

Response to Anonymous Referee #2

(Referee's comments in italics.)

Our response 2.0:

We would like to thank the referee for the very useful suggestions and comments.

In this manuscript Heinze et al. explore the feasibility of using measurements of dissolved ^{230}Th in the deep ocean to detect changes in calcification as a result of ocean acidification. The idea being it would be nice to be able to "check" the response of carbonate export/calcification to ocean acidification with observations of changes in both parameters. They argue that this approach is complementary to using measurements of alkalinity in the surface ocean to detect changes in calcification. This is an intriguing idea, especially the idea that surface changes are 'magnified' in the deep water through ^{230}Th . And I fully agree with the authors' concluding sentence that the full potential of ^{230}Th has yet to be realised. However, I have some reservations about this manuscript.

First, the idea is not actually new, having been proposed over 10 years ago by the first author (Heinze et al, 2006).

Our response 2.1:

The idea was brought up by Heinze et al. (2006). This study follows up on it and pursues it in greater detail focusing now on the possibility of detecting changes in carbonate production through the monitoring of deep-water dissolved ^{230}Th levels. See also our detailed response 1.1 in reply to Referee #1.

More importantly, and particularly as the current work can be considered a 'follow up' on the initial work, this work is missing a key sensitivity analysis. Stepping back, the authors have shown that it's quite possible that changes in carbonate production and export will result in measurable changes in ^{230}Th concentrations in the deep sea in coming decades or at least centuries. However, they have not adequately demonstrated that it will be possible to distinguish between the different 'parameterisations', or 'sensitivities' of calcification to OA, using the ^{230}Th measurements, which is the premise of this work. In their experiments, where only CO_2 and calcification change, yes there are clear differences in the time of emergence for the different parameterisations. However, in the real world there will almost certainly be changes in POC, CaCO_3 and opal that are independent of OA, driven by changes in stratification, temperature, dust deposition, even anthropogenic nutrient input. Do the different Th trajectories and times of emergence stay distinctly different when you add in these other changes? Furthermore, ocean circulation will likely change and that will change ^{230}Th distributions independent of changes in particle flux and composition. Again, can you distinguish between different OA sensitivities once you factor in a range of ocean circulations? It all comes down to the sensitivity of ^{230}Th distributions to OA compared to their sensitivity to other perturbations.

Our response 2.2:

We intend to remedy this point by adding additional sensitivity experiments as also suggested by Referee #1. We think that our study has a value even without such additional sensitivity experiments, but agree that these would strengthen the manuscript. Please, see our responses 1.2 and 1.3 in reply to Referee #1.

The authors mention that changes in deep ocean ^{230}Th concentration emerge at about the same time as changes in alkalinity, and point out that the two tracers offer complementary information. I think this is an excellent point. My feeling is a study that combines these two tracers, together with a

sensitivity analysis as suggested here, could potentially be quite useful in showing how the two tracers together (or perhaps just one or the other) can be used to distinguish, on the basis of time of emergence, between different sensitivities of carbonate export to OA.

Our response 2.3:

This is an interesting point – thank you. We will address this issue by evaluating the model results through consideration of simultaneous changes in alkalinity and ^{230}Th at various regions in connection with the regional assessment as suggested by Referee #1 (see also our reply 1.10 to Referee #1).

So my recommendation is to conduct some more model simulations to assess sensitivity to these other changes, and if possible to also consider a study that combines both ^{230}Th and alkalinity.

Our response 2.4:

See our responses 2.2 and 2.3 for this.

Other comments:

- The model does not include hydrothermal particles, which are important scavengers for Th (e.g. recent papers by Hayes)

Our response 2.5:

We will discuss this issue with additional graphical material. Please, see our response 1.4 to Referee #1.

- Although the authors cite Heinze et al. 2006 as evidence for a lack of sensitivity to the choice of particle phases scavenging Th, it would be good to actually show that result in the context of the present study, as part of the sensitivity analysis.

Our response 2.6:

We will repeat a few model runs with a change in particle specific scavenging and discuss whether this issue is of relevance for the results presented here. (See also our response 1.3 to Referee #1).

- I don't think figure 6 is necessary - Section plots would be more useful for the model-data comparison than the station by station comparisons.

Our response 2.7:

We would like to keep Figure 6, as not every biogeochemical trace element researcher is fully familiar with the greenhouse gas scenarios. Plots of observations and model results will be added for an Atlantic meridional cross section (see also our response to Referee #1).

REFERENCE:

Heinze, C., Gehlen, M., and Land, C.: On the potential of Th-230, Pa-231, and Be-10 for marine rain ratio determinations: A modeling study, *Global Biogeochemical Cycles*, 20, Artn Gb2018, 10.1029/2005gb002595, 2006.