Referee 1

The manuscript, "Tracking sulfate, magnesium, phosphorus and amorphous phases in Rosalina-like benthic foraminifera", by Paris et al., presents results of correlative TEM and NanoSIMS imaging to track the distribution of sulfur, phosphorus, calcium and magnesium in cultured foraminiferal cells. The foraminifera were cultured within ^{33}S-labeled seawater to track the incorporation of inorganic sulfate into the test walls.

The paper is generally well-written and the results are novel and exciting. Nevertheless, there are some serious issues with the interpretation of the results. While the interpretation of the sulfur distribution is generally fine, I have problems to agree with the interpretations about the phosphorus distribution. Many studies showed that inorganic phosphorus is completely discriminated in foraminiferal calcite, which is different to corals (see Science paper by Boyle, 2006). Most of the P in foraminiferal calcite is associated with the organic matrix and efficiently removed by bleaching of the tests and in my opinion the results of the study by Paris et al. show exactly this. In addition, there are certain other major points of revision that are in described in detail below. In my opinion, the paper is definitely suited for Biogeosciences, but it needs substantial major revisions before publication.

We thank referee 1 for their thorough and stimulating review. We provide here answers on their main points and criticisms. In red, the answer for which we have already modified the text (mostly the typos). In green, our answers with new elements that we will include in a new version of the draft should the editor wish us to do so.

The review led us to be more cautious about the description of P distribution within the cytoplasm and to be even more explicit about the limitations of the method we used, and thus of the observations we performed, which we hope will strengthen some of our conclusions and provide a more nuanced about the interpretations we provide within the cytoplasm.

Combined the comments of referee 2, the points raised by referee 1 led us to reevaluate the connection between P, S and phases distribution within the tests. We will provide new figures where we group the trends not by profile, but by trends, in order to calculate the correlation coefficients of each group to better explore the nature of the phase(s) possibly carrying the elements we analyzed.

Revisiting the data allows us to establish that trend B corresponds to the Primary Organic Sheet, and thus that the P, Mg and S enrichments correspond to the granules that we observe within the POS.

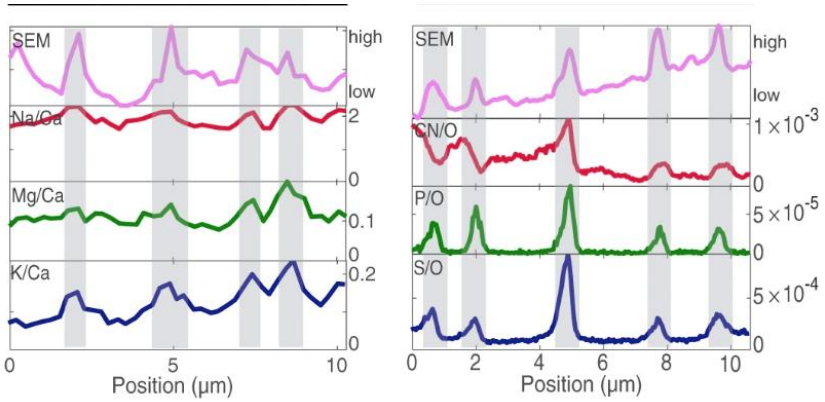
Below you can find my major points of revision:

1.: As mentioned above, the interpretation of the P distributions in the test walls are problematic and not really supported by the results of this study. Previous studies showed that P associated with foraminiferal calcite either is located in inorganic coatings (Boyle, 2006) or sits in the organic matrix of

the test walls (Geerken et al., 2019; Glock et al., 2019). Nevertheless, the authors interpret their results as evidence for P that is bound in the calcite lattice. This is questionable, especially by looking at the plots in figure 4. Figure 4b shows the plot of P vs. CN. CN tracks the distribution of nitrogen in the test and is basically a proxy for organic matter in the test. The plot shows that there are two or three slightly different phosphorus bearing phases in the test but in all these phases P is strongly correlated with organic matter. While this is similar to the correlation between S and CN (Fig. 4a), the intercept with the CN axis for all three different phases is zero or close to zero for the correlation between P and CN (Fig. 4b). This is not the case for the correlation between S and CN where the intercept indicates always significant S concentrations even when nitrogen is absent. In addition, the plot in Fig. 4g indicates, that S and P certainly are sitting in different phases, when normalized to CN. To really show that there is inorganic P within the calcite, I would have recommended to bleach the shells before the analyses to remove the organic matter in the walls. As it is now, I really think the authors have to do some major revisions to their interpretation here. Also, I have a bit of doubt about the interpretation of amorphous calcium phosphate (ACP) phases in the test walls.

The referee raises a very important point and we thank them for their comments.

First, we did not intend to say to all P sits in the mineral lattice. We said however that, in lack of raman data for instance, our data can be interpreted in different ways, and that some P in the mineral lattice and or as inorganic ion/ACP is possible.



