**Comment on bg-2021-114 Anonymous Referee #2**

Referee comment on "Seasonal flux patterns and carbon transport from low oxygen eddies at the Cape Verde Ocean Observatory: lessons learned from a time series sediment trap study (2009–2016)" by Gerhard Fischer et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-114-RC2, 2021

The response to the Referees is structured in the following sequence: (1) comments from Referee 2 (RC#2) and (2) *authors' comments (AC)*.

**RC #2**

**General comments :**

This paper provides a precious data set of moored particle traps (1 and 3 km) off the African West coast, North East of Cape Verde, in an area where low oxygen eddies are frequent. I find high value in the data set presented, and recommend publication, but feel a number of important issues need to be addressed prior to publication. I'm not sure that the authors really provide strong data to evidence the effect of low oxygen eddies on C export efficiency, since, as I understand it, not all eddies reported are low oxygen systems. This distinction lacks clarity in the manuscript. Some efforts on identifying these low oxygen eddies in the figure or the text should be made, or maybe reformulate the paper's title to better fit the paper's data content ? I also have some issues with a few of the hypotheses developed in the paper, as explained in details below. Also I have some issues reconciling the presence of a fossil (to the best of my knowledge, but I could be wrong) diatom (*Borogovia*) in the traps, with the dismissal of sediment resuspension events ? I was a bit frustrated not to have more details on diatom cell counts and sample imagery data which could have helped support some of the hypotheses made more strongly. I would also suggest broadening the literature citations to better place these data in perspective.

*AC*
*We greatly thank Reviewer #2 for the time-intensive and critical review and the helpful suggestions and comments. This input has improved the manuscript considerably. We have followed the helpful suggestions and comments in almost all points and will change the manuscript accordingly (see details below).*

**Detailed comments :**

RC#2:

The carbonate content is the dominant flux in the traps (from Figure 6), what is the origin of the carbonate, this is not discussed at all ? Is it from forams, pteropods, coccolithophores, and why is this flux not discussed in relation with the Corg flux ? Is the Carbonate included in the lithogenic flux component in Fig 7 (if not, could the results be added to this figure)? If this is calcite from organisms, this should be distinguished from lithogenic material, as BSi is.

*AC:*
*This is a good comment pointing to an important issue. The carbonate flux is of biogenic origin and is therefore not included in the lithogenic (= non-biogenic) flux in the old Figure 7. Therefore, we have now added the total carbonate flux in Fig. 7 which adds additional information. In the new version, we have mentioned carbonate fluxes in chapter 3.6. and discussed them in chapter 4.1.*
*The composition of the biogenic carbonate is critical and not simple to assess. Generally, planktonic foraminifera and coccolithophores constitute the major part and pteropods are less important. Calcareous dinoflagellates account for negligible percentages in the order of 1% or so.*
*However, coccolithophore-carbonate is extremely difficult to quantitatively assess as this needs, e.g., counts, size measurements and mass of individual coccoliths of different species. In our group, we use ca. 1/1000 wet split for counting etc, or even less (1/4000). In contrast, intact planktonic foraminifera are picked and counted and weighed from a 1/5 wet splits (detritus of forams cannot be picked). So, in essence, the different carbonate masses (coccos, forams, pterpods) mostly do not add up to 100% of the total carbonate measured (which is ok due to detritus from forams and pteropods). Various methods and approaches to tackle this problem have been proposed but all of them contain large errors. In the study by Guerreiro et al. (2021, L&O, doi: 10.1002/lno.11872) conducted in the tropical Atlantic, coccolithophores accounted for ca. 15 to 23% of the carbonate mass (minimum values) in the oligotrophic and mesotrophic oceans. This range of percentages may apply to CVOO as well.*
*This time-consuming analysis has not been done for the CVOO 3-7 record and carbonate fluxes. Since we have measured carbonate fluxes as a bulk value without distinguishing its individual components, we have not discussed the carbonate flux in more detail. However, we now added some sentences adressing this issue in the discussion section 4.1. We also discuss the findings of Guerreiro et al. (2021) mentioned above. In the detailed study on CVOO-3 (Fischer et al. 2016), we counted coccoliths, planktonic foraminifera and pteropods and showed some data in Figs. 7 and 8 in this publication. We found that pteropods account at least for 4-8% and forams for ca. 32-52% of the total carbonate fluxes to the 1290 m trap in winter (CVOO-3, suboxic ACME passage). We will include these findings from Fischer et al. (2016) concerning the composition of carbonate fluxes and new data from K.-H. Baumann on coccolithophore carbonate fluxes in the discussion section 4.1. Based on the Guerreiro et al. (2021) method, they show that coccolithophore-carbonate makes up between ca. 12 and 38% of total carbonate of the upper trap in winter (suboxic ACME passage) at site CVOO. As the coccolithophore carbonate fluxes are not published yet and we included them in the new version, we suggest to include K.-H. Baumann in the list of authors in the new version.*
*Reviewer #2 is right with his/her comments. But discussing the organic carbon flux in relation to the carbonate flux without knowing the contribution from different primary (=coccolithophores) and secondary producers (=forams) makes not so much sense as we have to make too many assumptions due to a lack of field data. This would lead to more questions than answers. The carbonate issue would require an entire paper which we presently prepare for the Cape Blanc time-series study site (Baumann et al.). To our knowledge, there is no single paper which determines the carbonate flux derived from all the different carbonate secreting producers (and then compares to total carbonate fluxes) and discusses this entire issue, the problems and large errors associated with the different methods. This issue is not within the scope of this paper.*

RC#2:
I have difficulties identifying in Fig 5 and 6 if “ACME” events are also low oxygen eddies

events. Could you make the distinction between eddies clearer in your figure legends ?

