Referee #3:

Ding et al., report dissolved organ carbon (DOC) data for the shelf-edge and slope regions in East China Sea (ECS) and the Kuroshio Extension (KE) in the northwestern North Pacific (NP) during two cruises carried out in 2014-2015. The main goal of this paper is to demonstrate that hydrodynamical processes play an important role in the distribution of DOC as well as of nutrients, impacting primary production and ecosystems functioning in this region (P2, L33-35). The main shortcomings of the manuscript are: (1) No data about nutrients, primary production, nor bacterial abundance or production are presented and discussed to support the main goal of the paper.

R：We totally agree with the referee #3 that the shortcomings of our study is that we didn't measure nutrients, primary production and bacterial abundance. The other Referees also pointed out this. When people study DOC in the surface ocean, especially in the euphotic zone, these parameters such as nutrients and primary production are very critical for DOC distribution and cycling. In our study, we try to see and compare the hydrodynamic influence on the distribution of DOC in the whole water column. Our focus is not on the biogeochemical cycling of DOC in the regions. On the same cruises, we did measure total dissolved nitrogen (TDN) but not inorganic N (DIN) and organic N (DON). As suggested by the reviewers, we should not discuss too much on nutrients due to the lack of the data. As our response to the other Referees' comments, we have used the data of integrated Chl *a* (35-44 mg m^{-2}) and primary production (483-630 mg C m^{-2} day^{-1}) from a spring April cruise in 2008 for reference in L 337-339. These data were reported by Nishibe et al., 2015 in Journal of Oceanography. We basically found that DOC was not correlated with DON for the stations. We believe that DON is a much more sensitive microbial parameter than DOC and the cycling of DON is more compleceted than DOC. In our future study, we will definitely consider all these parameters.

(2) hydrodynamical processes are investigated mainly using temperature. The authors use the linear correlation between DOC and temperature to demonstrate that hydrodynamical processes affect DOC distribution. I don't think this is a good approach since it is well known that vertical profiles of DOC and temperature have the same shape and that DOC accumulates in warmer waters (Carlson et al., 1994; Hansell and Carlson, 2001; Avril, 2002; Hansell, 2002; Santinelli et al., 2013), even if the mechanisms are still unclear. The correlation is therefore only due to DOC accumulation in the surface water when a well-developed thermocline occurs, but the increase in DOC concentration in the surface layer is due to a decoupling between

production and consumption processes, the low values in the deep cold waters are mainly due to DOC removal. As a consequence the correlation between DOC and temperature does not give any information about hydrodynamical processes.

R: We agree with the comment that the discussion of linking DOC distribution to physical mixing is not thoroughly developed in our discussion, especially for the correlation between DOC and temperature. The changes of DOC and temperature could be covariation with depth as suggested by the other Referee. On the other hand, there are must reasons to cause the changes. In physical oceanography, water temperature and salinity are important parameters related to the water mass movement and mixing in the ocean. As suggested by Referee #2, we tried to remove DOC and temperature data in the upper 200 m for the ECS and 250 m for KE to eliminate biological effect and to replot the data. We also tried to filter out the depth effect by comparing DOC *vs.* temperature at the same depth across all stations. In fact, the correlations were worse, or there was no good correlations at all for each depth. We believe that the correlation between DOC and temperature in the water column is due not only to the physical mixing but biological influence as well because temperature also influences microbiol activities. We discussed more biological influence for DOC in the revised MS. We also added a strong supporting evidence as Figure S2 (Supporting Information), as plotting DOC and T data cited from four nearby stations collected in the CLIVAR 2004 cruise P02 Line. There is a very strong correlation between DOC and T for these four stations ($r^2 = 0.95$, $p<0.001$) mainly because they were in a same water mass hydrodynamic region south of the KE. We believe that in addition to salinity and water temperature, hydrodynamic mixing is a dominant process affecting the observed distribution of DOC in the KE region, especially in the upper 800 m depth.

(3) The authors also use the linear inverse correlation between DOC and dissolved inorganic carbon (DIC) to support that hydrodynamical processes affect DOC distribution, but the correlation can be due to the microbial mineralization of DOC to CO2, as a consequence it is expected that in old waters DOC is low and DIC is high, whereas in surface waters DOC is high and DIC is low.

