Reply to comments

Review for Chen et al manuscript *Episodic subduction patches in the western North Pacific identified from BGC-Argo float Data*

In this manuscript, Chen and co-authors analyse the occurrence of subduction events using a dataset of 43 BGC-Argo floats in the Kuroshio Extension (western North Pacific). As demonstrated in Llort et al, (2018), BGC-Argo float profiles are a costly-efficient way to observe events of small-scale water subduction, also known as eddy-pump or eddy-subduction pump (ESP, Boyd et al, 2019). Recent studies have shown that this mechanism can contribute to the biological carbon pump but there are still large uncertainties on how important this contribution is compared to other pathways of carbon export (Boyd et al, 2019; Resplandy et al, 2019).

Chen et al contribute to this knowledge gap by thoroughly analyzing new data in an important region where the ESP has not yet been quantified. Besides, authors revise the only detection method published to date (Llort et al, 2018) and provide a new version. The paper is very well written, with great figures and appropriate citations. Although these original elements justify the publication of Chen et al at Biogeosciences journal, there are two major issues (described in paragraphs below) in the current version of the manuscript that needs to be addressed before acceptance. For this reason, I would recommend major revisions.

The two issues are related to the estimation of the Carbon and Oxygen inventories and fluxes associated with the episodic events.

Reply: Thanks for recognizing the value of this work, and thanks for the critical and constructive comments to help us improve the manuscript. With the lack of carbon data from the BGC-Argo floats and the challenges to estimate the transporting rates of subducted waters, it is difficult to quantify the carbon inventories and carbon/oxygen fluxes associated with the episodic subduction events. In the original manuscript, in order to estimate the carbon and oxygen inventories and fluxes, we made a few assumptions. However, as you pointed out correctly, we realized the uncertainties in these assumptions. To avoid any misleading quantifications and analyses, in the revision, we removed all the carbon-related and flux-related estimates, and focused on the oxygen injections (i.e., oxygen inventory) only. Please see our detailed replies below.

1) The first and most important issue is that the dataset used in this study contain no measurement of optical backscattering, the data generally used to estimate particulate organic carbon (POC) with BGC-Argo floats. Instead, the only biogeochemical variable sampled by the floats used here is oxygen. That's unfortunate and strongly impacts the estimates and conclusions on the role played by subduction events to export carbon into the deep ocean. Authors try to circumvent this handicap by applying a C:O ratio that is not up-to-date nor adapted to this purpose. Authors justify this ratio (C:O, 117:170) citing two publications (Anderson and Sarmiento, 1994 and Feely et al, 2004). None of the two publications are referenced in the manuscript, besides there's no reference to C:O ratio in Feely et al, 2004 paper. I did find the cited ratio in Anderson and Sarmiento, 1994 (A&S94 hereinafter), who concluded that at large scale there's no significant change on the C:O ration between 400 and 4000m depth. A&S94 include however a caveat about the use of this ratio

that Chen et al authors ignored or neglected. The very last paragraph of A&S94 states:

As these are long-term, basin-wide, net-ecosystem utilitzation ratios, ***they might not be applicable on short timescales or length scales*** *(...). Also,* ***these ratios may not be applicable to high-latitude regions or the ocean above 400m****.*

These two sentences, which are not addressed by Chen et al, suggest that the use of C:O for subduction events is inadequate. Authors should address this major issue, either by removing all the analysis of the carbon export, looking for other methods to estimate carbon from the data available, or incorporating additional analysis to propose C:O ratio that can be used in the context of episodic subduction in this region. In the latter case, an analysis of uncertainties will also be necessary.

The analysis on Oxygen injections is on the contrary valid as it is based on measurements from the floats. Besides it contains interesting thoughts on how these injections might ventilate low oxygen subsurface waters in low-to-mid latitude oceans. So, an option would be to focus the paper only on Oxygen injections and include one paragraph on why carbon export fluxes could not be estimated.

Reply: We appreciate your comments on the carbon estimates. Now we did realize that it is inappropriate to use the C:O ratio (C:O, 117:170) to quantify the carbon exports associated with eddy subduction. In fact, without measurements of carbon from the BGC-Argo floats, it is difficult to quantify the carbon exports. Based on the data we have, we did not find other better methods to estimate the carbon exports, and we did not find a better way to propose a C:O ratio to use. For that, here in the revised manuscript we decided to remove all the carbon-related estimates and analyses and focus on the oxygen injections instead. Please note that we also clarified in the revision why the carbon exports cannot be estimated quantitively.

