Authors corresponding reply to the Reviewer's comments.

Referee #1:

Ding et al look at the dissolved organic carbon (DOC), dissolved inorganic carbon (DIC), D14C-DIC and hydrographic (T/S/σ) values to assess whether DOC distributions at the shelf-edge and shelf-slope regions of the East China Sea (ECS) are more affected by hydrodynamic processes (mixing of Kuroshio and Oyashio currents) or biological processes. They find that DOC in the ECS largely reflects mixing as opposed to biological processes, and that DOC oxidation only accounted for 18% of the oxygen consumption, thus implying that POC plays an important role in maintaining the biological pump as opposed to DOC. While I find the study to be simple and straightforward and potentially suitable for publication, it requires some substantial revisions.

R: We thank the positive summary of the Referee #1 on the manuscript. The following are our responses addressed to each comment made by Referee #1.

1) First of all, the authors suggest that since DOC and temperature correlate, that mixing must play a major role in the DOC distributions. While I find their reasoning to be completely valid here, they don't counter their argument by showing that DOC does not correlate with microbial processes. Are there bacterial abundance (BA) data from those seven stations? Do the BA correlate with DOC? If they don't, that will strengthen their argument that POC would be what sustains the microbial communities in that region.

R: Yes, we found the overall correlation of DOC concentrations and water temperature in the studied region. We agree with the Reviewer that this correlation should be applied only to DOC below the euphotic zone in the deep water. When DOC is produced in the surface water, microbial degradation is the major process causes the rapid decrease of DOC with depth. Our data as plotted in Figure 3 clearly show this. In the deep water, bacterial activities, of course, still play important roles to regulate the distribution of DOC, but ^{14}C measurements of DOC have shown that the DOC in the deep ocean (>1000 m) are highly refractory with ^{14}C ages of 6000 years in the N Pacific and 4000 years in the N Atlantic. Therefore, DOC in the deep ocean, like DIC, can be treated as conservative. We added more discussion to clarify this.

2) Secondly, the authors discuss the DIC and $\Delta^{14}C$-DIC values from that region, but never report their values in the Results. The Results section only includes hydrographic data and

DOC. If the authors are reporting these original data to support that DOC is distinct in water masses, these data should be reported in the results and discussed in greater detail. AOU should be reported in the results as well.

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 discussed these results in Section 3.3 Concentrations and radiocarbon distribution of DIC in L 243-260.

Both Referees have concerns about the discussion of DOC vs. AOU. 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 and to better response to the referee's questions. We therefore deleted the whole section on AOU.

3) Thirdly, the data reported here are limited, and these data are from either the shelfedge, or near the KE (as seen in Fig. 1); there are no data in between these two extremes. As the authors show that DOC correlates with temperature, even though there are no DOC data, perhaps there are some publicly available temperature data along the Kuroshio current that the authors can use to support their claims.

R: This is a very good comment. We plotted DOC vs. temperature for four stations selected from the CLIVAR 2004 cruise Line P02 as Figure S2 as supporting evidence for our data. The plots showed a very strong linear relationship between DOC and T (r^2 = 0.95, p<0.001) for the four stations mainly because these four stations were in a same water mass and much stable hydrodynamic region south of the Kuroshio Extension.

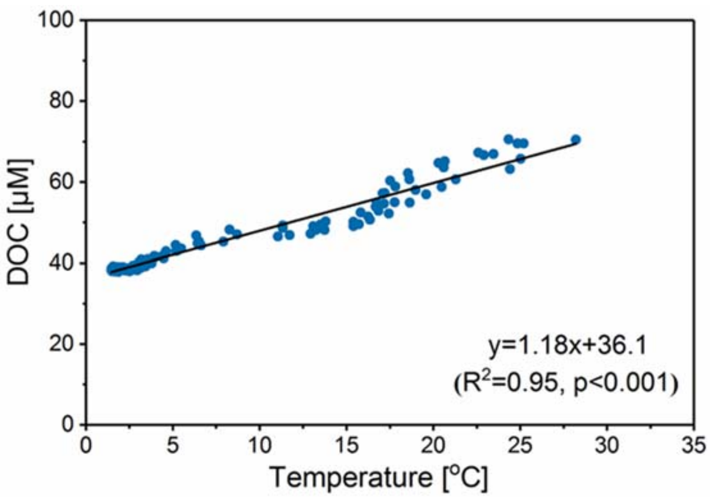

Figure S2. Correlation of DOC concentrations with water temperature for four stations selected from the CLIVAR 2004 cruise Line P02. The hydrologic and DOC data are from http://cdiac.ornl.gov/ oceans/ woce_p02. html.

4) In addition, Figures 5 and 8 look nice initially, but at a closer look, they are a bit misleading, as the data are quite spread out (and the data in the figures don't include all of the seven shelf stations and eight deep stations shown in Figure 1). Also, why is density listed as the conserved variable in figure 5, yet salinity is in figure 8? These two figures should be consistent. With those variables in mind, if the authors were to find more hydrographic data in the region to support the figures, that would be helpful (at least to show that the spreading of the data in the figures is a valid assumption). In addition, the x-axis on both of these figures is latitude, but the stations that are reported in each of these figures are not linearly spaced. I suggest at the very least putting a map with the section outlined in each figure to orient the reader.

