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Comment on bg-2021-145

Anonymous Referee #3

Referee comment on "Episodic subduction patches in the western North Pacific identified from BGC-Argo float data" by Shuangling Chen et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-145-RC3, 2021

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.

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.

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.

Specific comments

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

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.

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.

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.