

## ***Interactive comment on “Transparent exopolymer particle binding of organic and inorganic particles in the Red Sea: Implications for downward transport of biogenic materials” by Abdullah H. A. Dehwah et al.***

**Abdullah H. A. Dehwah et al.**

tmissimer@fgcu.edu

Received and published: 25 July 2019

Interactive comment on “Transparent exopolymer particle binding of organic and inorganic particles in the Red Sea: Implications for downward transport of biogenic materials” by Abdullah H. A. Dehwah et al. Anonymous Referee #1 Received and published: 2 April 2019 In this manuscript, the authors present survey data on organic matter pools as well as sensor data from the Red Sea. The study strongly builds upon the circumstance that it is the very first study recording the measured parameters in the given combination at the study region over depth (down to the base of the eu-

C1

photic zone). The authors are very open in this regard stating themselves the data presented "have not been collected in a systematic manner with spatial and temporal comparisons to assess the biogeochemical cycles within the Red Sea comprehensively." Hence, the study does not come up with substantial new concepts, but rather provides a starting point for following up analyses of matter cycling over depth in the study region. Below are detailed comments. Major issues are that the section describing the statistics needs to be elaborated to achieve appropriate quality of the paper. The study design is rather unbalanced, with three shallower stations sampled during spring and one deep station sampled during winter. Additional data from unrelated surveys in the region were compiled and included in the manuscript as an additional data source for measurements at surface depths. Response: We agree with this statement of paper content. \*\*\*\*\*General remarks\*\*\*\*\* - The title is rather misleading since the binding capacity of TEP was not examined explicitly, as the title implies. In addition, the term 'inorganic particles' seems not to be suitable here as solely the organic fraction is analysed. Response: We have changed the title of the paper as suggested by the reviewer. The new title is: Transparent exopolymer particle occurrence and interaction with algae, bacteria, and the fractions of organic carbon: Implications for downward transport of biogenic materials.

- The description of the results is not completely balanced. Some parts should contain fewer details while other more relevant parts are only scratched at the surface (see further down for detailed comment on this). Response: We will address each suggested revision suggested. -The authors should be more careful with the use of literature. For example, references indicated in the text are missing in the reference list (check for example Villacorte et al. 2009). Response: The missing reference has been added. - Overall the manuscript is clearly structured. The text is written in an honest way and the results are discussed critically. Response: We agree. \*\*\*\*\*Specific remarks\*\*\*\*\* - L48: Please specify the difference between sediments and POM. In my opinion the term sediments is not ideal for matter within the water column, especially not if it is contrasted to POM. The authors should consider to choose a

C2

different term. Response: We have eliminated the term sediment and revised the sentence to read: "... cycling of nutrients and particulate organic matter (POM) in general." - L55 'some' organic matter: Please be more precise. Response: "some" has been deleted. - Fig.1: Where is site D? The authors should be consistent and add similar labelling as for station A-C. In general, the labelling of sites does not seem optimal. My suggestion is to use different colors for the sites sampled during the present study in contrast to the stations incorporated from previous studies. Response: We have added the missing label for site D on figure 1 and made the previous study location a blue color as suggested. - Fig.2: This figure should rather go into the supplementary material, as it is only a technical validation/quality assessment rather than adding to the results of the study. Response: We do not agree that these parameters should be omitted from the text on the paper in that the pH and suspended sediment contribute to binding with TEP and can alter the rate of downward transport. All of the parameter should be shown for completeness. - L223 Overall remark on Section 2.6-Statistical methods: This section is poorly described. How did you deduce statistical significance from a scatter plot? Besides a p-value also the statistical method applied has to be indicated. Response: This section has been revised as suggested and a new, more advanced statistical analysis has been added. - L229 Overall remark on Section 3- Results: A substantial amount of text in the results is spent on the description of the physical/sensor data. However, these data do not contribute substantially to the following discussion. I would suggest to shorten the respective results section and to remove 'hitchhiking' parameters such as pH, dissolved oxygen and turbidity from Fig.2 and Fig.3. I guess these parameters have been recorded and published for the study region before. Otherwise the authors should put more effort into putting these parameters into context in their discussion. From the TS profiles it looks like different water masses could be present, which could have implications for organic matter cycling. In terms of statistics the study is scratching only at the surface. My suggestion is to include further statistical tests evaluating the difference between shallow and deeper water layers. One option would

