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Interactive comment on "Evaluation of isotopes and elements in planktonic foraminifera from the Mediterranean Sea as recorders of seawater oxygen isotopes and salinity" *by* Linda K. Dämmer et al.

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The authors present a comprehensive regional dataset of trace element and stable oxygen isotope data measured on foraminifera collected from plankton samples with rich contextual physical and chemical data. The analyses were carried out to test to what degree the strong salinity (or seawater oxygen isotope) gradient in the Mediterranean could have been reconstructed from shell chemistry. The results are sobering, which I believe is not to be taken negatively but as an extremely important result, confirming the growing body of evidence that there is something we fundamentally do not

understand about the way the proxy signal in the sediment is generated. In this way, the manuscript makes an important fresh contribution to the field and the data and analyses in my opinion warrant publication in Climate of the Past.

That said, I would advise the authors to place less emphasis on the aspect of lateral advections, for reasons explained below, and to provide a more explicit quantitative evaluation of the magnitude and direction of the various candidate processes invoked to explain the large scatter. Beyond the individual comments listed below, I would like the authors to explain if/how they dealt with the carbonate ion effect on all of the proxies (oxygen isotopes and Mg/Ca in particular), as this is not really clear from the text and I would like to draw to their attention the possibility that the analyses of the G. ruber from the plankton in the chosen size fraction could have been affected by differential contribution of specimens representing pre-adult G. elongatus, which may follow a different calibration line. Perhaps the results already contain some hints (bimodality or not of the single-shell measurements, for example)?

Finally, I would like to urge the authors to make sure that the data that will be make available on the Utrecht data server are as comprehensive as possible and that they are stored in a way that they will be found in any future attempts to synthesize seawater or foraminifera chemistry data.

Taken together, these points and the individual points below all aim to make the most out of the nice dataset that the authors have, which I believe they will be able to do without having to substantially restructure the paper or change its conclusions.

Comments to individuals points:

Title: Instead of "isotopes and elements", I would recommend to be either more specific (oxygen isotopes and trace elements) or less specific (shell geochemistry), or else the title appears to promise more than what is delivered.

Line 30: large scale

Line 54: continues to be

Line 66: please specify what exactly "has been shown for foraminifera". In my opinion the effect of expatriation on shell chemistry in foraminifera has been previously shown by the work of Ganssen and Kroon in the Red Sea, but not really outside of that extreme environment. The studies cited in this place were mainly concerned with attempts to use particle tracking in models and describe potential effects, rather than documenting these effects empirically, or the empirical detection was indirect, inferred from sediment trap material where the dwelling depth is unknown.

Line 87: there are no formally and objectively defined and biologically or ecologically meaningful morphotypes within the species G. ruber. The concept of "morphotypes", re-introduced into the literature by Wang, has been superseded by the discovery based on genetic data (Aurahs et al., 2011), that the species concept as introduced by Parker (1965) is incorrect and that the species G. elongatus, synonymised by her with G. ruber, should have been retained. The same genetic data have also revealed that the pink and white varieties of G. ruber are genetically distinct and these have been now formally distinguished at the level of subspecies. The correct label of the analysed taxon is thus Globigerinoides ruber albus (Morard et al., 2020), with morphology corresponding to what Kontakiotis et al. (2017) label as Morphotype A.

Line 88: I fully understand the decision to concentrate on the relatively small size fraction for analyses, as this likely yielded most material. However, I would like to point out that Aurahs et al. (2011), also working with plankton material, also from the Mediterranean, showed that the features distinguishing G. ruber albus from G. elongatus are not yet present among all specimens in the plankton, allowing separation of plankton-derived specimens to the ruber and elongatus only to about 75 % accuracy. Since G. elongatus is abundant (if not dominant) in the Mediterranean, the authors must consider the possibility that some of the analysed specimens may have belonged to that species.

Line 104: The methods section here is not entirely clear in how the oxygen isotopes were measured. Whereas it is clear that Mg/Ca was determined on final chambers of individual shells, the authors should specify if the isotopes were also measured on final chambers or whole shells, on single shells or multiple shells (and then how many) and whether the same shells as for Mg/Ca were used or different shells. This has all implications for the understanding of the origin of the apparent noise in the measurements.

Figure 1: I agree that the two regressions (correctly using a total least squares approach) are similar, but could the authors please provide a formal statistical test for the similarity of the slopes, to support their statement that the sensitivities are indistinguishable, and for the equality of the intercepts, to dispel the impression that the regression lines are offset, indicating different endmember composition? Also, I am not convinced that it is correct to consider the results of Gat et al. (1996) as being different, as all of their values fall within the range of the presented data.

Figure 2: Could the authors please state which regression has been used here and also provide a formal test for the lack of difference in the east and west and for the presence of a difference in the slope and intercept between their data and literature data? Also please provide R2 for all regressions in the figure caption and/or text.

Line 145: Considering that seawater oxygen isotopes and salinity only correlated with R2 od 0.2, the authors need an explanation for what the isotopes in foraminifera correlated more strongly with both variables. Could it be that each of the variables explains a different part of the total variance? Then, a multiple regression of foraminifera isotopes against seawater isotopes an salinity should explain significantly more variance. If it does not, it means that the two explanatory variables explain the same amount of variance. This could be because of a fortuitous choice of sampling and the authors should thus also calculate the R2 for salinity and seawater isotopes only for the samples shown in Figure 3.