R: In the old Figure 5a now Figure 7a, we have added the density variations of another two stations near the shelf-edge and slope regions in the ECS from a summer cruise in July 2011, in order to support the spread of the data in our results. In the old Figure 8a now Figure 9a, we have chosen the salinity as the conserved variable instead of density. The newly formed NPIW is characterized by a salinity minimum zone in the density range of 26.6–27.4 due to the along isopycnal mixing between the Kuroshio and Oyashio waters in the mixed region, and then the new NPIW is transported eastward by the KE as a low salinity tongue. The transactional distribution of salinity in Figure 9a could reveal the intrusion of fresh Oyashio water better than the density distribution in the KE region. Besides, we have also added the salinity from another five stations along the 35ºN transection in the NP to support our results. These data are downloaded from the Pacific Data Source in https://www.nodc.noaa.gov/ocads/. Also, as suggested by the Reviewer, we have put a location map in each new figure to illustrate the stations along the latitude transection in the ECS and KE regions (now Figures 7e and 9e).

5) Finally, there are some writing and style aspects of the paper that need to be improved. Several figure axes are miniscule and impossible to make out (see specific comments). In addition, the written English for the manuscript should be improved. There are quite a few grammar and wording issues that should be addressed. I pointed out some of them, but the authors would do well to send their manuscript to a proofreading service.

R: We feel sorry for this because we didn't send the manuscript for professional English proofreading and editing the first time. We will certainly do for the revised MS before we resubmit.

Specific comments:

6) Line 32: Suggest rephrasing sentence for clarification: "carried by the Kuroshio and Oyashio western boundary currents…"

R: Yes, we have rephrased this sentence as "carried by the Kuroshio and Oyashio, the two dominant western boundary currents in the region"

7) Line 41: "compounds" is not entirely correct because that is not considering the structural isomers…there could be more actual compounds than 20,000. Please replace "compounds" with "molecular formulae".

R: Replaced "compounds" with "molecular formulae".

8) Line 43: English: "plays", not "play"

R: Yes, corrected.

9) Line 65: English: replace "about a" with "there is a" and remove "was seen".

R: Yes, corrected as suggested.

10) Line 71: replace "such as" with "from"

R: Yes, corrected as suggested.

11) Line 75: add "the" before "Kuroshio".

R: We added "the" before "Kuroshio".

12) Lines 92-94: English: Consider correcting to: "DOC observations on WOCE (World Ocean Circulation Experiment) and CLIVAR cruises were collected at Line P02 stations along a 30 °N latitudinal transect, yet the distribution of DOC near the KE was not investigated during these cruises."

R: We have changed this sentence to "DOC observations in the WOCE (World Ocean Circulation Experiment) and CLIVAR cruises were collected at Line P02 stations along a 30° N latitudinal transect, but the distribution of DOC near the KE was not investigated during these cruises.".

13) Line 110: Needs clarification: "it is affected": What is affected? The DOC? The currents? Please clarify.

R: In the revised MS, we have clarified this. The hydrographic characteristics and oceanic processes are affected largely by the northward-flowing Kuroshio Current, which impinges on the shelf break, and a branch that enters the ECS

14) Line 117: Remove "which".

R: Yes, removed.

15) Line 152: replace "Dr. Hansell" with "Hansell Biogeochemistry Laboratory"

R： Yes, we replaced "Dr. Hansell" with "Hansell Biogeochemistry Laboratory".

16) Line 185: Replace "were" with "are"

R： Yes, replace.

17) Line 188: spell out the number 7

R： We have replaced "7" with "seven".

18) Line 222: Significantly? Are they statistically lower? Otherwise please avoid using that word.

R： We have replaced "significantly" with "visibly".

19) Line 226: Where are the results for the DIC and D14C-DIC data?

R： See the comment earlier, we have added the vertical profiles of DIC and $\Delta^{14}C$-DIC in the new Figure 5, and provided the results in Section 3.3 Concentrations and radiocarbon distribution of DIC in L 243-260.

20) Line 233: yes DOC has a good relationship with temperature, but does it also have a relationship with bacterial abundance? This seems like a pretty definitive statement, so at least provide some evidence that DOC does not correlate with a microbial parameter.

R：Unfortunately, the bacterial abundance was not measured for the studies. However, we found that DOC was not correlated with dissolve organic nitrogen (DON) for the stations. We believe that DON is a sensitive microbial parameter than DOC.

21) Line 290: Remove "apparent".

R： As responded to the earlier comment, we deleted the whole section on AOU including this sentence.

22) Line 292: "statistically significant", not "significantly statistical"

R：Deleted this sentence which is included in the AOU section.

23) Line 293: Of course AOU and temperature have a high correlation; the temperature of water plays a role in the solubility of dissolved oxygen. Please advise and adjust this statement.

R：As responded for the last two comments, we deleted the whole section on AOU in the revised MS.

24) Line 374: How is ΔDOC calculated? There is no mention of how the authors determine a conserved DOC? Please clarify.

