

Biogeosciences Discuss., referee comment RC3  
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## Comment on bg-2022-73

Anonymous Referee #3

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Referee comment on "Tracing differences in iron supply to the Mid-Atlantic Ridge valley between hydrothermal vent sites: implications for the addition of iron to the deep ocean" by Alastair J. M. Lough et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-73-RC3>, 2022

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The manuscript entitled "*The impact of hydrothermal vent geochemistry on the addition of iron to the deep ocean.*" by Lough et al., present an important issue regarding supply of iron into world oceans through hydrothermal sources. Authors have tried to present the impacts of vent geochemistry on such contribution. For that, authors have presented a few vent-specific case studies about the behaviours of hydrothermal iron in deep oceanic environments along the MAR. They have estimated the excess dissolved iron to dissolved helium-3 ratios and exchanges of iron between dissolved & particulate fractions around those vent fields and which would improve our understanding about dispersion/contribution of iron from active hydrothermal sources into deep oceanic waters. The observed variability of those parameters have also nicely correlated with the other geochemical factors of particular vent fluid. It's also nice to see that authors have mentioned critically about the possible error sources for estimation (and therefore misinterpretation) of ex-Fe/<sup>3</sup>He ratios in deep water columns. This concept would be useful for similar types of observation in future. According to me, in the present manuscript, except few technical issues, the overall presentation of the topic is fine.

The language used in many parts of the text are not very clear, and need some changes to make it more understandable to readers. The longer statements should split into simple shorter ones. Some of the other major lacking's are as follows:

The topic discussed in this manuscript is based on observations at the only four vents in norther MAR. However, the title of manuscript was made in very generalise way, as if the draft has information's of vents from global oceans. Author may think about this and may slightly modify the title accordingly.

In abstract there is mentioned "... *two methods of estimation....*" for checking variability in dFe/xs<sup>3</sup>He ratios. Is this really mean two separate methodologies?? Or indicates two types of hydrocasts which were made during sampling?? If these two different types of hydrocasts, author should mention the details of operation and significance of those casts

in the "sample collection" section (i.e., section 2.1).

In methodology, authors should mention about the techniques (with necessary references) used for helium isotope analyses in water samples at WOI. Those helium analyses details should appear in "Sample analysis" section (section 2.2.); instead of section 2.1.

Even the vent sites are very well known, still it is better to mention about the sampling station locations (lat, long); may be in Table 1.

Initially in the abstract and introduction authors are mentioned studies are carried out four vent field with different geochemistry. But latter it has found that the results and discussion (and figure) are restricted to the N-MORB hosted Rainbow and TAG fields only and the E-MORB hosted Lucky Strike and Menez Gwen fields are excluded- any specific reason?? In result and discussion section, the water column profiles of  $\delta\text{He}$ ,  $\delta\text{Fe}$ ,  $\delta\text{Mn}$  from the Rainbow are presented. What about the profiles of other three fields? For geochemical comparison those data of other fields are essential.

It looks the  $\delta\text{Fe}/\text{H}_2\text{S}$  ratios in the text and table might have an unit of nmol/mmol.

The first paragraph of conclusion (Ln: 447-455) which mostly mentioned about technical suggestion for deep water sampling doesn't looks very essential remarks to address about any "impacts of hydrothermal geochemistry on supply of iron" in deep ocean waters.