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Biogeosciences Discussions

Interactive comment on "Natural isotopic composition of nitrogen in suspended particulate matter in the Bay of Bengal" *by* S. Kumar et al.

S. Kumar et al.

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Answers to referee # 1's comments:

Comment 1. "Mainly, I do not understand why particulate organic carbon data was not investigated. The additional information provided by not only the d13C signature but even by the C: N ratio would have greatly strengthened the arguement for a 2 end member mixing model. . As presented, the argument for a 2 end member mixing model is not very convincing "

Reply: The d13C analysis was not performed because it does not provide any additional information in the present context. d13C values of terrestrial organic matter, in general, fall within the range of -23 to -28 per mil (C3 plants, predominant in the Indian subcontinent presently) and tends to overlap with the d13C of marine organic matter and may not be easily distinguishable. However, C: N measurements were performed at a few locations during the post monsoon of the present study (included in the revised manuscript). The C: N values at locations under coastal influence have been found relatively higher (9.5, 9.3 and 8.2 at stations 12, 16 and 18 respectively) com-

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pared to stations without influence (5, 3.4, 4.6, 6.2 and 6.4 for stations 3, 6,9,20 and 23 respectively) further strengthening our argument for mixing as suggested by the reviewer. However, in the revised manuscript we have avoided the use of the term "end member".

Comment 2. "The possible contribution to the nitrogen pool by nitrogen fixers such as Thichodesmium is dismissed too easily by the authors. Jyothibaba et al. (2003) found Trichodesmium blooms near stations 14 and 24 in April 2001 although there is no mention of this in the manuscript. Simple phytoplankton counts would have been useful in determining whether nitrogen fixers were important at the stations sampled in this study."

Reply: Unlike in 2001, no Trichodesmium bloom was found during our study in 2002 and 2003; the main phytoplankton species were diatoms. However, there were sporadic occurrences of Trichodesmium during premonsoon but it did not dominate in terms of N contribution to the PON (Dr. N.Ramaiah, National Institute of Oceanography, Goa, India). Our measurement of d15N of PON clearly indicates that if at all there is influence of Trichodesmium, it is very small. This aspect has been included in revised manuscript.

Comment 3. "It seems that a mixing model with three nitrogen sources may be as valid as a 2 end mixing model with the data presented."

Reply: Yes, we agree. When we say two end member we meant terrestrial and marine sources (mainly phytoplankton). However, the marine end member has a wide spectrum of values and can be split into two: one the phytoplankton that assimilate nitrate without fractionation (highest d15N) and other with a high degree of fractionation (lowest d15N). In this perspective we can say it as three end member mixing. But clearly, more than three end members cannot be accounted for by the data.

Comment 4. "In addition, there is no data presented from distinct river plumes to support the contribution of a terrestrial end member. When assuming a 2 end member

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mixing model, samples should be collected from both sources to better strengthen the 2 end member model."

Reply: Salinity gradient from north to south in the Bay of Bengal (low salinity near the coastal stations and high salinity in the open ocean) as depicted in the manuscript is clear proof of contribution from terrestrial sources. We have measured d15N of PON in salinity as low as 21 psu (~2per mil) and as high as 34 psu (~7.5per mil). These are clearly values close to the two end members.

Comment 5. "The authors also state that the d15N of NO3 is likely in the 3-7 per mil range that has been reported for NO3 in deeper waters lacking significant water column denitrification. According to Sundarvel, oxygen concentrations are low in the BOB water column. I do not understand why the authors assume that no denitrification is occurring. Is there other data from the cruise to support this assumption?"

Reply: The Bay of Bengal water is well oxygenated relative to the Arabian Sea (where denitrification is known to occur; Naqvi, 1991). During the premonsoon season oxygen concentrations $>$ 175 micro molar and 150-175 micro molar in surface mixed layer were observed in open and coastal regions. During the post monsoon in the open ocean oxygen minimum zone (OMZ) with oxygen concentration $<$ 10 micro molar was observed between depths of around 60 m to 400m from14 to 20°N. During premonsoon the OMZ was confined to a smaller area i.e. from 19 to 20°N between 80 to 120m which extends to 14°N between100 to 300 m. OMZ was much thicker and was seen from 11 to 20°N between approx. 120 to 500 m in the coastal region during post monsoon but shows shoaling up to 60m around 17°N during pre monsoon. Pockets of very low concentration of oxygen ($<$5 micro molar) are also observed in the coastal region. Though, such low oxygen contents are present we did not encounter significant secondary nitrite levels or decrease in nitrate levels in the region of OMZ to suggest denitrification during either seasons. So our contention that the Bay of Bengal water lacks significant column denitrification is reasonable. This has been included in revised text.