R：We referred the conservative DOC (DOC^{0}) as the concentrations of DOC derived from the two water masses mixing model as expressed in L 425.

ΔDOC can be calculated from the difference between the measured and conservative DOC concentrations, as clarified in ($\Delta DOC = DOC_{measured} - DOC^{0}$) in the revised MS L 426.

25) Line 404: Again, use of "significant".

R：Yes, corrected.

Figures

26) Figure 1: The font on the z-axis is especially tiny and unreadable. The fonts on the x and y axis should probably be larger as well.

R：We have adjusted the font in the figure.

27) Figure 2: Have the authors considered putting these figures in T/S space, as opposed to vs. depth? What is their reasoning behind using depth? With T/S space, they can distinguish the different water masses that are present in the system (and they would need less subplots).

R：As suggested, we have redrawn the T-S diagrams in new Figure 2 and put the hydrographic profiles in the attachment as Figure S1 for reference. We have also modified the Section 3.1 according to T-S diagram of the new Figure 2.

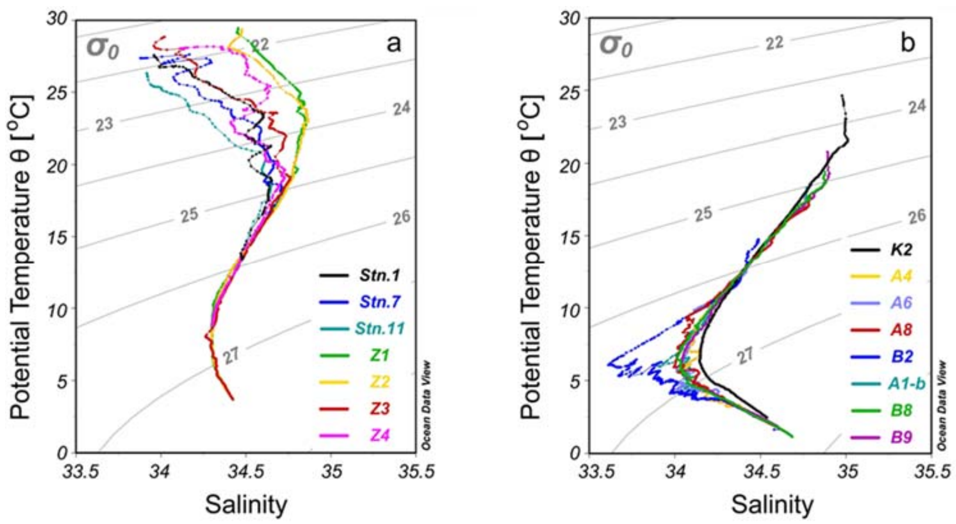

Figure 2. Potential temperature versus salinity plots (T-S diagram) for the sampling stations. (a) Seven shelf-edge to slope stations in the ECS and (b) eight deep stations in the KE of northwestern NP.

28) Figure 7. The font is tiny and impossible to see.

R：We have enlarged the font in the old Figure 7 now Figure 8.

29) Figure 8: This is intrusion is interesting and the data look nice, but it appears to be only five stations spread out across 8 degrees of latitude. I understand that sampling is limited here, but the colors are really spread out over a large range, which can be misleading. How do we know that this is truly what the hydrography looks like there? As salinity is shown there, there must be some other datasets around with more salinity in the region. I suggest that the authors expand their data for salinity at least, to show a more complete picture of the currents in the region. The same general idea goes for Figure 5.

R：We agree with the Reviewer's comments for the old Figures 5 and 8. As responded for the earlier comment, for the old Figure 8a now Figure 9a, we added more salinity data from five other stations along the 35 ºN transection to support the spread of the salinity variations. Again, the salinity data are downloaded from the Pacific Data Source at https://www.nodc.noaa.gov/ocads/. The same expand changes for density in the ECS were also made for the old Figure 5a, now Figure 7a as responded above for comment 4.

30) Figure 9: The x-axis range is odd. Why not zoom in to better reflect the regression?

R：We have zoomed in the X-axis by using "(mmol/kg)$^{-1}$" as the unit for [DIC]$^{-1}$ instead of "(μmol/kg)$^{-1}$".

Referee #2:

This manuscript provided the DOC concentrations and distribution in the East China Sea (ECS) and Kuroshio Extension (KE) region in the northwestern North Pacific. Through the comparison of DOC concentrations among different stations that located under the influence of Kuroshio current and Oyashio current and the DOC distribution superimposed on top of the other parameters such as temperature, salinity, DIC, Δ^{14}C-DIC, and AOU, it was concluded that the observed DOC patterns were most likely attributed to the hydrodynamic mixing by Kuroshio current and/or Oyashio current water. Since there is scarce data on the DOC distribution in the ECS and KE regions, this study can help to establish a valuable database on the DOC values in those areas. However, the discussion of linking DOC distribution to physical mixing is not thoroughly developed in the manuscript. For example, (1) the authors discussed the linear regression between DOC value and temperature or DIC (Fig. 4) and used this as an evidence to support the important role of physical mixing in shaping DOC distribution, this data and discussion should be reprocessed and readdressed as this correlation is mainly due to the co-variation of those DOC, DIC values with depth. And DOC decrease with depth is more likely to be controlled by biological processes. The authors need to tease out the effects of physical mixing after filtering out the depth effects first in those discussion. The salinity depth profile patterns could be potential evidence to support physical mixing and intrusion of currents to certain depths in ECS and KE (for instance, the high salinity around 200m in ECS may from intrusion of saline Kuroshio current, and the low salinity around 300-700m in KE may result from intrusion of fresh Oyashio current), but this is not fully discussed in the discussion or result section. Discussion around the Fig.5 and Fig.8 is more convincing to show the physical mixing, which should be emphasized.

