Reply to comments

Comments from Reviewer #1

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, we based our estimates of the carbon and oxygen inventories and fluxes on a few assumptions we felt reasonable for "back-of-the-envelope" calculations. However, as you pointed out correctly, we realized the uncertainties in these assumptions. To avoid any misleading quantifications and analyses, we removed all the carbon-related and flux-related estimates in the revision, 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. As the reviewer notes, there is great variation in the C:O ratio (C:O, 117:170) which raises the uncertainty of our estimates of the carbon exports associated with eddy subduction. Even if we did have optical backscatter data, these would provide an underestimate of carbon export during subduction because it does not account for remineralized carbon. As we note above, we initially felt there still was value in estimating the order-or magnitude export, but in the revision 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 (see Lines 515-540, clean version of the revised manuscript).

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. More results and analyses were added in Section 3.3 & 3.4 (see Lines 449-465, and Lines 483-514, clean version of the revised manuscript).

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 these subduction events, and 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 took a conservative approach by assuming a 1 year lifetime, where the observed subducted waters are renewed and dissipated on an annual scale (based on the observation that few subduction patches were identified in December). Actual lifetimes are likely shorter, but at this stage, we do not have any better way to estimate the subduction rates. However, we did foresee a reasonable estimate of the lifetime of the subduction patches from the MLD dynamics. Given this uncertainty though, we removed the flux-related calculations in the revision, but added a discussion on the need of flux estimates from the eddy-induced subduction (see Section 3.4, particularly Lines 534-540, clean version of the revised manuscript).

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 (see Lines 338-348, Lines 368-373, in the clean version of the revised manuscript). 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.

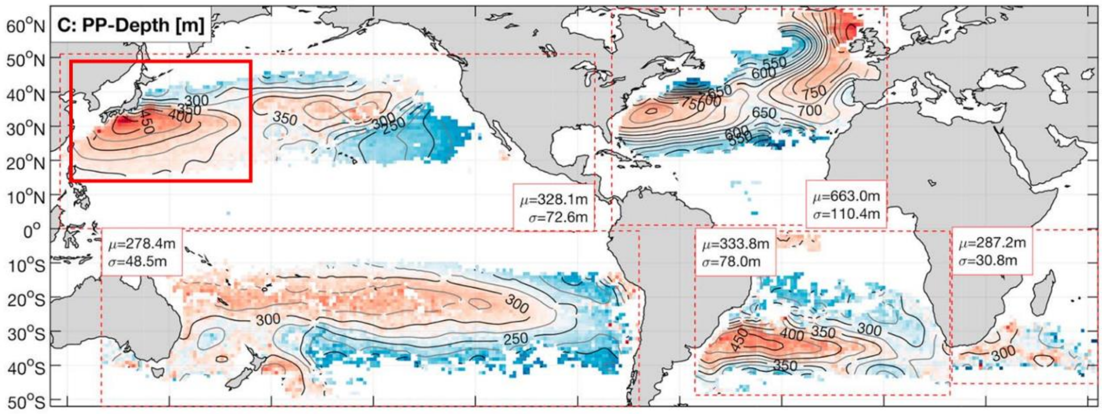


Fig. R1 The 2000-2015 Argo climatology of permanent pycnocline (PP) depth in the global ocean. Contours are every 25 m, labeled every 50 m. Cited from Fig. 2 in Feucher et al. (2019). Our study region is outlined in red.

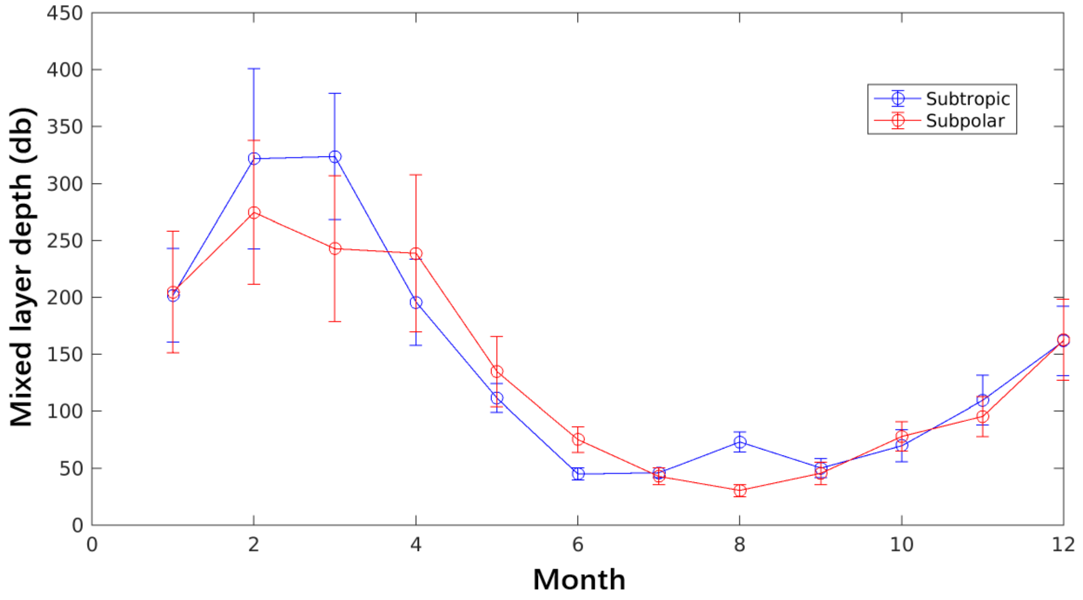


Fig. R2 The monthly variations of the maximum mixed layer depth (MLD) in the subtropical (i.e., south of 35° N) and subpolar (i.e., north of 35° N) sections of the western North Pacific, respectively, based on all the BGC-Argo profiles (N=7120, see Fig. 2). The errorbar represents one standard deviation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

