5 July 2026

Dear Editor,

We would like to sincerely thank you and the reviewers, Prof. Lennart de Nooijer and Prof. Hellen Thomas, for their careful evaluation of our revised manuscript and for their thoughtful and constructive comments throughout the review process. We are grateful that the reviewers recognized the substantial improvements made to the manuscript. We have carefully considered all remaining comments and suggestions and have revised the manuscript accordingly. Below, we provide a detailed, point-by-point response to each comment, and all corresponding changes have been incorporated into the revised manuscript.

Methods

It still remains unclear to me how the raw ICP-MS data was translated to ratios. The mentioned standard (Mix 16-8) is unknown to me. There is no reference included for this standard (and I couldn't find it by searching the internet). The supplementary table does show the deviation from published values, so I am curious as where to find the actual data for Mix 16-8. This is vital to know that the concentrations for all elements published here can be used in future comparisons, including some more 'exotic' elements like V and Fe.

We revised the text accordingly (section 2.2). In the revised text, we clarified that the two in-house drift solutions we used, one a homogenized deep sea sediment digestion ("Mix 16-8"), and the second, a digestion of *Amphistegina lobifera* tests. Both solutions have been previously used to monitor precision and accuracy (e.g., Yehoshafat et al., 2026; Hoober et al., 2022; Benaltabet et al., 2021; Lapid & Torfstein, 2025),and have been cross-calibrated against international standards with known values, e.g., BCR2. Thus, the results are well constrained and Table S1a provides precision and accuracy estimates. We further used this opportunity to hone this part of the text with a few additional technical corrections (see revised manuscript).

Values for Zn seem highly unreliable (table S1a), but still are discussed in the manuscript (e.g. figure 3).

We thank the reviewer for identifying this error. We indeed reported in the original manuscript Zn values that were offset by an order of magnitude. This has now been corrected.

In addition: the text (lines 120-124) is not very clear on this point: calibration against a reference material is usually done with an internal standard (e.g. 43Ca as its concentration in foraminiferal shells is known). Please clarify the text to avoid confusion.

The revised text now clarifies what internal standards were used for monitoring sensitivity drift (Sc, Re, Rh). We emphasize that Ca is not used as an internal standard per se, but rather, that we normalize all the elemental results to Ca in order to increase precision and accuracy, as well as report results that correspond to accepted practices in comparable studies. The text is now clearer on both points.

Discussion

Lines 220-223: it works differently. If Ca2+ is removed from the calcifying fluid by precipitation, the concentration of other elements in the calcifying fluid increases (relative to calcium). But if only Ca participates in calcification, the K (or D) for all TE's will be very low,

irrespective of their (increasing) concentrations in the calcifying fluid. If somehow TE concentrations -relative to Ca- increase over time, a constant K will make the TE/Ca of the calcite increase over time (which is essentially Rayleigh fractionation). The way it is written now, is unclear.

We have revised the text as the reviewer notes, progressive removal of Ca during calcification increases TE/Ca ratios in the residual calcifying fluid, but the resulting TE/Ca of the calcite also depends on the partition coefficient of each element. If partition coefficients remain constant, increasing TE/Ca in the residual fluid will lead to progressively higher TE/Ca in subsequently precipitated calcite, consistent with Rayleigh-type fractionation.

Lines 224-230: A more frequent renewal of the fluid at the site of calcification with seawater may likely increase TE/Ca (it actually is a bit more complicated and depends on the ion fluxes and precipitation rate), but it unlikely lowers TE incorporation. Also, kinetics are a way to explain (coupled) TE/Ca, which should be mentioned here.

We agree that the relationship between calcifying-fluid renewal and trace-element incorporation is more complex than implied by our original wording. The effect of fluid renewal depends on the balance between seawater-derived ion fluxes, precipitation rates, and element-specific partitioning behavior, and therefore does not necessarily lead to lower TE/Ca ratios. We have revised the text accordingly. We also agree that precipitation kinetics may contribute to coupled variations in multiple elemental ratios and have incorporated this possibility into the discussion.

Lines 231-235: Mg is indeed more variable between taxa than, say, Sr. Which made colleagues hypothesize that there may be specific Mg ion regulators (e.g. Bentov and Erez, 2006). The claim here that even in miliolids (and high-Mg/Ca rotaliids) Mg is suppressed, is however not necessarily true. Inorganic partition coefficients (although hard to compare directly) can have similar Mg/Ca (~200 mmol/mol). Species with high Mg/Ca may therefore lack the suppression of Mg2+ uptake (or its removal) that is present in planktic and low-Mg/Ca benthic species.