R: Yes, 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 agree with the Referee #2 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. This is also a question asked by Referee #1. Based on our data, we agree that in the euphotic zone, the rapid decrease of DOC was largely controlled by biological processes, namely microbial degradation of DOC. Therefore, we should consider this fact. 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 and replot the data. In fact, the correlations were worse than before. We believe that the correlation between DOC and T in the water column is due not only the physical mixing but biological influence as well because temperature also influences biological processes. 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.

(2) While the authors mentioned the mixing of Kuroshio and Oyashio water in the KE, they touched a little on the mixing model but did not provide well-developed discussion on the relative contribution of these two currents in terms of DOC on the surveyed stations. Also there is not enough comparison of the role of biological processes vs. physical mixing in shaping DOC distribution. Since one main conclusion from this study is to show the important role of physical mixing, some direct comparison or estimated percentage of each process that contributed to DOC would be helpful to support the conclusion.

R: Again, we agree with the Referee's suggestions that the relative percentage of the physical and biological processes should be estimated. We have calculated the relative contributions of each process in shaping the DOC distributions in the ECS and KE region, respectively.

For the ECS region, we have added the statements "Based on the calculated ΔDOC and the field-measured DOC, we further estimated that the bioavailable fraction of DOC could account for approximately 7% of the total DOC pool in this region. The value is comparable to the results (6.1% and 10% ± 5%) previously reported for the Kuroshio Current and the shelf-slope region of the South China Sea (Gan et al., 2016; Wu et al., 2017). Clearly, biological processes had a significant influence on DOC but were not the dominant controlling factor on the observed DOC distributions in the ECS." in L 325-331.

For the KE region, we added the discussion "the positive ΔDOC values (~6 μM) that accounted for approximately 11% of the measured DOC at Sta B8" in L 427-428, and "However, biological consumptions of DOC could 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 432-434.

(3) In addition, the discussion of DOC and AOU seems to be kind of random. The authors should lay out better what is the purpose of introducing the AOU in the manuscript here, is it to state the refractory quality of DOC or to show that dissolved oxygen is also more affected by mixing rather than biological process? If the authors want to include AOU to evaluate the DOC

oxidization, then more discussion is needed regarding what it really means and relating that to the DOC quality. Also the DOC vs AOU relationship should be evaluated on specific isopycnal layers to filter out the depth effects, rather than on pooled DOC data over different depth.
R: Both Referees have concerns about the discussion of DOC vs. AOU. 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 and to better response to the referee's questions. We therefore deleted the whole section on AOU.

Overall, major revision is needed for this manuscript especially in its results and discussion sections.
We thank Referee for the thoughtful review and detailed comments. These comments and suggestions are very constructive and helpful. Below are our responses to the specific comments.

Specific comments:
1) Abstract: The abstract should include some information of the DIC and Δ14C-DIC information, as those are important pieces of evidence in this manuscript to derive the role of hydrodynamic mixing.
R: Excellent comment. Yes, we have added the DIC and Δ^{14}C-DIC information in the abstract as "By comparing the DOC results with dissolved inorganic carbon (DIC) and dissolved inorganic radiocarbon (Δ^{14}C-DIC) measured from the same water samples," in L 27-29 and "Based on the previously reported DIC and Δ^{14}C-DIC values for the stations," in L 32-33.

2) Line 24: Any more details on what relative percentage of biological process vs. hydrodynamic mixing each contributes to the distribution of DOC?
R: Based on the DIC-Δ^{14}C isotopic mass balance, we calculated the conservative DOC concentrations of the two mixed water masses. Then by comparing the measured DOC concentration with calculated conservative DOC concentrations, we calculated the possible biological contribution of DOC in L 25-27.
"while the biological processes are estimated to account for 7% and 8-20% in shaping the DOC distribution in the ECS and KE regions, respectively."

3) Line 28: the sentence is not finished yet, so what does the 18% means, this suggesting of other processes (e.g. mixing) controlling AOU?

R: As response to the earlier comments, we deleted the whole section about AOU including this sentence.

4) Line 30: add below how much meters is defined as deep waters, "deep waters (below xxx m)"

R: We have added "(below 1500 m)" to define the deep waters.

5) Line 34: The manuscript doesn't talk about any nutrient, it is a little bit stretching here to say it is the important role of nutrient.

R: We agree with the Referee and we removed the "nutrients as well" from the sentence as suggested.