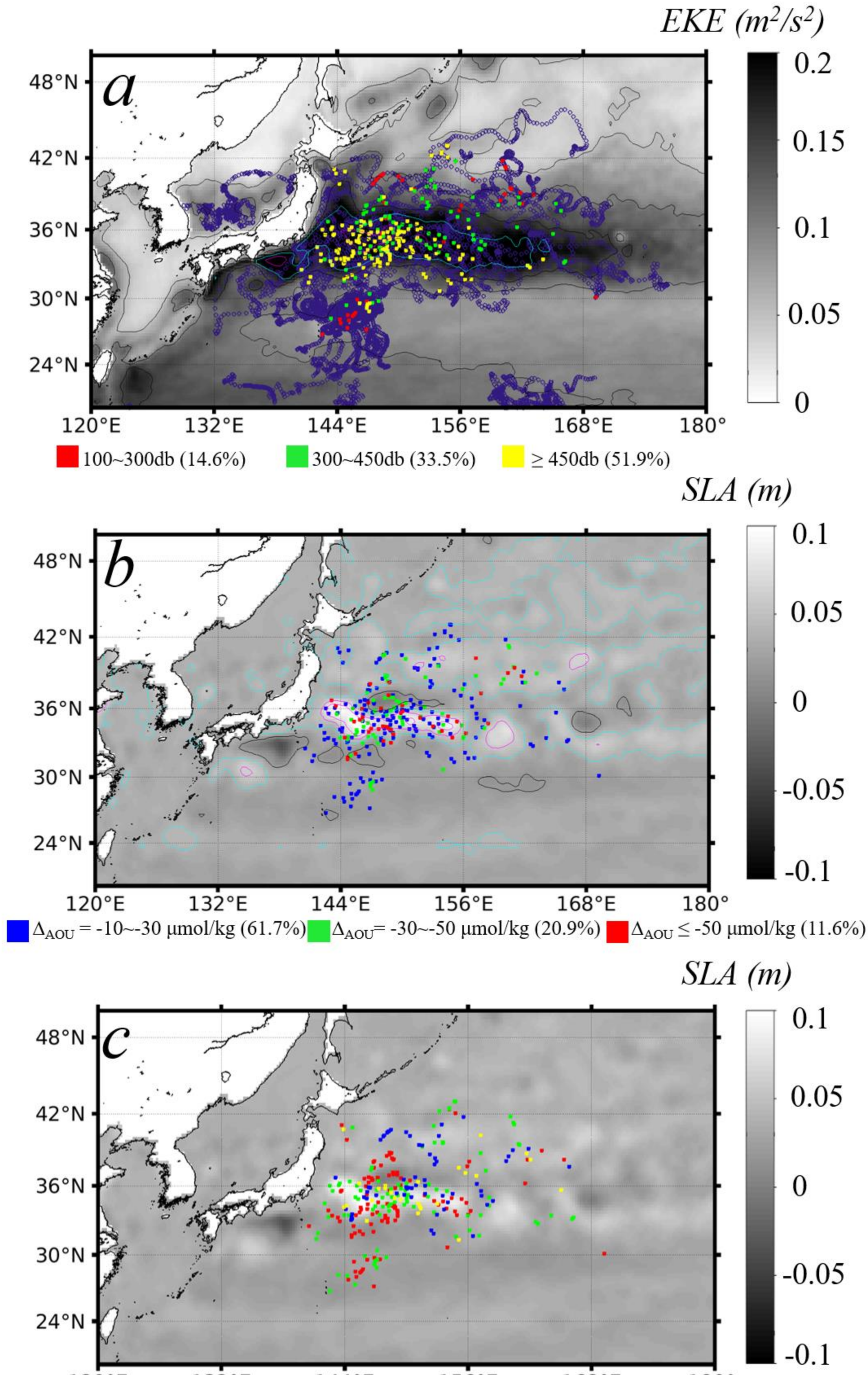
Comments from Reviewer #2

This manuscript describes a method for detecting water subduction associated with mesoscale eddies in the western North Pacific and estimates of the amount of material transported into the mesopelagic zone. The authors detected episodic subduction patches using spicity and oxygen anomalies from the BGC-Argo dataset. They found 288 subduction patches, most of which were observed between March and August in the Kuroshio Extension region. Most of subduction patches were found below the annual permanent thermocline depth (450 db). They estimated export rates of oxygen and carbon on the order of 175 to 417 mg O2 m-2 day-1 and 85 to 159 mg C m-2 day-1. The method used in this manuscript to detect subduction is very interesting. It is also an important finding that many subduction patches exist in the Kuroshio Extension region and that many of them are deeper than the permanent thermocline. On the other hand, it is important to know the distribution and amount of material transport by subduction, but the analysis and discussion are not sufficient. There are some problems with estimation the amount of material transported by subduction. I cannot recommend this manuscript for publication. I request significant revision and additional discussion.

Reply: Thanks for the critical and constructive comments to help us improve the manuscript. We carefully considered all the comments and addressed most of them in the revision, please see our detailed replies below.

Major comments

1) In this study, subduction patches were detected using a unique method. The spatial (horizontal and vertical) distributions of patches and seasonal variation of number of patches are discussed respectively, but it will be necessary to combine them. If the authors are going to discuss oxygen export by subduction, the patches must be considered separately shallower and deeper layers than the permanent pycnocline. Since the depth of permanent pycnocline differs between the subtropical and subarctic region, it is also necessary to distinguish between the regions.
Reply: This is a good point. In the revision, we examined how the subduction patches distributed over the study domain in each season, with results shown in Fig. 5c. In general, there were no spatial patterns of the subduction patches in each season. We clarified this in the main text.



EKE (m²/s²)
a
48°N
42°N
36°N
30°N
24°N
120°E
132°E
144°E
156°E
168°E
180°
0.2
0.15
0.1
0.05
0
100~300db (14.6%)
300~450db (33.5%)
≥ 450db (51.9%)
SLA (m)
b
0.1
0.05
0
-0.05
-0.1
Δ_AOU = -10~-30 μmol/kg (61.7%)
Δ_AOU= -30~-50 μmol/kg (20.9%)
Δ_AOU ≤ -50 μmol/kg (11.6%)
SLA (m)
c
Spring (38.9%)
Summer (31.7%)
Fall (21.0%)
Winter (8.4%)

Fig. 5 (updated). Horizontal distribution of the BGC-Argo data profiles associated with subduction patches (a and b) between 2008 and 2019 in the western North Pacific. The profiles with detected subduction patches are color coded by different intervals of depths of the subduction patches (a), AOU anomalies (b), and seasons (c), with percentages of detected patches in each interval annotated. The purple background data in (a) represent all the analyzed profiles as shown in Fig. 2a. The grey-scale background map in (a) is the annual mean EKE climatology, with EKE contour lines of 0.3, 0.2, and 0.1 m^2/s^2 shown in magenta, cyan, and black, respectively, and the grey-scale background map in (b) is the annual mean SLA climatology, with SLA contour lines of ≥ 0.06, 0.04, and 0.02 m shown in magenta, cyan, and black, respectively. The seasons in (c) is divided with Spring of March-May, Summer of June-August, Fall of September-November, and Winter of December-February.