In this figure from Geerken et al (2019), if S and P are strongly correlated, P is not so well correlated to CN. Thus, if CN is OM, and if S has a hybrid distribution between OM and calcite, so could P, and Mg.

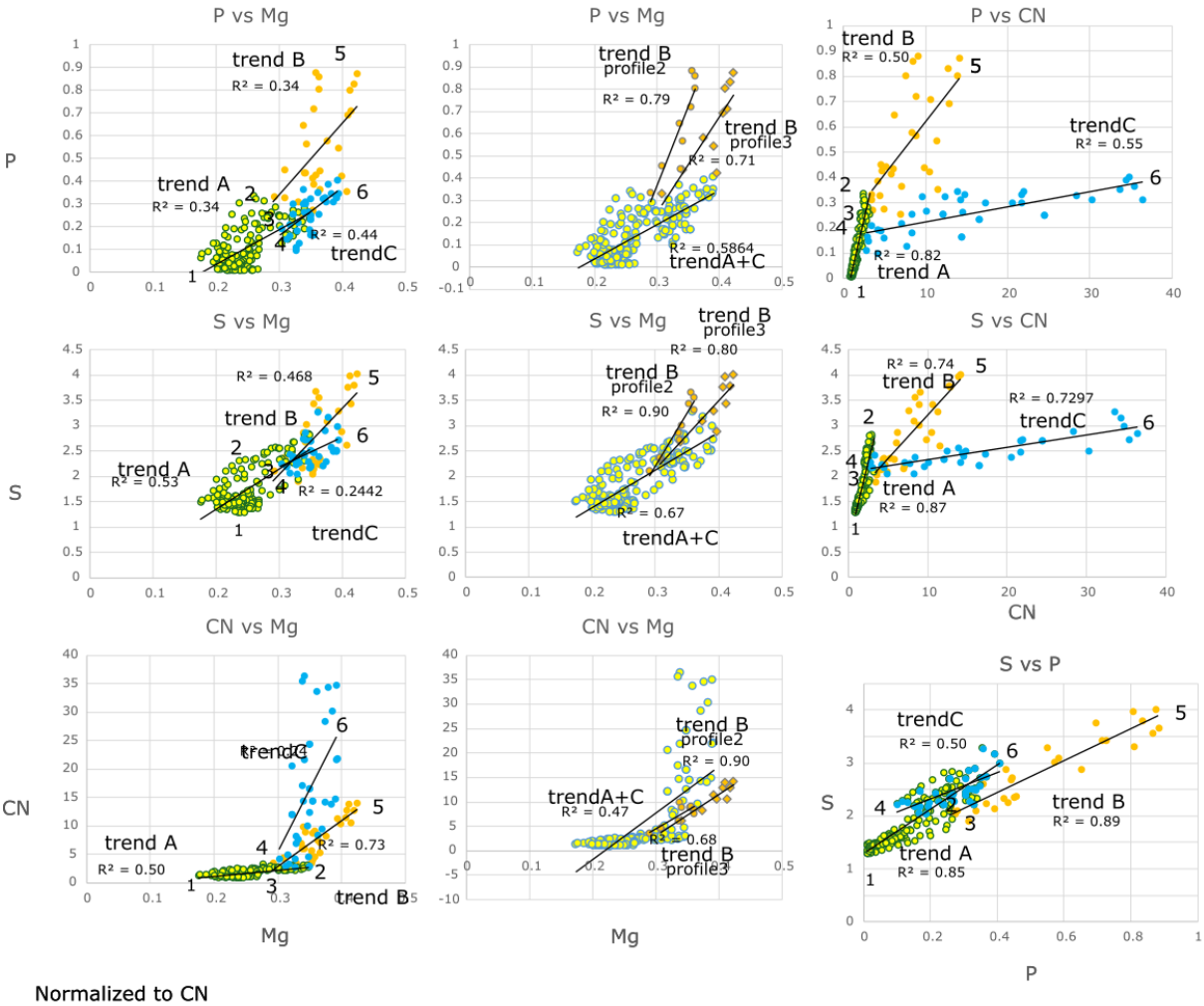
Furthermore, when compared to the actual positions of the OOLs, thanks to the SEM and STEM views, we note that no substantial N or P is seen, but instead that the element distribution is more complicated.

In any case, we went back to our data and produced new figures. We also now chose to plot figures grouped by trends, instead of profiles. This has the advantage of allowing us to calculate correlations for each trend, and thus to be more rigorous in discussing them.

The new figures confirm the existence of the trends across all profiles. Trend A correspond to both OCLn-1 and OCLn and is defined by endmembers 1 and 2. Trend B corresponds to the POS and is

defined by endmembers 3 and 5. Trend C corresponds to the edges of the tests and is defined by endmembers 4 and 6.