AC:
*We assume, Reviewer #2 refers to the Figs. 4 and 5. The corresponding oxygen levels are provided in Fig. 3 along with the accompanying low salinity events. As outlined in the text, former studies (e.g. Karstensen et al. 2015) reported that the low oxygen/high productive eddies carry low salinity waters associated with South Atlantic Central Water (SACW). As the moorings carried a lot more salinity sensors (four to six in the upper 100 m) than oxygen sensors (maximally one in the upper 100 m), we used both, low salinities (ca. 35.6 to 35 per mil) and low oxygen (ca. <5 to 110 µM) as an indication for the potential passage of eddies. We now show these eddy events with a duration of at least one month and longer as bars in the two Figures 4 and 5. Note that the events need to be considered carefully as eddies may strive the mooring only with their periphery and that the single oxygen sensor may miss the main signal of the eddy, hence, not resolving it properly with one sensor only. We further indicated the eddy events in Table 3 as an extra column at the right hand end.*

RC#2:
Line 19 : low oxygen eddies are mentioned a bit abruptly without introducing why they are of importance in this area. Maybe add a reformulation of line 40 "The eastern tropical North Atlantic hosts one of the major Oxygen Minimum Zone of the world oceans..." before this to give a bit more context.

AC:
We now added a sentence on this subject at the beginning of the introduction '*Mesoscale eddies with low oxygen concentrations have been suggested to play an important role in the biogeochemistry and carbon cycling in the oligotrophic eastern tropical North Atlantic'*

RC#2:
Line 24 : "quite consistent **sine-wave flux**". From your figures, the mass flux does not really match with this description, which is OK for Figure 2 (Chla/SST). Maybe change this term ?

*AC:*
*The term 'sine-wave-flux' flux pattern was introduced by Berger and Wefer (1990). It is used to describe flux patterns showing an intermediate seasonality. Flux records from different ocean basins may exhibit a constant flux pattern (no seasonality at all, oligotrophic systems) or a highly peaked one (e.g. in the Southern Ocean); these are the extreme settings. A sine-wave flux pattern is between them. We changed this term in the abstract (line 24) to avoid misunderstandings and use the more common and general term ('…we observed consistent seasonal flux patterns….). In the discussion, however, when referring to Berger and Wefer (1990), we maintained this expression. We provide some additional information on this issue in the method chapter 2.2. and we changed Fig. 8c and the caption to better explain this issue (see also below).*

RC#2:
Line 32 : "large diatom aggregates are formed due to strong sear" Sometimes turbulence or shear stress is invoked for aggregation or disaggregation processes". Please substantiate the hypothesis with references (maybe later in the text).

*AC:*

*A good point. We change this hypothesis in line 32 (Abstract) writing: …are formed due to strong shear and turbulence…. We later substantiate this with references in the discussion.*

RC#2:
Line 65 : "High phytoplankton and particle concentration, **high carbon degradation**... have been reported (Fiedler etc...)." How do you reconcile these data with your statement line 70 : "it is assumed that organic carbon attenuation of larger particles in the water column is reduced..."

*AC:*
*A good point and obviously a contradiction. It has recently been shown that increasing primary production in the surface ocean did not show a similar increase in export flux to deep sea (Cael et al. 2021, GRL). The authors suggested that this was due to a biological dampening of variability from primary production to deep-sea POC flux, likely due to higher intensity of zooplankton grazing during productive periods. These observations were in the near-surface ocean under normal (oxygen) conditions. However, if oxygen is strongly reduced in the sub-surface and deeper, degradation of organic carbon may be reduced. We clarified this in this part of the introduction.*

RC#2:
Line 85 : "(e.g. eddy OPAL)". Please add a reference for this information, is this a program, a particular eddy observed in one of the previously cited papers ?

*AC:*
*This refers to the previously cited papers; 'OPAL' is the name of a cyclonic eddy studied during the E-FLUX program (Benitez-Nelson and McGillicuddy, 2008, DSR) in the North Pacific. We now clarified this in the text providing some additional information and reference.*

RC#2:
Line 89 : "Oxygen to total nitrate stoichiometry in an ACME north of Cape Verde was found to be twice as high (16) compared with surrounding waters (8.A)" I don't understand this, do you mean nitrate to oxygen ? In a low oxygen eddy, wouldn't you expect the oxygen to nitrate ratio to be lower than in surrounding waters ?