Yes, one important factor about the sequestration time scales of the subduction patches is that whether these injections extend below the depth of permanent pycnocline (PP), which hinders their return to the atmosphere. The subduction patches above the depth of PP cannot be isolated for a long time because they would dissipate in the next year. The depth of PP differs between the subtropical section (i.e., south of 35° N) and subpolar section (i.e., north of 35° N) of the western North Pacific. Therefore, it is better to quantify the subduction patches separately for each section and for each depth (i.e., above or below the depth of PP).

Based on 16 years' Argo float data (which provides temperature and salinity profiles) (N=1,226,177), Feucher et al. (2019) investigated the distribution of the depth of permanent pycnocline in the world ocean. As outlined in the red box in Fig. R1, the depth of PP was shallower (≤300 m) and deeper (≥400-450m) depth of PP in the subpolar and subtropical sections of the western North Pacific, respectively. In addition, based on the 7120 profiles from the BGC-Argo floats (Fig. 2), we also analyzed the monthly dynamics of the maximum MLD with variance in both of the subtropical (i.e., south of 35° N) and subpolar (i.e., north of 35° N) sections of the western North Pacific. 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 the year in both sections, and values are comparable with those shown in Fig. R1 in Feucher et al. (2019), and more importantly, consistent with the spatial patterns shown in Fig. R1, our data also show that the annual maximum MLD (i.e., potential depth of PP) is shallower in the subpolar section, and deeper in the subtropical section. Mathematically, our data show that 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 quite preliminary based on a much smaller dataset than that in Feucher et al. (2019). With the analyses above, and to be conservative, we finally chose to use the maximum potential of PP depth of 300 m and 450m for the subpolar and subtropical sections, respectively, in our analysis of the subduction patches in each layer (i.e., above or below the depth of PP).

As a result, in the subtropical section (PP depth at 450 m), 104 (31.0%) and 56 (16.7%) subduction patches were found to below the depth the permanent pycnocline (i.e., 450 m); and in the subpolar section, 141 (42.1%) and 34 (10.1%) subduction patches were above the permanent pycnocline. These results were added in the revised manuscript (see Lines 338-348, Lines 368-373, and Table 1, in the clean version of the revised manuscript).

In addition, we also separately quantified the average AOU and DO anomalies and DO inventories below and above the depth of permanent pycnocline for each section. Please see our replies to the following comment below.

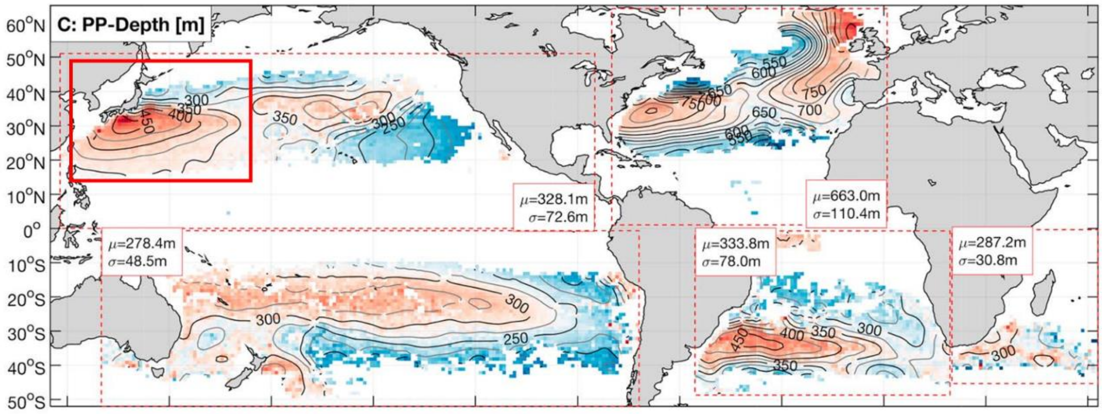


Fig. R1 The 2000-2015 Argo climatology of permanent pycnocline (PP) depth in the global ocean. Contours are every 25 m, labeled every 50 m. Cited from Fig. 2 in Feucher et al. (2019). Our study region is outlined in red.

