Reply to comments

In this manuscript the authors tried to detect the eddy-subduction signals in the mid-latitude western North Pacific by applying their improved method with spicity anomaly to total 7120 BGC-Argo float profiles during 2008-2019. Then, they used dissolved oxygen properties in the subduction patches detected successfully to estimate both carbon and oxygen exports due to subduction process. Based on these estimates, the authors argued for the significance of episodic subduction in the mid-latitude ocean in terms of carbon sequestration into the deep sea as well as supporting mesopelagic ecosystems. Carbon export due to subduction has recently gained importance as an overlooked pathway, and I believe that their new method for detecting subduction patches contributes greatly to its evaluation. However, there are some methodological concerns with their estimation of carbon exports, as described below. Unless these concerns are addressed, I cannot recommend acceptance of this paper.

Reply: Thanks for the critical and constructive comments to help us improve the manuscript. Recent studies have shown that eddy-induced subduction can be a pathway for carbon export, yet little is known on its significance compared to other pathways. Using an extensive dataset of BGC-Argo profiles in the western North Pacific, we investigated the occurrence of eddy subduction both spatially and temporally. However, due to the lack of carbon sensor on the BGC-Argo floats and the lack of knowledge of subduction rates, we are handicapped at estimating the carbon inventories and export fluxes. In the original manuscript, we tried to overcome these problems by using a fixed C:O ratio and a fixed lifetime of subduction patches with some assumptions. However, after carefully considering the comments raised by this and other reviewers, we noted the potential problems involved in our calculations. Indeed, it is quite difficult to quantify the carbon export and fluxes based on the 1-dimentional BGC-Argo profiles alone. To avoid any misleading results and analyses, we removed the carbon-related and flux-related calculations in the revision. Instead, we focused on the oxygen inventory associated with the subduction patches. Most of the published studies emphasized the importance of eddy subduction in carbon export, but we proved that the episodic eddy subduction could also be an important mechanism in oxygen transport into the ocean interior, which would relieve the oxygen demand in the deep sea.

In the revision, we carefully considered all the comments and addressed most of them, please see our detailed replies below.

The authors calculated the organic carbon anomaly by multiplying ΔAOU by the C:O ratio (eq. 1), then suggested that this corresponds to the amount of carbon exported by subduction. I don't easily agree with this even if the C:O ratio is reasonable. Maybe, authors think that ΔAOU, the difference between the AOU of subducted water and surrounding water, would reflect the time period (that a water to reach the depth of the subduction patches) shortened by the subduction, and the amount of organic carbon had been decomposed in the non-subducted water within that period (= ΔAOU x C:O ratio) would correspond to the amount of organic carbon exported to that depth by subduction. However, the organic carbon that was decomposed in the non-subducted water originated not only from the organic matters originally contained in the water when it was detached from surface mixing, but also from particles input from outside (especially due to particle aggregates settling and their fragmentation). Therefore, I believe that authors overestimated the subduction carbon export by this amount of the particulate organic carbon input that not associated with water movements. This is why that I think your method for estimating carbon export is logically unreasonable. Also, I don't think it was reasonable to calculate the daily flux by dividing the subduction carbon and oxygen export by 365 days (eqs. 5-6), as pointed by other reviewer.

Reply: Thanks for the comments to help us re-evaluate the carbon-related and flux-related calculations. Yes, we previously assumed that the difference of decomposed organic carbon (=ΔAOU x C:O ratio) between non-subducted and subducted water represents the organic carbon exported by subduction. Now we realized the uncertainties involved in using a fixed C:O ratio and a fixed lifetime of subduction patch in our calculations. To avoid any misleading results and analyses, we removed the carbon-related and flux-related calculations in the revision.

