

***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***

**Anonymous Referee #1**

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Sen and Bhadury, have monitored the state of Chikla lagoon for almost an year. The parameters monitored are comprehensive and provide an insight in seasonal changes in Chilka lagoon. The data collected is useful, but the interpretation is overstretched and contradictory. I feel that the authors have unnecessarily tried to sensationalize the findings, although the data suggests otherwise. I commend the authors for the extensive work but request them to re-look the data before the manuscript is accepted for publication. Besides the edits in the annotated pdf file, I've the following observations.

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

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all. One sampling stations should also have been from the northern lagoon, to make a true representative of the entire lagoon. 2. The authors claim that the lagoon is witnessing eutrophication, but I do not see any comparison with previous reports from Chilka lagoon.

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.

Line 28: delete 'production'

Line 51-52: Not clear, please rephrase.

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

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

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.

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?

Line 136: I think the sea access was subsequently, restored manually. Please add details. Line 140: Is it 'seepage' or discharge?

Sampling Stations: Why no station was considered in the northern part of the lagoon?  
Line 150: Please specify the depth.

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

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

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

Line 174: Even though the depth seems to be very shallow, there could be a big

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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?

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

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

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

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

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

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

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

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

Line 332: When was the concentration higher?

Please replace 'live' with 'living'

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

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

Line 481: These species belong to which group?

Line 483: 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,

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which clearly contests authors conclusion about a link between pH and freshwater plants.

Line 489-491: 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 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.

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?

Line 582: The  $\delta^{13}C$  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?

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?

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

Line 629: specimen

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.

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.

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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.

Line 655: Are there no studies utilizing benthic foraminifera for pollution studies from Indian Ocean?

Line 659: Such field based observations have also been confirmed by laboratory culture experiments.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/bg-2017-13/bg-2017-13-RC1-supplement.pdf>

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-13, 2017.