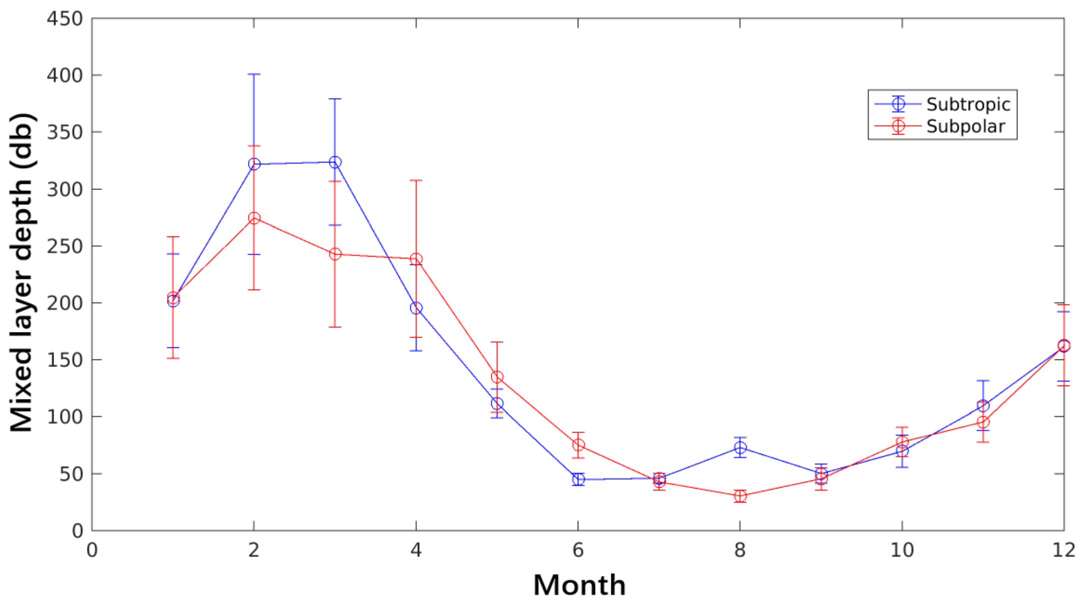


Fig. R2 The monthly variations of the maximum mixed layer depth (MLD) in the subtropical (i.e., south of 35° N) and subpolar (i.e., north of 35° N) sections of the western North Pacific, respectively, based on all the BGC-Argo profiles (N=7120, see Fig. 2). The errorbar represents one standard deviation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

2) Oxygen and carbon inventories should be estimated in each region and depth after classifying the water masses as described above. Subduction patches shallower than the permanent pycnocline are not isolated for long time because they dissipate in the next year. On the other hand, subduction below the permanent thermocline, where carbon can be sequestered from the atmosphere for long time, is the important transport process. It will be difficult to clarify the entire spatial distribution of patches below the permanent pycnocline and to estimate the amount of material transported by subduction using Argo dataset. However, it may be meaningful to calculate the mean value of the patches separately by area and depth, and compare them.

Reply: Following your suggestion, here we calculated the mean values of the patches separately for subtropical (i.e., south of 35° N) and subpolar (i.e., north of 35° N)) regions of the western North Pacific, and for each region, we also calculate separately for above and below the depth of permanent pycnocline (PP). The depths of PP in the subtropical and subpolar regions were referred as 450 m and 300 m, respectively (see our reply to the first major comment above, Figs. R1 & R2). For each layer (above or below the depth of PP) in each region, we quantified the number of subduction patches identified, mean ΔAOU, mean ΔDO, mean thickness (i.e., vertical extension) of the subduction patches, and the mean DO inventory based on the two methods (i.e., integrated area (IA), and peak height (PH) method).

The statistics are summarized in Table R1. Specifically, in the subpolar region, 34 (10.1%) and 141 (42.1%) subduction patches were identified above and below the depth of PP (i.e., 300m), and for each layer, the mean ΔAOU are -32.9 and -25.8 μmol/kg, mean ΔDO are 42.5 and 32.5 μmol/kg, mean thickness of 127.5 and 126.6 m, and the DO $inventory_{IA}$ are 92.6 and 61.2 g O_2 /m^2; in the subtropical region, 56 (16.7) and 104 (31.0%) of the subduction patches were identified above and below the depth of PP (i.e., 450m), and for each layer, the mean ΔAOU are -27.2 and -28.5 μmol/kg, mean ΔDO are 31.2 and 36.4 μmol/kg, mean thickness of 128.7 and 128.1 m, and the DO $inventory_{IA}$ are 51.7 and 64.3 g O_2 /m^2. In general, it is found that, (1) the vertical extension (i.e., thickness) of the subduction patches identified in each layer and in each region did not vary much between 126.6 m and 128.7 m; (2) the mean ΔAOU, ΔDO, and DO inventory were stronger above the depth of PP than those below the depth of PP in the subpolar region, yet the opposite case occurs in the subtropical region, where the mean ΔAOU, ΔDO, and DO inventory were weaker above the depth of PP than those below the depth of P; (3) the mean ΔAOU, ΔDO, and DO inventory in the subtropical region below 450 m were also weaker than those in the subpolar region above 300 m, which further supports the potential pathway shown in Fig. 7, where subduction occurred in the northern KE (i.e., > 35° N, subpolar section) and the subducted water traveled south and deeper along isopycnal surface as illustrated in Fig. 1.

Accordingly, these additional results and analyses were added in the revised manuscript

(see Lines 449-465, and Table 1, clean version).

Table R1. Statistics on the subduction patches in the subtropical and subpolar sections of the western North Pacific, for both above and below the depth of permanent pycnocline (PP) in each section. The depths of PP in the subtropical and subpolar sections were referred as 450 m and 300 m, respectively, based on Figs. R1 & R2. Note that the percentages in the third column (i.e., Number of patches) refer to the ratios between the number of subduction patches identified in each category and the total number of subduction patches identified (N=335), and "H" refers to the thickness (i.e., vertical extension) of the subduction patch.

Region	Layer	Number of patches	Mean ΔAOU (μmol/kg)	Mean ΔDO (μmol/kg)	Mean H (m)	DO $inventory_{IA}$ (g O_2 /m^2)	DO $inventory_{PH}$ (g O_2 /m^2)
Subtropical	< 450 m	56 (16.7%)	-27.2±17.7	31.2±20.4	128.7±27.1	51.7±45.9	132.1±106.2
	≥ 450 m	104 (31.0%)	-28.5±15.3	36.4±18.0	128.1±25.8	64.3±50.6	161.5±103.0
Subpolar	< 300 m	34 (10.1%)	-32.9±15.5	42.5±17.7	127.5±35.0	92.6±59.7	197.5±115.3
	≥ 300 m	141 (42.1%)	-25.8±15.9	32.5±20.9	126.6±23.2	61.2±53.1	142.1±108.1
Whole area	< 450 m	161 (48.1%)	-29.7±16.7	36.7±19.7	126.8±26.8	68.5±52.8	160.5±108.0
	≥ 450 m	174 (51.9%)	-25.6±15.2	32.5±19.8	128.2±25.1	59.4±52.5	144.3±108.0

Specific comments

Line133-136: The material transport by subduction is a topic of great interest to Biogeochemical scientists. However, "spicity" is not a familiar parameter, so a detailed explanation of spicity is needed.