R: DIC and its $\Delta^{14}C$ values have been used as conservative tracers to study the sources, movement and mixing of different water masses in the ocean. For example, in the WOCE and CLIVAR Programs. Decomposition of DOC in the seawaters could contribute to the correlation between DOC and DIC, but this effect is relatively small when comparing the DOC pool with the much bigger DIC pool in the ocean.

(4) The data set is of good quality and the data look interesting, but the paper cannot be published in this form. Most of the data are not presented in the results section and the discussion is confused and the main conclusions are not supported by the data. Most of the discussion should be reworked and additional data should be presented to support that hydrodynamical processes play an important role in the distribution of DOC as well as of nutrients, impacting primary production and ecosystems functioning in this region or the goal of the paper should be changed.

R: We thank the positive comments of the Referee on the data set. The main point we want to make based on this study is to link DOC distribution to physical hydrodynamic mixing in the East China Sea and especially in the Kuroshio Extension. We have added additional results of DIC and Δ^{14}C-DIC data, and tried to discuss the relative role of hydrodynamical processes and biological processes more in the revised MS as combined with the earlier comments by Referee #2. We estimated the conservative DOC based on the two water masses mixing with their DIC-Δ^{14}C values, and further compared the estimated conservative DOC with field measured DOC to estimate the biological contribution to DOC (production or removal). Correspondingly, we also added the DIC and Δ^{14}C-DIC information and the detailed biological contribution of DOC in the abstract. The goal of the paper now is not on primary production and ecosystems functioning in this region.

(5) The English needs an in depth revision.

R: Yes. The revised MS has been edited by professional English proofreading service.

Some suggestions, specific concerns, and questions are provided below.

Material and methods

(6) P7, L151-153, please indicate the batch of the CRM used, the expected and measured values and the statistics (number of samples analyzed, average values ± standard deviations).

R: Yes. We have added the used CRM batch and the statistics throughout the DOC analysis in L 154-157.

"(CSR Batch 13 with 41-44 μM DOC concentration, supplied by Hansell Organic Biogeochemistry Laboratory at University of Miami, USA). The standard deviation of deep-sea water reference throughout our measuring was ±1 μM, which was used as an index of our analytical precision."

(7) P7, L156-157, if you measured all the samples in duplicate, why don't you report the standard deviation on the vertical profiles in Figure 3?

R: Since the data lines in Figure 3 are close to each other, we do not plot the standard deviation on the vertical profiles. Instead, we have added the standard deviation for DOC (in the range of ± 0.1- 4.0 μM) in the main text L 162.

Results

(8) All the data discussed in the paper should be briefly described in this section, not only DOC and physical parameters (T and S). AOU, DIC and DICΔ^{14}C data are not presented at all.

R: In the revised MS, we provided the DIC and Δ^{14}C-DIC data and added the vertical profiles of DIC and Δ^{14}C-DIC in the new Figure 5. We have also discussed these results in Section 3.3 Concentrations and radiocarbon distribution of DIC in L 231-248.

In response to the earlier Referees' comments, we deleted the whole section on AOU due to the lack of measurements.

(9) P9, L206-209. This sentence is hard to follow, I recommend to rework it to improve clarity.

R: Yes. We have rephrased these sentences as following "Similar to *T*, the largest differences in salinity also appeared in the upper 700 m water column (the density range of 26.4-27.0 σ_t), where low salinity (34.49) was observed at the surface of Sta B2. The salinity decreased to 33.66 near 250 m and subsequently increased to values similar to those of the other stations at 2500 m. The salinity for the remaining seven stations (Stas. K2, A1-b, A4, A6, A8, B8 and B9) showed less variation in the surface layers (5 m) (34.76 to 34.98), and Sta K2 had the highest *S* (34.98) at the surface among all stations (Fig. 2b and Fig. S1)." in L 202-208.

(10) Section 3.2. it is really hard to follow the description of vertical profiles of DOC in Figure 3. The profiles are overlapped, making difficult to look at differences among the stations. Values between 700 and 1400 m at the stations located in the ECS are higher (45-54 μM) than those observed in the KE and in the oceans. Why? I think this is an interesting result that would deserve more attention and discussion.

