

Interactive comment on “Holistic monitoring of increased pollutant loading and its impact on the environmental condition of a coastal lagoon with *Ammonia* as a proxy for impact on biodiversity” by Areen Sen and Punyasloke Bhadury

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The authors would like to thank the referee for rigorously going through the entire manuscript and giving valuable comments on the work. The comments and suggestions provided by the reviewer have significantly helped in improving overall quality of the manuscript. We have addressed all the pertinent questions raised by reviewer. The observations provided by the referee have been sequentially addressed in the following section.

1. I wonder why did the authors not collect samples from the northern lagoon, at all.

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One sampling stations should also have been from the northern lagoon, to make a true representative of the entire lagoon.

We have selected the six stations after observing the benthic biota of the entire lagoon by performing monthly samplings from June 2013 across twenty three sampling stations. In the manuscript, the criterion for selecting the present stations has been detailed in revised version of manuscript.

2. The authors claim that the lagoon is witnessing eutrophication, but I do not see any comparison with previous reports from Chilka lagoon.

We have addressed this point by comparing the observed values with previously reported values in revised version of manuscript. We have also provided reference of a recent investigation that characterized Chilika lagoon as eutrophic in revised manuscript.

3. Line 25: 'era' is a geologic term designated for specific interval. Please avoid using it. Also, 'Anthropocene' is yet to be accepted as a time interval.

The authors thank the referee for pointing out this detail and the sentence has been rephrased without using the term 'era of anthropocene'.

4. Line 28: delete 'production'

The word has been deleted.

5. Line 51-52: Not clear, please rephrase.

The sentence has been rephrased in revised version.

6. Line 54: The reference is too old to suggest that this realization is recent.

The word 'recent' has been removed and the sentence has been rephrased in the revised version.

7. Line 61: 'also acted as a catalyst'. . . for what?

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We have rewritten the sentence to bring in clarity in revised version of the manuscript.

8. Line 117-118: I think there is plethora of data on the environmental factors that control Ammonia distribution in shallow water regions, both globally as well as from Indian Ocean. I request the authors to review the literature.

We agree with the referee regarding the existence of ample data on environmental controls on Ammonia distribution. Literature review stating global and Indian waters have been added in the revised version of manuscript.

9. Line 128: How do the authors quantify the 'increase' in nutrient influx? Is there any previous nutrient data available from the Chilka lagoon, to compare their recent data to suggest an increase in nutrient influx?

Comparison of present reports of nutrient concentrations with previous reports is constrained due to the different types of methodologies used. The authors have come across one single publication that has utilized similar methodologies to quantify nutrient concentrations from the lagoon. A comparison has been made with the publication and our study to demonstrate that there is an increase of nutrient flux in Chilika.

10. Line 136: I think the sea access was subsequently, restored manually. Please add details.

Details regarding the restoration of sea access have been added in the revised version of manuscript.

11. Line 140: Is it 'seepage' or discharge?

'seepage' has been changed to 'discharge'.

12. Sampling Stations: Why no station was considered in the northern part of the lagoon?

The authors designed the present six stations after observing the benthic biota of the entire lagoon by performing monthly samplings from June 2013 across twenty three

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sampling stations. In the revised version the criterion for selecting the present stations has been detailed.

13. Line 150: Please specify the depth.

Depth of all stations has now been included in Table 1 of revised version of manuscript.

14. Line 162: Please add make and precision of the thermometer.

Make and precision of the thermometer has been included in the revised version of manuscript.

15. Line 163: Please add precision of salinity and pH measurements.

Precision of salinity and pH measurements has been included in the revised version of manuscript.

16. Line 164: From how close to the sediments, the authors collected bottom water.

The details of bottom water collection by deploying Niskin sampler has been included in revised manuscript.

17. Line 174: Even though the depth seems to be very shallow, there could be a big difference in elemental concentration in surface water and that at the sediment-water interface. As the authors have measured the elemental concentration only in surface water, is it appropriate to use it to understand benthic distribution?

The authors agree with the concern raised by the referee and concur that it is not appropriate to use surface water elemental concentrations in deciphering the benthic environment. Hence, all methodological descriptions, results and discussions pertaining to the measurement of potentially toxic elements (PTEs) from the lagoon have been completely removed in the revised version of manuscript.

18. Line 182: Are you sure that the top 0-2 cm contains the entire living benthic foraminiferal assemblage in this lagoon?

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The choice of restricting the study to the top 0-2 cm of sediment was made following the finding of Sen and Bhadury (2016) where > 90 % of the living assemblage was found to be residing in the surface 2 cm. This reference has been included in the revised version of manuscript.

Sediment Composition: The authors should have removed the biogenic carbonate before measuring the different size fractions.

The influence of biogenic carbonate in determining the sediment fraction was minimized by manually removing the mollusk shells from the sediment prior to analysis. This detail has been included in revised version of the manuscript.

19. Line 244: Does the use of fresh water jet, does not result in test breakage?

The sentence has been reconstructed to better represent the protocol that was adopted in the study.

20. Line 272 and 274: The salinity range is same for both surface and bottom water?

The recorded salinity range from both surface and bottom water compartments did display same range.

