

Interactive comment on “Mg / Ca and $\delta^{18}\text{O}$ in living planktic foraminifers from the Caribbean, Gulf of Mexico and Florida Straits” by Anna Jentzen et al.

B. Metcalfe (Referee)

brett.metcalfe@lsce.ipsl.fr

Received and published: 3 July 2018

Brett Metcalfe (LSCE, FR & VUA, NL)

Jentzen et al. have performed extensive stable isotope and trace metal geochemical analysis on 8 species from samples from 5 multinetts, 4 plankton filter and 5 core tops. This is a vast dataset that merits publication, however, the paper could be improved in several ways. The first is to plot the $\delta^{18}\text{O}$ and Mg/Ca for individual stations so readers can see which datapoint goes with which station. Second the Mg/Ca- $\delta^{18}\text{O}$ and Mg/Ca data could be toned down a bit as it is a bit over sold, the high r value of the Mg/Ca- $\delta^{18}\text{O}$ data (Figure 9) is a misnomer and the measured salinity and $\delta^{18}\text{O}_{\text{sw}}$ are not reconstructed using foraminifera accurately enough for palaeoclimate reconstructions.

Printer-friendly version

Discussion paper



For instance, how would pooling of the various size fractions into a single mean for Mg/Ca-d18O (Figure 9) work practically in a proxy study down-core? Should shells >300 μm be pooled and measured to reduce the 'noise'? The paper is nicely written, with a valid dataset, the results are interesting and should be expressed a bit more openly (i.e. less focus on the combined plots and more on the individual stations). These comments are outlined in more detail below.

Major Comments

(1) Raw data. My major problem with the paper is the lack of presentation of the 'raw' station data, figure's 6 and 8 are a synthesis of the entire dataset which whilst interesting should not be how the reader sees the data. There is a 4 page table of $\delta^{18}\text{O}$ measurements and 3 pages of Mg/Ca which is a lot of work that the authors have done, that shouldn't just be in table format in the supplement! I would like to see the data plotted per station (I think in the main text, but also could be in the supplement though it might get overlooked) so that the reader can see how the various stations/species isotope values 'evolve'. For instance, a figure (5x8 panel) of the 5 multinet stations depth T, $\delta^{18}\text{O}$, and salinity profiles with the values of each species in a different panel. Or the authors could extend figure 2 to include the isotopes/trace metal geochemical values. I understand that these plots also include filter and sediment values which may explain the rationale of the authors for plotting the data like it is.

(2) Mg/Ca-d18O. Does it really work? The authors show that they have a r value of 0.78 and 0.77 for d18Osw and salinity observed vs expected and the results in Figure 9 gives compelling evidence for the use of Mg/Ca-d18O to estimate d18Osw. In figure 9a the range in d18O estimates is more than 0.5 per mil but its difficult to tell whether this could be due to the spread in the station data. As per my previous comment I believe you need to show the individual station data, as it's impossible to link the salinity, d18Osw and estimates from individual stations to one another.

The authors should check the significance of the r values, for $n = 6$ (degrees of freedom

[Printer-friendly version](#)[Discussion paper](#)

are $n-2$) the significance value of r at an alpha levels of 0.5 and 0.1 is 0.812 and 0.917 respectively. It would also help to propagate the errors between the two analytical measurements.

Furthermore, Figure 9b, the scale of the y and x axis differ considerably, the entire data's x -axis range is approximately one tick along the y -axis. It would also appear that were the authors to draw a 1:1 line it would be offset from the line the authors have drawn through the data. Combining these two points it appears there is a weak relationship that isn't statistically significant and the approach doesn't exactly predict the 'real values'.

In addition, in Figure 9b it would appear that only one axis is 'reversed', the current view of the plot, at a glance without looking at the absolute values along either axis, gives the impression of a negative correlation instead of the positive correlation that it is and the authors state (Pg. 10 Line 1).

Why not plot the Figure 3b line (d18Osw vs salinity) in Figure 9c? Just from eye-balling it I would say that the slopes are different. Or convert the d18Osw into salinity (though I will admit that might impose some circular reasoning)?