6) Line 38: Ocean is not the largest carbon reservoir on earth, crust is the biggest, and ocean is the second largest.

R: We have corrected the misstatement by adding "the second".

7) Line 39: not all DOC are active, delete "active"

R: Yes, we deleted "active" from the sentence.

8) Line 41: The FTICR analysis only capture the Solid phase extracted proportion of DOM and doesn't include isomers as well, so the actual individual compounds should be more than 20,000. To be safe, just say "over 20,000 individual compounds".

R: Referee #1 also questioned this statement. In the revised sentence, we stated "~ 20,000 individual molecular formulae".

9) Line 46: Talk more specifically on the biological processes, such as microbial respiration.

R: We have added the details of biological processes, as "biological photosynthesis and microbial respiration processes" in L 49-50.

10) Line 53-72: Here it talks about different processes (biological and physical) in shaping DOC distribution. Since this study will show physical mixing, rather than biological processes, dominated the role in shaping DOC distribution in the ECS and KE region. Would be helpful to provide some background on the relative role between biological processes vs. physical mixing *in other different ocean regions. Any literature on this comparison before?*

R: As suggested, we have added some background and references about the discussion of principal processes in controlling the DOC distribution in different regions in L 63-71 and L 79-81, and added the corresponding references in the reference list.

"However, many previous studies conducted in different coastal and open oceans have shown that the distribution of DOC appeared to depend, to a large extent, on the hydrographical structure and/or horizontal/ vertical water mixing (Hansell and Waterhouse, 1997; Hansell and Peltzer, 1998; Hung et al., 2007; Ogawa et al., 2003; Guo et al., 1995) and the secondary biological forcing superimposed on the physical forcing (Carlson et al., 2010; Wu et al., 2017). Based on a water mixing model, Wu et al. (2017) also reported that microbial degradation contributed 10% of the DOC removal and that physical mixing controlled the majority variation of the DOC pool in the northern South China Sea."

"Carlson et al. (2010) later confirmed DOC export by the meridional overturning circulation in the Atlantic Ocean and further estimated the export and decay rates of DOC during this water circulation."

11) Line 89: What does "reduce the very old DOC 14C-age" mean? The export of DOC makes it younger or older?

R: We have clarified that the export of young DOC would be enriched in Δ^{14}C-DOC values and make the DOC ^{14}C-age younger.

"the export of young DOC accompanied by the NPIW formation, resulting in an enrichment in the Δ^{14}C-DOC values and a reduction in the notably old DOC ^{14}C-age in the Pacific Ocean interior. "

12) Line 156: Samples were analyzed in duplicate sample from different vial or duplicate draws from same vial? Clarify.

R: We have added "from different vials" in this sentence.

13) Section 3.1: Should provide some information of the temperature and salinity on the end members of Kuroshio current and Oyashio current. It would help readers to compare these end member values with observed values in the studied stations.

R: Yes, we added the typical T and S values for the Kuroshio water and Oyashio water in Section 3.1 for comparative information.

14) Line 202-206: Interesting "S" shape, can develop some discussion on why salinity profile is in "S" shape. As mentioned above, it seems to me that the high salinity around 200m in ECS may come from intrusion of saline Kuroshio current, and the low salinity around 300-700m in KE may result from intrusion of fresh Oyashio current. This could be another evidence to show the important role of physical mixing in the studied regions.

R: Yes, this is the point of physical mixing. We have redrawn the T-S diagrams in new Figure 2 in order to distinguish the different water masses more clearly, and put the hydrographic profiles in the attachment as Figure S1 for reference. Accordingly, we have modified the Section 3.1 based on the T-S diagrams in the new Figure 2. It also can see the salinity minimum at the density range of 26.4-26.9 σ_t, indicated the intrusion of fresh Oyashio Current in the KE region.

15) Line 209: somewhere in this section, the authors should introduce the temperature and salinity of the end members from Kuroshio and Oyashio currents.

R: Information was added. See the response for the earlier comment of 13.

16) Line 215: Define your surface water? Top how much meters?

R: We have defined the surface water as the depth ≤10 m and $\sigma_t \leq 22.1$ in this sentence.

17) Line 217: Why sub-maximum? Related to subsurface chlorophyll max?

R: The subsurface DOC maximums at Stn. 1 and Z1 are not related to the chlorophyll maximum in our results. We have added a few discussions about the subsurface maximum in Section 4.1 in L 312-317 and added the corresponding references in the reference list.

"At Stn. 1 and Z1, the subsurface DOC maximums were not related to the chlorophyll maximum (data not shown) and could not accumulate in the developed stratification water column, as inferred from the σ_t distribution (Fig. 7a). Previous studies have confirmed that fixed sinking of particulate organic carbon (POC) could partition into the DOC pool, which could result in the subsurface DOC maximum usually observed below the euphotic zone (Druffel et al., 1992; Hansell et al., 2009; Karl et al., 1998)."

18) Line 226: Where is your DIC, 14C-DIC, AOU data? They are important component to support your physical mixing conclusion, should be included in the main text rather than the supplemental table. If this data have already been published in previous papers, just redraw the *figures or tables to fit into this manuscript and state that it is adapted from previous papers.*

R: As suggested, we have added the vertical profiles of DIC and Δ^{14}C-DIC in the new Figure 5 and briefly stated the results in Section 3.3 Concentrations and radiocarbon distribution of DIC in L 243-260.

