

## ***Interactive comment on “Sediment release of dissolved organic matter to the oxygen minimum zone off Peru” by Alexandra N. Loginova et al.***

**Anonymous Referee #2**

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Review of “Sediment release of dissolved organic matter to the oxygen minimum zone off Peru” by A.N. Loginova et al.

This manuscript reports assessments of benthic dissolved organic carbon (DOC) and dissolved organic nitrogen (DON) fluxes and pore water profiles from six sites on a transect of stations off central Peru. The chemical characteristics of DOM pools are also explored using absorbance and fluorescence spectral analyses. The work follows a series of other papers (e.g., Dale et al 2015 and 2016; Sommer et al 2016) reporting on benthic studies completed on research cruises to the Peruvian continental margin in 2017.

Generally, the manuscript was poorly prepared for external review. The English wording of sentences is often awkward, and many sentences contain extraneous words or are

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missing key prepositions. Some of these problem sentences are listed below.

The paper presentation is also lacking depth and rigor. A more focused introduction and a much more informative description of the study area under section 2.1 are needed to set the stage for this work. The study area description should summarize the already published and spatially variable sediment carbon accumulation rates and benthic remineralization rates (e.g. DIC and nitrate fluxes) that are critical to the later discussion. This information could be incorporated into a more informative Figure 1. Meanwhile, Figure 2 is not needed and only repeats information given in the text about routine sampling and flux calculation methods.

With respect to the analytical work there are other concerns. There is no reporting of analytical blanks, precision or accuracy. I note the authors used cellulose acetate membrane syringe filters rather than combusted GF/F, so there could have been blank issues. The authors themselves raise the possibility that the DON results may be in error due to incomplete or unmatched estimates of total inorganic nitrogen species that must be subtracted from total dissolved nitrogen (TDN). Rather than speculate about this as they do near the bottom of page 9, have they any samples remaining to test for elevated NO<sub>3</sub><sup>-</sup> stemming from either ammonia oxidation or bacterial sources? Any measurements of N<sub>2</sub>O? Were the samples completely processed under N<sub>2</sub> to prevent oxidation artifacts? Can they report both TDN and inorganic N determinations (at least as supplemental material) so a reader can evaluate these together?

The presentation of flux determination approaches comes across as though the authors do not trust either the diffusive gradient approach or the results from in situ chambers (see for example the last two sentences on page 3). If it was my data set, I'd have greater confidence in the chamber-based fluxes, and I would view the fluxes calculated from the concentration difference across the sediment-water interface as “potential diffusive fluxes” that could result if there is no DOM source or sink at the sediment-water interface. Since most sites had mats of sulfide-oxidizing bacteria at the interface, microbial utilization as presented through Figure 9 seems likely and worthy of emphasis.

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Differential diffusion rates and/or utilization rates of DOM pools are indicated by the FDOM components (Figure 8). These results are interesting, and they deserve more positive discussion.

The presentation of DOC and DON distributions and fluxes was uninspired. For some reason the authors simply compare mean  $\pm$  sd of measurements, over whole profiles or incubations, across the stations. With all the available dissolved and solid phase biogeochemical data from these sites, they should look for relationships tied to organic matter degradation processes. For example, what do DIC or sulfate versus DOC, or ammonia versus DON property-property plots look like? There is much more that can be done to interpret these findings. The final speculative link to denitrification rates is completely unsupported.

Sentences with particularly awkward construction or in need of minor edits are found at:

Page 2: lines 12-15, line 34. Awkward.

Page 3 line 9, "or" not "of fulvic-like".

Page 3 line 13. Is the Uiam Lake study really relevant to a marine environment?

Page 3 line 18. Explain "insolation shield".

Page 3 line 22. Change to: "from pore-water gradients using. . ."

Page 3 line 27. Your point is unclear here. The uncertainty is in the sediment diffusion coefficient and whether DOM pools with different molecular weights are subject to different diffusion rates.

Page 4 lines 13-14. Unclear.

Page 9 lines 3-5. Awkward construction.

Page 9 line 35. Awkward.

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Page 10 line 9. Change to "imbalance in production and consumption".

Page 10 line 15. Change to "agrees well with previous observations".

Page 10 line 24. Omit "to" before geopolymerization.

Page 12 line 18. Spelling "spatial".

Figure 7 caption, you use "stars" not pentagons.

Table 1. Units for dissolved oxygen are missing "micro"  $\mu$ .

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