Figure 4: Could the authors again specify what regression has been used and how exactly the regression lines were calculated (regression of individual values or of the means)? Please state R2 for all regressions. Also, the Mg/Ca to T relationship is known to be exponential, so why not fitting an exponential curve? The linearity of the relationship could simply reflect the fact that the regression is fitted over a relatively narrow temperature range.

Line 155: Considering that Mg/Ca is also changing as a function of salinity, why not plotting Mg/Ca against salinity and analyzing the strength of that relationship as well?

Line 160: it is true that the foraminifera may have travelled a long distance over the 30 days of the simulation, but I question the significance of the so derived variability for the interpretation of the shell geochemistry. Culturing observations indicate that G. ruber in the size range as analysed here produces a new chamber about every two days. Thus, the particle tracking result has no bearing on the laser-ablation data. For the isotope data, if we assume a total lifespan of 4 weeks and a life expectancy of the specimens in the analysed size range of two weeks, then the collected specimens would have only had two weeks to grow, not 30 days. On top of that, because of the exponential growth of the shell, almost all of the analysed calcite and thus almost all of the isotopic signal is present in the last few chambers of the shell, so it reality, the backtracking relevant to the analysed signal should not have been carried back for more than a week. This is not to say that the result stated here is wrong – it is just that the result is not relevant for the interpretation of the measured geochemical signals. I note that your discussion in 4.4.1 resonates well with what I write, but then I do not really understand what was the merit or the justification of showing the particle backracking results in figure 7 over 30 days?

Line 170 and onwards: please see the comments above as to the necessity to provide statistical tests to support the presence or absence of differences in regression shapes. Also, please consider the location of the sampling by Gat and yours: what if the apparent offset from your regression that he reports simply reflects the fact that

he sampled at locations where the relationship is unusually confounded by secondary variables and that your data would detect the same if you only had measurements at those locations? I am also concerned by the origin of the lower oxygen isotope values measured for the given salinity in your data: was the sampling method comparable between your data and those of the previous studies (collecting from the same depth)?

Line 202 and onwards: Considering all your specimens were collected from the surface and that you measured only the composition of the final chamber, would it not be logically at this place to reject some of the hypotheses that you list here? Otherwise, you would have to imply that the specimens migrate vertically tens of meters over a few days, or stay alive without adding new chambers for weeks to allow lateral transport to have an effect. So perhaps we are left with the variable biomineralisation as the only remaining candidate mechanism?

Line 216: I fear the Mg/Ca data are revealing more than what the authors imply. Firstly, since the authors have both temperature and salinity, they should derive the correction independently of Gray et al. (2018) or at least check if the relationship they obtain holds. Second, I wonder why the authors do not discuss the fact that once the salinity effect is removed, their Mg/Ca data are no longer correlated with temperature or if correlated then with a much steeper slope (at least this is what I see looking at Figure 8). Third, I do not agree with the statement that the corrected values are slightly higher than expected based on the global regression – I observe that they are all higher than predicted by the exponential regression (the linear regression in Figure 8 is in my opinion superfluous). Why is that? Could there be a salinity-temperature interaction affecting the salinity-Mg/Ca relationship? This is an important result that deserves some more thought.

Line 226 (and some figure captions): please make sure species names are always written in italics.

Line 228: an argument on the presence (production) of G. ruber in different seasons in the Mediterranean would benefit from references to sediment trap data. There is a nice long time series from the west (Rigual-Hernandez et al., 2012) and a new dataset from the east (Avnaim-Katav et al., 2020, Deep-Sea Research) that could be used to support these statements.

Sections 4.4.1 and 4.4.2: I believe the authors could do better in providing quantitative constraints on the strength of the processes invoked to explain the large deviations in trace metals and oxygen isotopes from the theoretical calibration curves. For example, in section 4.4.2 they seem to imply that the oxygen isotope signal should be much less affected by the individual variability, but not by lateral transport. Notwithstanding of what the value of the 30-day calculation is, one should then ask: how much lateral transport would be needed at each of the locations to explain the isotopic scatter? Where would the calcification have to occur? Is the offset due to lateral transport large enough or not to be considered the main mechanism behind the scatter. Similarly, if all other other processes do not act on oxygen isotopes then the scatter in isotopes (residuals) should be less than in the Mg/Ca. Is it? I feel the authors should take the discussion further and provide at least first-order assessment of the strength and direction of the invoked processes and evaluate the plausibility of those processes in explaining the scatter.

Line 265: on the same note: why is the lack of correlation “likely” due to all those uncertainties? How big are these uncertainties exactly? The reader needs to see the values to be able to evaluate statements like on line 271, which are intuitively correct, but not really supported by any calculations. Please provide R2 and p for both regressions shown in Figure 8. Also, the method by which the oxygen isotopes in seawater have been estimated is not sufficiently documented. For example, it is not clear if and how the salinity effect on Mg/Ca has been considered.

Line 281: why do the authors not take this opportunity to compare the performance of Na/Ca and the combined isotope and Mg/Ca on the resulting salinity estimates? There is no need to end with a general statement, when the authors have all the data to carry

out the comparison.

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