I would say that the authors data and perhaps this approach doesn't provide accurate d18Osw estimates. Nor do they elaborate upon the influence of test-size upon their estimates, how will this be influenced if foraminiferal size is not static through time (Peeters et al., 1999 Mar Micro; Metcalfe et al., 2015 Biogeosciences) and if this result is dependent upon pooling different size fraction (>300 μm) measurements.

(3) Mg/Ca. First the amazing supplementary figures: S2 Figure 1 and Figure 2 would be better in the text than Figure 7, however these figures show that there is a lot of scatter within the data generated that should be elaborated upon in the text. I disagree with the authors that (Pg. 8 Lines 3 – 5): "Our Mg/Ca ratios of eight species collected at specific ocean temperature ranges (corresponding to different water depth intervals) are in good agreement with established species-specific Mg/Ca temperature calibrations (Fig

7 cf. Supplement intervals) and further support the foraminiferal Mg/Ca-dependency on ambient water temperature". The plot of *G. menardii* in S2 Figure 2 could be best described as 'shotgun' like; *G. tumida* is recording 50C (Figure 8) and *G. unguolata* appears to be getting warmer with depth (Figure 8). The authors themselves state: (Pg. 9 Line 7) "the offset between... vary from -3C to 9C"; (Pg. 9 Line 28) "the Mg/Ca temperature of fossil tests (~19C) represent the calculated average habitat temperature (~21.7C) far better than the living foraminifers". But this is not a bad thing, this is the data and the authors should present it a bit differently (e.g. Pg 10 Line 19 "datasets agree well to published d18O and Mg/Ca calibrations", do they? if these are species specific calibrations should they have offsets?).

I think the authors should add an x-axis error bar in S2 Figure 2, but also consider that collected foraminifer may not have actually calcified in the collected interval so perhaps extend this error bar to incorporate the temperature of shallower depths. Such an approach might explain some of the high Mg/Ca values in the lower temperatures as specimens that had yet to calcify in the water they were caught in. Could this explain the discrepancy with recorded temperatures? One assumption the authors have made is that living foraminifera caught in a net interval are calcifying in it, for filter and the shallowest net it can be assumed that the bulk of the shell (considering that some foraminifera could ascend to the surface during their juvenile stage) comes from that net interval. This assumption doesn't hold true for the deeper nets.

It would be interesting to consider the difference between living and dead shell geochemistry (in this or another paper), and whether by choosing only living foraminifera the results could be biased (just because it is alive doesn't mean it has to represent the values it was caught in). Highlighting the shell concentration for each net interval could indicate whether the deeper depths are shells of living foraminifera sinking outside of their habitat zone but still alive (see Peeters et al., 2002; Global and Planetary Change – for examples e.g. Figures 5 and 6). This is alluded to by the dashed red-line in Figure 8, which shows the weighted average living depths shallower than some of

[Printer-friendly version](#)[Discussion paper](#)

the measured depth intervals for some species.

Minor Comments

Check the plural and singular of the word foraminifera throughout the text, as well as V-PDB and V-SMOW (sometimes it's PDB)

Pg. 1 Line 8 (first line of Abstract): The first line (slightly) contradicts the second line (if it is successfully approximated, why is refinement needed?) perhaps change 'are successfully' to 'can be' and add 'with varying success' to the end of the sentence

Pg. 1 Line 15 add 'with respect' between disequilibria and to

Pg. 1 Line 36: Avoid starting a sentence with Mg/Ca change to "The ratio of Mg/Ca"

Pg. 2 Line 5: add 'for proxy users' after critical

Pg. 2 Line 7: Perhaps a re-wording? Whilst, relatively few (isotope) geochemical studies have been conducted on recent/living planktic foraminifers, either collected from the water column or culture under controlled conditions, these studies are important for assessing different controlling factors on $\delta^{18}\text{O}$ calcite and Mg/Ca during biomineralization.

Pg. 2 Line 7: Also 'relatively few'? I would disagree with few $\delta^{18}\text{O}$ studies, though Mg/Ca maybe.

