

Interactive  
Comment

## ***Interactive comment on* “Trace metal/Ca ratios in benthic foraminifera: the potential to reconstruct past variations in temperature and hypoxia in shelf regions” by J. Groeneveld and H. L. Filipsson**

**W. Austin**

wena@st-andrews.ac.uk

Received and published: 26 April 2013

Trace metal/Ca ratios in benthic foraminifera: The potential to reconstruct past variations in temperature and hypoxia in shelf regions

Jeroen Groeneveld, Helena L. Filipsson

**Summary:** The manuscript describes field-based collection methods that attempt to show a link between foraminiferal shell geochemistry of modern bottom water conditions in the Skagerrak and Gullmar Fjord (SW Sweden); this is an important area of research because our current ability to understand the long-term response of these environments to recent change (both natural and anthropogenically forced) is poor and

C1272

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



relies on largely qualitative descriptors. The authors are well-respected in the field and have a strong publication track-record for this type of research.

The authors set-out the background review and make a clear case for the study; this is useful in that it attempts to broaden the scope and, hopefully, reader interest in the work. The details of the paper are only likely to appeal to a relatively specialised readership; however, the wider implications and potential to reconstruct long-term changes in these environments are likely to have broader reader appeal.

The data are somewhat limited and the inconclusive nature of the results is a little problematic because they do not move us very far forward, although I fully recognize the value of these preliminary data. I am therefore highly supportive of this work and I fully understand the challenges of working with live material and small sample sizes.

I recommend revision of the manuscript as follows:

(1) The manuscript could be significantly improved by the inclusion of more geochemical data, otherwise there is a risk that we simply end-up with confusion/uncertainty and potentially undermine the reputation of, in particular, the Mg/Ca proxy for temperature reconstruction.

(2) Alternatively, it might be possible to restructure the manuscript as a short note, highlighting the potential of Mn/Ca as a proxy in benthic foraminifera for past dissolved oxygen concentration; as noted below, there are difficulties in the interpretation of the Mn/Ca data as they currently stand, but this aspect is novel and its potential as a new tool deserves to be highlighted.

Some specific comments:

Line 168-170: “The CellTracker Green method ensures that the Mn/Ca values in this study are part of the foraminiferal test itself and not associated with diagenetic coatings.” How so? The possibility of trace-metal contamination is, I agree, minimized in ‘live’ foraminifera – but I don’t see that it ‘ensures’ 100% absence of coatings?

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Line 172-174: “Between 3 and 20 tests of *B. marginata* and *G. turgida* were selected from each sample and gently crushed. Due to the low number of living specimens available the size range of the selected specimens was not restricted.” It is worth highlighting that differences in the numbers of tests analysed can prove problematic in the comparison of samples measured on the same species – notably where reworking of an unknown number of tests is a possibility (as in any fossil assemblage) or where samples are mixed by bioturbation, bringing specimens from different depths within a geochemical gradient together. To be fair, this is acknowledged and briefly discussed on line 328 (‘bias’), but since we know about these problems already – I think they should acknowledge this point sooner.

Equally, the inclusion of specimens of different sizes can be problematic, given the possibility of ontogenetic changes in habitat-selection and hence shell geochemistry. Again, this issue is discussed in relation to the work of Hintz et al. (2006) around lines 332 – so there is already published evidence to suggest that this is likely to be problematic and so should be highlighted at the outset (i.e. earlier in the manuscript methods)?

Line 278-281: “It is essential to know for the discussion of any geochemistry data, especially acquired from benthic foraminifera from variable environments, when the used species calcify as this determines the geochemical signal” – useful to focus on theory of seasonal calcification and e.g.  $^{18}\text{O}$  equilibrium calcite calculations in relation to NW European shelf seas (Austin et al., 2006, Holocene).

Line 302-306: “Why are these differences in Mg/Ca between the different studies so large assuming that Mg/Ca in *B. marginata* in the Skagerrak is indeed representative of calcification temperatures? And additionally, why is the scatter in the results much larger than is commonly seen in planktonic or deep sea benthic foraminiferal Mg/Ca?” – these strike me as important and useful questions, which are being raised as a matter of open discussion rather than offering a solution to the problem – this is one reason why manuscript revision to focus on a review-style paper might be better at this stage?

Line 307-309: “One possible reason for the large spread in Mg/Ca could be the heterogeneous Mg distribution between and within shells from the same sample (e.g. Eggins et al., 2003; Rathmann et al., 2004; Hathorne et al., 2009). – misses the work of Allison & Austin (2003, G3 based on ion-probe analyses of benthic foraminifera). Incomplete cleaning, overgrowths etc. may all affect the results – these ‘unknowns’ are poorly constrained and worrying and one has to consider the value of publishing results (based on a limited data set) that might ultimately undermine this area of research. Have the authors given this careful consideration?”

Lines 407-410: “We speculate that in a lower oxygen environment the lower metabolic activity might also decrease the distribution coefficient of Mg which could potentially have contributed to the lower *G. turgida* Mg/Ca in the Gullmar Fjord compared with the Skagerrak.” – this is highly speculative.

Lines 441-443: “This is also the reason why the specimens used in this study have all been labeled by CellTracker Green ensuring that they were alive during collection and, thus, could not yet have been affected by diagenetic coatings” – again, take care with these bold statements. It seems that the Mn story is very complex and the question of developing a new proxy is still at an early stage? This point needs careful editorial consideration and further discussion with the authors.

Lines 480-485: “As CellTracker Green was used to ensure that analysed specimens were alive during collection we can make a direct link between Mn/Ca in the bottom water and in the foraminiferal calcite determining distribution coefficients. A potential bias on the possible distribution coefficients is caused by the lack of pore water chemistry data with likely even higher Mn-concentrations than the bottom water itself (Goldberg et al., 2012).” – this seems a big jump – complexity of bottom-water to pore-water Mn gradients?

Lines 532-533; “Staining with CellTracker Green ensured that the specimens were alive when collected and, thus, were not affected by diagenetic coatings.” – I must highlight

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

the problem of this statement – if they want to reach this conclusion, then they need to show data based on the measurement of ‘live’ and ‘dead’ specimens, where the ‘live’ specimens are clearly shown to be diagenetic-free; this is a prime issue in this field and cannot be ignored?

Lines 540-541: “Mn/Ca results on *G. turgida* show potential to record variations in dissolved oxygen content of the habitat where they calcify” – this is very encouraging and worth highlighting and publishing.

Lines 546-549: “Our study shows that trace metal/Ca ratios in benthic foraminifera from shelf regions have the potential to record past variations in bottom water temperature and dissolved oxygen concentrations, but calibration studies based on both core tops and culturing are needed to resolve the possible extend of variation.” – in many ways this final sentence of the manuscript (which should be a strong punch-line and take-home message) highlights the difficulty of my review – we know this already.

Finally, I consider a revised manuscript well-worth publishing, but some more thought must be given to the rather limited data available at this stage and their use in developing the arguments – a shorter, review style paper would work better at this stage, then collect more data and publish a follow-up?

This is an innovative new study and, to my view, the type of paper which is well-suited to the journal and I look forward to follow-up studies in this area.

---

Interactive comment on Biogeosciences Discuss., 10, 4403, 2013.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)