As response to the earlier comments, we deleted the whole section on AOU.

19) *Line 234: Again, the correlation between DOC and temperature is mainly just due to covariation with depth. Should filter out the depth effect first, for example, compare DOC vs temperature at the same depth across stations.*

R: We clearly realized that the correlation between DOC and temperature is a major concern for the Referee. It could be a covariation with depth but there must reasons to cause the change. As suggested by the Referee, we tried to filter out the depth effect by comparing DOC vs temperature at the same depth across all stations. We found there was no good correlations at all for each depth.

20) Line 241-243: Why this correlation indicates physical mixing? Not convincing.

R: Again, this is the major concern of Referee. 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. However, DIC and its Δ^{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. We believe that in addition to salinity, water temperature is also a good parameter for mixing processes as we observed in the KE region. We added more discussion on this.

21) Line 263: In line 261, it just said there are little effects of upwelling intrusion to <100m in the shelf stations. Z4 not included as a shelf station? But Line 213 said Z4 is defined as shelf-edge station. Need clarification here.

R: We agree that there are some confusion in our statements. We have modified these sentences to clarify the confusion. With water depth of 400 m, Z4 is a shelf station in the ECS, but also close to the shelf break.

22) *Line 275-277: As mentioned above, provide some quantitative percentage to compare the relative role of biological processes vs. physical mixing in shaping DOC distribution. More*

well-developed discussion related to the dominant role of physical mixing and its comparison with biological processes are needed overall. Also should include some literature comparisons here.

R: Yes, we tried to discuss these question more in the revised MS as combined with the earlier related comments by the Referee. As we responded above, we estimated the conservative DOC based on the two water masses mixing with their DIC-$\Delta^{14}C$ values. We compared the estimated conservative DOC with field measured DOC to estimate the biological contribution to DOC (production or removal). In L 325-331, we added the discussion as "Based on the calculated ΔDOC and the field-measured DOC, we further estimated that the bioavailable fraction of DOC could account for approximately 7% of the total DOC pool in this region. The value is comparable to the results (6.1% and 10% ± 5%) previously reported for the Kuroshio Current and the shelf-slope region of the South China Sea (Gan et al., 2016; Wu et al., 2017). Clearly, biological processes had a significant influence on DOC but were not the dominant controlling factor on the observed DOC distributions in the ECS."

23) Line 281: DOC vs. AOU regression should filter out the depth effects as well. For example, should be reprocessed on specific isopycnal layers.

R: As response to the earlier comments, we deleted the whole section on AOU.

24) Line 292: What is the dissolved oxygen value of the end member from the Kuroshio current? Any way to build a conservative mixing model to estimate what percentage of AOU pattern is attributed to the physical mixing? Is it just the rest of 18% (i.e., 82%)?

R: As response to the earlier comments, we deleted the whole section on AOU.

25) Line 310: Any chlorophyll data from CTD to get some idea on primary production in the region?

R: Unfortunately, there were no chlorophyll data measured by CTD during the same cruise. 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 350-351. These data were reported by Nishibe et al., 2015 in Journal of Oceanography.

"(483-630 mg C m^{-2} day^{-1}), accompanied by high Chl *a* concentration and high column integrated Chl *a* values (35-44 mg m^{-2}) in April (Nishibe et al., 2015)."

26) Line 312: "around the axis", what axis?

R: Clarified to "around the KE axis".

27) Line 315: Modify this part to say more clearly. You mean primary productivity should be high in the north stations like Sta B2 And A4 and result in higher DOC concentration there, but in reality, DOC is low at Sta B2 and A4, indicating it is due to physical mixing, right?

R: Yes. We have added the sentence "The relatively high primary production should result in a high level of DOC in the stations located north and around the KE, but the measured DOC concentrations were rather low at Stas B2 and A4." in the former part in L 351-354 to support the statements more clearly.

28) Line 320-325: Again, need to filter out the co-variation (with depth) factor, reprocess the correlation data here.

R: We tried this but results look worse.

29) Line 329-335 and Fig.7: Need to related back those water masses to your studied stations, thus can further evaluate the effects of physical mixing. For example, are the dots of water mass C with higher densities in Fig.7 the stations in the north that is more affected by Oyashio current? Otherwise it would still be the effects of water masses from different depths.

R: Yes. We have re-divided the water masses into four different parts (A, B, C and D) instead of the three parts, and have changed the text in L 375-382.

"The denser water mass C with density levels of 26.4-27.1 $\sigma 0$ near 500-800 m 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 subsequently 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 was most related to the Kuroshio Current."

30) Line 370: Where is the ratio data? I cannot tell which dot is which station on Fig. 9. Need better way to show the exact ratio data for each station. The mixing of two currents is touched upon a little here, but not well developed. This should be discussed more thoroughly.