On the other hand, the spatial distribution of detected subduction patches (shown in Fig. 7) is very interesting. It appears (at least to me, roughly speaking) that the subduction patches position was extending from northwest to southeast and from shallow to deep depth. This would indicate that subduction occurred in the northern KE (>35N) and that the subducted water traveled south (or west) and deeper along isopycnal surface as illustrated in Fig. 1. Besides, there were no relationship between depth of subduction patches and ΔAOU (L398), i.e. various ΔAOUs were found at any depth. This may be interpreted that the ΔAOU of the subducted water would be determined when subduction occurred, and it would be mainteined while the water traveled. If so, by using the average change of AOU (not ΔAOU) with depth (micro-mol kg-1 m-1) and the average oxygen consumption rate (micro-mol kg-1 d-1) in the mesopelagic layer, you can calculate the (vertical) travel rate (m d-1) of subducted patches, which may contribute to estimate oxygen export flux by subduction. Perhaps this is a largely misguided comment, but please consider it.

Reply: This is a good point. From Fig. 7, we also noted the likely extending patterns of the subduction patches from shallow to deep and from northeast to southwest. To avoid any misleading interpretation based on Fig. 7 only, here we gridded the depths of the subduction patches along latitude of 26°-44° N and longitude of 140°-167° E (in which range the subduction patches were detected), respectively, at an interval of 1°. Fig. R1 shows the latitudinal and longitudinal variations of the mean depth positions of the subduction patches. Along the latitude (Fig. R1a), it is seen that, despite a few deep subduction patches identified at 42°-43° N (at around 550m), the mean depths of the subduction patches observed show a clear increasing pattern from latitude 37°-42° N to latitude of 32°-37° N, i.e., 300m *vs.* 500m. However, the depth positions tend to be shallower and shallower south of 32° N. Along the longitude (Fig. R1b), the depth positions generally appeared to be deeper from east to west. Based on the analyses above, it is most likely that, for the subduction occurred in the northern KE (37°-42° N), the subducted waters traveled southwestward from shallow to deep depth, and these

waters could reach to 32° N. The increasing depth positions of subduction patches from 26° N to 32° N tend to suggest the gradually downward movements of the subducted water masses carried by the general trend of the anticyclonic gyre scale circulation, yet a further investigation is needed, which is out of the scope of this study.

Indeed, we did not find any relationship between the AOU anomalies (i.e., ΔAOU) and the depth of subduction patches. As we interpreted in the main text, the surface conditions (e.g., water temperature, primary productivity) really matter when these water parcels get subducted. Along the subduction pathway, there would be also organic carbon decomposition and oxygen consumption. Nevertheless, the AOU properties of the subducted water would maintain to some extent while the water traveled. With the subduction pathway from northeast to southwest, the reviewer suggested a method for calculating the vertical travel rate of the subduction patches using the average change of AOU with depth. Here we first examined the average AOU (not ΔAOU) variations along the latitude, and the result is shown in Fig. R2. Unfortunately, there is no distinct AOU changing patterns from latitude 37°-42° N to latitude of 32°-37° N while the subducted waters travel along. In fact, it is not surprising to see this result. As pointed out above, the AOU of the different subducted waters really depend on the local conditions (e.g., DO which is related to phytoplankton production, water temperature which determines oxygen solubility) when the waters get subducted, therefore, the averaged AOU of the various subducted waters did not necessarily show an increasing pattern when these subducted patches traveled from north to south. However, we would think that the AOU of the subducted water should be increasing along the subduction pathway if we follow a single subduction patch, in which case the vertical travel rate of the subduction patch can be estimated, yet there is no such data available in our study. With all these tested and considered, we removed the flux-related analyses in the revision, and only focused on the oxygen inventory associated with the subduction patches.

