

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

Anonymous Referee #1

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Overall, I felt that the study by Kumar et al to be interesting however I felt there was much that could be added to the manuscript. Mainly, I do not understand why particulate organic carbon data was not investigated. The additional information provided by not only the $\delta^{13}\text{C}$ signature but even by the C:N ratio would have greatly strengthened the argument for a 2 end member mixing model. As presented, the argument for a 2 end member mixing model is not very convincing. The possible contribution to the nitrogen pool by nitrogen fixers such as Trichodesmium 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. It seems that a mixing model with three nitrogen sources may be as valid as a 2 end mixing model with the data presented. 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 mixing model, samples should be collected from both sources to better strengthen the 2 end

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member model.

The authors also state that the $\delta^{15}\text{N}$ of NO_3 is likely in the 3-7 per mil range that has been reported for NO_3 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? NO_3 data is presented for surface waters but no mention is made of NO_3 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 NO_3 in the methods section.

In the results section, the authors state that the relationship of POC and $\delta^{15}\text{N}$ is more significant during pre-monsoon than post-monsoon season but only present the R^2 as evidence. A more thorough statistical test (such as a simple t-test) should be run on the data to better support this statement. 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.

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 the quality of the paper. In the last few years, many measurements have been made for $\delta^{15}\text{N}$ 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 relevant 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.

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