Normalized to Ca



Normalized to CN

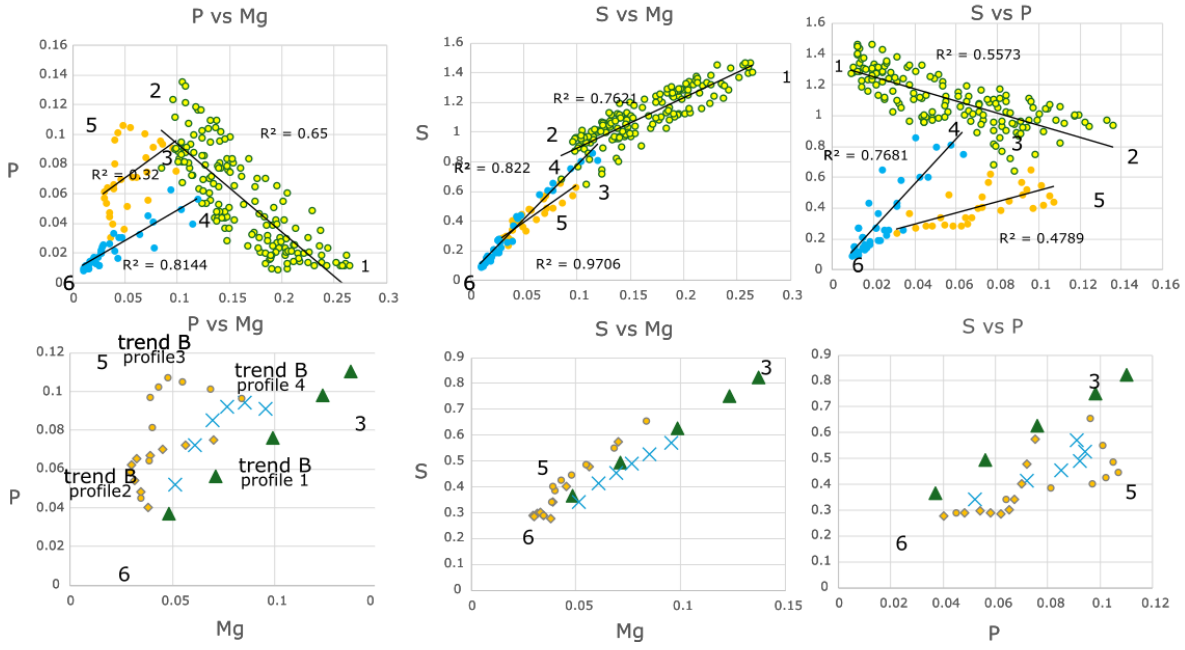


Figure 1 New (temporary) figure to be included in the next version of the manuscript In yellow : trend A, orange, trend B, blue, trend C. The location of each point of each trend is shown as profiles in the next figure.

We established a table of correlation coefficients (ratios normalized to Ca). It shows that the strongest covariation is between P and S, rather than between P and CN or S and CN, showing that these two elements share common patterns, that might not all be related to organic matter content. In green are the most significant correlation (>0.75), in orange are the moderately significant correlations ($0.6<R^2<0.75$) and in red are correlation factors $R^2<0.5$.

Ratios to Ca	P vs Mg	P vs CN	S vs Mg	S vs CN	CN vs. Mg	S vs P
trend A	0.34	0.82	0.53	0.87	0.50	0.85
trend B	0.34	0.50	0.47	0.74	0.73	0.89
trend B prof. 1	0.91	0.07	0.90	0.83	0.21	0.38
trend B prof. 2	0.71	0.74	0.80	0.90	0.9	0.93
trend B prof. 3	0.79	0.30	0.90	0.57	0.68	0.91
trend B prof. 4	0.90	0.55	0.60	0.86	0.75	0.95
trend C	0.44	0.56	0.23	0.73	0.24	0.50
trend A+C	0.58	0.44	0.67	0.43	0.50	0.85

