Reply to Reviewer 1 (Lennart de Nooijer)

We want to thank Lennart de Nooijer for his comments on our manuscript. His comments and suggestions helped us to improve our manuscript and eradicate mistakes that we overlooked.

Major comments:

Reviewer 1: 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.

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.

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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.

Answer: We agree with the reviewer about the problematic distribution coefficient for Mn. We have initially chosen a distribution coefficient from inorganic calcite due to the high range of distribution coefficients reported for benthic foraminifera (Barras et al., 2018; Groeneveld and Filipsson, 2013 and refernces therein). We have modified the figure to present the results using the distribution coefficients suggested by the reviewer as well as inorganic distribution coefficients (L 411). However, generally speaking the chosen distribution coefficient is not important for the point we are trying to make with this model. What we are trying to show with this model is why we see differences in the foraminifera grown on different hosts regarding Mn/Ca and Sr/Ca but not in the Mg/Ca and Na/Ca ratios, even though the hosts display different compositions.

Reviewer 1: 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 endmember (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.

Answer: Even if the foraminifera modified the seawater for calcification, which would probably mainly affect the magnesium concentration, this does not change the general outcome of the model and the point we are trying to make with it (See previous comment). Equally there is also evidence for seawater vacuolization without the need for a significant modification of the calcifying fluid (Evans et al., 2018; Bentov et al., 2009). The cited studies were conducted on *Amphistegina lessonii* and *Amphistegina lobifera*, two species that display a chemical composition that is similar to *H. sarcophaga*, at least with regards to Mg and Na (van Dijk et al., 2019; Geerken et al., 2018).

It is impossible with our results to judge if the mixture between host derived material and seawater happen inside or outside of the foraminifera. We think that the transport of dissolved material can happen similar to the transport of organic material to the calcification site, through vesicles and cytoplasm (Spero, 1988). We have added sections, about potential alterations of the vacuolized seawater and potential mixing outside of the foraminifera (L452-4368).

Reviewer 1: 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]).

Answer: We are very sorry, but the statement that ions have to largely derived from dissolution is based on a mistake in our manuscript. Based on the results we expect the foraminifera to gather between 100-1000 mg /l of dissolved carbonate from the host.

Reviewer 1: 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.

Answer: In *A. excavata*, tissue damage is reported in the mantle tissue close to the boring (Cedhagen, 1994). Therefore, we can be rather sure that the foraminifera feeds on the bivalve's soft tissue. However, we don't see why the foraminifera should not benefit from the extrapallial-calcifying fluid (ECF), which is also very nutritious (Yin et al., 2005) and additionally can provide Ca and CO_2 to aid calcification (Crenshaw, 1972). Feeding on extra pallial fluid of bivalves was also shown in *C. refulgens* with labeled tracer experiments(Alexander and Delaca, 1987). Amino acids in the mantle fluid are highly concentrated (2.5 mmol) and suffice to satisfy the foraminifera's max influx rate (Alexander and Delaca, 1987) The attachment etchings are considered to help the foraminifera to anchor on their host, but the host have no effective way of removing the parasites. *H. sarcophaga* is also reported to infest Leptochitonidae, where the etching activity is so intense that the foraminifera sometimes break the hosts shell and fall of as a consequence (Sigwart, 2009). This leads us to assume that there must be other reasons for the large etchings other than attachment. Experiments with labeled Ca would be great to show, what ends up in the foraminifera. We added a section that summarizes what is known about the boring and etching process including the processes used for etching, according to the reviewer's recommendation (L.351-373).

Reviewer 1: 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.

Answer: We want to thank the reviewer for this suggestion and suggested papers. We added a section about possible mechanisms utilized for boring by the foraminifera (L.367-373)

Reviewer 1: 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 nonphotosythetic symbiont-bearing species, from another family than Sorites, Amphistegina, Heterostegina, Marginopra, etc. is interesting in itself.

Answer: We added a short chapter, discussing the high Mg/Ca ratios we measured in H. *sarcophaga* in comparison to other foraminifera species(L.583-607)

Minor comments:

Reviewer 1: 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.'

Answer: The reviewer is completely right that parasitism is not a proper word to describe a feeding mechanism. We changed the wording to "parasitic feeding" (L.39)

Reviewer 1: Line 55: what does SRZ mean?

Answer: SRZ means shell repair zone (L.210), meaning the area where the bivalve repaired the foraminifera's boring by forming a calcified callus sealing. We added an explanation to L56.

Reviewer 1: 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.