C3

before example to pool the three stations A-C for a comparison of the different OM parameters at minimal depth against maximal depths. Response: The reviewer may be correct that there appears to be different water masses shown in the site D profile from 0 to 120 m versus 120 to 300 m. However, the location is not far from the coastline and no currents are known to exist in the region to create a second water mass.. - L232 biospherical licor: This aspect could be skipped from my perspective (see comment above). Response: We do not agree and we believe it should remain for completeness. - L244 'a slightly lower salinity gradient': What is the precision of the measurement? I guess the observed variation lies within the methodological range. Please correct me if I am wrong. Response: The precision of the instrument is based on the conductivity, temperature and pressure. The measurement error range for conductivity is  $\pm 0.0003$  S/m, for temperature is  $\pm 0.001$  °C, for pressure is  $\pm 0.015\%$  of full range. - L254 oxygen variability: Was the CTD device 'acclimatised' in the water for several minutes before starting to run the profile in order to avoid methodological biases? Response: The device was properly used based on the recommended standard practices. While on deck the instrument is always stored in water. - Fig.8: The figure headers are quite hard to read. The authors should consider to increase the font size. - Overall remark on Figures: The figure captions should be more elaborated in order to transport a message. Identical units should be used for all parameters within the same figures if applicable. For example in Fig.7 and Fig.8  $\mu\text{g/L}$  should be used also for TOC (instead of mg/L) to facilitate comparability across parameters. Response: We have increased the font sizes as recommended added and we changed the TOC values to  $\mu\text{g/L}$ . However, the PDF review copy is not to the resolution that would be published in the journal - L368 'highly effective': How do you define high effectiveness here? Response: The language was changed to read "...in this study was used in ... - L410: As you state that TEP is presumably a significant part of TOC, it would be valuable to also calculate the respective fractions and indicate them in the manuscript (maybe even as an additional figure). Response: Since the composition of TEP is known to vary in the extreme, this type

C4

of calculation cannot be done to any reasonable degree of accuracy. If the reviewer has some formula to do this calculation, please send it to us to assist us. - L414: Can methodological issues such as a bursted filter be excluded? Response: No, the laboratory analyses were performed very carefully and a failure of a filter would have been noticed. -L464: I would be careful here as this aspect was not measured within the study scope. The authors should replace 'which are food' with 'which can be food'. Response: The language change was made as requested. - L491 'unusual result': Nutrient concentrations would be interesting to check in this regard. The chlorophyll maximum seems to lay quite deep at station A-C. Response: We do not have data to check this. - LL503-504 offshore vs. nearshore: This statement in the conclusion can be misleading, as season may be the more influential factor than offshore versus nearshore. Response: We added a sentence "Seasonal differences during sampling could have also impacted the results" to address this issue. - L505: Which irregularities? Response: We reworded the sentence to respond to this comment as "Differences in local conditions, such as circulation and anthropogenic influences, in the nearshore zone can cause large variations in the organic parameters measured, not allowing statistically-significant relationships to be established. ".  
\*\*\*\*\*Technical corrections\*\*\*\*\* - L57 LANGUAGE: The term 'caused' seems not fitting here, probably better to be replaced with e.g. 'formed'. Response: Changed to "formed" - L116 TYPO: 'the characterize' C4 Response: The text was changed to read"... characterization of the natural...". - L156 TYPO: 'there different' Response: Corrected. It now reads "The different types of algae,..."

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2019-59/bg-2019-59-AC2-supplement.pdf>

---

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-59>, 2019.