Pg. 2 Line 12: 'Here we' instead of 'We here'

Pg. 2 Line 21 move 'and fossil' to the end of the line (so it reads 'below the ship, and fossil foraminifera from sediments')

Pg. 2 line 32 – 34: Why poison $\delta^{18}\text{O}$ samples? Does the addition of poison impact cavity-ring down / infrared spectroscopy, as there is some suggestion in the literature that salinity may have an impact on the vaporiser unit (especially changing salinity between samples but also certain salt solutions)

BGD

Interactive
comment

Printer-friendly version

Discussion paper



Pg. 2 line 29/Pg. 3 line 1: Bradshaw (1957) did this massive plankton net study/database of the Pacific, halfway through though he stopped using Rose Bengal to identify 'living' as cytoplasm in the shell was just as efficient. Out of curiosity, is there a reason why (Pg. 3 Line 1) 'cytoplasm-bearing' was picked rather than stained? Is there any potential analytical error associated with staining?

Pg. 2 line 40: perhaps add 'trilobus-like' to better describe forms of *T. sacculifer* with a 'spherical last chamber'.

Pg. 2 line 41: *Globorotalia unguolata*, the arch that distinguishes it from *G. menardii* did you perchance consider laser ablating the arch and the rest of the chamber separately? More out of curiosity, it looks like a chamber feature rather than an embellishment (like a keel) so I wonder is it identical to the rest of the chamber but thicker? Or does it have some structural modification that can be observed in the ablation profile.

Pg. 2 line 41: *Globorotalia truncatulinoides dextral*, have you seen the new paper by Reynolds et al (2018; Mar. Micro.)? Did you distinguish between crusted and encrusted *G. truncatulinoides*? Is there any ablation data that might shed light on the Mg/Ca content of encrusted and non-encrusted from your data?

Pg. 3 Lines 1-4. Add in size fraction used for analysis (from the table it appears not all size fractions picked on pg. 2 line 37 were included in the d18O analysis).

Pg. 3 Line 14. The magazine/turret of a Kiel device is quite small, perhaps give an indicator of the number of standards per run or in total measured? (as per the Mg/Ca analysis i.e. Pg. 4 Line 22)

Pg. 3 Line 18. Add Picarro before model (I assume it's a Picarro instrument based upon L1102-i).

Pg. 3 Line 26. Whose rearrangement of Kim and O'Neil?

Pg. 3 Line 31. Add in a mean number of specimens with a plus/minus.

[Printer-friendly version](#)[Discussion paper](#)

Pg. 4 Line 2. The steps were repeated several (1-2) times to completely remove the cytoplasm, could varying this step have any impact on the resultant values between samples that have had a single step and a replicate step?

Pg. 4 Line 21. Outliers are classified as those values that fall 2 stdev above or below the mean, but that would mean based upon the proportion without (assuming normality) that $\sim 4.55\%$ of the data could be removed. What's the rationale for this? How much does this influence the data?

Pg. 6 Line 18. The transition from juvenile to neanic to adult stages occurs between 100 and 200 μm (Brummer et al., 1986 Mar Micro; 1987 Nature), therefore foraminifera above $>200 \mu\text{m}$ (species dependent) should be considered as 'adult'. Could the vital effect, described here, instead be due to growth rate (adult specimens with different sized chambers could suggest variation in growth).

Pg. 7 Line 1. Regarding GAM, if you assume that a shell is a weighted average, if 25% of the shell is composed of gam-calcite and it has a 0.5 per mil offset from living, if you consider it proportionally the gam-calcite would need to have a significantly larger offset from the rest of the living shell.

Pg. 7 Line 34. Would it not be appropriate to (like the palaeotemperature $\delta^{18}\text{O}$ eq1) compare the expected Mg/Ca value by calculating an expected Mg/Ca value (assuming that Mg and Ca concentrations are not limiting and using the values in S2 Table 2)

Figure 1. It's a beautiful figure. However, two things, (1.) the map has a transparency that the scale bar does not, could make it difficult to interpret the color, (2.) The rainbow is a bit difficult for colour blind readers to see (I am guilty of this as well). See: <https://www.climate-lab-book.ac.uk/2014/end-of-the-rainbow/>

Figure 3. Why not plot the figure like Figure 2 (with isotope, salinity and equilibrium plotted per station) rather than as a composite plot?

Figure 9. (see comments above)

[Printer-friendly version](#)[Discussion paper](#)

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-176>, 2018.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