Answer: That is also possible. We added this information (L.65)

Reviewer 1: Line 221: 'specimens

Answer: Corrected

Reviewer 1: Figure 2: could you swap the results from the aragonite and the calcite of the *A.excavata* in all panels? Then the order (calcite/ aragonite/ SRZ) matches the layers as they are deposited

Answer: Good suggestion. We changed the figure according to the reviewer's suggestion (L.220).

Reviewer 1: Line 234: add ')'

Answer: Corrected

Reviewer 1: Line 256: please italicize 'L. pertusa', 'lower' should be 'more depleted'

Answer: Corrected

Reviewer 1 : 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.

Answer: We changed the colors of the figure but we refrained from using similar colors for related samples because we think it makes the distinction between samples harder (L.274).

Reviewer 1: Line 337: I think the paper of Schleinkofer et al. (2021) does list the Sr/Ca of *Lophelia*, but not that of *Acesta*

Answer: This citation was a placeholder for an accepted manuscript that awaited data upload to Pangaea. It is fully available now and properly cited in the reference list.

Reviewer 1: Line 345: the mixing model is not really a further investigation into the mechanisms, but rather an exploration of the consequences of the assumptions.

Answer: We changed the wording to read: “In order to further investigate the observed results,…..” (L.393)

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We want to thank the Reviewer Inge van Dijk for her thorough review, that helped us greatly in improving our manuscript by providing useful remarks and comments.

Mayor Comments

Reviewer 2:For the EPMA analysis, I believe some more investigation is necessary to support the claims of the authors on the co-variation of certain elements (e.g. high Mg corresponds with high Na and S.) This can be done several ways, e.g. plotting the map counts of Mg versus other elements, or calculating line transects of the different elements (see for example my own protocol for foraminifera in van Dijk et al., 2019, https://doi.org/10.3389/feart.2019.00281).

Answer: We agree with the reviewer's statement, regarding the EPMA analysis, which was not adequately described and lacked information. We have added a further figure as proposed by the reviewer and added more information to the results section to better explain the results (L.255-270).

Reviewer 2: For the mixing model, I would like to see some changes in the model parameters, mainly for Mn and Sr. The chosen DMn from Mucci, 1988 is based on inorganic precipitation, and DSr from Raitzsch et al., 2010 is from a high-Mg species, which in general have higher D values. I would propose to use DSr derived from a mid-Mg species like Amphistegina, which is in the same range of Mg/Ca. This will likely greatly change the results of Fig. 6 and the current interpretation and discussion. The model is a bit to simplified in my opinion, and it ignores a lot of observations on foraminiferal calcification of direct uptake of the elements during calcification through trans membrane transport (Nehrke et al., 2013; Toyofuku et al., 2017). Even when following seawater endocytosis model of Erez, 2003, the seawater is not unmodified when it is available for the foraminifer, as the model assumes. For example, Mg ions are likely pumped out of the seawater, before it is available at the site of calcification as calcifying fluid. I understand that there are still a lot of unknowns in foraminiferal calcification processes and the authors cannot include all in their mixing model, but maybe they can add a step/fractionation from seawater to calcifying fluid (which can be based on e.g. the offset between distribution coefficients observed for inorganic calcite and foraminifera).

Answer: In addition to the distribution coefficients we initially used, we also added plots with the distribution coefficients suggested by the reviewer (Fig. 7). As the reviewer says, changing the distribution coefficients used in the model results in different absolute values that are calculated. However, it does not change the statement we want to make with the model, namely presenting a possible explanation why we do see changes in the Sr/Ca and Mn/Ca ratios of *H. sarcophaga* influenced by the host, but not in the Mg/Ca and Na/Ca ratios (see also our answer to Reviewer 1).

Considering the composition of the initial calcifying fluid, it might be appropriate to use an initial composition that is different from seawater with regards to Mg. The reviewer states that this could be based on an offset between inorganic and foraminifera distribution coefficients. Again, this would not change the main point of the model. We think that such a correction would make the model less "simplified" but not necessarily more realistic. As the reviewer says, there are a lot of unknowns in the calcification mechanism of foraminifera and we think that if we make the model less simplified, we are introducing more assumptions. Nevertheless, we added a section which states the possible modification of the seawater composition and how it influences the outcome of the model (L.452-461).

Specific Comments:

Reviewer 2: Figure 1: Was this specimen embedded in resin and cut/polished? Please give some details about the preparation/treatment of the sample.

Answer: Yes, the specimen was embedded in resin, cut and polished. We added the information to the text (L129)

Reviewer 2: L89: This is a bit unclear for me. What does HAO stand for, and why is HAW (with callus according to L88) included in HA, which are H. sarcophaga that infested A. excavata without callus formation.