*AC:*
*We meant the Apparent Oxygen Utilization (AOU) and therefore the AOU/NO3 ratio. Indeed, this is a bit counter-intuitive but this is what has been reported in earlier papers. The surrounding waters of the eddies are close to Redfield stoichiometry and have an AOU/NO3 of 138/16 (equals 8.6). However, inside the eddies, the oxygen removal by respiration was found to be about twice as high as the expected NO3 increase (giving an AOU/NO3 ratio of 16). This is explained by an interplay between local/eddy periphery upwelling of waters originating from the low oxygen core, and a local recycling of nitrate/PP. We clarified this in the revised version.*

RC#2:
Line 105 : "additionally, we found a massive increase of organic matter flux with increasing depth, pointing to particle flux focusing (Fischer et al., 2016)." What about sediment resuspension ? Do you have data to substantiate this ? In Fischer et al 2016, the authors

state : « Resting spores of several coastal species of Chaetoceros, and tycoplanktonic/benthic Delphineis surirella, Neodelphineis indica and Pseudotriceratium punctatum, are secondary contributors. » Does the presence of benthic species such as these, also not point to sediment resuspension or nepheloid layer circulation from the African west coast plateau offshore ? Is it completely excluded that these species could originate from the Cape Verde's plateau ?

*AC:*
*In Fischer et al. (2016), the major findings are that (1) upper and lower traps show similar, strongly seasonal peaks in Feb-Mar 2010 and (2) the deeper trap showed ca. two to three-fold higher fluxes (no peak flux delay due to sinking), depending on the bulk flux component. If this would be attributed to lateral advection, it would imply that the amount of laterally advected material to be very high and - by chance - arrived at the same time in the 3 km than the signal from the 1 km trap (eddy derived) which is unlikely. Furthermore, the overall current direction at CVOO is from NE to SW and the origin of the eddy is traced back to the African coast (Karstensen et al. 2015 BG). We proposed that this material should not come from the Cape Verde Plateau. Dave Archer, the reviewer of the Fischer et al.(2016) paper strongly suggested to name the effect of two to three times higher fluxes in the open ocean 'flux focusing', a concentration effect (wine class effect) of fluxes within a natural funnel associated with the strong low oxygen ACME in winter 2010. In the rest of the record, we did not see this effect and these flux increases at CVOO 3-7 in winter. Dave Archer pointed out that this is the only reliable explanation for our observations. There is for instance an almost perfect relationship between the upper and lower trap lithogenic fluxes shown in Fischer et al. (2016). We are not aware of any open ocean site where all fluxes increased two to three times with depth (1 and 3 km) at the same time in an open-ocean setting. Even organic carbon is increasing dramatically which does not point to resuspension as a major factor. Interestingly, at CVOO this 'flux focusing' happened only once which could be coincidental because the traps may have captured an eddy directly moving over the study site.*
*We did not change much in the introduction with respect to flux focusing as it is discussed in Fischer et al. (2016) in more detail. However, we later provided some more discussion on this issue in the chapters 4.1. and 4.3.*
*We also added some sentences on the occurrences of benthic diatoms at CVOO in the discussion section 4.2. The presence of benthic diatom species in the 1 km trap cups is rather low (ca. 5% of total diatoms) and is attributed to coastal, shallow waters origin (eddy origin, Karstensen et al., 2015) which is transported from the African coast to CVOO within the eddy. The same benthic species have also been found offshore at the trap sites CBeu (Romero et al., 2020) and CBmeso (Romero et al., 2021). Their occurrence in trap locations in the hemipelagic strongly points to lateral transport from the inner shelf (< 50 m) from the African coast. Taken all our observations together, a transport of diatoms from the south of the Cape Verde Plateau is unlikely.*

RC#2:
Line 131 : "as a consequence, the mooring, which has it head buoy" typo "it**s** head buoy". And subsequent phrase : if the head buoy was mostly in an upright position, does it mean it was sometimes reversed ? and how do you infer from that, that the sediment trap had no tilt or that it did not affect collection efficiency ? I find no such details in Fischer et al. 2016 ?

*AC:*
*It is known from literature that several km long mooring lines deployed over a year do not always stay exactly in an exact upright position. Due to changing current intensities and*

*directions, they oscillate between 0 and ca. 15° around the vertical line. This was for instance measured by inclinometers (e.g. Fischer et al. 1996, L&O) or can be calculated from pressure sensors on the mooring array. At site CVOO, pressure sensors indicate a maximal deflection angle of ca. 12° in the upper ca. 1300 m which is in the normal range measured. This might affect trapping efficiency (e.g. in Buesseler et al., 2007, JMR) but it is difficult to quantify these changes (traps move forth and back…). Better studied and probably more important is the current regime around the trap. In a temporally and spatially constrained field experiment, Baker et al. (1988) showed that in currents below 12 cm s-1, the magnitude and characteristics of settling particles collected in moored cylindrical traps were similar to those collected simultaneously in drifting cylindrical traps (Buesseler et al., 2007, JMR). When currents were between 12 and 30 cm s-1, the moored traps under-trapped by 75-95% compared to an identical cylindrical drifting traps near the same site during overlapping time periods (Buesseler et al., 2007). Current speeds in 1 and 3 km were mostly clearly below these values of 12 cm s-1. We provided some more detailed information on this issue in the revised version.*

RC#2:
Line 147-148 : I find the justification for the positioning of the catchment area NE of the trap a bit light, could retro-trajectories modelling not be used to better infer the surface catchment are of the trap ? Have you tried varying different positioning of this rectangle to see if this modified your results?