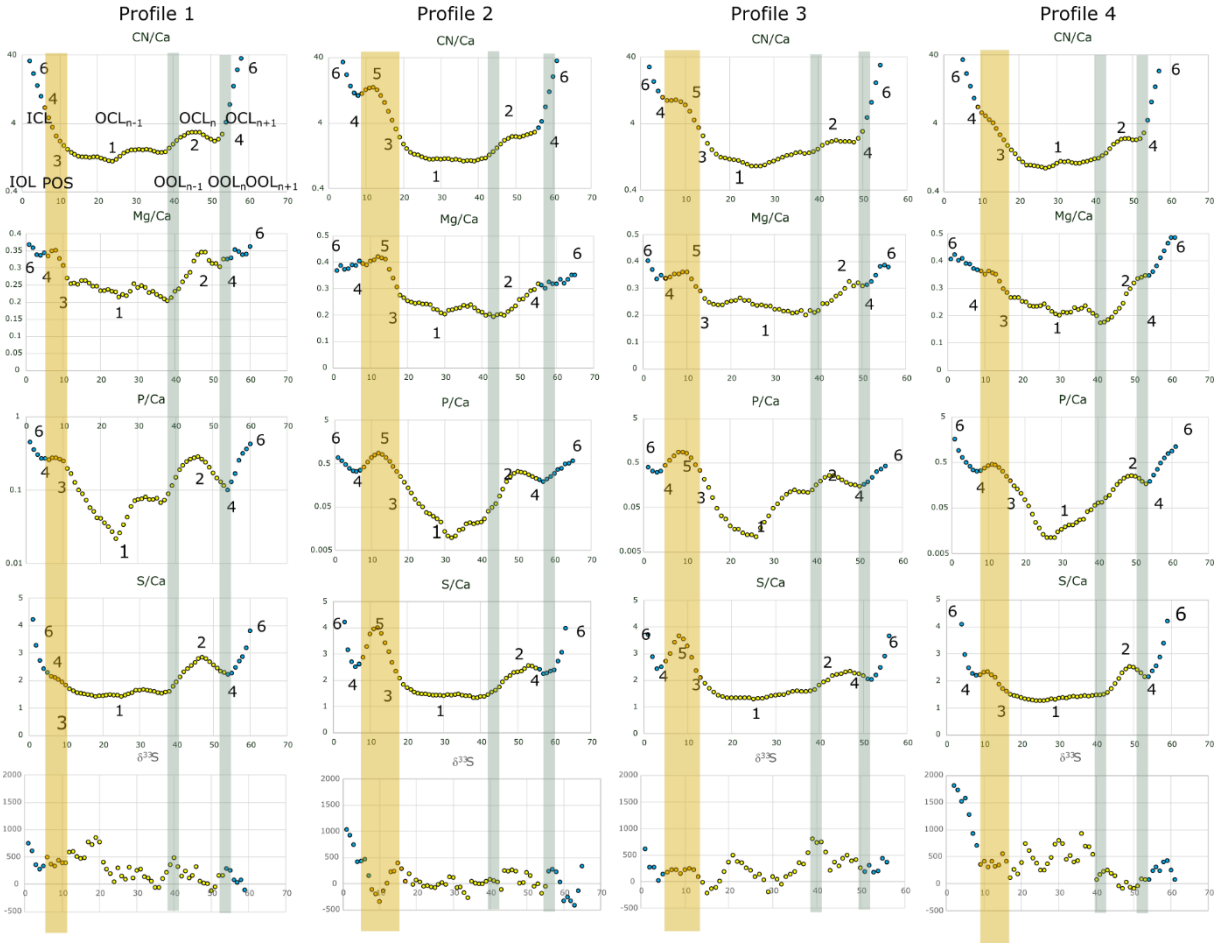


Figure 2 to be included in the next version of the manuscript. Points are color coded along the profiles according to each trend they belong to. In yellow : trend A, orange, trend B, blue, trend C. Numbers

indicates the extremities of each trend and are the same as in the previous figure. The orange vertical bar for profile 1 corresponds to the POS, and the gray bars to the OOLs (see fig. 5). For the other profiles, the orange bars is placed based on trend B, and the gray bars represent the transition from endmember 1 to endmember 2 (most likely OOLn-1 by analogy to profile 1) and endmember 4 (most likely OOLn), while trend C represents the transition from OOLn to OOLn+1, including OCLn+1 and the transition from the POS to IOL, including the ICL.

As pointed by the referee, S and Mg never reach the "zero" level. We note that P and CN don't reach zero either, even though we must point that N dynamic contaminations are common and requires extra care to avoid. Except in trend A, P/Ca ratios show little to no correlation with CN/Ca, which suggest that P and CN are independently enriched. Furthermore, in trend A, the variations of CN/Ca and P/Ca are not very important, only a factor of 3 or 4.

In trend A, the best correlations are between S/Ca and CN/Ca or P/Ca and CN/Ca. Yet, when normalized to CN, they are also moderately correlated, but negatively, while Mg/CN and S/CN positively covary. In detail however, trend A corresponds to two areas : OCLn-1 and OCLn. OCLn has varying Mg/Ca with mostly uniform S and P and OCLn has a higher Mg/Ca, higher S/Ca and higher P/Ca. Thus endmember 1 corresponds to OCLn-1 and is an endmember with a non-unique Mg/Ca ratio. Within endmember 1, P/Ca and CN/Ca may be stable or vary up to factor of 3 or 4, with phases of stability. Both calcitic layers appear distinct, but the mechanisms of enrichments might be different, with more organic matter in the OCLn area, as supported by the STEM and TEM views. In such a scenario, the enrichment in S is indeed consistent with more S- and P- containing organic matter. Though we agree this is not a shared view, trend A could nonetheless also be consistent with a change of inorganic S, N and P content in the mineral lattice.