R: Yes, we added more discussion on this. If we took Δ^{14}C-DIC value of 50‰ for the Kuroshio

water and -220‰ for the NPIW of Oyashio (Ding et al., 2018), we were able to calculate the relative mixing contributions of the Oyashio and Kuroshio Currents for the five stations (Stas. B2, B8, A4, A8 and B9).
"For example, 55-58% Oyashio water could contribute to produce the observed Δ^{14}C-DIC values at the depth of 500 m in Stas B2 and B8 and 100% Oyashio water at Sta A4 and 96-100% Kuroshio water at Stas A8 and B9, respectively." in L 419-421.

31) Line 376: Can you use this to derive the percentage of biological process vs. physical mixing?
R: Yes. We have calculated the relative contributions of each process in shaping the DOC distributions in KE region, and added in the main text.
"the positive ΔDOC values (~6 μM) that accounted for approximately 11% of the measured DOC at Sta B8" in L 427-428, and "However, biological consumptions of DOC could 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 432-434.

32) Line 394: After the separate discussion for ECS and KE, somehow the authors should connect the ECS and KE data together to derive some general pattern or their contribution to form the overall current that enters into North Pacific. Otherwise it is just like put two separate survey studies together side by side without any connection.
R: Agree. In the last paragraph in Section 4.2, we have added a few discussions combined the two different oceanic regions together in L 448-451.
"However, by comparing with the deep DOC results in the slope region of the ECS, it can be observed that the deep DOC level in the KE was 10-15 μM lower on average than that in the ECS, implying the possibility of lateral transport of DOC from marginal seas to the ocean interior and cycling in the deep ocean for a long duration."

33) Fig.3: Hard to look at the data since all lines are pretty close to each other. Need a better way to present this data. Maybe using color in ODV plots? Can leave this figure as a supplemental figure if needed.
R：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.

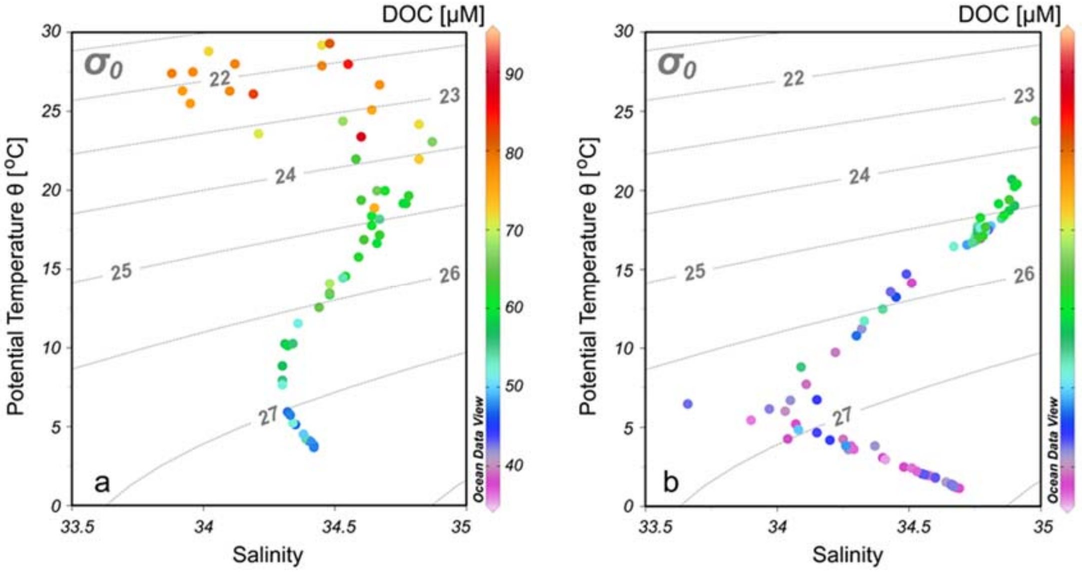

Figure 4. Field-observed DOC concentrations superimposed on plots of potential temperature versus salinity for the sampling stations in the (a) ECS and (b) Kuroshio Extension in the northwestern NP.

34) Fig.7: What about the leftover dots not in water mass A, B, C? How did you decide the grouping of those different water mass? Is it statistically different?

R：See the earlier comment. In the old Figure 7 now Figure 8, we have re-divided the water masses into four different parts (A, B, C and D) mainly referred to the different density and the DIC/Δ^{14}C-DIC values without statistically estimate.

Technical comments:

35) Line 21: Add "the" before ECS

R: Done.

36) Line 29: should be "lower in surface waters than that in the ECS"

R: Yes, we added "than that in the ECS" in the sentence.

37) Line 40: Clarify as "DOC in the ocean is. . .."

R: Yes, we clarified the sentence by adding these words as suggested.

38) Line 42: Delete "therefore", not a result caused by previous sentence.

R: We have removed "therefore" in this sentence.

39) Line 80: "exiting", not "existing"

R: We corrected the typo in this sentence.

40) Line 85: restructure this sentence

R: We have rephrased the sentence as: "The newly formed North Pacific Intermediate Water (NPIW) in the mixed water region has received attention due to its important role in the ocean circulation systems and its impacts on regional carbon cycle and climate variability".

