should take into account that scavenged Th and Pa are underestimated and proportionally more for Th than Pa

Since potential and scavenged are underestimated by exactly the same absolute amount, calculated scavenged/potential ratios should also be taken as minima.

P 9; line 20-22; I think it would be helpful for the reader to provide the details of this mass balance calculation. For instance, what volume for the North Atlantic did you use to calculate production? It seems that the results from this mass balance corroborate that scavenged (and therefore potential) Th and Pa calculated from equation 4.3 are underestimated because of the use of CFC ages.

I am a little surprised that so much Pa is scavenged in the North Atlantic. If 77% of the production is scavenged, then the average Pa/Th of N. Atlantic sediments should be ca.0.073, which is higher than measured in the central N. Atlantic basin. That means that boundary scavenging must be more significant than I thought.

Roger Francois

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-191>, 2018.

C6