

# ***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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The manuscript by Consolaro and colleagues is intriguing. The authors present various records from a core located on top of a pockmark in Fram Strait. They show that the benthic foraminifera  $\delta^{13}\text{C}$  record has two notable negative excursions. They present evidence that the excursions are not solely the result of diagenesis. They suggest that the negative  $\delta^{13}\text{C}$  excursions are the result of methane release from the seafloor, and may be temporally linked to other such records in the North Atlantic.

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The manuscript is, for the most part, refreshingly well written at the sentence to paragraph to section level. There are, however, three main issues that need rectifying before publication, at least in my opinion. I discuss these below, and obligatorily add a few additional comments.

In summary, I would accept this manuscript after modest revisions.

Sincerely,

Gerald (Jerry) Dickens Professor, Rice University

General Comment and Major Issues: I think it is fair to say that after 20 years of discussion, most people in the paleoclimate/paleoceanography community remain allergic to the idea that variable amounts of carbon flow between the ocean and seafloor methane systems over time. This is especially true for the Quaternary. I think this is for three main reasons: (i) many papers on the topic are not thought all the way through; (ii) the overall linkage between the ocean and seafloor methane systems is often poorly presented; and (iii) if correct, the concept of variable seafloor methane fluxes really forces our community to change many long-held ideas. Of course, it is also possible that seafloor methane systems are, as many believe, largely irrelevant to carbon cycle and climate discussions. The point here is that a paper on the topic should be held to a higher standard and crystal clear.

(A) The manuscript lacks an introductory conceptual framework regarding the fate of methane and the carbon isotope recorders. As a consequence, the aims of analyses and the discussion of results become confusing to follow. This will be particularly true to the broad readership of COP. I think a short new paragraph with an accompanying figure (new Figure 1) should be added to the Introduction (page 4193, between Lines 13 and 14). For this figure, I strongly suggest a cross-section from the sea surface to below the seafloor, that shows (a) a pockmark, (b) gas hydrates and the BSR, (c) variable upward CH<sub>4</sub> fluxes from the deeper CH<sub>4</sub> system, (d) the location and mode for the two potential fates of rising CH<sub>4</sub> – AOM and aerobic oxidation in the water column,

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and (e) the location and fate of planktic and benthic foraminifera. I have not seen such a figure before, although key components are scattered about in various works. In any case, this would clarify things immensely, as it would set the stage, hypothesis, and the rest of the writing.

(B) Related to the above comment, it is not obvious whether the manuscript is arguing for past times when AOM in shallow sediment was greater locally, when aerobic oxidation in the water column was greater across the North Atlantic, or something in between. At a root level, both end member processes could lead to greater carbon fluxes to the ocean, and could potentially change the  $\delta^{13}\text{C}$  of benthic foraminifera, pore water DIC and even bottom water DIC at multiple locations, although much would depend on the extent and flux of methane release. For example, enhanced AOM in shallow sediment over a wide area might produce  $^{13}\text{C}$ -depleted  $\text{HCO}_3^-$  that could diffuse to the seafloor.

(C) The manuscript lacks a compelling demonstration and discussion of its true significance. More specifically, are the negative  $\delta^{13}\text{C}$  excursions really similar in age and magnitude to those at sites shown in Figure 1? In general, I do not like paleoceanographic interpretations based on results from a single piston core. This is because most things of significance can and should be reproducible in multiple cores. In the present manuscript, comparison to other cores in the North Atlantic arises (page 4203), which is good. However, this is not rigorously demonstrated or discussed. An additional last figure, one showing and detailing the correlation, would be very welcome; indeed, I will suggest this is almost mandatory. If the correlation of benthic  $\delta^{13}\text{C}$  excursions is correct, then much of the discussion should be condensed and modified, as local processes are not the answer. Such correlation also should include cores away from pockmarks, to assess the alternative possibilities: (i) there are multiple locations with local carbon isotope perturbations at the same time (perhaps in pore waters); (ii) the deep water of the North Atlantic has had significant short-term changes in its stable carbon isotope composition.

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For fun, and to return to my opening comment, it is worth considering the true significance of the interpretations, should they be correct and if I am reading things correctly (although see above issues). If there is indeed widespread, correlative release of methane (or affiliated carbon fluxes) from the seafloor across the North Atlantic, enough to change the  $^{13}\text{C}/^{12}\text{C}$  composition of deep water, then the  $^{14}\text{C}/^{13}\text{C}/^{12}\text{C}$  ratios also have to be affected, because seafloor  $\text{CH}_4$  and its oxidative products (either  $\text{HCO}_3^-$  or  $\text{CO}_2$ ) are radiocarbon dead. Ideally, the authors can collect enough benthic foraminifera to measure and to test this idea (obviously in a different paper). If they could show that the  $^{14}\text{C}$  composition of North Atlantic bottom water was affected in the past by seafloor methane discharge, this would be revolutionary; without clarified rewriting or considering this possibility, the wolves of the paleoclimate/paleoceanography community will be bay at their doors and burn this house sometime in the future.

Some other comments: – Page 4193 – Line 14: Rewrite because the grammar is incorrect.

Lines 15-17: This needs expansion and rewriting. The various studies suggest incorporation of  $^{13}\text{C}$ -depleted carbon, but it needs to be presented if this is through pore water or through overlying seawater (See also Comment A).

– Page 4194 – Line 24: I think this is past tense – “has occurred”.

– Page 4195 – Line 5-8: Rewrite because awkward, including an odd verb.

Line 8: This sentence is not needed, because it is effectively repeated in the next sentence.

– Page 4199 – Line 21: Rewrite because the grammar is incorrect.

– Page 4201 – Lines 14-22: This section does not make sense to me. Why would AOM lead a coating dominated by  $\text{SiO}_2$ ?

– Page 4202 – Line 3: Something seems incorrect here.  $\text{AOM} (\text{CH}_4 + \text{SO}_4^{2-} \rightarrow$

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HCO<sub>3</sub><sup>-</sup> + HS<sup>-</sup>) generates bicarbonate (and consequently alkalinity) not CO<sub>2</sub>. This process favors authigenic carbonate precipitation, as abundantly clear at many locations. I suppose one might suggest that AOM drove FeS precipitation, and subsequent oxidation of these sulfides caused carbonate dissolution. All sites that I have looked at where AOM is occurring have significant amounts of FeS minerals.

Lines 12-27: This section needs rewriting. It is supposedly on methane release, which I would assume means to the water column; however, the section discusses AOM, which at least in my mind is the consumption of methane before it escapes to the water column. (See also comment A).

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

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