*AC:*
*Although retro-trajectories would be a good choice to better infer the catchment area, this needs knowledge of the currents at all depths layers over all seasons and years and additionally, and even more critical, the seasonal settling rates of the different types of particles (unknown to us) with depth (particle settling rates appear to increase with depth). Additionally, this is a region with passing mesoscale eddies with strong shear and turbulence and inside currents, which we do not know either on these time scales. Considering all these uncertainties, any assumption on the different setting conditions seemed unrealistic.*
*We made different tests with the size of the study box (1 or 2° boxes) and the location (above the trap, to the west or northwest) but did not observe significant changes of the seasonality of the environmental parameters. We clarified this in the text and added some additional information on this methodological issue.*

RC#2:
Line 154 : Can you please explicit what you use as “seasonality index” ? it is not clear from the cited reference what this refers to exactly ?

*AC:*
*We agree with Reviewer #2 in that this needs some clarification (see also comments below at the end). The approach of Berger and Wefer (1990) was to describe seasonality quantitatively. We show this in Fig. 8c. A summation of the fluxes over the year provides a line (constant flux/production) or a curve. The seasonality index (SI) refers to the production half-time (when 50% of the flux is produced). This means that a horizontal line is drawn at 50% to the flux summation curve. In the modifiedFFig. 8c, we now indicated the 50% flux line to better show this concept of production/flux half-time. At constant flux, 50% of the flux is produced within in six months and this results in a SI of zero (6-6=0, production half-time in months; see arrow, this practical definition also follows Berger and Wefer, 1990). If 50% of the flux is produced in only three months (=higher seasonality), SI increases and is three*

*(6-3=3). To illustrate this, we showed this in the modified figure. In addition, we explain this in the Figure caption and in the method chapter 2.2.*

RC#2:
Line 160 : BSi extraction method : how is the formula used to express BSi in mg ? SiO2 ? Si ? SiO2nH2O ? please explicit the molar conversion used. Also please add the total extraction time for the leaching method. How sure are you of the absence of leachate from labile LSi which can be digested in NaOH?

*AC:*
*We used SiO2 and not SiO2.nH2O for molar conversion as the water content of biogenic opal is unknown and varies within a few percent or so. Therefore, we slightly underestimated the BSi flux. The leaching time depends on the course of the continuous leaching curve (dissolution is fast in fresh diatoms) and we stop recording when reaching a linear dissolution course (see Müller and Schneider, 1993 for more details), mostly after ½ to one hour. Si from clay minerals are corrected for, which may be the other significant Si source. We added some more information on this issue in the method chapter.*

RC#2:
Line 206 : " Unfortunately, oxygen data were not available for the winter season in 2016 but the relationship to the salinity values (Fig 3b) suggest that a low-oxygen ACME was passing in winter 2016 as well." This relationship is not very clear from Fig 3B, could you substantiate this assertion with a correlation or another statistical test?

*AC:*
*Indeed, the relationship is not clear in Fig. 3 and this is not well described before – see comment above. Time series of oxygen (Fig. 3A) and salinity (Fig. 3B) were in the depth range between 30 to 100 m. Oxygen was recorded with one single sensor at nominal depth between 43 and 55 m, salinity with at least three but up to six sensors (depending on the respective deployment period). Well isolated eddies of coastal origin are assumed to correlate with low salinity events, and eventually may be paired with low oxygen concentrations. However, the potential for a productive event is only qualitatively evaluated by the minimal salinity and oxygen recorded and compared to the flux observations. No statistically significant correlation between low oxygen and low salinity as a clear sign for a flux event at the CVOO site is expected because of the complexity in underlying drivers, characterizations and intermittency in space and time. We clarified this inaccuracies in the new ms text and in the caption of Fig. 3.*

RC#2:
Line 240 : I have never heard of the diatom genus *Bogorovia* before, and looking up the literature this appears to be a fossil genus, mostly found in Neogene sediments. Worms also indicates this genus as fossil https://www.marinespecies.org/aphia.php?p=taxlist Could you comment on how you find these diatoms in the traps? Are there some species indeed still living (please substantiate), and if not, this rather proves, maybe also supported by higher mass fluxes in the bottom trap, that some sediment resuspension occurs from the seafloor. This should at least be discussed somewhere. Also how would this affect your BSi measurements with the method chosen if abundant sediments are found in the cups ? How can you distinguish between particle focusing from above, and resuspension from the sea floor/lateral advection ?