Reply: Sea water is a two-component system. Water mass anomaly is commonly analyzed in term of (potential) temperature and salinity anomaly. Isopycnal analysis is also widely used. By definition, temperature and salinity anomaly on an isopycnal surface is density compensated; thus, water mass anomaly on an isopycnal surface is commonly described in term of another thermodynamic variable, which is called spice, spiciness or spicity. Over the past decades, there have been different definitions of such a thermodynamic variable; however, a most desirable property of such a thermodynamic function is that it is orthogonal to the density. Recently, Huang et al. (2018) proposed a potential spicity function (π) by the least square method, which is practically orthogonal to the potential density, with the root-mean-square of angle deviation from orthogonality at the value of 0.0001°. Therefore, combining density and spicity gives rise

to an orthogonal coordinate system, and it is the thermodynamic variable we used in this study. More description on spicity was added in the revision.

Reference:

Huang, R. X., L.-S. Yu and S.-Q. Zhou (2018). Potential spicity defined by the least square method. Journal of Geophysical Research-Oceans, 123, 7351-7365.

Line242-244: The authors assume that the lifetime of subduction patches is one year. However, it is expected to vary depending on the depth and the region. This calculation should be reconsidered.

Reply: A similar comment was also raised by Reviewer # 1 (Joan Llort). Converting carbon and oxygen inventories to export fluxes requires some knowledge of subduction rates, which can vary exponentially from the surface to the deep ocean. To be conservative, we assumed the lifetime of subduction patches to be on an annual scale based on the seasonal statistics of the subduction we detected. Yet this would still involve some uncertainties, considering the dynamics of subduction at different depths and in different regions. However, we did not find any better way to resolve the subduction rates. As such, to avoid any misleading results and analyses, we removed the calculations of fluxes and clarified the reason in the revision (see Lines 529-540, clean version of the manuscript).

Fig.4b: The anomalies of spicity, indicated by boxes in Fig.4b, is unclear.

Reply: It could be caused by the reduced resolution in PDF file. The anomalies of spicity in Fig. 4b are clearly shown in our original figure at full resolution. We will make sure the original figure will be used when it is to be published.

Line297-303: The fact that anomalies were observed four consecutive profiles does not mean a sustained or a large spatial subduction event. A further analysis of the water mass using the surrounding water temperature and salinity would be necessary to show the spread of the subduction.

Reply: We did further analysis of the water mass. Fig. R3 shows the corresponding time series of temperature (a), salinity (b) and potential density (c). Corresponding to the anomalies identified in boxes 1-3 in Fig. 4b-4d, we found that salinity shows similar anomalous patches. As such, we suspect that the distinct and continuous water patches observed from the four consecutive profiles in box 3 were mostly likely resulted from one subduction event. We added this analysis in the revision (see Lines 302-305, clean version of the revised manuscript).

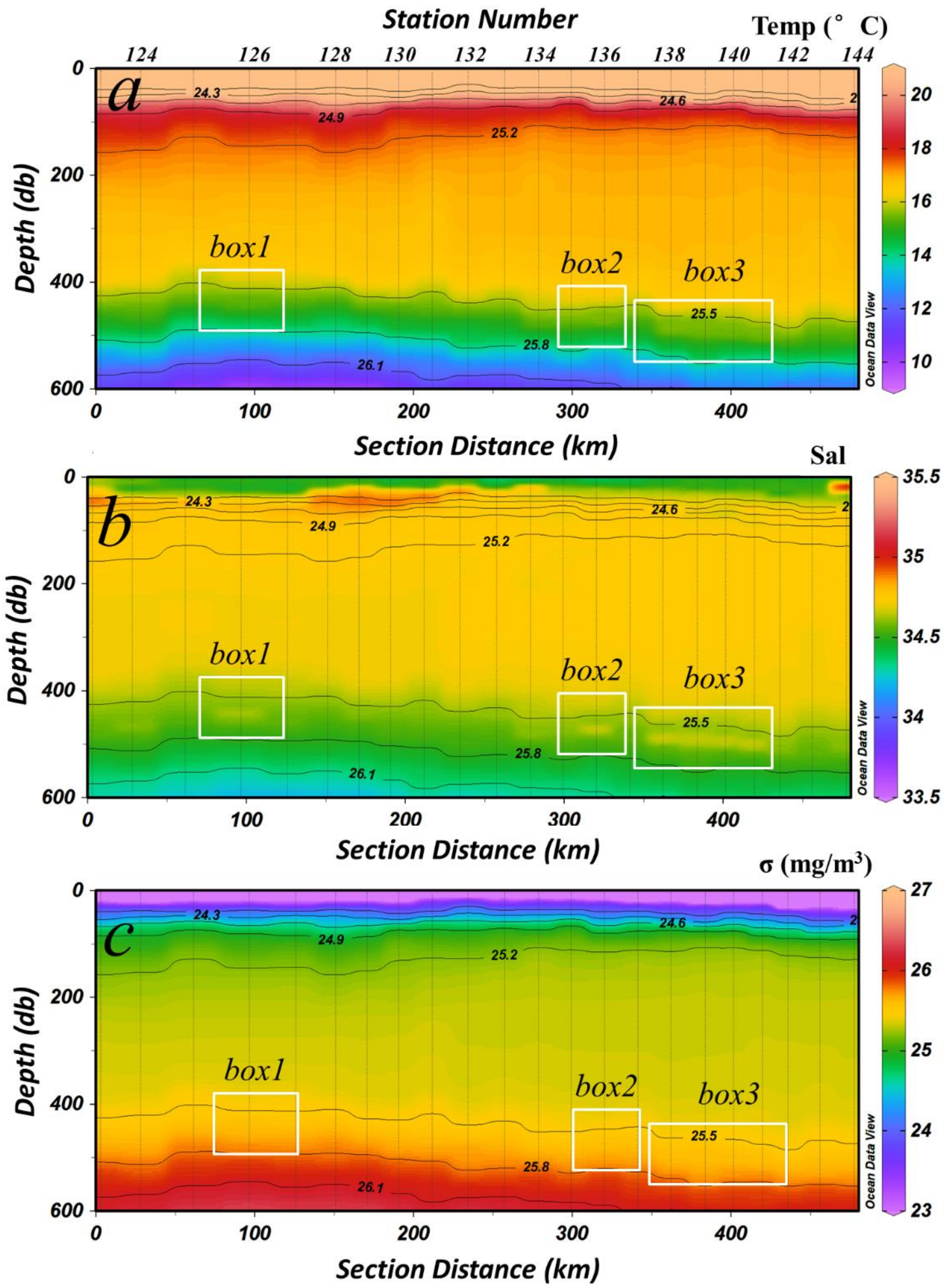


Fig. R3 Time series of temperature (a), salinity (b) and potential density (c) of float MR2901556 between July 28th, 2014 and August 18th, 2014.