Trend B is more complicated, especially when normalized to CN. However, trends exist, especially when separated by profiles, though they are not simply explained by a correlation. When normalized to CN, the data clearly reveal that endmember 5 is significantly more enriched in P than Mg or S. As a result, we interpret trend B to possibly include three phases: calcite (endmember 3), (organic matter endmember 5) and another phase, either a specific P-enriched OM, or a P-enriched inorganic phase, possibly ACP. Because of this additional enrichment in P, endmembers 3 and 4 are identical except for the P/Ca ratio

In trend C, CN/Ca appear as more correlated with S than P/Ca variations. This could be related to a P-poor and S-rich organic matter. However, when trend A and C are grouped together, we note that Mg and S are moderately correlated through both trends, as well as P and Mg, more than they are to CN, which could be also indicative of a control that is not connected to organic matter only.

In conclusion, we agree with referee 1 that part of the S and P variations are due to changes in organic matter content and/or with type of organic matter, at least in trend C and possibly within endmember 2 of trend A. We also confirm that some of the variation is likely independent from organic matter, especially in trend B and could be due to a P, S and Mg enrichment in non-crystalline phases, with P being the most enriched, as, for example, ACP. However, the complicated shapes of the cross plots also contain the possibility that calcites from OCLn and OCLn-1 are not characterized

by the same Mg, S and P/Ca ratios and also more Organic Matter and amorphous visible in OCLn-1 in TEM/STEM views

We will modify the text, images, abstract and introduction accordingly.

Then, the authors found some P-hotspots within the cells, which are partly associated with Ca and Mg. These have been interpreted as acidocalcisomes that have recently been found in foraminifera. In this context, those results could potentially be exciting. Nevertheless, there are some issues with the methodology, since classical drying for the preparation of thin sections for TEM, that the authors used, usually removes soluble content and dissolved ions from the cells (Ayache et al., 2010). This is also stated by the authors in line 174, although there is a reference missing there.

The reviewer is right, the sentence "This process removes all free ions and small molecules present in the cell." needs a reference. We will add the reference provided (Ayache et al., 2010) to the text.

That is also the reason why traditional fixation is not recommended, if acidocalcisomes are supposed to be visualized (Goodenough et al., 2019). Goodenough et al. (2019) recommend cryo-fixation to preserve acidocalcisomes and dissolved content. The only study that found evidence for acidocalcisomes in foraminifera so far also used cryo-fixation (Glock et al., 2025).

In addition, the authors used a phosphate buffer solution solution for the cell fixation (line 171), which potentially could create artificial P hotspots in the foraminifera. In line 372 it is written: "More phosphorus and sulfur are observed in the organic matter located close to the test compared to the center of the cell." Could this maybe be an indication that the P originates from the buffer but did not intrude deeper into the cell? Nevertheless, there could be a slight possibility that the P hotspots in the cell are no artifacts, since they have a similar size and shape as the recently found P-bearing structures in foraminifera. This would have been incredibly lucky since acidocalcisomes in foraminifera have not been found yet by using classical TEM-fixation methods. Therefore, I would recommend to tone down the interpretation of the P hotspots in the cell and recommend to add a discussion of the possible artifacts, related to the preparation method of the thin sections.

We understand the reviewer's point and agree that cryoscopy would have removed the ambiguities of our interpretations . We were not initially looking for acidocalcisomes specifically. Instead, we tried to interpret the P hotspots in agreement with the literature, hence the hypothesis that those hotspots are acidocalcisomes, based on their size and content. The cleaning should have removed the dissolved phosphate introduced during the protocol (Ayache et al., 2010), and its distribution, either in the cytoplasm or the test, is not random.

Because none of our interpretations are new as such (acidocalcisomes and other P-bearing organelles) and are based on previous work, we will add additional care to modulate our interpretation so that the concerns raised by the referee appear clear to the readers of the article. The interpretation of P-rich grains as acidocalcisomes or any other type of structure is not critical for the current manuscript and we will tune it down to reflect the uncertainties related to the method

we used, be more explicit about possible contaminations from the method, correct the figures, abstract and conclusion accordingly.

The next point is not that severe but should maybe be considered by the authors: I was a bit puzzled by the use of the term “Rosalina-like foraminifera”. Either the species that the authors cultured is a Rosalina or not. I can imagine, where this is coming from: Most likely a foraminiferal species that has been optically identified as a Rosalina species has been cultured and later genetic analyses revealed that it is actually not a Rosalina. In this case, I would recommend to give this species (and genus) a name, rather then to use the term “Rosalina-like foraminifera”, which is objectively not correct and misleading.

We thank the reviewer for this important point, that we had discussed in Thaler et al. (2020) and should have further discussed here. The For1C1 strain (MNHN-CEU-2016-0075) has been designated '*Rosalina*-like' due to its morphological resemblance to the *Rosalina* genus.

