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Interactive comment on "Physical controls on the storage of methane in landfast sea ice" *by* J. Zhou et al.

Anonymous Referee #3

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The manuscript by Zhou et al. delivers an import contribution to our understanding of the physical controls on methane within sea ice, something which has previously been shown to be important for CO_2, too. I think some parts of the article are well explained, and I agree partly with their conclusions. However, other parts are less convincing presented. Alternative explanations for the observed concentration patterns, such as biological activity, are too easily dismissed. The intuition of the authors that biological processes are minute might be correct, but I'm not convinced that their argument - based on a statistical analysis - shows it, too.

This statistical analysis was done on 4 samples taken in the course of 10 days, and only on the impermeable part of the ice core. Mehanogenic and/or methanotrophic activity might have been different in the top and bottom part of the sea ice, but this is not shown. Nonetheless, as the methane standing stocks over this subset of the ice-column did not differ statistically over this small period, it was concluded that biological activity played no role. However, it might just be that those 10 days in April represented a period of low biological activity, and this does not exclude methane production and consumption processes outside of this period, such as early winter. One way to better assess this, would've been to take samples and incubate them to assess a potential methane production rate under controlled conditions (e.g. varying brine and temperature conditions).

In fact, such measurements have been done for sea ice from the same location (Barrow, Alaska), and these were presented last year at a large meeting of the Pergamon Arctic methane group in Kiel. I'm aware of the fact that these results are not peer-reviewed yet, but they will probably show up in the literature soon. If so, this shows that biological production and consumption of methane is in fact happening in the ice. For more information, see page 15 of the abstracts of this conference: http://www.geomar.de/fileadmin/content/service/veran/wissenschaftlich/2013/All_abstracts_PERGAMON-symposium.pdf

Microbiological controls can, therefore, not be so easily dismissed. Perhaps the authors are less experienced in that field, as evidenced by the mixup of 'methanotrophic activities' in line 16, page 123, where it should've read 'methanogenic activities', which is the exact opposite. The same goes for line 25 of that page. I'm willing to forgive such a mistake, but it does lower confidence in the authors' ability to properly exclude a microbiological explanation behind their results. Perhaps teaming up with a microbiologist who has experience with processes within the sea ice would've helped to strengthen the argument that microbiological processes are minute, and physical processes are dominant.

I still think, however, that this publication adds significantly to our knowledge of the physical controls on methane fluxes in sea ice, but microbiological activity should be more seriously considered as a viable contributor, rather than dismissing this on a simple statistical test on a small subset of the data. I would therefore recommend for a major revision which addresses the remarks above, and explores alternative hypotheses behind the observed concentration patterns more thoroughly.

Some other, more specific remarks, are as follows:

- page 122, line 17-22: perhaps it's better to cite the latest IPCC report. The GWP of methane has been raised, as well as the contribution to radiative forcing. Also, concentrations of CO2 are more close to 400 ppm these days.

- page 122, line 23-25: again, the IPCC AR4 is seriously outdated. Besides, the quoted numbers are not mentioned in that IPCC chapter. I would suggest to refer to Kirschke et al (2013), who recently presented updated numbers for both the ocean, as well as global emissions.

- page 123, line 16 and 25: as mentioned above, this should be 'methanogenic', not 'methanotrophic'. Methanotrophy is the consumption of methane, not production.

- page 123, line 27: why such old references on this issue? Much has been written since about the likelihood of methane release from clathrates (which decreased). A good review on the matter is O'Connor et al. (2010), for example.

- page 125, line 18: I see that the N_2 came from a Belgian supplier. Can I therefore assume that the analysis was done in Belgium, too? What kind of influence would this have had on your analysis? Were the samples transported at -30 degrees from Alaska to Belgium, or not?

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- page 126, line 14: are you sure there's no methane left behind in the pure ice matrix? What if you would take the pure ice separate, melt it, and repeat the procedure, would there be absolutely no methane released?

- page 132, line 25 to page 133, line 2: These processes all probably contribute quite a lot to the observed profile. But the consumption and production of methane by microorganisms has to be considered as well.

- page 133, line 3: The use of the word 'believe' is a bit colloquial here. Better would be to hypothesise it or, better still, to show it.

- page 133, line 15: 'The presence of bubbles is suggested', but at line 20 you say they were observed. So are you certain they are there or not? Are there pictures? Perhaps show some more from your 2013 paper which is referenced in line 21.

page 133, line 28-29: Could you elaborate a bit more on the physical processes behind the reduction of bubble nucleation efficiency? This is mentioned very briefly, but not really explained, which will make it harder for readers to understand why this is. Besides, this is your argument for why the concentration of methane reduces with depth. So this should be explained better, supported with references.

page 134, line 2-4: Please explain the brine volume effect better. At the moment this part is written rather confusing and more difficult to comprehend than necessary. Please rewrite in a way that it's clear how this works.

page 136-137, conclusion: why convince the reader that biological processes don't matter, when you admit right here that microbiological activities do affect the concentration of methane in the ice? How does this affect your results?

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One final remark: first time readers might be confused about the mentioning of bubbles in your paper without additional context. Sometimes it would help to say 'bubbles formed in the ice' so readers don't confuse this with bubbles from ebullition

References:

Kirschke, S. et al. (2013), Three decades of global methane sources and sinks, Nature Geoscience, 6(10), 813–823, doi:10.1038/ngeo1955.

O'Connor, F. M. et al. (2010), Possible Role of Wetlands, Permafrost, and Methane Hydrates in the Methane Cycle Under Future Climate Change: a Review, Rev. Geophys., 48, RG4005, doi:10.1029/2010RG000326.

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