Fig.5: The distributions of EKE and SLA are unclear. It might be better to use only contour line instead of color gradients.

Reply: Fig. 5 is updated in the revision. We added contour lines on the color map to better show the distributions of EKE and SLA.

Fig.6: I do not think it is necessary to apply smoothing to these graphs; it would be easier to show them in a monthly summary, as in Fig.2S.

Reply: Different from Fig. S2, we want to resolve the temporal distribution of each term (i.e., the number of patches, integrated AOU anomaly, integrated spicity anomaly, and integrated DO anomaly) on a finer time scale (i.e., daily) in Fig. 6. The main reason for a 7-point smoothing was to reduce the noise. For example, Fig. R4 shows the data plots with and without smoothing. Clearly, the time series are somewhat noisier without smoothing, making it more difficult to interpret. However, with a 7-point smoothing, the temporal variation patterns were much clearer in each quantity, with most signals being observed between March (the maximum) and August.

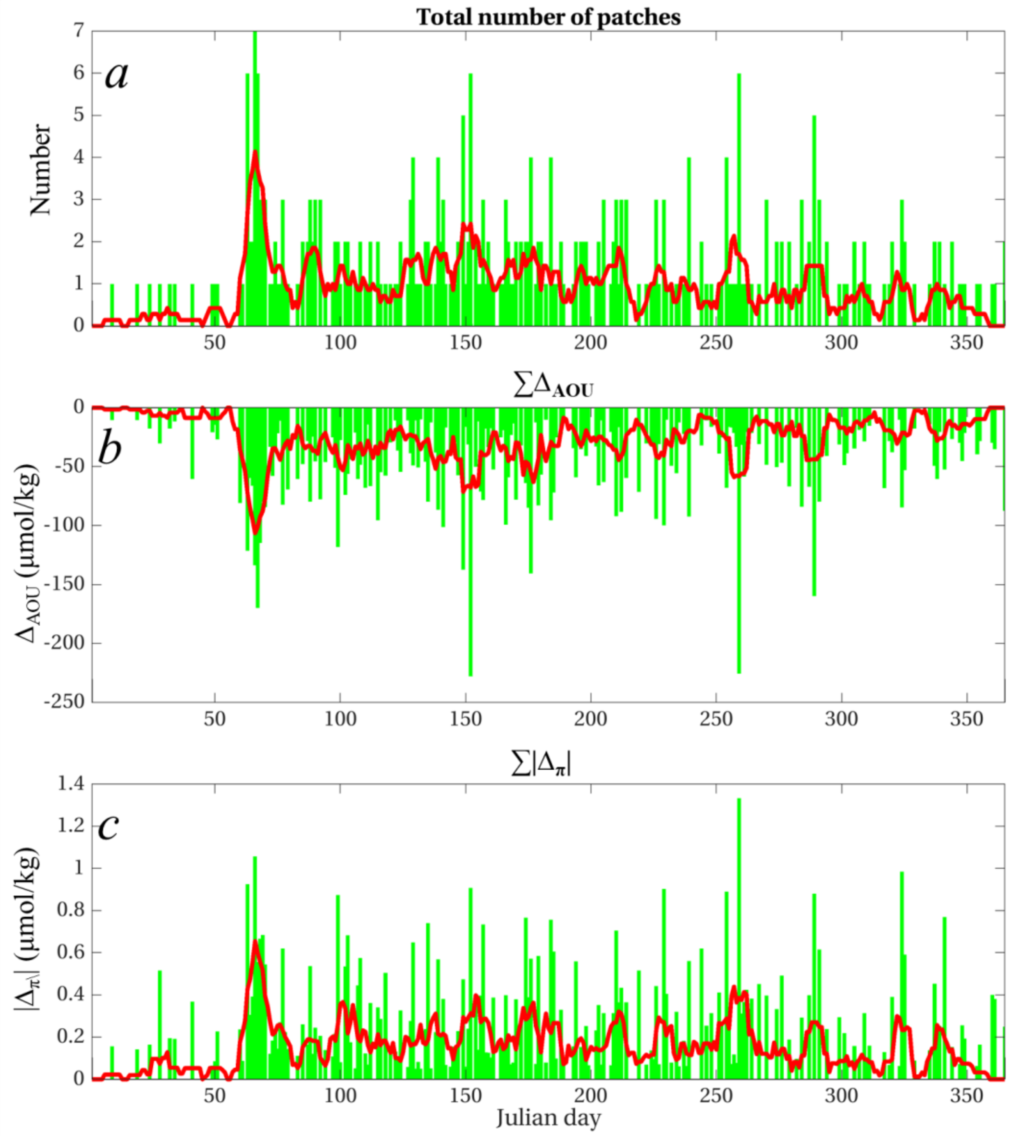


Fig. R4 Temporal distribution of the number of patches (a), integrated AOU anomaly (b), and integrated π anomaly (c), by Julian day. Note that the green bars are the quantities on each day without smoothing, and the red curves are based on 7-point smoothing.

Line 398: AOU or ΔAOU?

Reply: Corrected. It is ΔAOU.

Line 403-404: The authors mention high levels of phytoplankton production as the cause of the strong AOU anomaly from March to August, but it should be clear whether this indicates the release of oxygen from phytoplankton production or the consumption of oxygen by decomposing large amount of organic matter.

Reply: Clarified in the revision. High levels of phytoplankton production (i.e., higher levels of phytoplankton biomass) results in a greater degree of respiration in the subducting waters. More respiration means a greater degree of oxygen consumption and thus a more negative offset from the surface-saturated concentrations before subduction.