R: Agree. Referee #2 also questioned the description way of DOC data in Figure 3. As suggested, we have redrawn the T-S-DOC diagrams in new Figure 4. Correspondingly, we have modified few statements for the DOC results in Section 3.2 based on the new T-S-DOC diagrams in the new Figure 4.

In addition, in the last paragraph in Section 4.2, we have added discussions combined the different DOC levels in the deep layers in L 443-446. “On the other hands, comparing with the deep DOC results in the slope region of the ECS, it can be seen that the deep DOC level in the KE was on average 10-15 μM lower than that in the ECS, implying that the possibility of lateral transport of DOC from marginal seas to the ocean interior and cycled in the deep ocean for very long time.”

(11) P10, L219. This sentence is not correct, the highest DOC values are at station K2, whereas the lowest ones at station B2.

R: We have changed this sentence to “The highest DOC value (65 μM) and the lowest DOC level (43 μM) were measured at the surface at Sta K2 and Sta B2, respectively.” in L 222-224.

(12) P10, L224-226, This sentence is not clear.

R: In the revised MS, we have clarified this sentence in L 227-230.

“whereas the concentrations were slightly higher in the 500-800 m depth at Sta B8 and Sta A8. The T-S-DOC diagrams showed that DOC concentrations decreased to much lower levels (36-44 μM) at all stations at $\sigma_t > 27.5$ (approximately below 1500 m depth) and remained constant in deep waters (Fig. 3b and Fig. 4b).”

(13) Figure 2. There is a mistake in the letters reported above the graphs. I would add to the figure the name of area the profiles refer to (ECS and KE). I would use KE instead of NP, since it is used in the text and the use of a different abbreviation is confounding.

R: As suggested, we have added the ECS and KE in the Figure 2, and used the KE region in the northwestern NP in the figure in L 685.

Discussion

(14) P10, L232-234. As reported in the general comments, the positive correlation between DOC and temperature does not imply that physical processes affect DOC distribution more than biological properties. Biological properties are also affected by temperature and without data about biological parameters the authors cannot exclude that DOC concentrations is controlled by biological processes.

R: We agree with the referee that the correlation of DOC with temperature should not be caused by physical mixing alone. Biological processes could also influence the distribution changes of

DOC especially in the upper 400 m depth. As response to the earlier comment, we tried to remove DOC and T data in the upper 200 m for the ECS and 250 m for KE to eliminate biological effect (production and decomposition) and replot the data, but the correlations were worse. We discussed more biological influence for DOC in the revised MS and also added a strong supporting evidence as Figure S2 (Supporting Information).

(15) P10, L237-239. Since this is a part important for the goal of the paper the results from Ge et al., 2016, should be presented and discussed more in depth.
R: As response to the earlier comments, we provided the vertical profiles of DIC and Δ^{14}C-DIC in the new Figure 5 and discussed these results more in Section 3.3 in L 231-248 as supporting evidence.

(16) P11, L242-243. The correlation between DOC and DIC could be explained by the biological mineralization of DOC to CO2.
R: Again, this is the major concern of Referee. As response to the earlier general comments, decomposition of DOC could contribute to DIC, but considering the pool sizes of DOC and DIC in the ocean, DIC is ~53 times higher than DOC especially in the deep (>1000 m) water where most of the DOC is refractory. This effect, therefore, is relatively small and will not affect the observed correlation between DOC and DIC.

(17) P11, L247-248. DOC values below 500 m in the ECS are higher than in the ocean. This observation would deserve more discussion.
R: As response to the earlier comment *(10)*, we have added discussion combined the different DOC levels in the deep layers in the last paragraph in Section 4.2 in L 443-446. The processes influence the DOC in the ECS is different than those in the KE. In the slope region of the ECS, the intrusion of the Kuroshio Current plays an important role affecting the observed DOC in the depth of 500 m.

(18) P11, L256-265. This paragraph is very confused and hard to follow. As an example it is not clear how "density showed the water mass in the studied area is composed of mixed Kuroshio and shelf waters."
R: We agree that there are some confusion in our statements. We have modified these sentences to clarify the confusion in L 285-299.