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Comment 6. "NO3 data is presented for surface waters but no mention is made of NO3 concentrations at depth. Were these samples collected? If so, the data should be shown. Also, there is no description of the methodology used to measure NO3 in the methods section."

Reply: Samples for NO3 concentrations at depth were collected but have not been incorporated in figure form in the present manuscript as the arguments put up during the present study require only surface NO3 data which has been presented. However, average NO3 concentrations at depths, below which it increases/decreases significantly, have been mentioned in the revised text (reply 5).

Reference to the standard methodology used by us to measure NO3 has been included in the revised manuscript.

Comment 7. "In the results section, the authors state that the relationship of POC and d15N is more significant during pre-monsoon that post-monsoon season but only present the R2 as evidence. A more thorough statistical test (such as a simple t-test) should be run on the data to better support this statement."

Reply: We believe the referee means PON and not POC. The suggested t-test has been performed with the data and R2 = 0.42 reported for post monsoon has been found significant at p = 0.005. R2 = 0.21 reported for premonsoon is significant at p = 0.025 level. This has been added in the revised manuscript.

Comment 8. "On a technical note, the caption for Figure 5, a scatter plot, states that the annotations are the same as for Figure 2 which is a bar graph. I think it should read that the annotations are the same as Figure 3."

Reply: We thank the referee for pointing out this error, which has been rectified in the revised manuscript.

Comment 9." Lastly, I feel that the authors need to compare their data to more recent papers in the literature. Comparisons to other recent studies would greatly add to

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the quality of the paper. In the last few years, many measurements have been made for d15N of not only particulate matter but also DIN have been made in the world's ocean. Although data may not be available from the BOB, there are many recent studies that would be relavent to cite. The references presented here such as Miyaka and Wada (1967) and Minagawa and Wada (1986) are indeed landmark papers in the historical context of stable isotope studies but they are quite old (18-37 years old). The methodologies used to investigate stable isotopes has evolved significantly in the last decade or two and there are more recent papers to which the data collected by Kumar et al. could be compared."

Reply: Studies as recent as 2002 regarding d15N in PON has been referred by us (Mino et al., 2002; Rau et al., 1998; Wada and Hattori, 1991, Altabet, 1996, Altabet and Francois, 1994). Referee agrees that there is no data available for comparison in the Bay of Bengal and ours is the first report of such data from the region. However, we have included a small paragraph along with a table for comparison with studies in the other oceanic regions as suggested by both the referees.

Answer to referee 2's comments:

Comment 1. "The manuscript by S. Kumar et al. on del 15N values of suspended PON in the Bay of Bengal is said to constitute the first such detailed measurements in the Bay of Bengal. However, no reference is made to earlier, more cursory or limited measurements for comparative purposes which would have been of interest and perhaps supportive of some of their hypotheses regarding the observed patterns."

Reply: The data reported during present work is the first measurement from the Bay of Bengal and no data exists for the comparison purpose from the study area. However, only one data point reported by Saino and Hattori (1980) for the far eastern Indian Ocean has already been cited in the manuscript.

Comment 2. "This is symptomatic of a larger problem of not placing their data in a broader context. I would have liked to see the authors to at have least produced

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a table that compared del 15N values in the Bay of Bengal to other large regional bodies of water around the globe. I believe the manuscript suffers from too regional an approach."

Reply: We have incorporated a paragraph and a table comparing the present study with the d15N in PON of other oceanic regions in the world as suggested.

Comment 3. "Perhaps it is a failing on my part, but I found the presentation of the results did not clearly point out the most important findings of the study."

Reply: The main aim of the paper was to highlight seasonal changes in nitrogen isotopic composition of surface PON in the Bay of Bengal and how the influence of riverine discharge affects the value. Our idea was also to highlight the observed differences in isotopic composition at depth and role of mineral matter brought in by rivers in modifying the isotopic signature compared to regions with no such influence.

Comment 4." The figures are poor, particularly Fig. 3 which is almost impossible to decipher. The map with station locations drawn for Fig. 1 should be enlarged and should include the major rivers discharging into the Bay of Bengal, especially since the latter plays a prominent role in the authors' interpretation of the data. In addition, there are too many latitude and longitude lines."

Reply: All the figures have been modified in the revised manuscript with suggested modifications. Fig.1. has been modified with showing the major Indian rivers draining into the Bay.

Comment 5. "The pre- and post-monsoon sampling took place over two months. Could this have influenced the results? Were there any cyclones or major rainfall events along the Indian coast during these time periods? Were the discharge rates of terrestrial-dervied materials from the continent low or high preceding the taking of samples along the coast? Do any such measurements exist from the mouths of these major rivers for the sampling periods in question?"

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Reply: The pre-monsoon sampling took place from 17th Sep to 11th October 2002 (span of 24 days) and post-monsoon sampling took place from 16th April to 6th May 2003 (˜ 20 days, details included in the revised manuscript). It did not take place for a period of over two months.