Table2: I cannot understand what inventories and exports on the peak height mean.

Reply: More description on the peak height calculations was added in the revision (see Section 2.2.2). Take the oxygen inventory as an example, we firstly integrated the DO anomaly against the referred baseline (Eq. R1), this integrated anomaly represents the inventory of anomalous DO resulted from the subduction patch, this quantity was illustrated as the area with dash lines crossed on the left panel in Fig. R5. We also quantified the DO inventory using peak height method (Eq. R2). Here Δ_{DO_peak} is DO anomaly referred to the baseline at the peak and H is the vertical scale of the subduction patch [see Section 2.2.1 for how a peak was detected and how H was defined]. The shaded area on the right panel in Fig. R5 represents the derived DO inventory using Eq. R2. This derived quantity represents the maximum potential of DO inventory within the subduction patch.

$$\textbf{Oxygen Inventory}_{IA}\ (\text{g } O_2/m^2) = \sum_{z=p1}^{z=p2} \Delta DO_Z \qquad \textbf{(Eq. R1)}$$

$$\textbf{Oxygen Inventory}_{\_PH}\ (\text{g } O_2/m^2) = \Delta_{DO\_peak} \times H \qquad \textbf{(Eq. R2)}$$

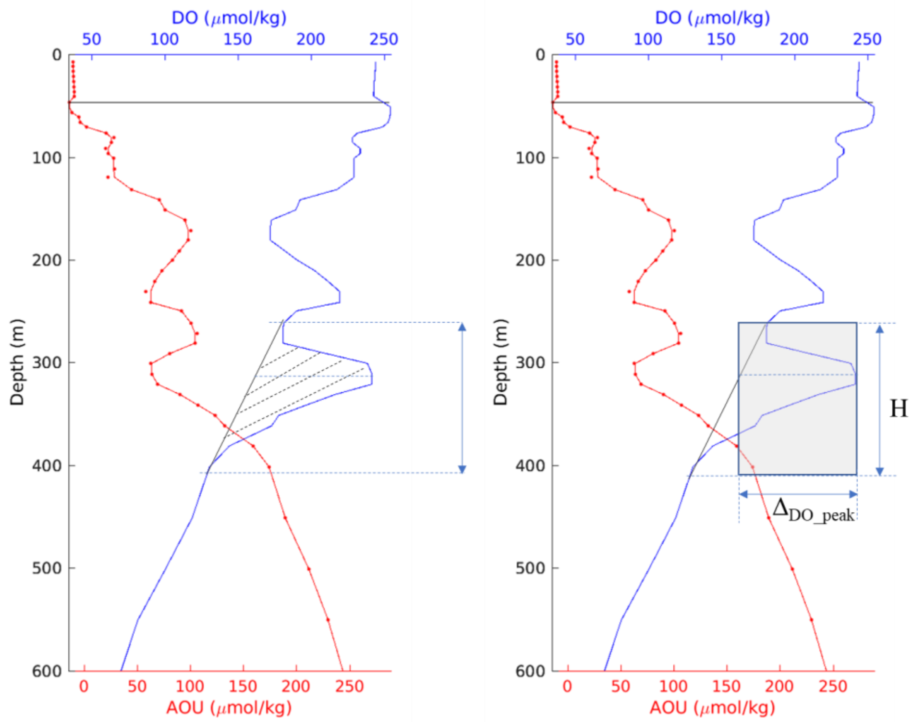


Fig. R5 Illustration of DO inventory calculated from Eq. R1 (left panel) and Eq. R2 (right panel). The DO inventory using peak height method (Eq. R2) represents the maximum potential of the DO inventory from the subduction patch.

It should be noted that, following suggestions from Reviewer #1 comments, we removed the calculations of fluxes in the revision.

Line 463-469: The oxygen export rate calculated here should not be compared to the oxygen consumption rate in the Southern Ocean.

Reply: Considering the difficulty in parameterizing subduction rates and the large potential uncertainties involved, we removed the fluxes-related results and analyses in the revision. Please see our related replies to the comments from Reviewer # 1 for details.

Comments from Reviewer #3

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. As the reviewer points out, the AOU in subducting water would include the respiration of "transient" particles sinking through the subducting finger of water, and it is difficult to estimate this contribution. However, as these deeper events appear to be associated with highly productive eddy margins, this transient contribution likely will be a smaller percentage of the AOU, and could be assessed by examining AOU across a broader spatial scale. Nevertheless, the uncertainties involved in using a fixed C:O ratio and a fixed lifetime of subduction patch in our calculations led us to now remove the carbon-related and flux-related calculations in the revision. Please see our replies to the major comments from Reviewer # 1 for more details.

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. R6 shows the latitudinal and longitudinal variations of the mean depth positions of the subduction patches. Along the latitude (Fig. R6a), 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. R6b), 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. R7. 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. We note that respiration rates of organic matter in the patch will be non-linear with time, as the quality of organic matter changes, and will not "cease" (over many months), which would add further uncertainties to these calculations (actually probably more that the uncertainties associated with C:O ratios). With all these considerations, we removed the flux-related analyses in the revision, and only focused on the oxygen inventory associated with the subduction patches. We added more results and analyses in Section 3.3 and 3.4 in the revised manuscript.

