

Interactive comment on “Artificially induced migration of redox layers in a coastal sediment from the Northern Adriatic” by E. Metzger et al.

Anonymous Referee #3

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Review of “Artificially induced migration of redox layers in a coastal sediment from the Northern Adriatic” by Metzger et al. bg-2013-328

This study reports geochemical evolution of sediment, and effect on benthic fauna, during prolonged anoxia using benthic chambers on the seafloor of the Northern Adriatic for 9, 30 and 315 days of incubation. The main results of the study were that decomposition of benthic macro-organisms on the seafloor generated important production of sulfides within the chamber, which generated a downward flux of sulfide towards the sediment where sulfides were quickly oxidized by metallic oxides or precipitated as FeS. Sulfide was found to be no longer detectable in the water column and pore water at the end of the experiment. The authors therefore claimed that sulfide enrichment in the water column in coastal systems is strongly controlled by the biomass of benthic

C6798

macrofauna and its decay during hypoxia, while its residence time in the water column is controlled by iron content (as solid oxides or as dissolved reduced cation) within the sediment. I have a number of problems with this paper. First, there have previously been several similar in situ experiments in other marine environments studying negative redox turnovers at the sediment-water interface (SWI), which the present authors appear not to have cited appropriately and may not even know about. These previous experiments include studies by Balzer (1982 in GCA), Balzer et al. (1983 in Oceanol. Acta) and Balzer (1984 in L&O) in the Eckernförde Bight, southern Baltic Sea; studies by Rolf Hallberg and coworkers, including Nils Holm and Anna-Greta Engvall, near the Askö Laboratory in the NW Baltic proper; studies by e.g. Anderson et al. (1986 in L&O), Rutgers van der Loeff et al. (1984 in L&O), Sundby et al. (1986 in GCA) and Skoog et al. (1996 in GCA) in the Gullmar Fjord, western Sweden; and studies in Chesapeake Bay, USA, by e.g. Boynton and coworkers. Many of these studies reported fluxes of sulfide from sediment to water column (measured in benthic chambers) as a result of a negative redox turnover, and the sulfide accumulated in the chambers. Some of them reported dying benthic fauna at the SWI due to the induced oxygen depletion, and it may be that some of these papers reported that the dying fauna contributed to the sulfide production. The present authors should thoroughly check these papers, and make it very clear how their study is different (if at all) from these previous studies of which some were carried out already in the 1970's, i.e. about 40 years ago, and in a substantially revised version of their manuscript (MS) clarify what new knowledge (if any) their study has generated in this regard. Second, if there was a downward flux of sulfide towards the sediment where sulfides were quickly oxidized by metallic oxides, then it needs to be shown that metallic oxides were present below a zone of sulfide production. I could not see that this was made clear in the MS. Thirdly, the authors claimed that sulfide enrichment in the water column in coastal systems is strongly controlled by the biomass of benthic macrofauna and its decay during hypoxia. Two of the most well known anoxic and sulfidic marine basins in the world are the Black Sea and the Baltic Sea, of which at least the latter is a coastal system. These basins contain

C6799

no benthic macrofauna below the oxycline, but still there is active sulfide production in these anoxic sediments, which significantly contribute to the sulfide enrichment in the water column of these basins. I can thus not find that the claim the present authors did is justified at all; at least it can not be generalized in the way the authors did. If this claim is to be trustworthy, the experiments should have been made in sediment with and without benthic fauna, and the sulfide production (and possible accumulation in the water of the chambers) be compared between zoic and azoic experiments. Fourthly, the authors claimed that the residence time of sulfide in the water column is controlled by iron content (as solid oxides or as dissolved reduced cation) within the sediment. I would like to see (in the revised version of the MS) a calculation or a budget in which the authors show how much sedimentary iron is needed stoichiometrically in their system to control the residence time of sulfide (with regard to oxidation or precipitation), and compare that with the iron content actually being present. There are a number of grammatical and/or linguistics errors in the MS (too many to list here). The MS should therefore be language corrected by a person with English as her/his mother tongue. My recommendation is that the MS undergoes a major revision and then is resubmitted for a new review.

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