Answer: HAO stands for *Hyrrokkin* on *Acesta* "ohne" (german for without) callus. HAW on the other hand for *Hyrrokin* on *Acesta* with callus. HA is used for samples that grew on Acesta but without a further distinction if a callus was formed -> HAW +HAO = HA (L89)

Reviewer 2: L91-93: For this study, you picked/selected only specimen that were firmly attached to the host?

Answer: Yes, every specimen was still attached to the host when we picked them for analysis. We added this information to the manuscript (L92).

Reviewer 2: L94-113: I prefer to put sample preparation with the paragraph of the method, e.g. L94-98 in 2.4. It makes the methods section much easier to read.

Answer: We agree with the reviewer and have included the sample preparation in the methods section.

Reviewer 2: L96: Which type of resin was used and was the embedding done in vacuum to avoid air bubbles etc.? Where the shells cleaned in anyway?

Answer: We used Araldite 2020 in a vacuum. The shells were ultrasonically cleaned in MilliQ-water prior to embedding. We added this information to the manuscript (L103).

Reviewer 2: L97: I am normally polishing until 0.2 um using a diamond emulsion. With 3 um, is the surface smooth enough? Was a previous method followed for the preparation?

Answer: The sample polish can have significant effects on EPMA measurements. Very rough ground samples (140 µm) can lead to intensity deviations of up to 20 %, which, however can be accounted for by choosing the right measurement parameters (Rönnhult et al., 1987). When using high acceleration voltages (15 keV) the effects of rough surface polish are already minimized (Merlet and Llovet, 2012). "Out of focus" effects are only evident when using grits above 20 µm and rough surface polish does also mostly affect L intensities, whereas K intensities are very tolerant towards surface roughness (Rönnhult et al., 1987). In our case, only Sr was measured using L intensities. As

our results considering Sr (and Na and Mg) are consistent with the results from ICP-OES we do not expect that the surface polish has significant influences on the results. Finer grid polishing can lead to problems as well, especially in porous biogenic samples, like diamond or corundum grains embedded in the sample.

Reviewer: L103 and again at 108: No kind of e.g. reductive and/or oxidative cleaning?

Answer: We cleaned the samples ultrasonically, but did not perform oxidative/reductive cleaning on the samples. We added this information to the manuscript (L. 112)

Reviewer: L105: Were the Eppendorfs acid cleaned?

Answer: Yes, every vial in contact with the samples was acid cleaned with 5% HNO_3 at 45°C (L. 131).

Reviewer 2: L125: type of microscope?

Answer: The microscope used was a KEYENCE VHX-S660E (L. 99)

Reviewer 2: L135-142: Samples were carbon coated? How many maps of how many specimens were taken? What kind of areas were selected?

Answer: Yes, the samples were carbon coated. We added this information to the manuscript (L 115) We only took maps of two separate specimen (which are both in the manuscript). The maps were taken in the area of the callus to get a better understanding about their composition.

Reviewer 2: L141: You preformed maps, lines or point measurements, and how many? With maps, how did you exclude resin / pores?

Answer: We made two maps of the callus area of two specimen of *A. excavata* that are included in the manuscript. We excluded resin/pores by excluding areas under a certain Ca cps level (7000 cps). In addition, we performed point measurements (n = 49) in certain points of interest (fluorescent, non-fluorescent layers, foraminifera) to provide quantitative measurements in addition to the semiquantitative maps. These measurements are the basis for Fig. 2. We added the number of samples to the figure caption.

We also did point measurements on the samples far qualitative measuring (Fig 2) in the different shell regions with n=11, 5, 17, 16 for calcite, aragonite, SRZ and *H. Sarcophaga*, respectively (L. 225)

Reviewer2: L149-157: Sample were diluted to solutions of e.g. 10 ppm Ca?

Answer: Yes, sample material was diluted to reach a concentration of 25 ppm Ca (L. 137).

Reviewer 2: L152 and 159/160: What was the accuracy of the standards?

Answer: The accuracy of the ICP-OES measurements, reported as %-deviation from standard reference materials MACS-3 and JCP (Jochum et al., 2005) was better than 1% for Mg/Ca and Sr/Ca, 3% for Mn/Ca and 5% for Na/Ca (L.138-142)

Accuracy of The Mn/Ca ICP-MS measurements is 7% (%-deviation from standard material ECRM 752 (Greaves et al., 2005)). We added the information to the manuscript (L140 & L 154)

Reviewer 2:L159: What was the measurement precision?

Answer: Precision of the Mn/Ca ICP-MS measurements, reported as relative standard deviation (RSD%) is better than 1%. We added this information to the manuscript (L156)

Reviewer 2:L187: What test was used to compare the point measurements by EPMA (Fig. 2)

Answer: We added a table with the results of a Wilcoxon-Mann-Whitney Test, to investigate if the different regions have significantly different means (Table 1, L. 227).

