

Interactive comment on “Strong linkages between surface and deep water dissolved organic matter in the East/Japan Sea” by Tae-Hoon Kim et al.

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Dear Editor and Reviewer

Thank you for sending the reviews of our manuscript (original ms#: bg-2017-8) entitled "Strong linkages between surface and deep water dissolved organic matter in the East/Japan Sea". Our response to each point suggested by Dr. Hawkes is as follows:

Major comments:

-Page 2 line 30: There is no mention of the use of reference materials (e.g. the sea-water reference material from Hansell's lab). If no references were used, this should be mentioned. Maybe the results can be compared with another study from the same waters.

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=> We used reference materials for verifying DOC analysis. In the revised version, we stated that “The reliability of the measurements was verified on a daily basis by analysis of DOC-certified seawater samples (DSR: 44–46 μM for DOC, University of Miami). The results were in good agreement with certified DSR values (deviation: <5%)”.

- Page 4 Line 15 and many other places: When values are stated as \pm something, are they referring to one standard deviation? This should be stated (i.e. (mean \pm SD)). If this is the case, I don't think the values are significantly different on page 4 line 15.

=> Yes, they are standard deviations. In the revised version, we stated that “Most values are expressed as the mean \pm standard deviation (SD) in this study” in Materials and Methods section. The lowest concentration of DOC as well as THAA was found in the southern area of the EJS and linked to the water circulations in the EJS. We stated that “There is no notable DOC anomaly in the EJS that would indicate significant lateral inputs from rivers or other sources (Kim et al., 2015). This is consistent with the fact that there are no major rivers that drain into the EJS, which is fed exclusively by the Pacific Ocean. Abyssal circulation in the EJS has relatively strong cyclonic flows along the basin periphery and sluggish flows in the interior region (Fig. 1; Senjyu et al., 2005). The lowest concentration of DOC was found in the southern area of the EJS along the abyssal circulation, with a similar trend of AOU (apparent oxygen utilization) (Kim et al., 2015)”.

-Page 4 Line 31 onwards: They talk about the reactivity of THAAs in the EJS compared with BATS and HOT, but there is no indication of how long a time passes between the sampling points in each case. A comparison of decrease in concentration alone cannot be used to talk about reactivity, without time. This needs an extra paragraph of discussion.

=> The reactivity of THAA is indicated by the C-normalized yield (%DOC as THAA). We stated that “Bioassay experiments have demonstrated that THAA DOC yields decline

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with increasing biodegradation over time and are indicative of THAA reactivity (Davis and Benner 2007, Davis et al. 2009). In addition, the global ocean distribution of THAA yields is consistent with bioassay observations (Kaiser and Benner 2009)".

-Page 5 Line 31 onwards: They mention a 'significant but weak' correlation between two variables without providing any statistical data or graphs, only a table. I think this sentence should include at least an r^2 , p value and n.

=> In the revised version, added r^2 value (0.001) and sample number (n=66).

-Page 6 lines 9-25: They state that D-amino acids are more resistant to decomposition, yet the ratio of D:L is lower in the deep water than surface water. These two things don't seem to connect, please clarify.

=> In the revised version, t-tests were done in order to compare the ratios of D:L between the surface and deep waters. We stated that "the average D:L ratios between surface layer and deep layer were not significantly different ($p > 0.05$)". Deleted "with a slight decrease observed at depth".

-Section 3.5: I don't understand how they make the step from the reported data to the statement: "our results suggest that the shallowing or slowdown of deep-water formation in response to atmospheric warming would bring about a considerable decrease in the oceanic storage of bioavailable DOM and a consequent positive feedback in the climate system". This needs to be discussed more thoroughly.

=> In the revised version, we added a figure about the variability of sea surface temperature and air temperature from 1932 to 2009 in the East/Japan Sea in order to explain linkages between storage of bioavailable DOM and climate change. We stated that "The increasing trend in average annual sea surface temperature and average winter sea surface temperature near Vladivostok, the most northern part of the Japan Basin, was synchronized with the warming trend (2oC) of winter air temperatures (December – February) from 1932 to 2009 in the EJS (Fig. 7)".

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General comment: There is a lot of discussion of the trends in shallow vs. deep waters, but very little discussion of the lateral data between stations (i.e. figures 2,3 and 5). Some of the stations seem to be significantly different from each other and I would like to see a little discussion on whether these changes fit with the other trends, for example degradation of L-amino acids in comparison to D- or loss of DON preferentially over DOC. What is the circulation time difference between the different stations in deep water?

=> In the revised version, we stated that “There is no notable DOC anomaly in the EJS that would indicate significant lateral inputs from rivers or other sources (Kim et al., 2015). This is consistent with the fact that there are no major rivers that drain into the EJS, which is fed exclusively by the Pacific Ocean. Abyssal circulation in the EJS has relatively strong cyclonic flows along the basin periphery and sluggish flows in the interior region (Fig. 1; Senjyu et al., 2005). The lowest concentration of DOC was found in the southern area of the EJS along the abyssal circulation, with a similar trend of AOU (apparent oxygen utilization) (Kim et al., 2015)”. In addition, the DOC and DON concentrations in the deep EJS were remarkably stable, and neither systematic nor significant differences were observed among the stations. However, the THAA data provide a sensitive indicator (%DOC) of bioavailable DOC throughout the water column.

Minor comments: - Page 1 Line 26: Should this read 'surrounded by the Korean Peninsula...?' – The Authors presumably know better than I.

