Reply to revision comments on "Constraints on ocean circulation at the Paleocene-Eocene Thermal Maximum from neodymium isotopes"
A.N. Abbott, B.A. Haley, A.K. Tripati, and M. Frank

General Remarks
We appreciate the efforts that have been made to find reviewers for this manuscript. The initial submission had one review suggesting minor revision (Dickens), and one review suggesting major revision. We feel that we made a reasonable effort to address both the minor and major revisions. The second reviewer sent back further comments on our revised manuscript, which we attend to here.

We respond to specific reviewer remarks below. We have also made several minor changes in syntax and wording at places in the manuscript: none of these changes are contextual, and are only made to help readability.

Anonymous Referee #1

The text and the figures were improved for the clarity although more efforts will be required to demonstrate that the proposed hypothesis is supported by the data.

We thank the reviewer again for her/his continued effort to improve our manuscript. We feel that at this point our interpretations follow the data as close as possible. As reviewer 1 (Dickens) had pointed out, we are largely offering supporting data for interpretations that are consistent with ideas previously suggested, as is reflected in the title of our manuscript: "Constraints on circulation based on eNd observations".

Now the authors acknowledge the eNd offsets between the sediment leachates and the fish teeth/debris, possibly because of different uptake or preservation of eNd records. Consequently, they decided to use the trend of eNd changes of leachates and of fish eNd, instead of absolute eNd values. With this treatment, temporal coverage of reconstructed bottom water eNd values is further uneven with different archives, and it becomes ambiguous which size of variability can be considered as sign of circulation changes.

Our interpretations are based on our data in combination with the evidence from the variability in fish teeth data, as it has been from the original submission. We recognize that there is some scatter in either/both records, but we do not interpret at this level of variance. Our interpretations are rather conservative in that we do not discuss any changes below about 1 eNd unit. Indeed, considering only our data, we have likely under-interpreted the variability observed.
With regards to the fish teeth data: while slightly offset from our data, the teeth data generally show the same trends - even in magnitude of change (where a direct comparison can be made). The intention of our paper was not to investigate the possible causes of the differences of the absolute values, for which this data set is not suitable: such was the topic of studies such as those cited in the original review (Elmore, Wilson, Martin etc.). These works used dedicated core top sediments to investigate the potential

mechanisms of Nd uptake into fish teeth versus metal-oxides with a direct comparison to ambient seawater signatures, which we can of course not offer. Here we simply apply what is known about the proxy to pose a hypothesis based on the assumptions that we provide.

I asked several points about the relationship between the proposed circulation changes and data described in section 3 in my previous review (ex. specific comments 5 and 6). The authors answered them but I am sorry to say that the responses are not enough precise.

(We could not find a specific comment 5 or 6 in the previous review. If the reviewer could clarify, we would be happy to reply.) We respectfully disagree in that we do not think that our manuscript is appropriate for a synthesis of data and model results. We have shown data that is appropriate those cores for which we made eNd measurements. We hope our data will be useful for such a comprehensive analysis of the PETM, and we fully agree that more data would certainly be useful.

I think that careful overhaul revision, in particular for section 3, Figures 2 and 3, is required.
Taking Figure 3 as an example, I explain my concern because this is the key figure of this work that summaries the main finding.
I appreciate that the authors added eNd values in Figure 3. However there is no explanation how the reconstructed bottom water eNd values are calculated.
Since temporal coverage of different records is uneven, the considered period and data points to estimate the indicated eNd values should be explained. When eNd data are not available ("n/a" in Figure 3), I do not know how the authors determined the circulation patterns shown with the black arrows.

Please note that these values provided are all observational data - we did not calculate any values. We added the values based on comments made previously by this reviewer. Originally, we deliberately avoided making such data labels, because our data set is not dense enough to make observations along the flow paths of the water as detailed as we all would like to see. This limitation is because the samples required for this approach simply do not exist, so we are working with what is available. In general, as the title of the manuscript states, we are simply offering the most likely possible interpretation in terms of changes in circulation documented by the eNd variability. We have nevertheless now denoted in red the arrows of the circulation pathways in those cases where no data (n/a) are available for particular sites in Fig. 3

The circulation pattern is very similar between "pre-PETM" and "PETM", which does not correspond to the hypothesis of ocean circulation changes as a trigger of negative carbon isotope excursion. One noticeable eNd change is a lower value (eNd of -6.0) during the "trigger" period (age range?) of site 1220 in the Pacific. But this low value can be rather interpreted to more contribution of southern origin water in the Pacific, which is contradictory with a shift of deepwater formation zone from the southern ocean to the

North Pacific related to the PETM. Please clarify which data support the hypothesis throughout the whole text.

We appreciate this comment, and have added text to be more clear (added lines 346 - 358). Indeed, we argue that the pre-PETM and PETM do have generally similar circulation patterns. Fundamentally, our idea is that the circulation in the Pacific was switching between two states just prior to the PETM - as defined by a single, later d13C excursion. As mentioned above, these are all the samples that are available, and so we do not unequivocally know if a strong or weak southern Pacific deep water source was prevailing prior to what we define here as the "trigger." We agree that it would be more satisfying to have these data, but regardless, our general observation is that Pacific circulation changed (dramatically) during the period of time just prior to the PETM, which is clearly still valid.

Figure 2: It is necessary to show the trend of eNd variability based on fish teeth separately from the leachate results (Figure 2) as already suggested by reviewer #2. The authors discarded this suggestion because the figure became too busy. However, the difficulty can be overcome by using different eNd scale adapted to each oceanic basin. The main message of Figure 2 is the different timing of d13C and eNd variability. It is not compulsory to use the same eNd scale for the three basins.

Figure 2 shows what the reviewer is asking for: more detail on the eNd evolution of these archival sources. Figure 1 is more intended to demonstrate the longer-term trends of eNd before and after the PETM and to illustrate where/when the eNd signals changed (and where they do not). We originally had drawn Figure 1 as suggested, but we realized that a direct comparison of all these basins with respect to eNd and $d^{13}C$ (on the same respective scales) best illustrates the relative changes in circulation indicated by our proxy records.

Figure 3: Since bathymetry constrain is discussed in the text, it will be helpful to add bathymetry as background in the map. It is not clear how the starting points of the black arrows are determined and what are the expected eNd values of the area of staring points.

The schematic arrows are generally based on a consistent interpretation of the observed Nd isotope variability and with starting points of the arrows consistent with presumed areas of deep water formation. We also think that bathymetry is not required for such a coarse schematic map, also given that it is not our intention to reconstruct pathways in detail but only to outline the general circulation patterns.

In the answer to the reviewers, the authors stated that there was no evidence for the presence of volcanogenic material in the sediments. It is necessary to clarify how the absence of the phase was examined.

Volcanic material is easily identifiable macroscopically and our statement is simply based on the lack of observations of ashes or volcanic glasses, which we would not have overlooked during homogenization of the sediments.

In the revised version, the authors indicate that acetic acid leachates had comparable eNd values with HH leachates within 0.5 e-unit. This information is important and should be added in Table 2.

We only ran only a few paired acetic acid and hydroxylamine leachates, during method development (<5 samples total). We stopped very early given that the data of both methods were indistinguishable. We have removed this text, as it is not critical to the information and interpretations presented.

Table 1. I do not think that core sites are all "IODP" (mostly DSDP and ODP).

Corrected in table heading.