

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

Received and published: 18 May 2018

We really appreciate comments from anonymous referee #2 on our manuscript. Comments are fruitful for us. Our reply is below.

Referee #2 suggests that the manuscript expands interpretation of nitrogen molecules concentrations and isotopic composition. Several sentences presenting preliminary interpretation of the dataset obtained have been added into Results section of the revised version. To be honest, however, it is difficult to expand discussion about nitrogen dynamics due to two aspects. Firstly, for abyssal-hadal waters, concentrations and isotopic composition of N₂O and nitrate of the IOT water varied little and were

C1

consistent with those of the deep-sea Pacific water reported so far (Page7Lines16-18 and Lines25-26). It means negligible transformation of nitrogen molecules in the IOT deep-sea water. It is supported by nitrification rates examined that were too low (<0.02 nM/d) to impact on vertical profiles of natively abundant nitrate (~35μM) and N₂O (~20nM) and their isotope composition. Secondly, for shallow waters (0 - 2,000 m depth), vertical intervals of seawater samplings are too rough (see Figure S1 and Table S1) to provide fruitful discussion about the nitrogen dynamics that are very complicated spatio-temporally (e.g., Casciotti 2016). Nevertheless, all the analytical results including shallow water are available as TableS1. We believe that the dataset can be contributed for future studies focusing on nitrogen dynamics in shallow water at IOT region or Pacific Ocean if we provide little discussion in this study. Again, a bit more interpretation of nitrogen molecules has been added into Results section of the revised manuscript.

Followings are replies to specific comments.

*A reference and details of DO measurement have been described in the revised version manuscript.

*About name of the d¹³C value of background CH₄, we cannot catch the meaning of referee#2 comment. We name "deep Pacific seawater-sourced background CH₄" in the revised manuscript. Transformation processes and isotopic effects on methane d¹³C in the IOT hadal water have already been discussed at Page3Lines3-15 and Figure 6 while those in the deep Pacific seawater cannot be discussed in this study (it requires basin-scale observation and detection of spatial changes of conc./da¹³C of CH₄).

*Preliminary analyses of hydrographic properties and interpretation from a viewpoint of physical oceanography such as current directions are supportive for our conclusion as well as previous observations. However, detailed analyses are now conducted and will be presented as a separated paper focusing on hydrography (Uchida et al. in

C2

preparation) as stated Page7Lines1-2.

*We are very interested in (but cannot distinguish) whether the hadal water CH₄ had been present in porewater as dissolved gas or sediment surface as sorbed gas before the release to water column. For this issue, we will collect sediment cores at trench slopes and axis bottom and analyze CH₄ concentrations and isotope composition with different ways for sediment processing.

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