*AC:*
*Although Reviewer #2 is correct stating that Bogorovia is a diatom genus originally described as fossil by Josué (1973, Beiheft zur Nova Hedwigia , no. 45, p. 333), it has been found in samples from several trap locations in the Atlantic Ocean (e.g., Romero et al., DSR II 47(9): 1939; Romero et al., 2002, JPR 24, 1035; Romero et al. 2016, PO, 147, 38). Based on light microscope observation of raw trap materials, Bogorovia cells contain chloroplasts, this being indicative of non-fossil diatoms. For the sake of this manuscript, we will re-name it as Bogorovia cf. spp. until more observations are gained and an updated description and geographical distribution of this planktonic diatom is provided in the near future. One additional argument against Bogorovia as only fossil diatom is the fact that none other "fossil" diatom species are found in any of CVOO samples or any of the other trap locations studied.*
*Resuspension from the seafloor (rather flat in this area) may always occur but is strongest in the Benthic Boundary Layer, some tenths of meters above the seafloor (McCave papers). As traps were several hundred meters above the seafloor, the amount of resuspension flux in the deep traps should be minimal and will mostly contain lithogenic material, not e.g. rather fresh organic phytodetritus (e.g. C/N ratios of 8-9) or empty diatom frustules. Additionally, currents in the deep ocean in this area are much lower (around 1 cm s-1) than e.g. over the shelf of West Africa (several tens of cm s-1) were strong undercurrents persist and resuspend e.g. diatoms. Resuspension should provide material rather continuously to the traps and is unlikely to provide a seasonal high and coincident peak in **both** traps in winter-spring.*

RC#2:
Line 247 :

In this first sentence you mention overall composition (is this an average value for all trap samples, a yearly average ? please explicit) in the upper and lower traps, but this does not refer to Fig 6 ? I am missing a figure that would translate Fig 7 for instance into relative % contribution of each fraction along a temporal line.

*AC:*
*The overall composition is given as yearly averages. We clarify this in the revised version at the beginning of chapter 3.5. Fig. 6 refers to the winter situation which is most important with respect to eddy passages and flux peaks and oxygen/salinity minima. This is indicated on top of the figure as well and in the caption. We now also present the percentages in numbers in Figure 6, from which the differences to the annual percentages can be better seen.*
*Fig. 7 is important with respect to the seasonality during different years, which -instead of percentages- needs fluxes. We believe that an additional time series figure with % of all components would not add further information to this issue. We already show three tables and 10 figures. We now plotted %BSi and %Corg on a temporal line in Fig. 5 (see also comment of reviewer #2 below).*

RC#2:
Line 257 : "only organic carbon content remains rather constant, despite...". What about absolute amounts, are these constant also, or do they decrease with depth?

*AC:*
*This is a chapter about the relative composition of the fluxes (%) during winter, Corg and its*

*contribution remained rather constant in the winter months in all four sampled winters (Fig. 6). In the second half of the sentence, we indicate that the absolute total and organic fluxes changed significantly between years (as indicated in Table 3 and Fig. 6). However, changes with depths of the organic carbon is discussed in an extra chapter 4.3; it is also not shown in Fig. 6. We now slightly modified this part referring to Table 3 as well.*

RC#2:
Figure 6 : I'm not sure I understand the rationale for choosing these particular dates in the upper trap only, please elaborate in the text as to why.

*AC:*
*Fig. 6 shows the changes of fluxes (total mass, see numbers) and its composition during winter for the upper traps only to show the potential flux output of the eddies passing in the winters 2010, 2011, 2012 and 2016 at CVOO. We do not discuss changes with depth here, this is done in chapter 4.3. In the new version, we elaborate this in chapter 3.5.*

RC#2:
Line 274 : the authors cite Fisher et al 2016 to corroborate the focusing hypothesis, but I find nowhere in this paper any proof of that. It is also a mere suggestion in that paper. The authors should discuss the different possible explanations (resuspension, lateral advection from another catchment area?, sediment trap collection efficiency .... ) as the sediment focusing theory is not convincing as presented. Could the composition of these particular deep samples help in anyway ? For instance is the proportion of *Bogorovia* high in this sample (could point to resuspension) ? Is it ever found in the upper trap or in the lower trap only ?

*AC:*
*Since flux focusing is not the major subject of this paper and it was already discussed in Fischer et al. (2016; chapter 5.5: Increase of mass fluxes with depth on this issue), we preferably refer to this publication and did not thoroughly discuss issues of lateral advection, resuspension, etc. in this manuscript. We are convinced that this goes beyond the scope of this paper.*
*Moreover, flux focusing only refers to the first deployment (CVOO-3). We already addressed this issue before to explain the three-times higher lithogenic fluxes with depth (also the two-three times higher biogenic ones). In the originally submitted version of Fischer et al. (2016), we did not use the term 'flux focusing'. However, one reviewer suggested to apply this term as the best and most probable explanation for our observations. We are aware that this interpretation needs further studies and observations, but some studies show mass transport within funnel like structures of eddies (e.g. Karstensen et al. 2008, BG; Zhang et al., 2014, Science; Waite et al., 2016, GRL) which might lead to flux focusing at depth. We inserted a short section on this issue at the beginning of discussion chapter 4.3.*
*For Bogorovia, see the comments above.*

RC#2:
Line 320 "Overall, only winter organic carbon fluxes showed a tendency to increase with satellite chlorophyll (table 3) but this relationship is statistically not significant."
Indeed,there are only 5 points in this table, and lack of correlation is obvious. Please add p values or reformulate this sentence according to data. Also please specifiy if you mean organic fluxes in the upper trap only or both upper & bottom traps.

