

Review of the paper
“Iron isotope fractionation in marine invertebrates in near shore environments”
By S. Emmanuel et al.

This short paper provides 18 $\delta^{56}\text{Fe}$ in chiton (marine molluscs living in the near shore coastal environment) that accumulate Fe biominerals in their radula's teeth. Different species have been analysed (including two from the same site) at 4 locations (2 in the Atlantic, 2 in the Pacific). The authors compare their values with published seawater data, even though they are far away from the chitons sampling time and location.

Data seem to be of good quality and the article is generally well written and illustrated. The authors acknowledge honestly several times that their study is preliminary. There exist indeed few data on $\delta^{56}\text{Fe}$ in marine environments so any new dataset is welcome. This is why I would recommend publication. I do however have a significant concern regarding the discussion when the authors try to interpret the differences in $\delta^{56}\text{Fe}$ measured in these samples. They present three speculative hypotheses in an imbalanced way: e.g. the feed regime is preferred to explain the differences in two species from the same site whereas this hypothesis is not better supported by data from this paper or from previous studies. In the main text, the three hypotheses should be equally discussed (as in Fig. 3). Organising the discussion into sub-sections would help. Abstract and conclusions should be modified accordingly. Some other points could be also clarified (e.g. seawater sites). My detailed comments are listed below.

Damien Cardinal

Title: add in brackets after “.... in marine invertebrate (chitons, mollusca)... ” to have more accurate information on the study which is very restricted to this type of invertebrate only.

Abstract and conclusions should present the three hypotheses to interpret the data.

P. 5537 line 25. Please provide a range of sample size: how much dry weight of radula and Fe have been processed for a single analysis?

P. 5538. Specify whether Apex has been used with or without membrane.

P. 5539 Line 4: typo: delete one ‘the’

P 5542, Line 8: typo: 2σ instead of $y\sigma$

P 5542 and 5543 “*Assuming that the isotopic difference between T. lineata and M. muscosa does indeed reflect their contrasting diets*” is indeed highly speculative, so going a step forward, i.e. finding an explanation on why red algae would have a different isotopic signature than green algae, goes very far since no algae data were measured. Similarly P. 5543 can the authors provide a reference to support this statement: “*relatively high Fe(II) concentrations in the eulittoral zone and low Fe(II) concentrations in the sublittoral zone.*” ?

Fig. 2. What are the distances to the chitons' sampling sites the locations of SW signatures d) and e)? Unclear also if 500km apply for a, b and c. See also my comment on table 1 where this information could be provided.

In Fig. 3 Here, the three potential processes yielding to Fe isotopic fractionation are presented in an equal way. The legend correctly underlines that those 3 mechanisms are not mutually exclusive. However, in the text, mechanism a (and in a less extent, b) is preferred and this is imbalanced. This is especially true in abstract and conclusion, where only hypothesis involving diet is provided.

Text on Fig. 3 is too small.

Table 1.

Provide exact locations: latitude and longitude of sampling sites.

Provide also locations and $\delta^{56}\text{Fe}$ of seawater data (a,b,c,d,e) that are compared with chiton isotopic signatures. Eventually four small maps of each site could be helpful.