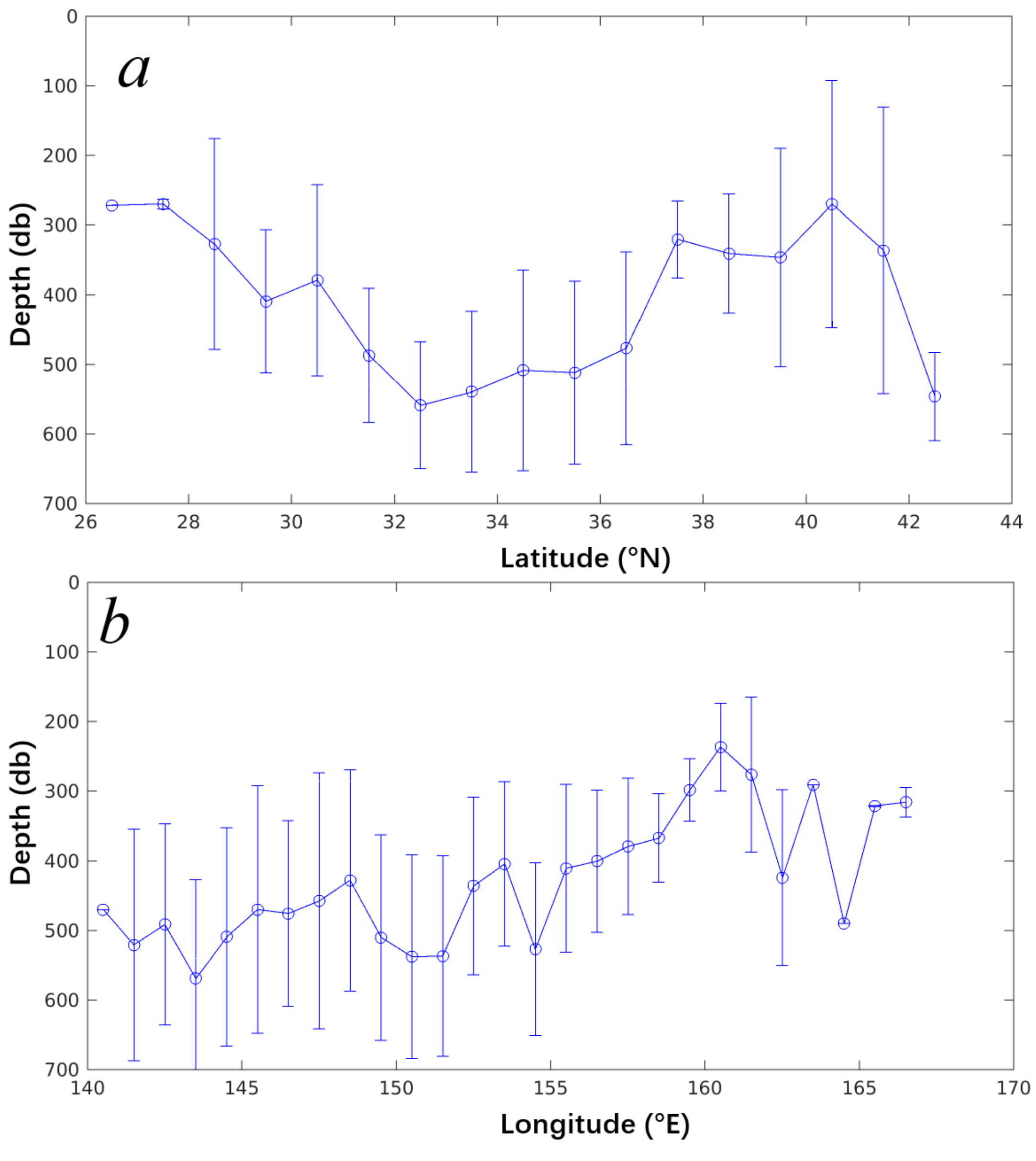


Fig. R6 Variation of the mean depths of the subduction patches observed in each 1° interval of latitude between 26° N and 44° N (a) and of longitude between 140° and 167°

E (b). The overlaid errorbar represents one standard deviation of the depths of the subduction patches in each latitude/longitude interval.

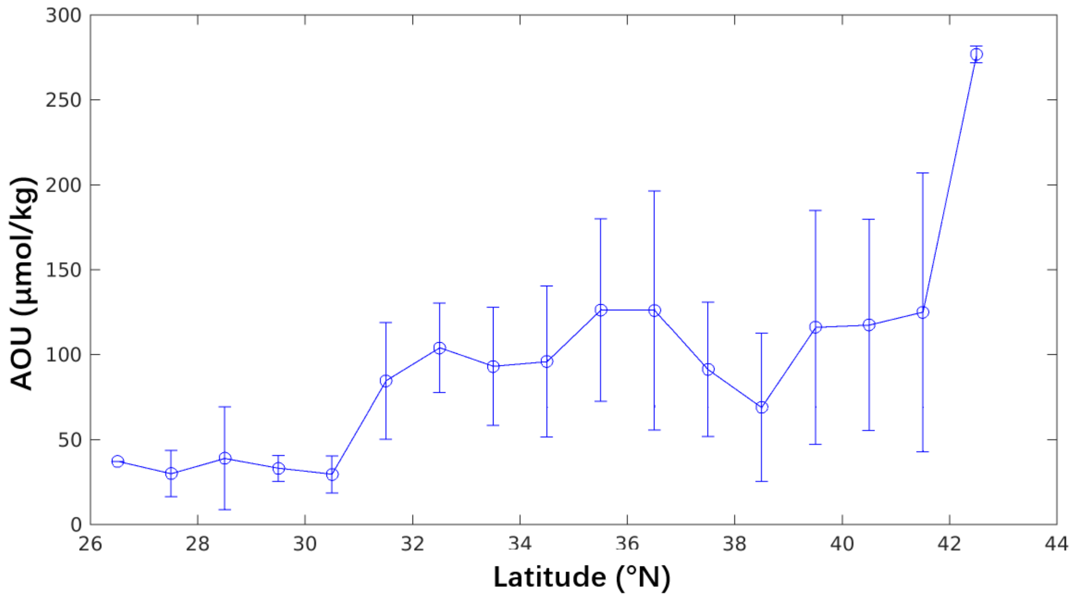


Fig. R7 Variation of the averaged AOU of the subduction patches observed in each 1° interval of latitude between 26° N and 44° N.

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. The reviewer is correct that supersaturation of a few percent can occur when phytoplankton productivity (rate of carbon fixation/oxygen release) is very high, and productivity (vs. production) may have been high enough in the source region of the subducting waters to generate supersaturation. However, this biological enhancement accounts for only a few percent above saturation. We took the conservative approach by assuming there was no supersaturation, meaning we may have underestimated AOU in the subducted waters by up to a few percent in some cases. The reviewer is correct that AOU is highly dependent on surface temperature; temperature and salinity are the primary factors affecting oxygen solubility seawater in contact with the atmosphere). Temperature is largely conservative after subduction, certainly for much longer than these time scales.

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.