*AC:*
*This refers to the upper traps only whose fluxes are compared to surface chlorophyll concentration. We rephrased this accordingly and provided the p-values and $R^2$ values as well.*

RC#2:
Line 325 and paragraph : can you comment on the differences in seasonality between your study and others carried out in the North Atlantic (for instance PAP where high fluxes are usually recorded in summer) ?

*AC:*
*We partly commented on this issue above. We believe that such a comparison is not really worth of as the PAP site shows a typical spring bloom (early summer sedimentation), a different flux seasonality (not oligotrophic) and is not characteristic for low oxygen eddies. Instead, we applied carbon export fluxes and attenuation from the nearby oligotrophic site NASG (Marsay et al., 2015) for comparison to our oligotrophic setting at CVOO (chapter 4.3.).*

RC#2:
Line 333 : the "flux focusing within a funnel-like structure (Fischer et al., 2016) " is again not explained nor substantiated in the cited paper. Can you find other references for this process maybe?

*AC:*
*See the discussion above and below about 'flux focusing' (Fischer et al., 2016). We did not add other references because 'flux focusing' refers to particle fluxes recorded by sediment traps and there are no other direct observations available yet. However, we now added a few other references (chapter 4.1 but also 4.3.) dealing with this issue in mesoscale eddies (Zhang et al., 2014, Science; Waite et al., 2016, GRL) which are partly based on modelling. To our knowledge, no direct/in situ observations on the transport and pathways of particles within ACMEs have been performed up to now.*

RC#2:
Line 334 : Table 2. Please add the legend to your column headers (full name of the parameter and unit). In table 2 you use Carb, in table 3 $CaCO_3$, please use consistent terms. I seem to better understand now that Figure 6 and Table 2 show the fluxes composition or correlation during the passage of an ACME. But is there any information to be given about their oxygen content ? are they all low oxygen ? Maybe indicate this somehow as also suggested for Figure 4 & 5 (see below).

*AC:*
*We changed Table 2 accordingly. As mentioned above, the coverage of the oxygen and salinity sensors is very different, and eddy events are now defined with low salinities (35.0-35.6 per mil) and low oxygen (<5 to 110 µM). This is indicated in Tables 2 and 3, and Figs. 4 and 5 (see comment above).*

RC#2:
Line 342 : the authors state the hypothesis that large diatoms aggregates form at the eddies edges and sink passively. Why are grazing and transfer via fecal pellets ruled out in this explanation? Were the cup samples observed in microscopy for fecal pellets or aggregates?

*AC:*
*Reviewer #2 is right. The major problem is that most larger particles such as diatom aggregates or fecal pellets are generally not at all or not completely preserved in classical sediment trap cups. Fecal pellets, however, sometimes reach the trap cups undestroyed and remain preserved. Some fecal pellets counts (CVOO -3 upper trap samples #1-5) from microscopic analysis for the extreme low oxygen ACME in winter 2010 are available. We actually observed higher contribution from zooplankton fecal pellets when the low oxygen eddy was passing over the trap position compared to non-eddy conditions. This was quite surprising but needs further observations and studies. Obviously, some zooplankton might tolerate low oxygen conditions within the eddy; some zooplankton might also feed on particles at the eddy edges with slightly higher oxygen compared to the very low oxygen core of the eddy? Studies on these issues are underway (e.g. in REEBUS, CVOO) but we have not enough flux data to discuss it in detail. As requested, we mention now this scenario.*

RC#2:
Line 357 and following : This is an interesting result, more than half the yearly diatom flux reach the traps in less than 70 days. Again why is the grazing hypothesis ruled out so swiftly ? The cleaning treatment of diatom valves is most likely to have disaggregated any organic material covered aggregates or fecal pellets, any direct observations made?

*AC:*
*A strong seasonality of the diatom flux is typical of open-ocean traps (see review in Romero & Armand, 2010. In: The Diatoms, Ed: Smol & Stoermer, Cambridge University Press). This is mainly due to seasonal nutrient supply, which defines the diatom productivity season. Observations of the raw material (before chemical treatment for permanent slide preparation) allow us to see almost none aggregates and/or fecal pellets.*

RC#2:
Line 362 : “the decline of the diatom maxima” where ?

*AC:*
*It reads now “The decline of diatom maxima by mid-January 2016…”*

RC#2:
Line 372 : I don’t really understand the explanation for the discrepancy between the two BSi peaks in the trap in 2016 and the diatom valve peak. The two distinct BSi peaks are associated to super low diatom valve flux, how is this possible? The difference in Si content between small but highly silicified cells such *Chaetoceros* resting spores or *Thalassionema nitschioides* var *parva* and slightly larger pennates does not seem like a credible explanation. Are pictures, maybe SEM images or cell counts data (with cell size) available for each of these cups? The fluxes in BSi and diatom valves seem completely anticorrelated for 2016. Has a mix up of cell count samples been envisaged? BSi and Corg fluxes seem consistent.

