Biogeosciences Discuss., 9, C2348–C2351, 2012
www.biogeosciences-discuss.net/9/C2348/2012/

Biogeosciences Discussions

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

Anonymous Referee #2

Received and published: 9 July 2012

Tesdal and co-authors present in this manuscript a compilation of seafloor and subseafloor d15N data from the global ocean. They first list locations where d15N records spanning different periods of time (Holocene, Pleistocene and Pliocene) are available. They secondly highlight the robustness of the bulk d15N proxy for reconstructing nitrogen cycling in the past. Finally, they aim to provide a global picture of changes in the isotopic ratio between 5 and 0 kyrs BP. Studying nitrogen cycle in the past is of primary importance as it might strongly influence global climate. In this way, information about its evolution during warmer-than-today to cold climate episodes, glacial-interglacial variations or over the last millennia is absolutely needed.

Although such a database represents a major effort to elucidate the impact of nitrogen cycling on climate change, as such, I have to admit I am not very excited about the manuscript. I think it is imprecise, confusing and not really well organized. The manuscript seems to be a summary of observations without giving any explanations

about potential mechanisms involved. Perhaps, this is due to the shortness of the paper. I do not understand after reading the text what the authors exactly want to show. My impression is that the manuscript is a mix of data from a mix of periods of time, not really connected to each other, corresponding to a suite of observations with no logic reasoning. In addition to offering nothing new, question the significance of the information offered to the reader. Is the aim of the paper to show the robustness of the proxy used? Except experts on nitrogen cycles and d15N who may be interested by this work, I am not sure what the 'common' reader will gain by reading this manuscript. I think the manuscript is not mature enough and significant reorganization of the manuscript is necessary before considering publication. I therefore cannot recommend in its current version the publication of this manuscript.

First, I question what new information this study brings to the scientific community that we do not already know. Exactly what do the authors intend to show and during which period of time are they focusing? Holocene, Pleistocene, Pliocene? I can perfectly understand that the authors wish to provide an overview of d15N records but why did the authors not only focus on Holocene records for instance instead of including records from everywhere and across a multitude of time periods? 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.

More intriguing to me is that the authors assert that the good correlation between

BGD

9, C2348–C2351, 2012

seafloor and sub-seafloor d15N is highly encouraging for the use of downcore d15N as a proxy for past nitrogen cycle (p. 4073-4074). Why do they expect a correlation? Between 5 kyrs and today, should the denitrification zone for instance be similar to today while they report later (p.4074) differences between periods of time? How do the authors precisely date 5 kyrs?

The authors assert that seafloor d15N values are in good agreement with sub-seafloor results. However, again, I question how well-dated are these core-top sediments? Could there be a difference of 10, 100, 1000 years? Is it unlikely that all the seafloor samples correspond to the present, they may well be older. At the very least the authors should describe their means of chronological control (210Pb,. . .). 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.

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? Explanations are missing throughout the manuscript. What is the relevance of this study?

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,. . .)?

BGD

9, C2348–C2351, 2012

Interactive 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?

To summarize, although I see the merit of this work, I have the impression that this manuscript presents a (quite interesting) database without giving any explanations about mechanisms, processes... which makes the manuscript frustrating to read. The authors have a powerful tool for resolving part of the crucial history of nitrogen cycling but unfortunately they underexploit it.

Interactive comment on Biogeosciences Discuss., 9, 4067, 2012.