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Ocean Science Discussions

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

Shinsuke Kawagucci et al.

kawagucci@jamstec.go.jp

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We really appreciate fruitful comments from anonymous referee #1 on our manuscript. Our reply to referee #1 is below.

We agree with referee#1 on confusing description about a minimum threshold for microbial methanotrophy in Discussion paper. To clarify meaning, we revise it as "CH4 concentrations as low as 0.25 nM are ubiquitous in deep-sea water (Hirota et al., 2010; Son et al., 2014) and are probably regulated by a minimum threshold in microbial uptake for aerobic methanotrophy. " in a revised version of manuscript.

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Discussion paper

Referee #1 presented concerns about origin of anomalous methane in the hadal watar mass. At first, during this Discussion term, Mn concentrations of the IOT waters were obtained (new Fig 2). The Mn concentrations were more enriched in IOT hadal water than IOT abyssal water, which strongly supports our conclusion in Discussion paper, occurrence of sediment resuspension in the IOT hadal water. We thus add Mn concentrations and their interpretation into Results and Discussion sections of a revised version of manuscript. As stated in Page10Lines3-15 of Discussion paper, variation of CH4 concentrations and isotopic composition in the IOT hadal water were consistently explained by three processes: local CH4 input (from sediment resuspension), microbial CH4 consumption in water column, mixing with background CH4. Even if the resuspension providing CH4 is common in hadal water column, CH4 concentrations could decrease by microbial consumption and the dilution down to $\sim$0.25 nM (it is shown in Figure 6). Although referee #1 mentioned about thickness of trench bottom sediment, sediment resuspension we expect occurs not only from the axis bottom but also trench slopes (Page11Lines23-24). Even in macro-scale sediment having significant amount of O2, micro-environment within the organic particle could be highly reducing and a situation for local methanogenesis as discussed previously (e.g., Sasakwa etal., 2008). The methanogenesis occurring in such microenvironment within the sediment and its release associated with resuspension would be a possible mechanism by which the observed CH4 anomaly was formed. The D14C-DIC change associated with DIC production through methanotrophy was considered negligible because concentrations of CH4 was at sub-nanomolar level, 6-7 orders of magnitude lower than DIC (2.3 millimolar).

As stated by referee #1, the Discussion parer lucks both description about our motivation for measuring nitrogen molecules and discussion about nitrogen dynamics. Our motivation analyzing nitrogen molecules came from previous microbiological study (Nunoura et al. 2015). That study revealed "hadal biosphere" and considered that the lateral supply of sedimentary organic matter from the trench slope and the relevant nitrogen metabolisms were the key mechanisms to develop the biosphere. We describe

this motivation into the revised version. On the other hand, we cannot further discuss nitrogen dynamics in the hadal water because concentrations and isotope composition were constant demonstrating little processes driving nitrogen cycle there.

About definition of "hadal" depth, this study uses 7,000m based on accurate and precise observation of hydrographic properties with CTD-CMS (Page6Lines23-27). We disagree with referee#1 who stated "no big change in potential density". As stated at Page6Lines23-27, there was significant changes in the density. We believe that the redefinition for the IOT hadal water boundary of 7,000m in this study is reasonable. On the other hand, of course we know that 6,000m or 6,500m are usually used and accepted in general. To avoid confusing readers, we thus add statement into Introduction section as "the hadal waters (deeper than 7,000 m in this study but usually 6,000 or 6,500 m (Jamieson et al., 2010; Watling et al., 2013), see Section 3.1)".

A term "mixed layer" usually means hydrographically homogeneous water mass or like that regardless to the driving force for mixing. In fact, "bottom mixed layer" for example has been used in numerous papers (we can easily find them on Google Scholar). We thus decide to keep using a term "hadal mixed layer" in this study.

We agree with the other minor specific comments, and the manuscript has been revised accordingly.

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

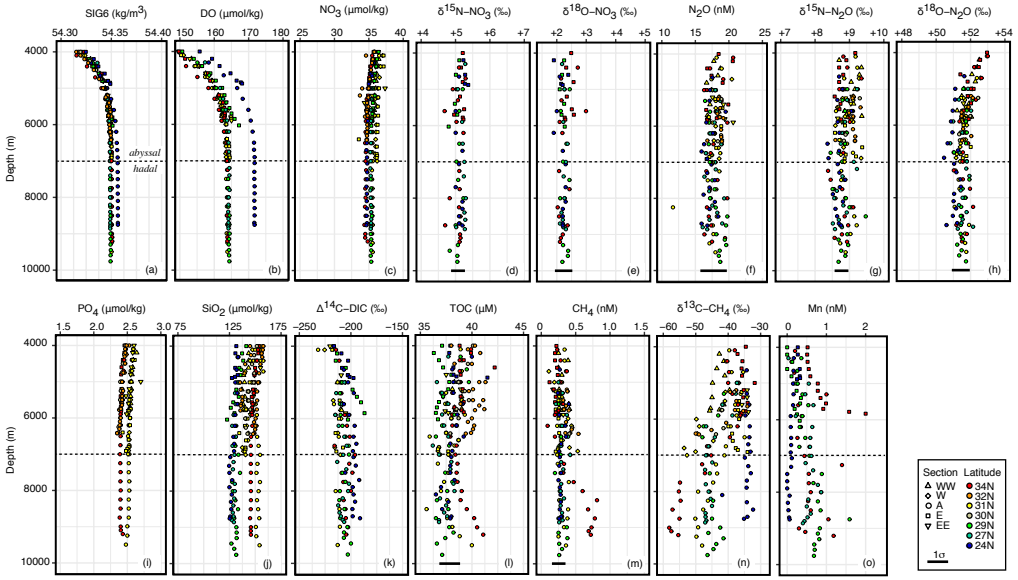

Figure 2 (Kawagucci et al.)

Fig. 1. new Fig 2

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Discussion paper