*AC:*
*It is well known that larger valves quantitatively contribute more to the BSi flux than smaller ones. It happens that mostly small valves contribute to diatom flux maxima during some intervals, although this is not necessarily matched by maxima of total BSi flux. Therefore, it is not expected that diatom maxima always match the occurrence of BSi peaks. It also needs to be considered that the methods used for quantification of diatoms (census) and*

*BSi (leaching sequence technique) are based on different approaches, hence, a perfect match between both diatoms and BSi fluxes must not always be the case.*

RC#2:
Line 383 : "However, small particles seem to react differently and show a normal to higher attenuation". A normal to higher attenuation compared to what ? Large particles ? Is this phrase addressing your results, or those of Rasse and Dall'Ormo, I'm confused.

*AC:*

*We rephrase as follows: However, small particles seem to react differently and show a normal-to-higher attenuation inside the OMZ compared to the outside with sufficient oxygen availability. This refers to Rasse and Dall'Ormo (2019).*

RC#2:
Discussion overall : I find the discussion section a bit confusing, contradictory hypothesis are invoked but no clear data really helps one way or another. There is a relative smaller use of the literature in the discussion section to help explain potential processes, and it is mostly focused on companion papers and does not really place the study in a broader perspective. I feel this section could be reworked a little to make its conclusion more credible.

*AC:*

*These are important points raised by Reviewer #2 and we reworked this overall section including other studies as well (e.g., site BATS as oligotrophic setting for comparison (Conte et al., 2001, site NASG for oligotrophic export fluxes and carbon attenuation in the North Atlantic Subtropical Gyre, Marsay et al., 2015) to put our studies into a broader perspective and make the conclusions more credible. However, flux studies of eddy structures with low oxygen are still rare and this is one explanation for the lack of comparison to other sites. The intension of this study was not a comparison to other sites in the Atlantic Ocean or in other ocean basins with often much longer records.*
*Reviewer #2 is also right in his/her statement that we have contradictory hypothesis and that the flux data raise several new questions instead of explaining the respective processes within the eddy structures. We admit that some issues are not fully understood and further studies of particle dynamics under eddy conditions are needed. It seems that eddy studies with respect to particle fluxes and particle transport pathways are at the beginning and we need further records to shed light on the internal, upper and deep water processes within the different types of eddies. It is clear that major processes of attenuation occur in the epipelagic and mesopelagic (e.g. Marsay et al., 2015) which is above the 1 and 3 km traps. We have started a drifting flux program with a larger GEOMAR program to study particle formation and particle attenuation in the upper ca. 500 m. However, we are at the beginning of evaluating the data.*

RC#2:
Conclusion : I would suggest editing your conclusion as to remove the bullet points format.

*AC:*

*We removed it.*

RC#2:
Line 447 : still not convinced by this. If smaller diatoms are conveyed to depth within larger fast settling diatom aggregates, then there should also be a lot of OrgC associated to these aggregates, and figure 5 shows a low point in Corg fluxes as well during this valve peak event similarly to low BSi.

*AC:*

*A good point of course and an issue not solved yet. This is only an assumption, and we are also not convinced whether this true or not. In any case, small diatoms found in the traps need larger and faster sinking particles to be transported downwards to 1 and 3 km water depth. Besides larger diatom aggregates, fecal pellets might also play some role. We changed that sentence accordingly and invoked the potential transport of other vehicles e.g., fecal pellets. We discussed this point above at the end of section 4.1.*

RC#2:
Flux data such as these are precious, I suggest adding a supplementary in which all flux data per cup for all fractions (total mass, BSi, Carbonate, Corg, Lithogenic) can be found.

*AC:*

*These data are already there and will be made available after acceptance of the ms. [https://doi.pangaea.de/10.1594/PANGAEA.931052](https://doi.pangaea.de/10.1594/PANGAEA.931052) (data policy of BG)*

RC#2:
**Discussion/References**

RC#2:
Other historical long term surveys of mass flux, with specific composition, at PAP but also at BATS have been highly documented for the North Atlantic, yet I did not really see citations or comparison of your data with these studies, I believe the discussion could improve with this perspective.

*AC:*

*Reviewer #2 is right, we did not compare our flux data to PAP nor to BATS as we intend to show the record of a site strongly influenced by low oxygen eddies from the African coast (high production coastal region) and we have only a few years with flux records, albeit with interruptions. For comparison with other flux data from oligotrophic settings, we compared our data to the oligotrophic EUMELI site nearby (Bory et al., 2001) and the oligotrophic site of a dust dynamics study south of the Cape Verde Archipelago (Korte et al., 2017). Both PAP (a spring bloom setting, not a typical oligotrophic site) and BATS have a much longer flux record but are not influenced by low oxygen eddies from a coastal setting. Therefore, we deliberately omitted this comparison.*
*However, we now included some BATS data (Conte et al., 2001) to characterize oligotrophic fluxes in the Atlantic in the discussion section (e.g. seasonality) and used the nearby NASG site in the North Atlantic Subtropical Gyre for comparison of organic carbon flux and attenuation in the epi- and mesopelagic (Marsay et al. (2015) under normal oxygen conditions. We hope that the discussion has now improved and that our data are now presented in a broader context, at least for the Atlantic Ocean.*

**Figures**

RC#2:

Fig1 : 1B. What is the time coverage of the composite chla images ? one month, 3 months ? please specify. Also could you add the white rectangle to your Fig1b for better assessment of surface heterogeneity ?