41) Line 92: change to "have been collected. . ."

R: Yes, we changed this sentence to "DOC observations in the WOCE (World Ocean Circulation Experiment) and CLIVAR cruises were collected at Line P02 stations along a 30° N latitudinal transect, but the distribution of DOC near the KE was not investigated during these cruises.".

42) Line 97: delete "a"

R: Done.

43) Line 112: add "that" after "branch"

R: Yes, added.

44) Line 113: "higher primary productivity" higher compared to where?

R: We replaced the "higher primary productivity" with "high primary productivity" .

45) Table 1: Sampling data for ECS not clear, Stn.1,7 both on 12 July? Z1,Z2,Z4 all on 14 July?

R: Yes. We added the sampling date for each station.

46) Line 143: change to "rinsed with seawater three times"

R: Changed

47) Line 151: change to "standards"

R: Changed.

48) Line 154: delete "before", just "every five samples". What does the content in parenthesis *mean? The blank is also run before each deep station sample?*

R: We changed to “between samples” and “before every sample for the deep seawater”, respectively.

49) Line 164: Change “was” to “were”.

R: Yes, changed.

50) Line 173: Add “the” before “DOC and DIC analyses”.

R: As responded for the earlier comment, we deleted the AOU section including this sentence.

51) Line 210: Change to “Concentrations”

R: Yes, we corrected to “Concentrations”.

52) Line 214: “fewer variation” than what? Compared to KE?

R: We replaced “fewer variation” with “less variation”.

53) Line 222: Is 36-53uM the DOC value for Sta A4 and B2?

R: Yes, the concentration range is for Sta A4 and B2. We moved the DOC values (36-53 µM) after the Sta A4 and Sta B2 to include both stations.

54) Line 228: Change to “Processes controlling the DOC distribution. . .”

R: Yes, we changed the title of Section 4.1 to: “Processes that control the DOC distribution in the ECS”.

55) Line 237: delete “depth”

R: Yes, deleted.

56) Line 258: Change to “high concentrations of DIC and. . .”

R: Yes, changed as suggested.

57) Line 259: restructure this sentence

R: As suggested, we restructured this sentence to “This intrusion of Kuroshio intermediate water diluted the DOC at Stn. 11 and Z4 (Figs. 7b-d).” in L 301-302.

58) Line 262: Change to "the well mixed shelf water not only contributed to . . .."

R: We have deleted the sentence "As shown in Fig. 5d, the well mixed shelf water could not only contribute to the ^{14}C-depleted DIC signature in the upper 100 m layer at station Z4, but also elevate DOC concentrations, as compared with the DOC levels in the upper water column at the other three slope stations (Stas. Z1, Z2 and Z3) as influenced by the Kuroshio Current (Figs. 3b and 5b). The river influence and inner shelf export of DOC appeared to be limited in the deep slope stations.". We also added "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." in L 305-310.

59) Line 292: Change to "statistically significant"

R: Deleted this sentence which is included in the AOU section.

60) Line 295: Change to "Processes"

R: Yes. We changed the title of Processes that influence the DOC profiles in the Kuroshio Extension.

61) Line 305: "among these stations" means "spatially", right?

R: Correct. We have rephrased the sentence as "large spatial variations for DOC concentration among these stations".

62) Line 307: 'significantly lower than other stations"

R: Yes, we added "than those of other stations" for the sentence.

63) Line 310: "with values that are 28% higher"

R: Changed.

64) Line 318: delete "most"

R: Yes, we deleted "most" from the sentence.

65) Line 375: change “modulated” to “modulating”

R: Done. We replaced “modulated” with “modulating”.

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 350-351. 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 166-169.

"(CRM Batch 13 with 41-44 μM DOC concentration, supplied by Hansell 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 173-174.

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 243-260.

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 214-220.

(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 448-451. “However, by comparing with the deep DOC results in the slope region of the ECS, it can be observed that the deep DOC level in the KE was 10-15 μM lower on average than that in the ECS, implying the possibility of lateral transport of DOC from marginal seas to the ocean interior and cycling in the deep ocean for a long duration.”

(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 234-236.

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

R: In the revised MS, we have clarified this sentence in L 239-242.

“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 694.

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 $\Delta^{14}C$-DIC in the new Figure 5 and discussed these results more in Section 3.3 in L 243-260 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 448-451. 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 297-311.
"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 297-303.

In L 303, 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 375-382.

"The denser water mass C with density levels of 26.4-27.1 σ_0 near 500-800 m 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 subsequently 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 was 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 383-385.

(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 "the positive ΔDOC values (~6 µM) that accounted for approximately 11% of the measured DOC at Sta B8" in L 427-428, and "However, biological consumptions of DOC could 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 432-434.

(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 $\Delta^{14}C$-DIC values have been published in Ding et al., (2018). Briefly, the significantly lower $\Delta^{14}C$-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 $\Delta^{14}C$-DIC values carried by the Oyashio Current. We have clarified the statements by changing the sentence as "Using the significantly low $\Delta^{14}C$-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 448-451.

(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 $\Delta^{14}C$-DOC values in the northwestern North Pacific would be discussed in details in another paper.