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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 #2

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This is an interesting and well written paper, which highlights the physical controls on methane dynamics within sea ice and the authors have gone to great lengths to verify their observations and data. Their major conclusion is that methane storage in sea ice is the primary source of elevated methane levels compared to the underlying water and they basically rule out that biological processes lead to elevated methane concentrations within sea ice. At first reading this is indeed a convincing conclusion resulting from the available and presented data. However, I do have some major contentions regarding the way they arrive at these conclusions and their take home message. The authors, in a previous publication on physical and biological properties of landfast sea ice, done at the same locality and during the same period as described in this paper and most likely on parallel if not the same cores, have not at all referred to the chlorophyll data obtained during this study let alone considered or discussed these. The

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authors refer to the previous paper, but only in the context of physical properties and Argon. Nor is any reference made to the other biogeochemical data, particularly the interesting information on nutrients for example (See Zhou et al. 2013). The authors go to great lengths to assess the biological activity by calculation and argumentation. I don't understand why they don't compare the CH4 bulk measurements with the chlorophyll and nutrient data they have, particularly since there is considerable variability in concentration and distribution between cores, within cores and with season. Furthermore, an analysis and comparison of CH4 standing stocks is to my mind not an efficient and credible method to exclude biological activity. It is unfortunate that the authors do no have rate measurements to support this, especially since considerable biological activity must have occurred, judging by the Chlorophyll concentrations measured. Needles to say this information would have been crucial, even if it was only to substantiate their major findings. Currently, however, the omission of this available information weakens the paper. This study is on first year landfast sea ice, which has specific and unique properties that may differ considerably from those of pack ice, both first year and multiyear. One major aspect in this case was the proximity to the sediment underneath, which was 6,5 meters. This has connotations with regard to methane sources, which differ markedly from those under oceanic pack ice, for instance, where different water masses with different methane loads occur, and where the sea ice is multi-seasonal and has a different history. This is not discussed or reflected on in this study The impression the authors give is that their results reflect that, which occurs in sea ice in general - see conclusion -and that biological activity in sea ice is not likely to play a role in methane production in sea ice anywhere and at any time. This is very misleading and needs to be constrained. One should be careful to extrapolate information gained from landfast sea ice over a water column of 6,5 meters, to the entire Arctic sea ice cover or elsewhere, not that this is what the authors are explicitly conveying, but there is a danger that this may be taken to be a fact. The authors need to account for the uniqueness of the ice they studied as well as the locality relative to other sea ice covered zones. The fact that the authors in their conclusions discuss that CH4 dynamics in the permeable layers need further investigation, points to the uncertainty as to what role biology may play there. This information is not mentioned in the Abstract. Furthermore it would have been appropriate to distinguish more clearly about the possible differences between processes in the impermeable and permeable layers. This information needs to be in the abstract too. In this context I also found discrepancies between the Figure 13 in Zhou et al. 2013 and Fig 4 of this paper where the zone of permeability in the former is shown to reach the ice surface, whereas it does not do so in the latter Figure. There is an apparent transition zone during May, shown in the former figure, which is not displayed in this paper where the impermeable zone extends well into May. This is a little disturbing and needs to be clarified, particularly since there are differences in processes in these layers. Some additional points: Fig 3: How do you explain the higher methane concentrations in the water column during June? Analyses where done within one year, which means cores were stored for long periods at -30°C. Can it be ruled out that such storage has no effect on gas bubbles and brine within cores. The cores were frozen to prevent brine drainage, but what about brine expulsion at -30° C? When sea ice is frozen under these temperatures, brine can be expelled, particularly from the first few centimetres along the edges of the core. Where were analyses carried out and how were cores transported. Was there an uninterrupted cooling chain at -30°C? The authors stored their cores at – 30° C to limit biological activity-what were they concerned about? That biological activity could affect the measurements? At what temperatures were the cores really stored? -35°C as in Zhou et al. 2013 or as described in this paper -30°C? Line 13 Page 6 "Providing that there is no CH4 in the pure ice matrix". This needs some explanation or reference. Line 5-10 page 7 "This is allowed providing that the relationship of Wiesenburg and Guinasso (1979) is valid for the ranges of brine temperature and –salinity". Is the relationship valid or not? Important conclusions are not well represented in the abstract. In the conclusions, "seems" is used more than once i.e that CH4 did not seem to be affected by biological processes. This reflects uncertainties, which weaken the final conclusions. Be more succinct. I recommend publication of the paper after significant revision, taking into account the questions and problems mentioned above.

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