

Interactive comment on “Carbon isotope ($\delta^{13}\text{C}$) excursions suggest times of major methane release during the last 14 ka in Fram Strait, the deep-water gateway to the Arctic” by C. Consolaro et al.

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Dear Editor,

the manuscript presented by Consolaro and co-workers reports interesting patterns in past (benthic) foraminiferal $\delta^{13}\text{C}$ which, as they argue, may well be a consequence of variations in methane release. I wrote my comments (below) on this manuscript after Prof Dickens' review was posted and therefore focussed on complementary issues. In short, I recommend publication of this manuscript after minor revisions in addition to

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those highlighted by the first reviewer.

Sincerely,

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Main comments:

EDS is not a common tool to quantitatively analyze element concentrations in (carbonate) samples. Accuracy of obtained concentrations could have been ok by using a set of reference materials. The Methods section (page 7, lines 8-15) is very brief on this topic and does not mention such a strategy. Table 2 summarized the EDS results and suggest a level of accuracy that is unrealistic (and it is confusing that the wt% of the recorded elements apparently does not refer to the real wt% in the material, which is for example 40 wt% for Ca in any piece of calcite). In short, the EDS measurements only allow distinction between mineral phases on a broad level (e.g. between CaCO_3 and SiO_2).

Mapping the elements (e.g. Mn) on embedded, sectioned and polished specimens would have allowed detection of (contaminant) surface phases. Spatial recognition of a contaminant phase throughout the chamber wall would make a valuable addition to the discussion of overgrowths and potential distortion of the primary signal. I hope the authors can include such maps (e.g. using ion microprobe) to replace the EDS-results which should either completely be removed or used only to show that the outer surface of some foraminifera is covered by something rich in Si.

Part of section 5.2 should be in the Results rather than in the Discussion.

Minor comments:

Page 3, line 14: ‘that lives’ should be ‘living’.

Page 4, line 8: ‘warm, saline water’ should be ‘relatively warm, saline water’.

Page 5, line 5: ‘at’ may better be replaced by ‘from’.

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Page 5, lines 22-23: approximately how much of the total number of foraminifera were specimens of *N. pachyderma* and *T. quinqueloba*?

Page 6, line 10-14: difficult to understand this sentence: please rewrite.

Page 6, line 15 and on: what standards were used and how were the carbonate results corrected? I assume all values are expressed relative to VPDB...

Page 6, line 17: what size fraction? Picked from same size interval as the planktonic foraminifera?

Page 9, line 6 and 10: 'lighter' should be 'more depleted'.

Throughout 5.2 and in the (caption of) figures 6 and 7, the authors state that specimens from certain intervals are 'altered', whilst others are not. Figure 6a-c nicely shows that this alteration is likely confined to the very surface: most of the inner parts of the shell wall looks just like that of the other specimens (d-f and g-i). Therefore, please refer to this alteration as a 'surface alteration' or something: by far most of the shell does not appear altered.

page 12, line 14: 'strong enough and long enough' reads better as something like 'prolonged and sufficiently high'.

Interactive comment on Clim. Past Discuss., 10, 4191, 2014.

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