Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-390-RC2, 2020

Biogeosciences
Discussions

Interactive comment on “The control of hydrogen sulfide on benthic iron and cadmium fluxes in the oxygen minimum zone off Peru” *by* Anna Plass et al.

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Received and published: 3 February 2020

I generally agree with most of the authors conclusions, particularly with respect to the Cd data, but I do have a few issues concerning the calculation of diffusive Fe (and Cd) fluxes and the discussion of the Fe data. To adress these, I suggest the authors expand and adjust their discussion to deal with these aspects.

1) The input data required to verify the authors‘ flux calculations from concentration gradients between the uppermost pore water sample and the water column sample is absent from the manuscript’s figures and tables. That includes the porosity used in equation 3 and the tortuosity calculated therefrom in eq. 2. Nor are the input concentration values given. I cannot glean them from fig. 4, as the scale is too small and most pore water profiles appear to show an enormous spread in the uppermost two data points. What values were used for the calculation? Which one is the true surface in fig. 4? What is the reason for this spread in pore water concentrations so close to the surface - e.g. St1_M136 (Fe: ∼ 100 nM and >1 nM) and St4_M136 (Fe: low nM range? and > 6 μM). Comparing bottom water column data (fig. 3) does not suggest one of these plotted pore water values could actually be the bottom water concentration. I think it would be better, to calculate concentration gradients from more than just two data points for Fe and Cd. Perhaps the authors would also consider to show the 0-500 nM range in high resolution, and the few numbers above at lower resolution with a break in the axis between?

2) I also don’t like the way these gradient-based diffusive flux calculations (lines) are shown over the flux chamber time series data in fig. 5. Better use the water column data as a starting point - since chambers were flushed with ambient bottom water - than the first chamber time series value. There often is quite a difference between the ambient bottom water and the first chamber value that the authors do not explain. Sometimes that difference is hard to understand. In Sta 1 ambient water is higher than the starting value of the chamber (flushed with ambient water). At Sta 9, chamber values (∼ 80 nM) are much higher throughout than ambient water (∼5 nM), but apparently there is no diffusive flux of Fe. That discrepancy does not agree with the authors’ general statement in the abstract that the two flux estimates agree with each other.

3) Generally, I see an unaddressed problem in that the Fe concentration data for the near-bottom water column and the flux chambers were filtered at 0.2 μm and acidified for later analysis. That would dissolve all colloidal Fe, which I would expect to be quite abundant near the bottom (see e.g. Fitzsimmons et al. 2015, Mar Chem 173 pp 125 for a similar problem in ocean water underneath dust plumes, and Heller et al. 2017, GCA 211, pp 174 for a discussion of particulate - and colloidal - removal of dFe in the Peruvian OMZ). I am aware of the authors citing the latter manuscript, but they

do not cite it in the context of colloidal/particulate removal of dFe. I realize, that the necessary ultra-filtration to quantify colloidal Fe was not implemented, and of course, cannot be provided in hindsight. However, I would like to ask the authors to deal with this problem in the discussion. There is so much scatter in the Fe concentration time series from the flux chambers, that I am certain the dFe data suffer from colloids. Colloidal Fe would not count for flux calculations based on pore water - bottom water concentration gradients, and would very much complicate flux estimates based on the linear trend calculation from the flux chamber time series. My quick suggestion would be to provide two estimated for fluxes based on minimum and maximum estimates, but perhaps the authors can come up with a better way to deal with this issue. A discussion of this aspect would really benefit the manuscript. I suggest to discuss this aspect at the end of paragraph 4.1.1. In addition, bioirrigation should contribute considerably to resuspension of particles and colloids as well - a process which the authors briefly mention in lines 441-446 in general terms but do not seem to connect to their scattered flux chamber data.

4) In the abstract, lines 20-23, I would suggest the authors phrase the suggested “agreement” of different flux calculations somewhat more careful. This is not a very convincing statement even without considering the colloid problem. At best, there is agreement for some of the stations.

5) In the conclusion, I don’t think it is appropriate to claim that Fe just diffuses out of the sediment column. The authors themselves have made the case for biorrigation and bioturbation as a significant process for Fe enrichment in the flux chambers.

Technical aspects that should be corrected: a) Please show cruise AND station number in figures 1 and 2. b) I believe it would be useful to show the H2S data along with the Cd data in fig. 6, since they are discussed together.

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