While Mg incorporation is clearly suppressed in many low-Mg/Ca foraminifera, particularly planktic species and some benthic rotaliids, this interpretation may not apply universally to miliolids and high-Mg/Ca rotaliids. As the reviewer notes, inorganic calcite may have Mg/Ca values comparable to those observed in some high-Mg foraminifera. We have therefore revised the text to avoid implying Mg suppression in all foraminifera and instead emphasize that differences among taxa may reflect varying degrees of Mg regulation, exclusion, or removal from the calcifying fluid.

Referee Report, revised version, Ellen Thomas

Overall, the authors took the comments by both reviewers seriously, and worked on improving the manuscript along suggested lines. I indeed think that the manuscript has improved considerably, and could be accepted with relatively minor adjustments. As I read it, my main comments on this version is that the authors in several instances sound a bit more positive than justified by the data (need to insert words such as 'possibly' or 'potentially' or even 'probably'. Along similar lines, the authors (see e.g. comment on line 35, below) use sentences containing statements such as 'Rayleigh-type enrichment, Mg exclusion, and element-specific transport processes' – without making it clear whether they want to invoke one of these processes, or all of them, or are just uncertain and it could be either or several of them combined. I suggest that they make clear whether they have an opinion whether one of these is more probably involved, or whether they leave this fully open.

A minor irritant for me (though many authors do this): the use of 'El/Ca ratio': El/Ca IS a ratio, thus one should use 'El/Ca', or 'El/Ca value', but not 'El/Ca ratio'.
Done

In my earlier review I commented on a lack of environmental data; these are now included (to some extent). However, the authors in my opinion do not sufficiently address the question of composition of equilibrium calcite under such conditions. How far off are the biogenic calcites investigated here?

A direct comparison with inorganic equilibrium calcite is challenging for most trace elements investigated here because both seawater concentrations and partition coefficients are insufficiently constrained for the study site. However, Mg/Ca provides a useful first-order reference.

Assuming Mediterranean seawater Mg/Ca ≈ 5.4 mol/mol (Lebrato et al., 2020) and published inorganic calcite Mg partition coefficients D_{Mg} ≈ 0.017-0.02 at temperatures relevant to the eastern Mediterranean (Mucci, 1987), equilibrium inorganic calcite would be expected to contain approximately 92-108 mmol/mol Mg/Ca. In comparison, the rotaliid species investigated here average ~64 mmol/mol, whereas the miliolid species average ~138 mmol/mol. Thus, rotaliids are depleted in Mg relative to inorganic equilibrium calcite, whereas miliolids are enriched relative to this inorganic reference. Importantly, both groups were collected from the same habitat and sampling period, yet they fall on opposite sides of the inorganic equilibrium estimate, indicating strong biological control on Mg incorporation.

We emphasize that the goal of this study was not to determine equilibrium calcite compositions, but to compare co-occurring taxa living under similar seawater conditions. Consequently, the observed differences in El/Ca ratios primarily reflect biological controls on elemental incorporation. While comparisons with inorganic equilibrium calcite may provide valuable additional constraints, especially in future culture or experimental studies, they are not required to establish the relative differences in elemental incorporation documented among the taxa examined here.

I like the compilation of published (supplement), but still miss seeing a reference to Blackmon and Todd 1959 (J of Paleontology 33, 1-15), in my opinion a groundbreaking discussion of Mg/Ca including in miliolids.

We have now included this reference in the published Mg/Ca compilation presented in the supplementary (figure S2).

34: “rare earth elements (REEs) with order of magnitude differences (up to 45 times), and moderate but systematic **differences** for other elements (e.g., Zn, Cd, Fe).” Suggest to insert ‘differences’ before ‘for other elements’.
Done, see line 34.

35: “This dichotomy likely reflects fundamental differences in biomineralization pathways between the two orders, **potentially** including differences in calcifying-fluid regulation, Rayleigh-type enrichment, Mg exclusion, and element-specific transport processes.” I do not think we know that all the elements of ‘calcifying fluid regulation’ listed are involved in the specific differences in El/Ca, so maybe add ‘potentially’ before ‘including’?
Done, see line 36.

