

Interactive comment on “A system in balance? – Implications of deep vertical mixing for the nitrogen budget in the northern Red Sea, including the Gulf of Aqaba (Eilat)” by C. Häse et al.

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The manuscript reports observational findings of the February/March 1999 Meteor cruise on the nitrogen deficiency of the northern Red Sea-Gulf of Aqaba system. The manuscript relates the presence of significantly lower nitrate deficiency in the Gulf to regular ventilation of its deep waters during the winter seasons whereas strong stratification of the Red Sea prohibits such deep winter mixing. The authors then propose intense benthic denitrification as the major cause of nitrogen deficiency in the Red Sea. The manuscript deals with an interesting subject on how the internal physical-biogeochemical coupling might be different for inter-connected systems even though

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they are regulated by similar external forcing. Therefore it is worth publishing in “Biogeosciences”. I however have some comments which may help to improve clarity of the manuscript. 1) The role of benthic denitrification as the cause of nitrate deficiency is suggested only qualitatively. In order to support their arguments, the authors should provide some estimates of benthic production and make an assessment on the role of benthic denitrification for the deep nitrate deficiency. They can work out how much benthic denitrification is necessary to withdraw observed nitrate from the system and see if such amount of denitrification is supported by the observed benthic production. I also wonder about strength of benthic-pelagic coupling for such deep basins, as argued in the manuscript. The calculations which I suggested, even if they might be crude and only provide an order of magnitude assessment, might be helpful to demonstrate importance of benthic denitrification. 2) In fact, the authors’ suggestion on the possible role of benthic denitrification for nitrogen deficiency reflects the outcomes of biological processes on short time scales. On the other hand, as observed in sub-pycnocline water of the Black Sea, nitrate deficiency may arise even in the absence of benthic processes at millennial time scales as a gradual process due to building up of the water column stratification and oxygen deficiency. The authors should therefore discuss why similar processes Sea did not play a likely role on the nitrate deficiency in the Red Sea. 3) Another possibility for different subsurface nitrate structure of the Red Sea proper and the Gulf is their different rates of primary production and phytoplankton species structure. The comments on these issues will be helpful for the clarity of the manuscript. 4) I found their statement that “ the data strongly support the concept of phosphate as the ultimate limiting nutrient...” is a bit far-reaching conclusion from such a limited data set. They only report the observational findings of a single cruise, and they do not provide additional data to support their arguments. This conclusion generally applies for ocean ecosystems and stronger observational support is necessary to claim that it is also valid for semi-enclosed/marginal sea ecosystems. I hope they can come up with additional data to firmly demonstrate that the system has different nutrient controls on short and long time scales.

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