

Interactive comment on “Hadal water biogeochemistry over the Izu-Ogasawara Trench observed with a full-depth CTD-CMS” by Shinsuke Kawagucci et al.

Anonymous Referee #1

Received and published: 30 April 2018

This manuscript reports reasonably interesting and unique dataset regarding the hadal water biogeochemistry in the Izu-Ogasawara Trench. Indeed, this kind of dataset is relatively rare I think it should eventually be published, but before that some of my concerns should be appropriately addressed:

The authors state that 1 nM of methane is the threshold for microbial aerobic methanotrophy in Page 9 Lines 17-18, but the measured CH₄ concentrations are all <1 nM within hadal depth and they attributed the change in $\delta^{13}\text{C}$ of CH₄ to the kinetic isotope fractionation induced by microbial methane consumption in Page 10 Lines 5-10. This is somewhat self-contradictory to me.

C1

I am not fully convinced by the conclusion that the slightly elevated CH₄ is due to sediment resuspension because 1). Not all transects within hadal depth show increased CH₄ concentrations. Since sediment remobilization is common in hadal sediments, I would expect most of the hadal water has higher than background CH₄ concentrations if not all. 2). O₂ penetration depths were shown to be around 15-20 cm in hadal sediments (Glud et al., 2013; Wenzhoefer et al., 2016). How thick is the sediment layer do you expect to be removed in order to expose methane to the bottom water? 3). If CH₄ is sourced from sediments and oxidized to DIC in the hadal water, I would anticipate to see $\Delta^{14}\text{C}$ of DIC exhibiting a more depleting trend. But from your profiles, I don't see any old DIC input into bottom water.

The authors provided profiles of N and O isotopes of NO₃ and N₂O which are indeed quite rare dataset in the deep ocean. But it is a pity the authors did not discuss those data in detail and did not state why they measured them.

Minor comments Page 2 Line 12: People usually take water depth >6000 or 6500 m as hadal (Jamieson et al., 2010; Watling et al., 2013). Better to be consistent with previous publications.

Page 2 Line 29: Rewrite as “Manganese and iron were slightly enriched in the hadal water only and were background levels in the overlying. . .”

Page 3 Line 15: Rewrite as “Ultimately, a full-depth rated CTD-CMS. . .”

Page 3 Lines 15-16: This sentence is unclear. Modify it.

Page 4 Line 28: Precision of NH₄ analysis is not mentioned.

Page 5 Lines 26-27: This sentence is incorrect. Rewrite it.

Page 6 Section 3.1: There is no big change in potential density anomaly between 6000-7000 m. Instead, a marked gradient change is observed between 6000 m and above. A re-defined hadal water depth is not warranted and is unnecessary to me.

C2

Page 6 Line 24: Rewrite as “which was called the hadal mixed layer”. Actually I don’t feel comfortable to call it “mixed layer”. Because surface mixed layer is a well-mixed layer driven by wind, but what the mechanism for the homogeneous hadal water?

Page 7 Lines 7-8: This sentence is incorrect. Revise it.

Page 9 Line 24: “became constant below 8000 m”? below or above?

Page 11 Lines 25-26: Some more recent studies should be cited, e.g., Luo et al., 2018 Luo, M., Glud, N.R., Pan, B., Wenzhöfer, F., Xu, Y., Lin, G., Chen, D., 2018. Benthic carbon mineralization in hadal trenches: Insights from in-situ determination of benthic oxygen consumption. *Geophysical Research Letters*, 45. <https://doi.org/10.1002/2017GL076232>.

Page 12 Line 5: distinct from the abyssal counterpart

Page 12 Lines 24-26: This sentence is incomplete. Rewrite it.

Page 13 Line 5: in previous studies

Interactive comment on *Ocean Sci. Discuss.*, <https://doi.org/10.5194/os-2018-37>, 2018.