=> As shown in Figure 1, the East/Japan Sea is surrounded by Korea, Japan, and Russia.

- Page 1 Line 29: The Korea/Tsushima Strait is not indicated on the map, which would be useful.

=> In the revised version, indicated the Korea/Tsushima Strait in Figure 1.

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- Page 2 Line 30: 'Based on colorimetric analysis' (no 'the'). Is it possible to reference the method used here?

=> In the revised version, deleted 'the'. We stated that "The reliability of the measurements was verified on a daily basis by analysis of DIN and DOC-certified seawater samples (MOOS-1: 23.7 ± 0.9 μM for DIN, National Research Council; DSR: 44–46 μM for DOC, University of Miami). The results were in good agreement with certified DSR values (deviation: <5%)".

-Page 3 Line 22-23: This implicitly assumes that all organic matter in these deep waters comes from surface waters. Can this be substantiated?

=> According to Kim et al. (2015, Scientific Reports), the $\delta^{13}\text{C}$ -DOC in the East/Japan Sea (EJS) ranged from -20.4 to -21.7% (avg.: $-21.3 \pm 0.4\%$) and showed constant values in deep waters. This result indicated that the source of DOC in the deep EJS is found to be of marine origin. The DOC and DON concentrations in the deep EJS were remarkably stable, and neither systematic nor significant differences were observed among the stations.

-Page 4 Lines 10-11: Amino acids are stated in μM , I think this should be nM.

=> corrected in the revised version.

-Page 4 Line 24: I don't think the 'normalized yield of THAA' is a commonly understood concept, so it should be briefly explained here.

=> In the revised version, we stated that "The DOC- and DON-normalized yield of THAA was calculated as the percentage of DOC and DON measured as amino acids". The formula for calculating the THAA yield could be shown in the Materials and Methods.

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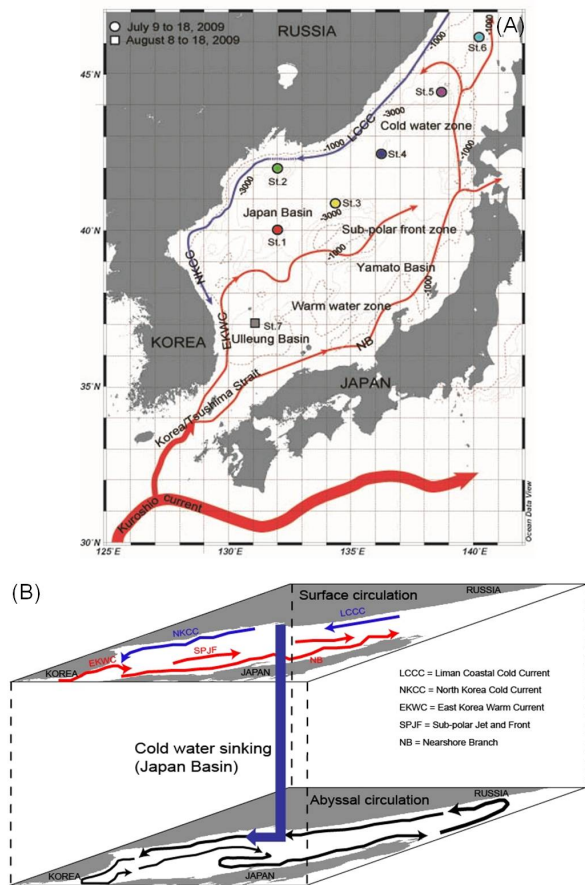


Fig. 1. Figure 1

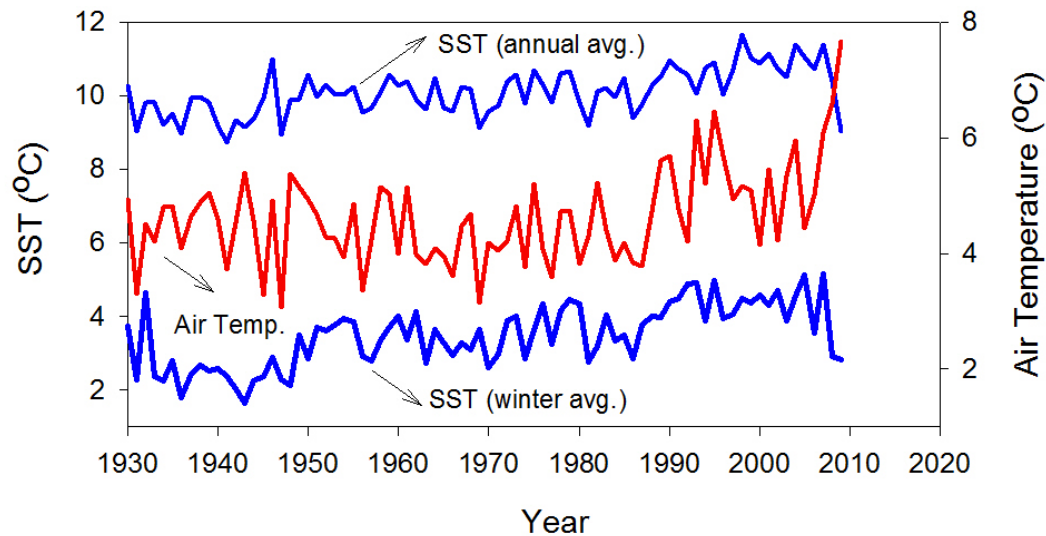


Fig. 2. Figure 7

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