Reviewer 2: L190: Was a Bonferroni correction applied to the obtained p-values?

Answer: Yes, Bonferroni correction is applied during calculation of the p-values (L. 203).

Reviewer 2: L197: Supplement S1, not S2.

Answer: Corrected

Reviewer 2: L203: You mean especially in the foraminifer?

Answer: No, we mean foramen. The organic matter is mainly concentrated in the openings of the test. We adapted the wording to “test apertures” to avoid potential misreading (L205)

Reviewer 2:L205: Is there also a high magnification image available of the SRZ? Maybe this can be added as an insert in Figure 1, to show the thickness of the organic layers.

Answer: No, there is no higher magnified picture. We added the original .CR2 and .tiff pictures to the supplement that have a better resolution

Reviewer 2: L206-211: The authors describe in detail the shape and size of e.g. the bore canal and layering in SRZ. Did they also obtain images from other SRZ and canals on other specimens to check the variability?

Answer: We did also obtain picture from other specimens, where we observe no strong variability. We kept this section rather short, because the morphology of traces produced by H*. sarcophaga* on the host is already extensively covered in Beuck et al. (2008)

Reviewer 2:L223: How many measurements per sample? – could be added to the method section

Answer: Each sample was measured three times. The reported values are the means of these three measurements. We added this information to the method section (L131 & 162)

Reviewer 2:L225, L228, 235 and in general throughout 3.3. as well as 3.4.: Are these similarities or differences significant or not? These statements need to be supported by statistics (e.g. Kruskal-Wallis test with obtained (Bonferroni corrected) p values).

Answer: We added a table, displaying the results of a Wilcoxon-Mann-Whitney test to show if the differences are significant (L227)

Reviewer 2:L231: Na/Ca in both region E1&2 is exactly 14.8 mmol/mol? Or on average for both area?Please clarify, or better, give values for both regions and show they are not significantly different.

Answer: We want to thank the reviewer for that remark. The mean value of 14.8 mmol/mol for Na/Ca is only true for the microgranular calcite layer (E_2). We have not measured the fibrous shell layer by EPMA, because it is basically completely dissolved by the foraminifera. The fibrous shell layer is commonly slightly depleted in Na/Ca in comparison to the microgranular layer (Schleinkofer et al., 2021)

Reviewer 2: 3.3.: Could you add the standard deviation to the mean ratio in the text? For comparison with other studies this comes in handy.

Answer: We added standard deviations in the text.

Reviewer 2: 3.4.: There is a lot of information in the EPMA maps, but the authors use merely 2 sentences to describe the results. Please add more information, like what is the width of the high intensity layers, and do the correspond exactly to the fluorescence image (I presume Fig 1 and Fig3A are the same specimen?). What are min and max values of the different elements and values of the high and low intensity bands. Also: how were porous areas removed from the calculations, e.g. observed with Ca counts in Fig.3 panel A.

Answer: We agree with the reviewer that this section is too short. We have added information about the different composition of fluorescent and non-fluorescent layers, size of the layers and correlations between the elemental ratios. Generally, the fluorescent layers are enriched in Mg, Sr and S and are congruent with the layers revealed by fluorescence microscopy. Max Na concentration are also higher in the fluorescent layers than in the non-fluorescent layers. We observe significant correlations between Mg and S and Na and S in the fluorescent layers, indicating that these elements are incorporate into the bivalve's organic phase. We also added an additional figure to better present these results (L. 255- 270, Fig. 4).

Reviewer 2: L247: The covariation or colocation of elemental bands is difficult to distinguish by eye. Authors claim Na and S are enriched at the same location, but looking at Na and S of Fig. 3, I disagree For instance, Na and S of Panel A: the green band of S in the middle of the map seems to correspond to the blue band (low intensity) in the Na map. How did the authors check the covariation of elements? Merely by eye is not sufficient in my opinion.

Answer: We added a Figure (Fig. 4.) that shows the correlation between the measured elements as well as extending the results section for the EPMA maps. Covariation plots show a significant correlation between Mg and S and Na and S in the fluorescent layers. The reviewer is right with the statement that the described layers in Panel A apparently do not follow the described distribution. However, on close examination one can see that the described bands (green S band, blue Na band) are not exactly congruent. The blue Na band starts slightly above the green S band. These layers are the first layers of the callus, we expect this to be an effect of the disturbed calcification process.

Reviewer 2: L253: Again, how many measurements are included in fig 4?