21. Line 277: Please modify this sentence as the salinity cannot go down below 0 psu.

The sentence has been modified.

22. Line 279: 'entirely devoid of salinity'..... please modify.

The statement 'entirely devoid of salinity' has been modified to 'displayed values at 0' in revised version of manuscript.

23. Line 291: How is it useful and what does it signify?

The significance of the observed correlation has been added in the discussion section of revised version of manuscript.

24. Line 307: The sentence is not clear. Please change.

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The sentence has been reframed in revised version of manuscript.

25. Line 332: When was the concentration higher?

The time duration of higher values of orthophosphate has been clearly mentioned as 'during monsoon' in revised version of manuscript.

26. Please replace 'live' with 'living'

Throughout the revised manuscript 'live' has been replaced with 'living'.

27. Line 453-461: Please delete as it looks like the abstract.

The entire section has been deleted.

28. Line 470-471: Did the authors measure precipitation? If not, then please cite the source.

The source of the precipitation values have been added in revised version of manuscript.

29. Line 481: These species belong to which group?

The species mentioned are plants and have now been mentioned in revised manuscript.

30. Line 483 and Line 489-491: As I understand, the authors mention that freshwater plants are abundant during the monsoon months. Then why the pH is not high during the same time, if the presence of these plants governs the pH. The pH is consistent throughout March to October. In fact, a sharp decrease in pH is observed during November-December, which clearly contests authors conclusion about a link between pH and freshwater plants. Again, the explanation for pH drop during November is not convincing. First, both the bottom and pore water have higher DO during both the monsoon and post monsoon months. Therefore, the question of anaerobic degradation of organic matter does not arise. Secondly, the surface pH is lower than both the bottom

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and pore water pH, which once again suggest that the organic matter degradation in bottom sediments is not the reason for low pH during November.

The pH profile of the lagoon appears to be complex. The authors agree with the referee that the previous explanation of pH variability was not adequate and therefore detailed explanation of most likely pathways that may influence pH across different depths have been provided in revised version of the manuscript.

31. Line 498-499: Just in the previous line, authors mention that the pH values fall well within the CPCB range. Then how do you suggest that the lagoon health is under severe threat?

The sentence clearly states that 'severe threat from increased nutrient loading.' and therefore the previous sentence has been modified in revised manuscript to clarify that the mentioned CPCB range is only valid for pH values and the condition is optimum with respect to pH only.

32. Line 582: The $\delta^{13}\text{C}$ varies from -21 - -24 , which as per the range of values listed by the authors should indicate a mixed origin. How does the authors conclude that it indicates autochthonous origin?

Majority of the $\delta^{13}\text{C}$ values fall within the autochthonous range. The entire section has been modified to support the autochthonous nature of the carbon in revised version of manuscript.

33. Line 595-598: I think this statement is contradictory to what the authors stated above (Line 583). How can the TOC be autochthonous, when the material is brought from outside the lagoon, as the authors ate in this paragraph?

The sentence has been modified in revised version of manuscript.

34. Line 622: foraminiferal taxa as indicators'.... Of what? Please slit split the sentence as it is too long.

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The sentence has been restructured in revised version of manuscript.

35. Line 629: specimen

The correction has been made in revised version of manuscript.

36. Line 628: The authors should elaborate the factors affecting dead assemblages, especially the currents and also whether it is a likely factor in the closed lagoon like Chilka.

The impact of currents and taphonomic processes on the dead assemblage has been added in revised version of manuscript.

37. Line 641: I seriously doubt the validity of this statement. These stated species might be the most abundant in the shallow water regions, but definitely can't be 'most abundant foraminiferal genera globally', as they are absent in deeper regions. Please recheck and modify.

The statement has been changed to 'is the most abundant shallow water foraminiferal genus..' in revised version of manuscript.

38. Line 650-653: I do not agree with the authors as 1) there is hardly any seasonal change in TOC, 2) there is no consistent trend in benthic assemblage at all the stations, 3) the seasonal change in nutrients does not match at all with the change in benthic foraminifera.

(1) Seasonal variations in TOC was not present throughout the entirety of studied lagoon but was restricted to stations where possible sources of nutrient influx existed and has been detailed in revised manuscript. (2) The benthic assemblage did not display a seasonal trend but more so a spatial trend as discussed in the manuscript (3) In the present version, the objective of the investigation is to test the utility of Ammonia with respect to sedimentary organic carbon content and hence the relationship of assemblage with dissolved nutrients is not the focal point of the study. Significant correlation observed between the assemblage and the TOC value support the conclusion drawn

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by us in our study . 39. Line 655 and Line 659: Are there no studies utilizing benthic foraminifera for pollution studies from Indian Ocean? Such field based observations have also been confirmed by laboratory culture experiments.

The entire section has been removed in order to provide a more composite investigation.

40. Please also note the supplement to this comment: <http://www.biogeosciences-discuss.net/bg-2017-13/bg-2017-13-RC1-supplement.pdf>

The authors would like to thank the referee for highlighting the grammatical and typographical errors in the PDF format as provided in supplement. All changes suggested by the referee have been performed in the revised version of the manuscript.

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