The polyphyly of the ribosomal DNA sequences attributed to *Rosalina* and the uncertainties surrounding the morphological identification of the related specimens (Schweizer, 2008, 2009; Holzmann, 2017) make them unusable to confirm a taxonomic assignation.

Nevertheless, we agree with the referee 1 that 'Rosalina-like' may be removed, as the For1C1 strain can be assigned to the *Rosalina* genus based on its morphological characteristics according to the current knowledge. We propose to make this correction in the text, leaving the species-level identification open and using the designation *Rosalina* sp., as also suggested by the referee 2.

Here below the proposition of the text, that we will include in the revised version of the manuscript, in section 2.1 (or likely as new supplementary information a description of the main morphological characteristics that allow the For1C1 strain to be assigned to the *Rosalina* genus. We also explain why, at present, we prefer to leave the species-level identification open and use the designation *Rosalina* sp.

To be included :

The For1C1 strain can be assigned to the Rosalina d'Orbigny, 1826 genus because it exhibits the typical morphological characteristics described by Loeblich and Tappan (1987) for this genus. The test is concavo-convex in shape with a trochospiral pattern and is rather compressed. The spiral side is gently convex and the umbilical side is slightly concave. All the rapidly enlarging chambers are clearly visible on the convex spiral side, particularly under an optical microscope, the sutures are depressed and oblique, curving back at the periphery. On the umbilical side, the chambers appear subtriangular due to the presence of an acuminate extremity named 'flap' or 'folium', which extends over the umbilical region. The sutures are slightly curved. The interior of the chambers is simple and undivided. The periphery is subacute with no keel present. The aperture is a low interior marginal arch bordered by a lip and located near the periphery on the umbilical side. It is separated from a small secondary opening by the umbilical folium. The test is hyaline calcitic with a smooth, perforated surface.

However, there are certain differences compared to the type species of the genus, Rosalina globularis Orbigny, 1826. These differences can be attributed to the morphological variability within this genus. These include a more evolute spiral coiling in the final whorl, which results in less overlap of the chambers on the umbilical side, and an ovoid rather than circular outline. Furthermore, significant morphological variation is observed in adult forms with more than 12 chambers, where the final chambers tend to become more elongated, arranged annularly and/or irregularly. In a previous article (Thaler et al., 2023), we highlighted significant morphological variability within the For1C1 strain and identified two distinct morphotypes. Those that reproduce asexually every 12–15 days once their test has developed 11–12 chambers resemble the Rosalina genus morphologically. In contrast, those that survive for several weeks and add more than 12 chambers exhibit a more irregular arrangement of the chambers in the final whorl. Both morphotypes of the For1C1 strain are attached to the substratum. High morphological variability is typical of attached forms, and this characteristic appears to be even more pronounced in larger specimens with more than 12 chambers. Reproducing individuals of the For1C1 strain are likely to be diploid macrospheric schizonts that have entered an apogamic cycle of successive asexual reproduction (Thaler et al. 2023). During this cycle, the new generation of schizonts is produced by schizogony (i.e. the multiple fission of a multinucleate parental cytoplasm). Therefore, as already discussed in Thaler et al. 2023, it is difficult to identify them morphologically at the species level because the morphology of the diploid microspheric agamont and/or the haploid macrospheric gamont parent generation, on which species descriptions are often based, is, to the best of our knowledge, unknown. Indeed, the morphology of the macrospheric diploid schizont, macrospheric haploid gamont, and microspheric diploid agamont generations can differ considerably. For this reason, at present, we prefer to leave the species-level identification open and use the designation Rosalina sp.

Moderate points of revision:

The introduction appears to be a bit long and detailed. It almost reads like a review. I have the feeling that the introduction can be shortened quite a bit and/or some parts could be moved to appropriate sections in the discussion. This is not completely obligatory, since Biogeosciences does not have a length limit but keep in mind that the length could deter some readers to read the text in detail.

Thank you for this suggestion. We will try and shorten the introduction for the sake of clarity and simplicity and remove superfluous details.

Minor points of revision:

Line 62: "CO_2" instead of "CO2". Done, thank you for pointing it out.

Line 70: Add "a" before sulfate reduction pathway. Corrected, thank you.

Line 113: See Boyle (2006) regarding phosphorus discrimination in foraminiferal calcite in contrast to incorporation of P in aragonite of corals.