43: “The direct coupling between ambient seawater chemistry, temperature and the biomineralization pathways that lead**s** to the precipitation”: ‘lead**s**’, not ‘lead’ (subject is singular)
Done, see line 43.

54: “with miliolids and rotaliids ”**presently**” representing the most widespread and ecologically dominant calcifying orders characterized by multichambered tests (de Nooijer et al., 2023; Pawlowski et al., 2013).” Suggest to insert ‘presently’, since this has differed over geological time (or ‘since the Middle Cretaceous’)
Done, see line 52.

154-155: I do not like this sentence ‘More moderate miliolid enrichments, up to fourfold, are observed for Cd, Mg, Cu, and As.’, because for Mg/Ca there is a small group of values in rotaliids which is as high as that in miliolids. It thus is not always true that miliolids show moderate enrichment relative to rotaliids (although it is true for many cases). It seems to me from Figure 2 that his puts Mg/Ca in a category by itself.

In the summary presented in Figure 2, Mg/Ca was included together with Cd, Cu, and As solely to provide a broad generalization of the overall geochemical pattern distinguishing miliolids and rotaliids. We agree that Mg/Ca differs from Cd, Cu and As because only a small number of high-Mg rotaliid species exhibit Mg/Ca values comparable to those of miliolids. As the reviewer correctly notes, Mg/Ca represents a special case, and this distinction is discussed separately in the dedicated Mg/Ca section of the Discussion, where we explicitly acknowledge the occurrence of high-Mg rotaliids and the limitations of this generalization.

Caption fig. 4: “**Distribution** of **El/Ca between different taxa**”. Suggest to insert ‘of’ after ‘Distribution’.
Done, see line 194.

254: “probably evolved under different oceanic conditions (Loeblich and Tappan, 1987), **possibly** leading to calcification mechanisms adapted to distinct seawater chemistries”. This is a very positive statement, but I personally do not think that there is truly convincing evidence that seawater composition at the time of origin of a taxon fully defines its calcification mechanisms, and the data in this paper could be used to argue against that hypothesis – and

you indeed argue so (e.g., section 4.2; and the sentence in line 357: 'This pattern suggests that rotaliid calcification should not be treated as a single uniform mechanism', and line 372: 'Although Mg/Ca has been proposed to reflect evolutionary adaptation to changing seawater composition (de Nooijer et al., 2023; and your discussion of *Amphistegina-Pararotalia* – lines 372-375), our data suggest that species-specific biological regulation exerts a dominant control') After all, there are considerable differences between rotaliids – do you want to argue that the origin of a species or a genus (or whatever other taxonomic level) reflects sea water composition at time of origin? Maybe insert 'possibly' before 'leading to' to indicate some uncertainty as to this point?
Done, see line 218.

335: "For example, the relatively low Mg/Ca in *Lachlanella* sp. compared to other miliolids has previously been interpreted as a record of colder winter temperatures (Titelboim et al., 2017)." I do not follow this argument – are you arguing here that the different miliolids dominantly calcify at different times of the year? Please make that clear.

We would like to clarify that the interpretation is based on the observations of Titelboim et al. (2017), who compared *Lachlanella* sp. only with the rotaliid species Pararotalia calcariformata. Thus, while their results suggest that Lachlanella continues to calcify during the colder winter months whereas P. calcariformata enters a period of quiescence, it is not known whether the same pattern applies to other miliolids such as Sorites or Peneroplis. We have added this reservation to the revised manuscript.

343: "Published data from both rotaliids and some miliolids show a broad range of Mn/Ca values, typically spanning from <1 to ~300 mmol/mol, with *Lachlanella* sp. representing a notable high-end outlier (van Dijk et al., 2020) and in the current study (Fig. 4)." Something missing in the underlined part of the sentence: do you mean '**also** in the present study'?
Done, see line 303.

351: 'Thus, *Lachlanella* sp. illustrates how the general miliolid enrichment pattern can be further amplified or modified by microhabitat conditions, particularly for redox-sensitive elements." To me, this reads like a plausible hypothesis, for which some evidence is presented, but it is in my opinion not definite that indeed microhabitat defines El/Ca; different explanations are still possible.

While the available evidence is consistent with microhabitat conditions influencing elemental incorporation in *Lachlanella* sp., alternative explanations cannot be excluded. We have therefore revised the text to present this as a hypothesis supported by the current observations, rather than as a definitive conclusion.