There has been a lot of emphasis on carbon export via subduction, however, few studies ever gave a close consideration to oxygen in the subducted waters. Our results show that roughly half (51.8%) of the episodic events injected oxygen-enriched waters to the depth below 450 db, exceeding the annual maximum of mixed layer depth, which would be an important pathway to support the metabolic oxygen demand of mesopelagic organisms. Meanwhile, we found high prevalence of these oxygen injections in the midlatitude. Since weak ocean ventilation is leading to declining oxygen concentrations in the tropical and subtropical mesopelagic zone, the prevalent oxygen supply via these episodic subduction events could be an important mechanism in relieving the oxygen demand in the ocean interior in the future.

2) The second issue impacts the estimates for both Carbon and Oxygen fluxes. While authors took great care on the detection and analysis of anomalies and its associate inventories (Eq 4) the assumption used for transforming these inventories to export fluxes is not convincing. In lines 241-244 authors assume that the average lifetime of subducted water patch is 1 year. Authors argue that they apply this assumption to avoid choosing an arbitrary vertical velocity, but this average lifetime seems arbitrary to me too. To my knowledge, there's no estimates of how much time these water masses maintain differentiated properties in the mesopelagic zone and we can imagine numerous physical and biogeochemical processes influencing them. These processes cannot be considered with the current dataset but authors could do a

detailed analysis of the mixed layer depth variability. How deep is mixing penetrating? How often do storms reset vertical distributions? Which is the regional variability of the maximal MLD and its variance over the region of interest? This analysis would provide some insights on which water masses will remain below the permanent pycnocline (in the current version of the manuscript authors used the value 450db but this is not justified), and on the average lifetime of anomalies above the permanent pycnocline.

Reply: The estimates of carbon and oxygen fluxes associated with the eddy subduction requires the estimates of eddy subduction rates. However, the BGC-Argo profiler only captures snapshots of the subduction patch, and it cannot record the entire process. Thus it is impossible to quantify the vertical transporting rates of the subduction from the BGC-Argo float data alone. In addition, the subduction rates could vary substantially along the subduction pathways from the ocean surface to the ocean interior. For that we tried to use the lifetime of subducted waters to calculate the subduction rates. The choice of 1 year (i.e., a general estimate of the lifetime of subducted waters) was based on the assumption that the subducted waters are renewed and dissipated on an annual scale considering that few subduction patches were found in December, but surely this would also involve some uncertainties. At current stage, we do not have any better way to estimate the subduction rates, and we did not foresee a reasonable estimate of the lifetime of the subduction patches from the MLD dynamics. As such, to avoid any misleading estimates and analyses, in the revision we removed the flux-related calculations, but we added a discussion on the need of flux estimates from the eddy-induced subduction.

In our analysis of the subduction patches shown below the permanent pycnocline (PP), in the revision, we justified our choice of the depth of PP based on the study of Feucher et al. (2019) and the MLD dynamics. Specifically, in Feucher et al. (2019), the permanent pycnocline properties in the world ocean were thoroughly investigated based on 16 years regular Argo float (which provides temperature and salinity profiles) data (N=1,226,177). Figure 2 in Feucher et al. (2019), provided as Fig. R1 below, shows the spatial distributions of the depth of PP over the global ocean. Indeed, the depth of PP varies from region to region, and it ranges from 300 to 450 m in our study region, with a relatively shallower (≤300 m) and deeper (≥400-450m) depth of PP in the subpolar and subtropical area, respectively. To be conservative, we chose to use the maximum depth of PP (i.e., 300m for subpolar region, and 450 m for subtropical region) as the reference depth of PP in our analysis. In addition, based on the 7120 profiles from the BGC-Argo floats (Fig. 2), we also calculated the monthly maximum MLD and the variance in both the subtropical (i.e., south of 35° N) and subpolar (i.e., north of 35° N) regions of the western North Pacific (Fig. R2). We took the annual maximum MLD as the potential depth of PP. It is seen that, in February and March, the maximum MLD was the deepest over a year in both regions, and more importantly, consistent with the spatial patterns shown in Fig. R1 (Feucher et al., 2019), our data show that the annual maximum MLD (i.e., potential depth of PP) is shallower in the subpolar section, and deeper in the subtropical section. Our data indicate that, in the subtropical region, the annual maximum MLD with one standard deviation could reach 400 m, which is smaller than the maximum depth contours of PP (i.e., 450 m) identified in Feucher et al. (2019).

However, it should be noted that, our result in Fig. R2 is based on a much smaller dataset than that in Feucher et al. (2019). Again, to be conservative, we chose to use 300m and 450 m as the depth of PP in the subpolar and subtropical regions in our analysis.