*AC:*
*MODIS winter chlorophyll means are DJFM mean values. We provided this information and we added white rectangles in Fig. 1b in the new version.*

Fig 2 legend : line 677 : “wi sp su fa” there seems to be a typing or format glitch here. Oh this means winter spring summer fall... this did not occur to me before long, please clarify this text.

*AC:*
*We explained in the caption now.*

Fig 3. Maybe add a panel c for statistical correlation/relationship between these datasets ?

*AC:*
*Indeed, the relationship is not clear in Fig. 3 and this was not well described before – see comment above. Time series of oxygen (Fig. 3A) and salinity (Fig. 3B) were in the depth range between 30 to 100m. Oxygen was recorded with one single sensor at nominal depth between 43 and 55 m, salinity with at least three but up to six sensors (depending on the respective deployment period). Well isolated eddies of coastal origin are assumed to correlate with low salinity events, and eventually may be paired with low oxygen concentrations. However, the potential for a productive event is only qualitatively evaluated by the minimal salinity and oxygen recorded and compared to the flux observations. No statistically significant correlation between low oxygen and low salinity as a clear sign for a flux event at the CVOO site is expected due to the complex interplay among underlying drivers, characterizations and intermittency in space and time. We clarified these inaccuracies in the new version in the text and in the caption of Figure 3.*

Fig. 5 the relative contribution of each fraction (to 100%) in a panel d, along the same temporal axis would be a helpful addition to your figure.

*AC:*
*We now plotted the % of Corg and BSi of total flux (diatoms are given as numbers) in Fig. 5, so this additional information requested is provided.*

Figs 4 and 5. I find the legend of ACME difficult to relate to one cup measurement in particular. Could you maybe draw an arrow to show which bar is concerned by the passage of an ACME. Also, I would find it useful to identify on these figures somehow the passage of low oxygen eddies ? It’s very difficult to overlap Fig 3a to Fig 4&5 mentally.

AC:
*Reviewer #2 is right with this statement, even if we do not find a relationship between e.g. oxygen minima in winter and carbon fluxes in 1 km. Due to the different coverage of oxygen and salinity sensors (see several comments above), we now defined eddy events based on low salinities (35.0 to 35.6 per mil) and oxygen (< 5 to 110 µM) with a > 1 month duration and plotted them into Figs 4 and 5 to better illustrate them. It is now easier to overlap Fig. 3*

*and Figs. 4/5 for the readership, and this allows to more easily relate the passage of low oxygen (low salinity) eddies to the winter flux events at site CVOO.*

Fig6. the choice of these events is not very clear from the text nor from the legend, what do the authors want to show with this particular figure ?

*AC:*
*We do not understand this comment as Fig. 6 indicates the % of bulk flux components of total mass flux in winter.*

Fig 8 . the grey shaded area is very faint when printed. But OK on the electronic version. I also suggest choosing two disctinct colors for your sinusoids and fig C. Such subtle shades of green are difficult to distinguish for many people.

*AC:*
*This is right. We changed the gray shaded area to a stippled line and the green colors as well.*

I find Fig.8c very briefly invoked in the discussion (paragraph from line 322-339), and again I don't really understand what is this seasonality index, and how are the values 2.3 and 2.6 calculated ? this figure needs to be better explained/justified, or removed.

*AC:*
*We do not agree that Fig. 8c should be removed. Fig. 8c explains exactly what Reviewer#2 is asking for (see above: what is the seasonality index?). This is shown here using the total fluxes and it is graphically constructed from the 50% flux line as indicated by the arrows leading to half-time production and the SI (6 minus half-time production in months). However, we tried to give some additional information in the Figure, in the caption and in the text in chapter 4.1 to better explain this tool to describe particle flux seasonality in a more quantitative way (Berger and Wefer, 1990). We hope that this now easier for the reader to understand.*

Figure 9 : is there any way to estimate a C transfer efficiency from your dataset ? Can the rectangle data with Chla averages be used to convert into surface POC to calculate this ?

*AC:*
*In chapter 4.3. we now use carbon fluxes from the nearby site NASG (Marsay et al. (2016) to compare to our fluxes and estimates for 100 m water depths (export).*

*Transfer efficiency needs the conversion of satellite chlorophyll images into POC concentrations and primary production, which is difficult enough and associated with many errors in normal oligotrophic settings (without eddies). We think that an estimation of production in these complex eddy systems at CVOO (with a high regional variability of chlorophyll with maxima often at the eddy edges) is associated with many assumptions and errors. Furthermore, it is known that satellites measuring chlorophyll, only capture the uppermost water layer of a few decimeters to meters. No deep chlorophyll maxima or the subduction of chlorophyll at eddy fronts can be recorded/measured. Finally, there is a need of a correlation between chlorophyll and POC and chlorophyll and primary production. Based on these arguments, we decided to not estimate the transfer efficiency.*