We thank the reviewer for pointing out this article we had chosen not to include, as its target is about fossil foraminifera and thus its conclusions about biomineralization are not direct. However, Boyle (2006) states that « Foraminifera engulf vacuoles of seawater into their interiors and then chemically modify these vacuoles as they are transported to the site of calcification (8). In contrast, coral anemone polyps transport Ca2+ and H+ ions and gaseous CO2 across their cell membrane into an

extracellular reservoir at the base of the polyp, where the fluid is drawn from external seawater (9). As a result of this difference in the mode of calcification, the chemical compositions of corals are quite different from those of foraminifera, with coral elemental concentration ratios values close to seawater values for many elements such as Sr/Ca and U/Ca, whereas foraminifera—including an aragonitic species— strongly discriminate against these elements. ». We do not fully agree with this statement. It was shown in many studies that elemental ratios in corals are not "close to seawater", if this means that the elemental coral ratios should be close to elemental ratios in inorganic aragonite precipitated from seawater. For example, Sr/Ca ratios in corals are also strongly affected by vital effects (see for example Corrège, 2006, Case et al., 2010, Gagnon et al. 2007). Moreover, many more recent studies have shown that Centers of calcifications, or Rapid Accretions Fronts, have extremely variable ratios of those elements, depleted relative to expected from inorganic experiments, and thus that corals do actually significantly control the elemental composition of their aragonite and discriminate against some elements. However, we agree with Boyle – and with the referee's point – that foraminifera do also control the chemical composition of the precipitating fluid, from which they actively exclude Mg, as well as other elements, possibly including phosphate, sulfate, or nitrate. Yet, sulfate and Mg, despite being calcite inhibitors, are found in foraminiferal calcite, which in itself shows that the removal is not complete. As a result, traces of inorganic phosphate or nitrate could also be found in foraminifera, just as in corals, though to a lower level. Furthermore, as pointed by Boyle (2006), "LA-ICPMS does not determine the chemical form of the phosphorus, so we are not certain whether the phosphorus is bound in the inorganic lattice or in organic phases or ferromanganese coatings. The authors argue against the importance of the latter phases, but others will demand direct proof eventually" and thus, does not exclude the possibility of inorganic phosphorus.

We agree that assuming all P to be inorganic would be too strong, but we stand by the interpretation that the presence of some inorganic P in the calcite lattice is possible and coherent with the non-zero level of phosphorus in the lowest concentration levels. In addition, the enrichment of P is not only connected to CN, opening the possibility of an additional phase carrying P in the test.

Line 171: If the authors used phosphate buffer for cell fixation: Couldn´t P distribution in the cell be partly artificial? P in acidocalcisomes is usually lost during cell fixation, based on dehydration that removes free ions in the cell and cryo-fixation is the preferred method (see longer comment above).

The referee raises a very important point, which is critical for our interpretation. We agree that cryo-fixation would be a more relevant method and that the method we used removed some information. As a result, following the points made by the referee in their main comment, we will tune down our interpretation regarding P distribution in the cell, as explained in reply to the referee main point.

Line 220: This transparency regarding the problem of god standard materials is really appreciated. This is often a problem. I think it is indeed very unlikely that the strong fractionation of the S incorporated into the test is caused by artifacts from machine drifts. Thank you for your comment.

Line 244-245: "The laterally heterogeneous lightness or darkness of the grey in the outer calcitic layers is intriguing." In my opinion this sentence is a bit redundant and intriguing is a bit to pictorial for a scientific paper. We agree that the sentence is superfluous and we removed it. We would argue however that being intrigued is a good incentive for scientific research but this debate is probably beyond the scope of research on foraminifera.

Line 262: “chemically lighter” sounds a bit strange. Do the authors mean that the material is “composed out of lighter elements” or that it is “isotopically lighter”? To avoid ambiguity with the phrasing, we change it to “composed out of lighter elements” as suggested by the referee.

Line 264: “show” instead of “shows” (plural). Corrected. Thank you.

Line 271: Again “chemically lighter”. See above. Just “lighter” would be also enough, I guess. We hesitated because lighter seemed more ambiguous. However, with the previous change suggested by the referee, “lighter” is probably clear enough here. Thank you for the suggestion.

Line 302-303: I would write “to the strongest P, S and Mg enrichments in relation to CN” instead of “to the strongest P, S and Mg enrichments compared to CN”. We changed the sentence as suggested. Thank you.

Line 331: I don´t see that those distributions are very similar in fig. 5. Actually at the edges the distributions are very different. P gets really high at the edges and Mg not. S is in between. Also the minima and maxima in the inner parts are sometimes more or sometimes less pronounced in the different elements. I would suggest to show a correlation plot and test if the correlation is significant. This could very well be the case but it is very hard to see in fig. 5.

We thank the reviewer for their comment, as this is an important point. Correlations of four profiles across the test shown in fig. 5 are provided in fig. 4. We realized that the relation to figure 4 was missing from figure 5 caption, so we added it. We also now refer to figure 4 in the sentence pointed out by the referee. Correlations will be provided in the text following the table presented above.

Line 351 and 352: Replace “foraminifera” with “foraminifer” in both cases, since it is used for singular here and not for plural. We corrected it, thank you.

Line 396: The cited study doesn´t really show “Ca-phosphate” but most likely polyphosphate granules and gels that are associated with Ca in addition to dissolved pyrophosphate. Thank you for pointing this out. We have corrected the sentence to be more accurate.