In the revision, we explained the choice of the depth of PP (i.e., 300 m for subpolar region, and 450 m for subtropical region), and discussed more on MLD dynamics. Moreover, considering the differences of the PP depth between subpolar and subtropical regions (which is also raised in the first comment from Reviewer # 2), we added more detailed statistics of the subduction patches based a PP depth of 300 m and 450 m for the subpolar and subtropical region, respectively. Please see our detailed reply to the second major comment from Reviewer # 2.

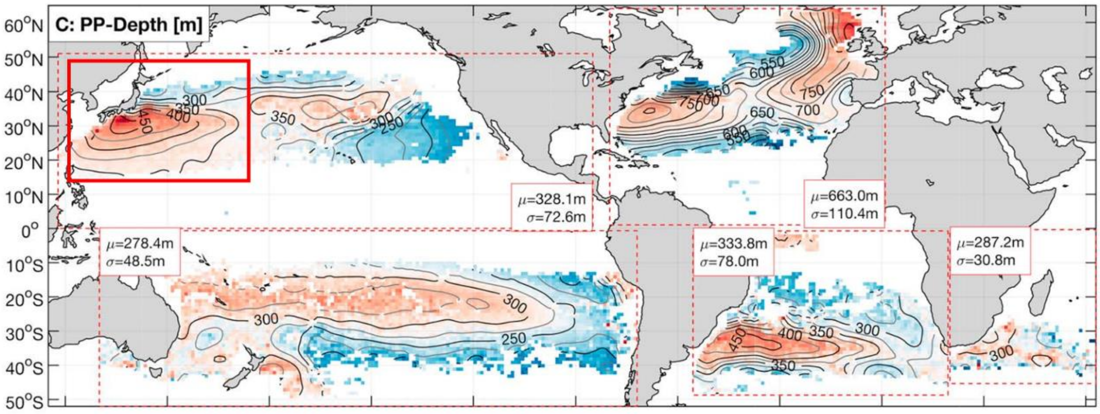

Fig. R1 The 2000-2015 Argo climatology of permanent pycnocline (PP) depth in the global ocean. Contours are every 25 m, labeled every 50 m. Cited from Fig. 2 in Feucher et al. (2019). Our study region is outlined in red.

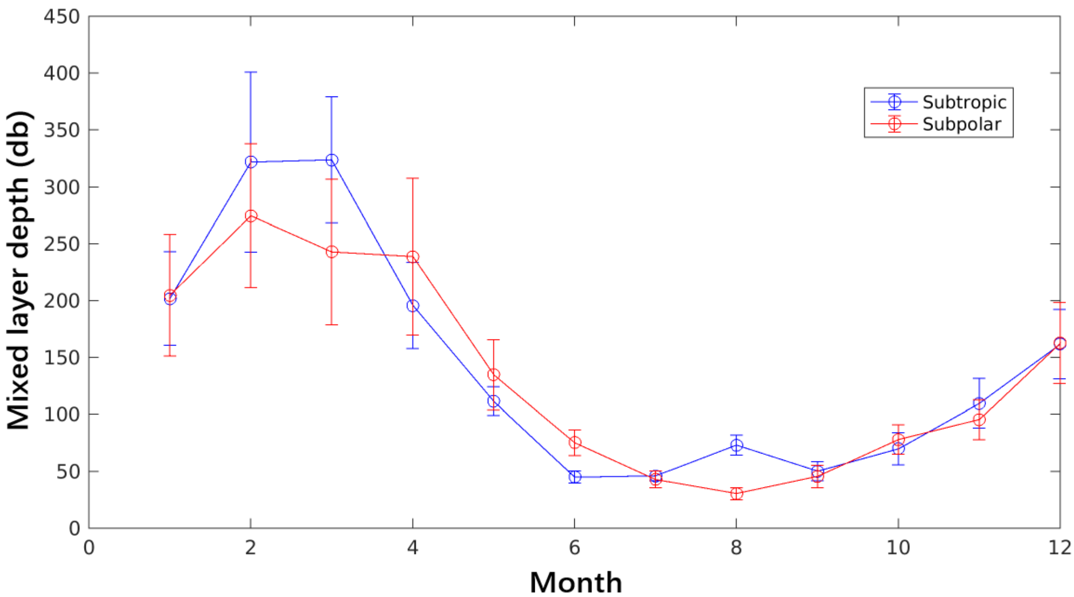

Fig. R2 The monthly variations of the maximum mixed layer depth (MLD) in the subtropical (i.e., south of 35° N) and subpolar (i.e., north of 35° N) sections of the western North Pacific, respectively, based on all the BGC-Argo profiles (N=7120, see Fig. 2). The

errorbar represents one standard deviation of the mean MLD in each month.

Reference:

Feucher, C., Maze, G., & Mercier, H., 2019. Subtropical mode water and permanent pycnocline properties in the world ocean. Journal of Geophysical Research: Oceans, 124(2), 1139-1154.