“The cross-section density (σ_t) plot (Fig. 7a) showed that the water mass in the studied area was composed of mixed Kuroshio and shelf waters. It appeared likely that the influences of Kuroshio intermediate water (500-800 m) on the bottom water at station Z4 and Stn. 11 brought low concentrations of DOC, high concentrations of DIC and low $\Delta^{14}C$ values of DIC. This intrusion of Kuroshio intermediate water diluted the DOC at Stn. 11 and Z4 (Figs. 7b-d). However, it appears that this upwelling intrusion had almost no effect on the surface water (<100 m depth) for the shelf stations. The intrusion of Kuroshio intermediate water could reflect a smaller-scale or eddy effect rather than a large-scale influence beyond Stn. 11 and Z4 (Ge et al., 2016). In contrast, as shown in Fig. 2a and Fig. 7, the intrusion of the saline Kuroshio water in the density range of 23.2-24.9 σ_t instead of the intermediate Kuroshio water not only contributed to the salinity maximum at approximately 150 m water depth at Stn. 1 and Stn. 7 but also affected the concentrations of DOC/DIC and the DIC-$\Delta^{14}C$ values, compared with the upper waters at the other three slope stations (Stas. Z1, Z2 and Z3) influenced largely by the Kuroshio Current (Figs. 7b-d). The river influence and inner shelf export of DOC appeared to be limited in the deep slope stations.”

(19) P11, L256-261. The description of Figure 5 is really confounding. At L261 the authors speak about upwelling intrusion, but the vertical distribution of density does not show any upwelling of waters.

R: See the last comment, we have modified these sentences to clarify the statements in L 285-291.

In L 291, we stated that the upwelling intrusion had almost no effect on the surface water (<100 m depth) for the shelf stations, which is consistent with the uniform density distribution in the upper 100 m layers in the new Figure 7a (old Figure 5a).

(20) P12, L278-294. It is not clear why the authors report the correlation between DOC and AOU. This correlation just reflects the vertical distribution of the 2 parameters that is driven by both biological and physical processes. In order to investigate the contribution of DOC mineralization to oxygen consumption, the correlation should be investigated in the core of the different water masses, not putting all the data together.

R: All Referees have concerns about the discussion of DOC vs. AOU. In our response to the earlier Referees’ comments, since dissolved oxygen concentrations were only measured for some stations in the ECS, not in KE. We feel that there are no sufficient data set to discuss the correlation between DOC and AOU. We therefore deleted the whole section on AOU.

(21) Section 4.2. It is not clear to me what is the main goal of this section. As above reported, the correlation between DOC and temperature and between DOC and DIC does not say anything about the control of DOC distribution by physical processes such as water masses circulation and mixing as stated by the authors at L323-325.

R: Again, we agree with the referee that the correlation of DOC with temperature should not be caused by physical processes alone. Biological processes could also influence the distribution changes of DOC. As response to the earlier general comments, we added a supporting evidence as Figure S2 (Supporting Information). The very strong correlation between DOC and T for the four nearby stations collected in the CLIVAR 2004 cruise P02 Line, mainly because they were in a same water mass hydrodynamic region south of the KE. In addition to salinity, water temperature is also a good parameter for mixing processes as we observed in the KE region. Furthermore, DIC and its $\Delta^{14}C$ values have been used as conservative tracers to study the sources, movement and mixing of different water masses in the ocean. For example, in the WOCE and CLIVAR Programs.

(22) P14, L329-331. It is expected that surface layer is characterized by high DOC concentration, low DIC and high $\Delta^{14}C$-DIC, this observation does not say anything about water masses mixing and its impact on DOC distribution.

R: Yes, higher levels of DOC were associated with lower DIC concentrations, and high $\Delta^{14}C$-DIC values were found in the surface layer with lower density waters ($\sigma_0 < 25.5$, water mass A). However, the lower density water mass A with high temperature and salinity only correspond to six stations (K2, A1-b, A6, A8, B8 and B9) in the south of KE axis, and do not include the surface layer of Sta B2. In order to clarify the water mixing influences, we have re-divided the water masses into four different parts (A, B, C and D) instead of the three parts, focused on the discussion of water mass C, and have changed the text in L 363-370.

