Review Schleinkofer et al – BG

Dear editor,

With pleasure I read the manuscript of Schleinkofer an co-workers on the parasitic foraminifer *Hyrrokkin*. I think it may be fit for publication in Biogeosciences, but only after the authors have answered to some of my concerns and redone some of the calculations they present. In short, other explanations for the differences in Sr/Ca and Mn/Ca should be discussed alongside the model's outcome. This is particularly necessary since the model outcome shows that for the hosts' $CaCO_3$ to have an influence on the Mn/Ca and Sr/Ca, the ions for foraminiferal calcification have to be derived from their hosts by >99%, which I think is unlikely. Despite these concerns, I am looking forward to see the revised version of this manuscript.

Sincerely,

Lennart de Nooijer

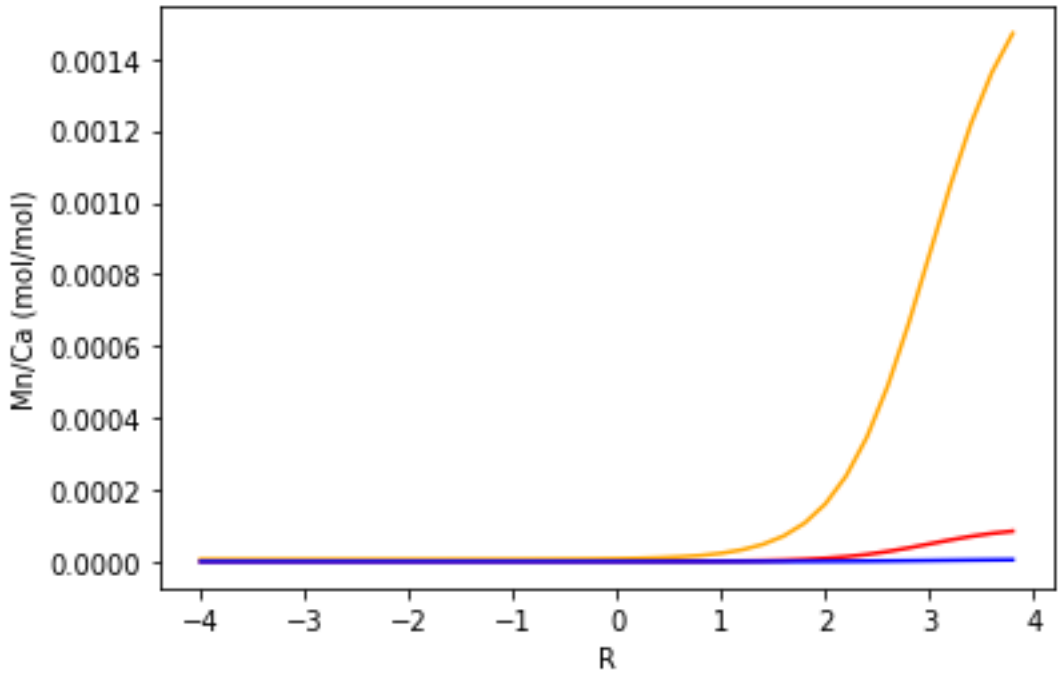
Major comments

The authors use their mixing model to explain the difference in Sr/Ca among the foraminifera, but I think the assumed values for partition coefficients may have to be adjusted. Below, I will outline why this is important. Also: unless I am mistaken, for the model outcome to be used as an explanation for the observed Sr/Ca, the ions for calcification have to be largely (>99%) derived from dissolution of the host, which is something the authors should stress and argue that this is likely. I find it unlikely that the foraminifera would need the Ca ions from dissolution of their hosts in order to calcify, given the relatively high seawater Ca-concentrations (at least high compared to the [CO_3^{2-}] or the [DIC]).

Mechanistically, the model is problematic since there is evidence for selective ion transport over membranes during calcification, so that the contribution of (unmodified) seawater as one end-member (at least for rotallid foraminifera) is unlikely true. I am not against involvement of seawater *per se*, but for many El/Ca, their ratios in seawater unlikely match those in the calcifying fluid. Secondly, I don't think the El/Ca of the hosts' shells'/ skeletons is directly entering the calcifying fluid of the foraminifer. Rather, the dissolution of the $CaCO_3$ will alter the [Ca^{2+}], [Mg^{2+}], etc. in the foraminiferal environment. Especially if the authors see seawater uptake as the main source of ions for calcification, the ions from the dissolution should modify the composition of the seawater surrounding the foraminifera before it affects the foraminiferal site of calcification. Such a less direct connection between the host-derived ions and the composition of the foraminifer's site of calcification will reduce the difference between the two end-members of the model and reduce the difference between the two groups of foraminifera.