More specific comments:

L202 I suggest removing the last sentence of the paragraph. The idea that the improved detection method would detect more events in other datasets (I understand that authors are referring to Llort et al, 2018 dataset) appears several times in the manuscript. I don't see the interest of this statement without providing any data to back it up. It would be interesting to test the new detection method on the same dataset used in Llort et al, 2018 to quantify the improvement in detection. Without this quantification it doesn't make much sense to compare the two studies as Llort et al, 2018 dataset covered the whole Southern Ocean with lots of profiles in low EKE regions, while Chen et al focuses in a much smaller area and with floats more localised in a region of mid-to-high EKE.

Reply: Removed as suggested. In fact, we surely accept the validity of the approach in Llort et al. (2018). Potential spicity (π) defined by Huang et al. (2018) is able to distinguish water masses with similar density due to its orthogonal coordination with density. Thus, we mainly want to convey the message that spicity instead of spiceness should be used to detect subduction signal.

L293-4 Again, a speculative comparison in "more signals of subduction (...) that had been previously recognized." Previously by who? If authors refer to Llort et al, 2018 I think the comparison is not valid for the arguments exposed above.

Reply: Modified.

L297 I don't understand this paragraph. In particular, I don't understand why authors argue that "The ephemeral nature" of the anomalies suggest that "they stemmed from distinct subduction events". Some paragraphs above authors assumed that these anomalies could last up to 1 year...

Reply: The 1-dimensional BGC-Argo profiling only captures instantaneous signals in the water column, for that, it is hard to justify whether a subduction patch represent a discrete subduction event or not. However, for the same subduction event, subduction patches should be identified in consecutive profiles, for example, the continuous subduction patches shown in Box 3 in Fig. 4. From this regard, if a subduction patch was captured in one profile, but was not captured from the adjacent profiles, we would suspect the subduction patch most likely stemmed from a discrete subduction event, for example, the subduction patch shown in Box 1 and Box 2 in Fig. 4. Still with the 1-D profiling dataset, it is very difficult or impossible to prove this reasonable argument. We modified our description in the revision.

L305 I feel that the use of "modified" here is not clear. Modified respect to what? I assume authors refer to Llort et al, 2018 but I don't see the necessity to compare both methods, except if authors decide to properly quantify the performance of one against the other. This comes back to the comments for L2XX. It should be better to talk about "our method" or create some acronym/code to be sure the reader understands which method is being used or referred to.

Reply: Changed accordingly.

Fig 6 The peak in March is surprising and it's not clear to me how can be explained by "large-scale subduction" (L339). Large-scale subduction should not be detected by your method, why it is then affecting to the number of event detected? Also, could you plot the average MLD dynamics of all floats in the dataset to show the shoaling during this time of the year.

Reply: In a pioneering work, Stommel (1979) argued that a demon working in the ocean by selecting the later winter (typically for later March in the North Hemisphere) water mass properties and injecting them into the subsurface ocean. This mechanism is now called the Stommel Demon in dynamical oceanography (Huang, 2010). Mesoscale and sub-mesoscale eddy activities are prevalent when large-scale subduction occurs, as such, more episodic eddy subductions should be detected during large-scale subduction.

As suggested, we investigated the MLD dynamics based on all the BGC-Argo profiles (N=7120) in our dataset (Fig. 2). Fig. R2 shows monthly variations of the maximum MLD in both subtropical (i.e., south of 35° N) and subpolar (i.e., north of 35° N) sections of the western North Pacific. Clearly, the MLD is shoaling from March to August.

In the revision, we added more explanation on the high occurrence of subduction in March, and discussed more on MLD dynamics.

References:

Huang, R. X., 2010. Ocean Circulation, wind-driven and thermohaline processes, Cambridge Press, 810pp.

Stommel, H. M., 1979. Determination of water mass properties of water pumped down from the Ekman layer to the geostrophic flow below. Proc. Natl. Acad. Sci. U.S.A., 76, 3051-3055.

L483 Remove word "currently" as the sentence starts with "Current global-scale...".

Reply: Removed.

L485 Replace "added" by "additional"

Reply: Done.

As recommendation I would suggest making the detection method public and available by uploading the scripts in GitHub or similar. I didn't do that when I published my paper and I strongly regret it.

Reply: That's a good point. Thanks for the recommendation. In fact, we are developing an application tool to interactively or automatically identify subduction patches with any BGC-Argo profiles loaded, and it is going to be published as a code & technology article, then the code will be deposited in GitHub.

Thanks for your work!
Joan Llort

Thank you again for providing the constructive and thorough review, we really appreciate your effort and time spent on reviewing our manuscript.