

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

Anonymous Referee #1

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I like the originality of this finding as well as the experimental approach implemented by the authors to collect and interpret the reported data. They have finally shed greatly needed light on the origin of methane (CH₄) measured within the sea ice, which some authors in earlier published papers attributed to biological processes involving in-situ production within the ice body (such as CH₄ production, including methanogenesis under aerobic conditions, and CH₄ oxidation). Neither of these recently published papers left me convinced about the origin of CH₄ measured within the sea ice. On the contrary, the authors of this study suggest that the landfast sea ice serves as a temporal storage depot for CH₄ and is one of the major factors controlling atmospheric emissions of CH₄, acting as a physical barrier restricting these emissions during the period between ice formation and ice breakup. They demonstrate that the buildup of CH₄ stored in the sea ice (standing stock) is due to incorporation of CH₄ from the

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seawater and from gas bubbles, the formation of which, they suggest, is triggered by strong solubility changes. Most of their arguments are logical and convincing to me. Their data on subsequent evolution of CH₄ storage in the ice layers, which demonstrate that levels of CH₄ in the standing stocks are not affected by biological processes in these layers, serve to demonstrate how negligible the influences of biological processes are, compared to the physical controls of the sea ice. These findings are very important, because they improve our understanding of the role of sea ice in the exchange of CH₄ between the Arctic Ocean and atmosphere during the period from ice growth to ice melt. Thus, the authors are dealing with a topic of highest interest. I am generally happy with the methodological approach used by the authors. Methods used by authors allowed them to clearly show that mean concentrations of CH₄ as well as standing stocks were correspondingly increasing as sea ice thickness was growing while the ice remained impermeable for gases (from February to April). They further showed that both parameters decreased in June when sea ice became permeable for gases and started venting CH₄ to the atmosphere. The authors logically attribute the relative stability of the CH₄ standing stocks to the ability of impermeable sea ice to physically control CH₄ accumulated within the ice during its growth. They also showed that even if some biological activity could have taken place, its importance was negligible in comparison with the importance of the physical processes responsible for CH₄ accumulation within the ice. They also reasonably argued that levels of CH₄ supersaturation and rates of ebullition, where ebullition occurs, would determine the levels of CH₄ incorporated into sea ice. This is a very important conclusion that has not been suggested by other authors who are measuring CH₄ in the sea ice. I agree with the conclusions as phrased except for those regarding biological processes that may affect the CH₄ in the sea ice. As authors do not exclude possible CH₄ production in the sea ice, a question arises: what would be the source of methanogenesis in those theoretically-possible anoxic micro-niches in the ice, if the authors reported that annual ice rejects a major fraction (85%) of all impurities, including most likely organic substrates, during its formation? In this regard, I would like to remind, that there were

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few reports of possible methanogenesis occurring in the anoxic lenses accumulated on the bottom of the pycnocline in the highly productive tropical waters. Nevertheless, levels of dissolved CH₄ measured within those sub-surface maxima varied in the range from 4 nM to 9 nM. Moreover, these rates of methanogenesis required accumulation of organic pellets from the water column of >100 m in thickness! Note, thickness of landfast sea ice is only 2 m. Regarding CH₄ oxidation, recently reported data from adjacent parts of the Arctic Ocean suggest that the dissolved CH₄ pool turnover time is much longer than the lifetime of the fast ice. In addition, the authors need to know that $\delta^{13}\text{C-CH}_4$, modernly produced in the Arctic environment, exhibits an isotopic signature much lighter than -50‰ or -70‰; this signature could be as light as -100‰ or even lighter. If CH₄ is produced in-situ in the sea ice, its isotopic signature should reflect the process appropriately. Nevertheless, I agree that all these processes should be scrutinized. Regarding the manner of presentation, I would suggest that the authors re-think their presentation of data in Figures 2, 3, and 5 in order to improve readability. With these corrections, I recommend publication of this manuscript.

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