Answer: The number of samples used is stated in the method section (L. 128). We added information about repeated measurements on the same samples (L. 131).

Reviewer 2:L261: Maybe add d18O and d13C values as a line to Fig. 4

Answer: We added lines that show the isotopic composition of the ambient water to the figure.

Reviewer 2: L324: how many specimens and host?

The amount of specimens is stated in the method section (L128 &149 & 159). We added more information about the number of host specimen (L143)(n= 3)

Reviewer 2: L329: Any idea if this is a chemical or a mechanical penetration of the host shell?

Answer: An extensive discussion of the traces produced by *H. sarcophaga* can be found in Beuck et al. (2008). These authors state that there is indication for chemical penetration indicated by the xenoglyphic surface texture of the boring and variability with the penetrated host microstructure (Beuck et al., 2008). We added this information to the manuscript

Reviewer 2: L369/Table 3. The authors use partitioning coefficient of inorganic precipitated calcite. Please use published values of foraminifera i.e. 1) for small benthic foraminifera see Barras et al., 2018, or 2) for Amphistegina lessonii (which has similar Mg/Ca as the studied species), which can be calculated from van Dijk et al.,2020 Frontiers in Marine Science 8 (DMn ~1). Also, it should be noted that the partitioning of Mn is not stable and increases with decreasing seawater Mn/Ca (e.g. Barras et al., 2018).

Answer: We agree with the reviewer about the problematic distribution coefficient for Mn. We have chosen a distribution coefficient from inorganic calcite due to the high range of distribution coefficients reported for benthic foraminifera (Barras et al., 2018; Groeneveld and Filipsson, 2013 and refernces therein). We added the distribution coefficients suggested by the reviewer to the figure to present an alternative. However, different distribution coefficients do not change the message of this model. Even with the adapted distribution coefficient the model predicts changes in the Mn/Ca ratio of *H. sarcophaga* influenced by the host organism.

Reviewer 2: L549-551: Is this confirmed by the EPMA measurements of this study?

Answer: Yes, it is, we observe a significant correlation (r^2= 0.34, *p*=0.014) between Mg/Ca and S/Ca in the callus region as well as a significant inverse correlation (r^2= 0.5, *p*=0.0018) between Mg/Ca and Ca wt%. We added a new figure to show this information (L. 267)

Reviewer 2: L560: As mentioned before, in e.g. Fig3A I could not clearly see this simultaneous increase in Mg with other elements. Consider making line transect over this area to see and compare the location of the high intensity bands of different elements. This is also necessary to understand if S is increased in the carbonate, or present in organics between carbonate layers (The authors did not perform EPMA on samples which were oxidative cleaned for comparison?)

Answer: We added a Figure (Fig. 4.) that shows the correlation between the measured elements as well as extending the results section for the EPMA maps. We observe a significant correlation between Mg/Ca and S/Ca ratios as well as significant correlation between Na/Ca and S/Ca indicating the presence of Na and Mg in the organic phase of the bivalve.

Reviewer 2: L551: Again, did the authors made this comparison by eye, or by e.g. plotting them overlapping?

Answer: We added an additional figure (L. 267) to show the stated correlations.

Reviewer 2: L617: How will the data be made available? Through doi / supplement?

Answer: The data will be included in the supplements.

Figures/Tables:

Reviewer 2: S1_Meigen Test. Please add a proper caption to this image, in the same style as the other figures.

Answer: We added a proper citation to the supplement image (S1)

Reviewer 2: Fig. 2 and 4: Maybe add the n above the boxplot. Explain that we are looking at boxplots, e.g. Boxplot distributions (line = median, boxes = interquartile range, whisker = min/max values). Are the measurements on different samples significantly different or not?

Answer: We added further descriptions about the boxplot in the figure caption. We also added a table with the results of a Wicoxon-Mann-Whitney test to show if the measurement results from different regions differ significantly from each other.

Reviewer 2: Fig. 3: Include more details in the caption. These are SEM images? What does A and B stand for (I presume different specimens)? Elements are in counts? What were the min and max counts per element (maybe give each of the their own quantified scale bar.

Answer: Yes, A & B are two different specimens of *A. excavata.* We added the counts to the caption of the figure.

Reviewer 2:Fig. 4&7: Please consider choosing another color for either HAW or L.pertusa.

Answer: We changed the colors to improve visibility.

Reviewer 2: Table 2/3. Tables and Figure should be readable without the main text. So please add the full names oF HAW, HAO, D etc in these captions.

Answer: We added descriptions of the abbreviations to the captions (L. 323 & 411).

Additional changes: We changed the species name of *Lophelia pertusa* to *Desmophylum pertusum*