No, there was no specific cyclone or specific major rainfall event along the Indian coast during sampling time to influence the results. During post monsoon, there is intense river discharge due to monsoon rains, which is well known.

During the monsoon (SW monsoon ˜ May to September) period, there is heavy rainfall in the Indian subcontinent and major rivers like Ganga and Brahmaputra bring a lot of freshwater leading to change in the salinity of coastal region during post monsoon season. However, the discharge by rivers during premonsoon period is low and hence its influence on coastal locations is much less as evidenced clearly by salinity variation.

Comment 6. "When differences were claimed to be significant or insignificant what statistical approaches were employed to analyse the data? What were the p values for the regressions characterising the data points of Fig. 3? What were the regression equations? Also, an r2 of 0.21 (premonsoon regression) is fairly low even if it is significant."

Reply: The t-test was performed to check the statistical significance of the data. R2 = 0.42 reported for post monsoon has been found significant at p = 0.005 whereas R2 = 0.21 reported for premonsoon is significant at p = 0.025 level. The equations for post and premonsoon were (d15N = 2.35*PON + 0.78) and (d15N = 4.05*PON + 0.74) respectively. This has been included in the revised text.

Comment 7."Finally, the authors point out that postmonsoon oceanic stations show a bimodal distribution, but it seems to me that they could be better described as forming two clusters of data points."

Reply: Yes, we agree with the referee's point that post monsoon oceanic stations are

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two clusters of data points and has been incorporated in the revised manuscript.

Comment 8. "The authors state that the nitrogen-fixing cyanobacterium Trichodesmium can be assumed to be absent because the observed 15N values. Granted, this is probably the case, but why didn't the authors at least take a cursory look through the microscope and identify the dominant groups of algae (and perhaps species) composing the phytoplankton? After all, the del 15N values are single data points summarising the del 15N values of what are assumed to be phytoplankton-dominated organic matter. These are complex and highly dynamic assemblages of organisms that may be quite different in species composition between seasons and stations, and this could have a bearing on the observed spatial and temporal patters for del 15N."

Reply: The phytoplankton species identification was performed by NIO (Goa, India) colleagues (Dr. N.Ramaiah, National Institute of Oceanography, Goa, India) using microscope and diatoms were found to be abundant species during the study period (Please see also the reply 2 for referee1). This has been included in revised text.

Comment 9. "The authors state that they have no measurements of del 15N for nitrate or ammonium in the Bay of Bengal and no information is presented regarding rates of denitrification, nitrification, or nitrogen fixation during the times of sampling. Such information seems to me to be critical to interpreting patterns in del 15N and concentrations of PON in the Bay of Bengal. Thus, the data as presented here are of a preliminary nature until additional parameters can be measured."

Reply: We agree with the referee about the importance of the processes like denitrification, nitrification, or nitrogen fixation during the time of sampling. However, as we discussed in our answer to referee 1 's comment, denitrification did not occur during the time of sampling in the Bay of Bengal. Since Trichodesmium was not the major species during study period we do not expect direct nitrogen fixation as a dominant process. Not much information could be gathered regarding nitrification during time of sampling. However literature suggests that elsewhere, nitrification (Dore and Karl,

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1996) occurs in the water column at the base of euphotic zone; and the significance of this process in the Bay remains to be assessed.

Comment 10. "I am a bit uncomfortable with the authors' approach that a two-end member model can be assumed with the end members being continental input and marine phytoplankton. First, the situation may well involve several end members that remain to be discovered from additional work and the adding of del 15N values of parameters not yet measured."

Reply: It is clear from figure 5. that there cannot be more than three end members. These are: Continental (salinity 21psu and d15N = 2 per mil) and marine (salinity 34.5psu and d15N = 7.6 per mil; salinity 33.4 and d15N = 2.0per mil). Please also see reply 3 & 4 to referee 1. These are not "pure" end members but end members that can explain the data presented in the manuscript. However, we have dropped the "end-member" term from our revised manuscript.

Comment 11. "Second, I'm not sure that something as undefined as continental input should be considered as a possible end member and I think marine phytoplankton is more of a result or compositing of end member uptake."

Reply: Low salinity is a clear indication of continental input. "Continental input" has been considered as end members in literature. Mariotti et al. (1984) have explained the suspended organic matter of the Scheldt estuary as a mixture of two components: continental component characterized by low d value and marine component with high d value.

Comment 12. "Perhaps it might be better to talk more about possible influences on del 15N of coastal and oceanic phytoplankton and for now abandon the idea of end members until much more is known about the system and its dynamics."

Reply: Yes, we agree with the reviewer.

Interactive comment on Biogeosciences Discussions, 1, 87, 2004.

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