

This manuscript presents new interesting silicon isotope data from sediment pore fluids ($d_{30}\text{Si}_{\text{pf}}$) in different marine environments from Guyamas Basin and Gulf of California. The data are of high quality and include $d_{30}\text{Si}_{\text{pf}}$ from sites located into Oxygen Minimum Zone or under the influence of hydrothermal vents both of which are particularly new in terms of Si isotopic system. This work certainly deserves publication in Biogeosciences after improvement / clarification of some parts of the discussion. I have compiled my concerns under the scope of the three main concerns below, which do not necessarily imply much work to be implemented.

1) The role of **bioturbation** is never mentioned in the main text (e.g. 76-77) despite it is an important process in the upper cm (cf. e.g. Rabouille et al. 1997 for Si). It is however a parameter of the model. However, the model is used only for the OMZ site where we could expect null or negligible bioturbation because it is very low oxygen environments. How could bioturbation not affect the porefluid profiles of the other sites? In contrast why this has been considered at the OMZ site? Some discussion should be added in the main text.

2) **Identification of minerals under or over saturated** in the pf is not sufficiently directly addressed in the discussion. See e.g.:

- 457-459. It is unclear how "*Si concentrations lower than amorphous Si and quartz solubilities, indicating precipitation of the respective mineral phase during pore fluid ascent*". If the DSi_{pf} is lower than equilibrium concentration, then Si should rather dissolve than precipitate.

- What are the minerals which are actually oversaturated in pf? Why saturation indexes of primary and secondary minerals have not been calculated e.g. using data of Table 1S?

- 480-481 and 499-512. Could dissolution of primary minerals (e.g. feldspars) supply DSi in pf? If not, justify why this can be ruled out. Even though primary minerals are less prone to dissolution than clays, they are likely to be undersaturated in the pf and may have some impact on DSi and $d_{30}\text{Si}_{\text{pf}}$.

- Why there is no mineral data on the OMZ core (Table 2S) despite this is the site for which the discussion is the most developed?

- Fig.3. What is the uncertainty we have on Si concentration for dissolved bSi to build the mixing line that has been taken at equilibrium (900 μM)? The equilibrium concentration is theoretical however why would the dissolution of bSiO_2 be at equilibrium and give Si_{pf} at 900 μM for the end-member chosen in Fig. 3?

3) **Model set up** from line 524 and in the Supplement. The sensitivity of the model to its main hypotheses is not sufficiently discussed in the main text and there is a lack of justification for some of its core parameters.

- The average value of clay used in the model is -2 pmil and reference to Frings et al. (2016) is given for this. However, average clays in Frings et al. is not at -2 pmil. I'm not sure it is actually calculated, but from the figure, it should be more between -1.5 and -1 pmil. This would be also consistent with the review of Sutton et al. (2018) in which the world average value of secondary minerals is at -1.08 pmil. Similarly, in Bayon et al. (2018) the average clay d30Si from river sediment fluctuate from -1.5 to -0.32 pmil depending on climatic regimes. How does it affect model outputs when using a more realistic d30Si of clay (i.e. -1.5 or -1 pmil) and/or propagate the uncertainty of this value? In any case, the use of -2 pmil as average clay value is not properly justified.

- Similarly uncertainty on the -2 pmil for the isotopic fractionation during precipitation of authigenic clay should be discussed and taken into account

- Role of bioturbation in the model for the OMZ (cf. first comment)

- Could the model be applied to the other sites, e.g. basin?

Minor comments

Throughout the ms, better use heavier / lighter than higher / lower when reference is made to isotopic composition.

176 : « *The bSiO₂ samples were stored in Milli-Q water* » Does it mean that once separated by Morley et al. (2004) method, the bSiO₂ samples were kept in water ? For how long? Dissolution could have occurred with some isotopic fractionation?

238. Typo, 2 times « and »

301 Is it worth keeping MUC-22-04 whose bottom SW has been contaminated ? (likely by surface SW)

366: the kinetics of reverse weathering is poorly known, especially in situ and is certainly not immediate. So do not to use such wording « *as soon as Si is released (...), it reprecipitates* ». Moreover not all Si reprecipitates, otherwise there won't be more DSi in pore fluids than in bottom water (indeed in their previous work, Elhert et al. 2016 have quantified that only 24% of dissolved bSi reprecipitates). This sentence needs to be corrected.

371-372 From the three references cited here, only Elhert et al. 2016 has estimated fractionation factors for reverse weathering, the other two refer to continental weathering (Georg et al., 2006 and Opfegehl et al., 2013). Remove them or specify it since this sentence is misleading.

395-398. Sentence unclear / grammatically incorrect

397. At least one reference should be cited for Si isotope fractionation during bSi dissolution (e.g. DeMareest et al., 2009).

440-441. This sentence is too affirmative given the level of discussion at this stage of the ms. It could be changed to e.g. "Thus, at Basin sites both K/Al ratios of sediments and the heavier $\delta^{30}\text{Si}$ are in agreement to recognise bSi dissolution followed by authigenic clay formation as significant processes taking place".

527: typo in isotope

Table S5 Typo « auf »

Fig. 5. It should be mentioned in the caption that Fig. 5b is from another study (Ehlert et al. 2016?)

Fig. 6. Need to define red and black dashed line in the caption without having to go through the text in the ms.