"The denser water mass C with density levels of 26.4-27.1 σ_t around 500-800 m was likely originated from the subarctic gyre which had low temperature and salinity; and was transported by the south-flowing Oyashio Current along the western boundary to the KE region. This water is then mixed with the warm saline water mass transported by the northeast-flowing Kuroshio Current (Fig. 2b and Fig. S1). In contrast, the lower density water mass A with high temperature and salinity corresponding to the six stations (K2, A1-b, A6, A8, B8 and B9) in the south of KE axis are most related to the Kuroshio Current.”

(23) P14, L338-339 This sentence is not correct. Usually DOC accumulation occurs in high stratified waters, so it is not clear to me how “deep vertical deep vertical convection possibly affected the DOC accumulation [. . .]”

R: In general, DOC could accumulate in the surface layer due to the water stratification. But in the high latitude, DOC concentrations in the surface waters may be kept at low levels due to deep water penetrates into the surface by deep vertical mixing. We have modified this sentence to “Many results suggested that hydrodynamic processes, such as the deep water penetration by vertical mixing, possibly affected the DOC concentrations within the surface waters in the high latitude despite high primary production.” in L 371-373.

(24) P15, L351-364. Looking at figure 8, it is clear from salinity vertical distribution the occurrence of a layer characterized by a salinity minimum at about 700-1000 m. No clear pattern in DOC distribution is observed, indicating that there is no link between the occurrence of this water mass and DOC distribution. In addition, DOC at station B8 shows high values up to 1500 m, but there is no clear correspondence with the occurrence of different water masses, nor with the water column structure.

R: In the old Figure 8b now Figure 9b, it can be seen relatively high DOC from ~200 to 1500 m depth at stations B9, B8 and A1-b, but low DOC concentrations in the entire water column at Sta B2 and in the upper 100-700 m water column of Sta A4.

For Sta B8, the positive difference (~6μM) between the measured and conservative DOC concentrations can represent other biological processes that secondarily modulate DOC in the KE region, and account for approximately 11% of the measured DOC indicating a net DOC increase from biological processes, accompanied by the relatively low DIC concentrations shown in Fig. 9c. We further estimated the relative contributions of each process in shaping the DOC distributions in the ECS and KE region, respectively. For the KE region, we added the discussion “positive ΔDOC values (~ 6 μM) that accounted for about 11% of the measured DOC at Sta B8” in L 422-423, and “However, the biological consumptions of DOC would account for 8-20% of the total DOC pool based on the negative ΔDOC values (2-8 μM) and the measured DOC at Stas. B2 and A4.” in L 427-429.

(25) P15, L364-366 It is not clear to me, how using dissolved inorganic radiocarbon

measurements the authors demonstrated the "same strong influence of the southward Oyashio-transported subarctic intermediate water mass via meso-scale eddies [. . .]".

R: More detailed discussions of the DIC concentrations and Δ^{14}C-DIC values have been published in Ding et al., (2018). Briefly, the significantly lower Δ^{14}C-DIC values at stations B2 and A4 than that at other stations in the upper 700 m could reflect the intrusion of the subarctic water with high DIC concentrations and low Δ^{14}C-DIC values carried by the Oyashio Current. We have clarified the statements by changing the sentence as "Using the significantly low Δ^{14}C-DIC values at stations B2 and A4 in the upper 700 m depth in the KE region, we also demonstrated the same strong influence of the southward Oyashio-transported subarctic intermediate water mass via meso-scale eddies."

(26) P16, L381, The authors discuss DOC data in the KE region, but they don't discuss the data in ECS, that in my opinion deserve more attention since the values are higher than those usually observed in deep waters.

R: In the response to the earlier comments, we have added a few discussions combined the different DOC levels in the deep layers of ECS and KE in the last paragraph in Section 4.2 in L 443-446.

(27) P16, L388-390. If radiocarbon data on DOC are available they should be included in the paper and discussed.

R: The old DOC-^{14}C ages presented here was mainly used to illustrate the refractory nature of DOC in the deep water, and the distribution of Δ^{14}C-DOC values in the northwestern North Pacific would be discussed in details in another paper.