Line 443: Maybe use “primarily” instead of “first”? We modified the sentence, thank you for the suggestion.

Line 444: Replace “foraminifera” with “foraminifer”, since it is used for singular here and not for plural. We corrected it, thank you.

Line 460: “but also to…”. To what? I have the feeling a word is missing in this sentence. The referee is absolutely correct, the word “control” was indeed missing. Thank you.

Section 4.1.1: There is a difference between precipitation of calcite and ion exchange between the lattice of already formed calcite in the surrounding medium. I have the feeling that both processes are a bit mixed up in this section. We will reword this section in order to avoid ambiguity.

Sections 4.1.1-4.1.3: I don´t really understand, why these three previously proposed biomineralization mechanisms have to be discussed and reviewed in such a detail in this section. They are partly not really related to the authors results. It felt necessary to us to explain the different ways

ions could be brought to the tests, because we wanted to be conservative. We also believe that because we bring the possibility of active control of sulfate, we have to explain the main pathways in order to explore how this control could come into play in each of them. The reminder here is also necessary, because it shows that vacuolization, and passive transportation, both have the opportunity to deliver S, Mg, P and N as seawater ions to the site of calcification. However, we will try to reduce this section to the minimum.

Line 510: I think this sentence could be formulated a bit better.

Line 512 and following: Could the mixing of the labeled with the unlabeled P be related to mixture of unlabeled organic S with the labeled inorganic S that is incorporated into the test walls? Would the percentage fit to the plot in fig. 4a? For example in phase 1 it looks like up to 30-50% of the sulfur would sit in the organics. This would roughly fit to your 1/3 of non-labeled S.

We thank the reviewer for this reflection that will be added to the discussion

Line 514: There is a word missing before "than". More than? Less than? We wrote "than" instead of "that". This is now corrected, thank you for spotting it.

Line 528-530: What would that mean? This section is a bit confusing. We will read and correct the section.

Line 567: The use of "on the other hand" needs also "on the one hand". We modified to sentence and used "Compared to trend A" instead.

Line 581: "Locally dark local"? Please reformulate. We changed the sentence to "locally dark areas"

Line 581-591: See my long comment above about the association of P to organic matter in foraminiferal tests and the discrimination of inorganic P in the calcite lattice. I really do not understand why the authors interpret their results as an evidence for inorganic phosphorus in the test. In my opinion all their results show the opposite and former literature is completely ignored. Especially the older works of Boyle (2006) and the studies that are cited within… The older works of Boyle specifically address the question of cleaning on corals, though it also discusses the occurrence of P in foraminifera, though mostly to point the discrimination against P that is stronger in foraminifera than in corals. We certainly agree that foraminifera discriminate against many elements more than corals do. We hope that our longer response to the referee main comment, as well as the detailed correlations, make our interpretation stronger.

Line 606: Why co-distribution? P and S are not even clearly correlated when they are normalized to CN (the organics). They are only co-correlated, if they are not normalized to CN, because both elements are at least partly associated to organic matter (P more and S to a lesser content).

This sentence will be reworded following the discussion presented to the first point of referee 1. We stand by the observation that there are common trends,

Line 608: Phospho-aminoacids are not really polyphosphates. Polyphosphates are chained phosphate molecules, which could be completely inorganic or have an organic rest such in ATP.

Thank you for pointing this out. We must correct the text to be accurate.

Line 616: I cannot agree with this hypothesis and don´t see that the data are justifying this hypothesis. We don't see any reason to exclude the idea there is at least traces of inorganic phosphate ions in calcite. If it can be included in inorganic calcite, and if there are phosphate ions in seawater, it is possible to find it in foraminiferal calcite, as we presented in our previous comments. We will however tune this point down, as discussed previously.

Line 619: See above: The patterns are not similar, if normalized to CN. Thus, P and S are only co-correlated in the organic content.

As discussed previously to answer the main comments of the referee, our observations support a more complicated pattern. This text will be modified nonetheless.

Line 689: "plays" instead of "play" at the end of the line (singular).

Corrected, thank you

Line 698: How can you design an observation? The sentence has been corrected to "designed the experiments and established the sequence of observations to perform" for better understanding.

References from that review that are not cited in the paper:

Ayache, J., Beaunier, L., Boumendil, J., Ehret, G. & Laub, D. in Sample Preparation Handbook for Transmission Electron Microscopy: Methodology (eds Ayache, J. et al.) 125–170 (Springer, 2010).

Edward A. Boyle: A Direct Proxy for Oceanic Phosphorus? Science 312, 1758-1759 (2006). DOI:10.1126/science.1129723

Corrège, T. (2006). Sea surface temperature and salinity reconstruction from coral geochemical tracers. *Palaeogeography, Palaeoclimatology, Palaeoecology*, *232*(2-4), 408-428.