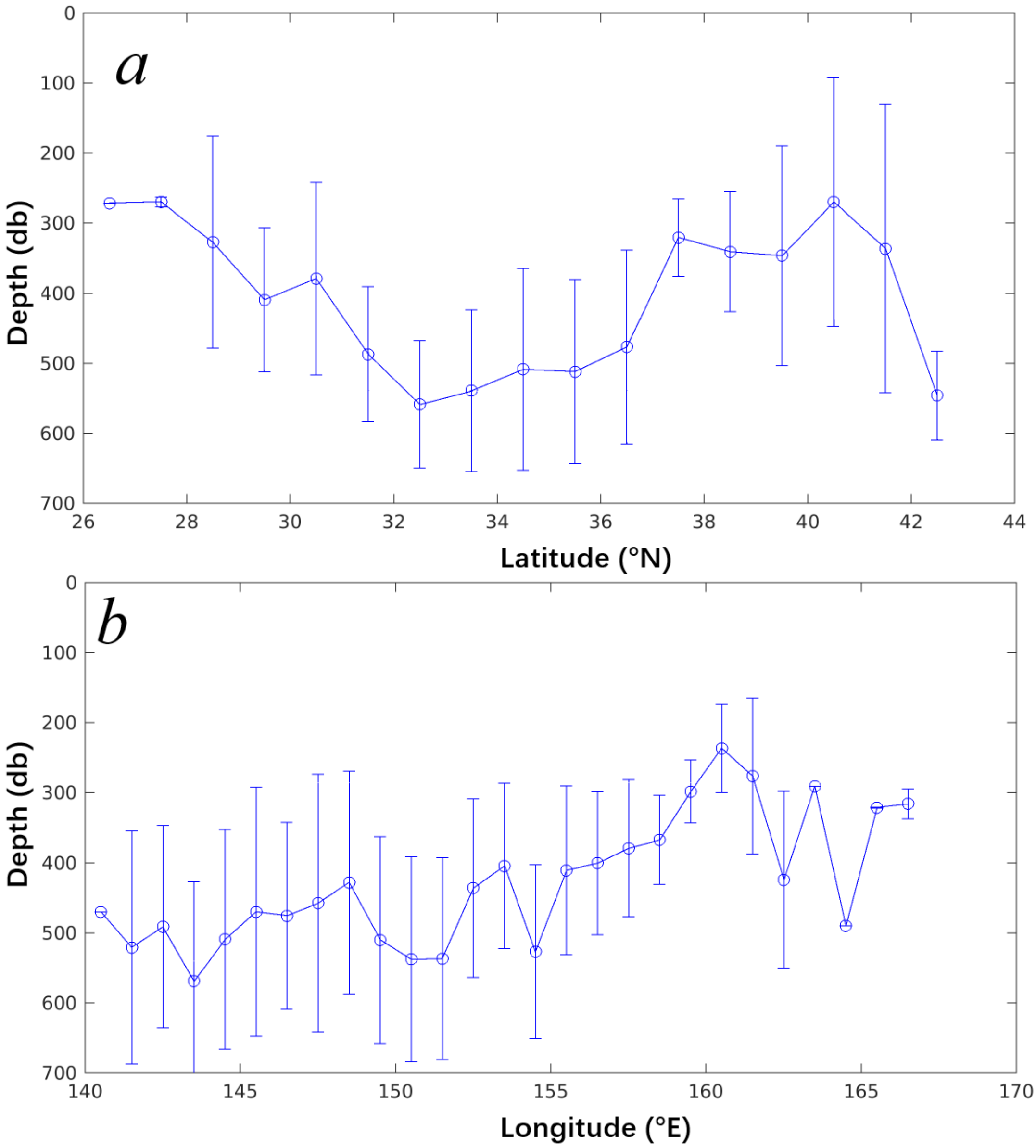

Fig. R1 Variation of the mean depths of the subduction patches observed in each 1° interval of latitude between 26° N and 44° N (a) and of longitude between 140° and 167° E (b). The overlaid errorbar represents one standard deviation of the depths of the subduction patches in each latitude/longitude interval.

