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Biogeosciences Discussions

Interactive comment on "The marine sedimentary nitrogen isotope record" *by* J. E. Tesdal et al.

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We would like to thank Referee #2 for the helpful comments. It is apparent that we did not sufficiently clarify the purpose of our article, which is twofold: 1) to provide a full description of the NICOPP database, a new data product available to the community, and 2) to compare the seafloor and downcore data in a general way, in terms of understanding the spatial consistency in the data and the fidelity with which it is transferred from the seafloor to the subseafloor sediment. In addition, Referee #2 called for further mechanistic interpretation of the database.

We are currently preparing a significantly reorganized manuscript in order to address all of the referees' points, and to point toward future work that can be carried out by the community using the database. Some specific responses follow.

Comment: *Exactly what do the authors intend to show and during which period of time are they focusing? Holocene, Pleistocene, Pliocene?*

Response: The NICOPP database represents a global repository of all available bulk sediment data of d15N measurements, for public use. It spans the late Cenozoic, and, as such, its description simply summarizes all available d15N data that has been measured over a range of timespans. Common to all timespans is a concern that diagenetic alteration might have compromised the primary marine signal – therefore, we focus on the fidelity with which the subseafloor signal is generated, as illuminated by this compilation. In the original submission we focused on the Holocene data only, but in the revised manuscript we will discuss how Pleistocene data can also help with this.

Comment: *Why have they decided to show only the comparison between 5 kyrs and today? The premise of the manuscript is at no point explained. Why 5 kyrs BP? Why do the authors not restrict the picture to several key periods of the Holocene. To my knowledge, 5 kyrs BP is not the most exciting period in terms of climate change. The early Holocene warmth (prior to 8 kyrs), the 6 kyrs event, or the last 2 kyrs would merit further consideration (in addition to the 5 kyrs time window) in a way that they do represent rapid climate shifts of interest to the reader and the scientific community at large. How the nitrogen cycle reorganized during these intervals is important and would be more useful. I would prefer to see a suite of maps similar to those in Fig.6 showing d15N values during different major events of the Holocene rather than a comparison with 5 kyrs.*

Response: In fact, this is a misunderstanding - we compare the average d15N between 0 to 5 kyrs and the d15N value of the surface sediment (which we assume to be close or equal to the present-day sedimentary d15N). The maximum age of 5 kyr BP as the start of the late Holocene was chosen to include as many downcore records

as possible, since many cores are missing the most recent millennia. Due to these old core tops and different resolution, some records contain several d15N measurements in that period while other only one or two, hence we chose to take the average d15N between 0 to 5 kyrs to ensure a more robust signal. Since we are only concerned on the translation of d15N between surface and downcore, we only looked at the top section of the downcore (0-5 kyr).

Following the logic of Freudenthal et al. (2001), the late Holocene should have been quite constant in d15N (also in agreement with Altabet, 2007). In this case, diagenesis should be the explanation for any discrepancies between the 0-5ky interval and the seafloor d15N. This reasoning should have been better clarified, and will be much better elaborated upon in the revised manuscript. Since the study's purpose was not to investigate climatic changes of the nitrogen cycle in the Holocene or any other period, we did not provide further comment about this. However, we will do so in the revised manuscript.

Comment: *The nitrogen cycle is quite variable from time to time and between regions, and we suspect that over the last century it may have drastically changed. Such variability is much more likely in regions located along the continental margins where changes in wind-driven costal upwelling might be strong enough to induce some important isotopic signature changes at a decadal scale (e.g. Eastern Equatorial Pacific). This might provide an alternative explanation for the discrepancies between seafloor and sub-seafloor d15N records.*

Response: We agree, and will discuss this more clearly in a revised manuscript.

Comment: *I also find the tables slightly irrelevant. The selected sites include many records from the Holocene, Pleistocene and Pliocene. Not all of them are continuous and some only focus on the early Pleistocene (e.g. Site 1082, Robinson et al., 2002).*

It would be useful to at least mention which periods of time the records span. Why are so many Pleistocene and Pliocene records listed when the authors focus only on the Holocene? Why even cite records for the Pliocene if the authors do not intend to use them?

Response: We feel the table is an integral part of the NICOPP database description, especially given that it includes a number of unpublished records that require documentation. This will be better explained in the revised manuscript. We agree that the corresponding time spans for each record should also be given in the table, and will make this change as suggested. In addition, as mentioned above, we will also discuss a greater fraction of the Pleistocene records in our revised analysis.

Comment: *The authors consider the Eastern Equatorial Pacific for insisting on the relevance of d15N records but I do not see the point. What is it that the authors want to show if not a global pattern? Why do the authors not show the several key regions mentioned in the text (China Sea, Arabian Sea,. . .)?*

Response: The Eastern Equatorial Pacific focus was chosen because of the high data density. The Arabian Sea was also examined, but not included in the original manuscript for conciseness. We will mention it, however, in the revised manuscript.

Comment: *The authors also mention that diagenetic alteration strongly affects the d15N bulk. This is true. However, when available, other data could be compared with diagenetic-resistant d15N records (diatoms, foraminifera,. . .). Could the authors represent some comparisons with other d15N proxies?*

Response: We agree that this would be great, and have been collecting diatom and foram-bound d15N data in order to make comparisons with the bulk, wherever possible. However, it remains to be seen if sufficient data can be retrieved at the present time in

order to achieve a meaningful comparison.

Again, we thank Referee #2 for the input, which we are confident will lead to a stronger revised manuscript.

References

Altabet, M. A.: Constraints on oceanic N balance/imbalance from sedimentary ^{15}N records, Biogeosciences, 4, 75–86, doi:10.5194/bg-4-75-2007, 2007.

Freudenthal, T., Wagner T., Wenzhöfer F., Zabel, M., and Wefer, G.: Early diagenesis of organic matter from sediments of the Eastern Subtropical Atlantic: Evidence from stable nitrogen and carbon isotopes, Geochim. Cosmochim. Ac., 65, 1795–1808, doi:10.1016/S0016-7037(01)00554-3, 2001.