On top of that I think the partition coefficient for Mn is not correct. The D_{Mn} for foraminifera (not from inorganic precipitation experiments; Mucci, 1988) is in the order of 0.5-1; see Van Dijk et al., 2020. Frontiers in Marine Science 8: 567701). This D_{Mn} is the one for an intermediate-Mg species (*Amphistegina*), which may well be not unlike that of *H. sarcophaga*. Using this D greatly reduces the difference in Mn/Ca of the foraminifers when assuming that they derive part of their calcifying fluid

directly from the host (see figure below). The discussion should consider this smaller difference in Mn/Ca.



Model output given eq 4. In orange is the original output (D_{Mn}=13) for the foram on the bivalve and in red (on the bivalve) and blue (on the coral) the output with a D_{Mn} of 0.75. Also note that the Mn/Ca hardly changes for the foram on the coral as a function of R (blue).

Similarly, the difference in Sr/Ca becomes (much) smaller if the D_{Sr} is chosen differently. The listed D_{Sr} from Raitzsch et al. (2010) is for *H. depressa*, which is a high-Mg calcite precipitating species. The same paper notes a D_{Sr} of ~0.16 for another species, which would reduce the difference in Sr/Ca by approximately 50%. For *Amphistegina* (see above and e.g. Van Dijk et al., 2017, Biogeosciences 14: 497), the D_{Sr} is somewhere inbetween.

The authors should be clear about the *reasons* for boring into the bivalve shell/ coral skeleton. I think there are two options, which the authors seem to mingle in their discussion (lines ~230-235). Either dissolving the $CaCO_3$ is somehow or profitable, or the resulting access to the hosts' insides is profitable. It could also be both, but that cannot be the case for the same reason. This means that they do not likely extract pre-concentrated fluid for their Ca-need and at the same time dissolve the $CaCO_3$ for their Ca-need. Given the specific tunnel made in the *Acesta*, it does not look like that dissolving the shell in itself is the main business of the foraminifera. Rather, the tunnel seems to be made to provide access to something that the foraminifer is after. This may suggest that the dissolved Ca, DIC and other ions may at best have an 'unintentional' effect on the foraminiferal shell chemistry. Despite the speculative nature of this discussion, I hope that the authors can spell out more clearly what may be and what may not be happening when dissolving and calcifying.

Related to this, I think the discussion would benefit from outlining the possible mechanism of dissolution by these foraminifera. Normally, I try not refrain from promoting my own work when reviewing, but in this case, the reduction of environmental pH by the foraminifer (Toyofuku et al., 2017) may provide the mechanism that foraminifera employ when dissolving calcium carbonate. There is similar work on the mechanism of bio-eroding sponges showing that pH regulation is the basis of bio-erosion by sponges (e.g. Webb et al., 2019; Scientific Reports 9: 758). Interestingly,

excavating sponges also can trigger calcification in their hosts: this similarity is maybe something to mention in the discussion as well.

Finally, what I miss in the manuscript is a discussion on the elemental composition of these species as such. Compared to other species, the Mg/Ca and Na/Ca are relatively high. If I am correct, all intermediate- or high-Mg/Ca Rotallid species are larger tropical foraminifera. That this is a non-photosythetic symbiont-bearing species, from another family than Sorites, Amphistegina, Heterostegina, Marginopra, etc. is interesting in itself.

Minor comments

Line 39/40: I am not sure parasitism as such is a 'feeding mechanism'. Without going into abstract discussions, parasitism is more a (symbiotic) relationship between organisms. Parasitism is not of the same 'level' as e.g. grazing. More accurate would be something like '…or their feeding mechanism is related to a parasitic lifestyle.'

Line 55: what does SRZ mean?

Line 64: given the balance between Ca and DIC in seawater, it may be more likely that dissolution of the *Lophelia* serves the need for DIC rather than Ca to calcify by the foraminifer.

Line 221: 'specimens'

Figure 2: could you swap the results from the aragonite and the calcite of he A.excavata in all panels? Then the order (calcite/ aragonite/ SRZ) matches the layers as they are deposited.

Line 234: add ')'

Line 256: please italicize 'L. pertusa', 'lower' should be 'more depleted'.

Figure 4: it is confusing that two samples have a similar color (blue), while they are unrelated. I can imagine that the samples that have something in common (e.g. the HL and the *Lophelia*) have a similar shade.

Line 337: I think the paper of Schleinkofer et al. (2021) does list the Sr/Ca of *Lophelia*, but not that of *Acesta*.

Line 345: the mixing model is not really a further investigation into the mechanisms, but rather an exploration of the consequences of the assumptions.