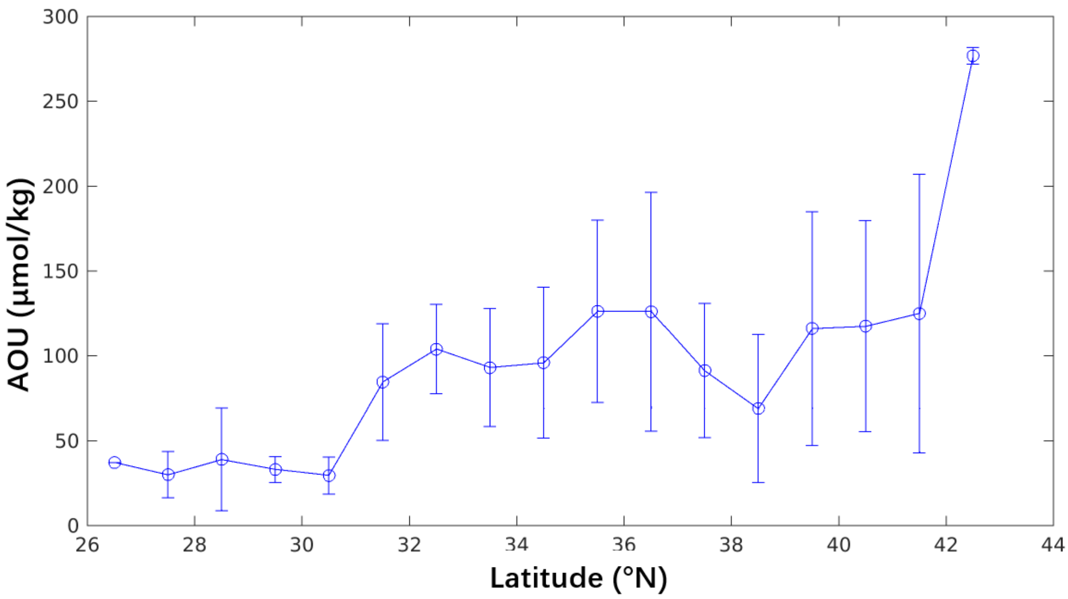

Fig. R2 Variation of the averaged AOU of the subduction patches observed in each 1° interval of latitude between 26° N and 44° N.

Specific comments

Fig. 5: It would be better to add new diagram showing the subduction patches colored by season when these were detected.

Reply: Added in Fig. 5c.

L335, L341: Authors reported that most of subduction patches were found during March and August, and discussed the reason. However, it should be discussed using the detection rate (the number of detections divided by the total number of profiles). Looking at Fig. S2, I agree that the detection rate was high in March, but I suspect that the it did not change after May (to December) since monthly fraction (%) of the number of detections was almost parallel to the number of available profiles.

Reply: This is a good point. In the revision, we added Fig. S3 about the subduction detection rate in the supplemental file and added more discussion accordingly. Indeed, there is a higher detection rate in March (around 10%), and the detection rate is relatively low in January and February (< 2%), and did not change much from May to December (3.5-6.4%).

L399, L404: Authors may think that there should be a relationship between ΔAOU and the surface productivity when the water was subducted: strong ΔAOU for high productivity water, but I don't get it. Did you consider the supersaturated dissolved oxygen in productive waters? (it can result in low AOU thereby high ΔAOU) Please clarify your idea in the text. Rather, I think ΔAOU would depend strongly on the water temperature (which determines gas solubility) when it is subducted.

Reply: Both are clarified in the revision. With high productivity in the upper layer, oxygen could be enriched and even supersaturated. When these waters are subducted into the ocean interior, the subducted water patch would show anomalously higher DO and lower AOU than the surrounding non-subducted waters at depth (as demonstrated in Fig. 1 & 5). We agree that ΔAOU would also depend strongly on the water temperature when the water is subducted.

L491: Negative Δπ indicates not only "cold" but also "less saline" for the subduction patches, which should be noted. I think that the water subducted in the northern KE may include partly low salinity (subarctic) water.

Reply: Modified as suggested.