I find that the authors have made a substantial effort to take into account my comments. I think the paper could be published after the minor revisions I listed below.

L176: *open squares*

R: Corrected.

Figure 5: *These budgets can be difficult to interpret. I think it would be helpful to explain why there is less grazing during this phase. Besides, it is not clear to me why vertical mixing contributes positively as the mixed layer increases: what is your interpretation of this process? I would not comment on entrainment and add this weak term to vertical mixing.*

R: In an attempt to improve the clarity of argumentation, we have modified Fig. 5 such that the colours of the bars (indicating changes between t1 and t2) now match the colours of the time series.

Less grazing during this phase could be due to a series of processes, including the amount or composition of food that is available to zooplankton for grazing. As grazing is not contributing to the seasonal paradox, we prefer not to go into detail on grazing but instead focus on the budget terms that do contribute to the seasonal paradox.

Regarding mixing, throughout the year, mixing continuously mixes phytoplankton out of mixed layer, as phytoplankton concentration above the mixed layer depth is consistently higher than the concentration below the mixed layer. However, the vertical gradient across the base of the mixed layer is getting smaller from t1 to t2, as the "above the base of mixed layer" phytoplankton concentration is decreasing more dramatically as the mixed layer is deepening compared to "below the base of mixed layer". Therefore, though "mixing" is always negatively contributing to biomass accumulation, the absolute value of "mixing" is decreasing from t1 to t2.

We agree with your suggestion to combine entrainment and mixing, and have done so in the revised manuscript.

L235: *refer explicitly to the figure in the text please.*

R: Done.

L236: *please explain how you obtain 60% (and 40% for temperature) using equation 3.*

R: The contribution is calculated based on the multiplicative function of the light-, temperature-, and nitrogen-related growth factors. We estimated based

on the product rule and the change of each single growth factor over the decline phase how much each factor would contribute to the total growth factor change. To make this clearer, we have added in L237:
"Estimated from the product rule for differentiation and the multiplicative relation of growth factors shown in Eq. 2 & 3".

L255: *The minor role of the seasonality of radiation was also shown in Echevin et al 2008.*

R: Thanks for pointing this out. We have added that the results agree with findings by Echevin et al 2008.

L257: Please refer to figure 5c explicitly in the text.

R: Done.

Section 3.5: Figure B5 is not mentioned anywhere in the text, which is a shame as the authors made the effort to compare with observations. Besides, it is really difficult to evaluate the model's skill to simulate zooplankton biomass from Figure B5. A scatterplot and a correlation value would give a clearer more quantitative view.

R: Thank you for this suggestion. As suggested by the reviewer, we have added the scatter plot (Fig. B5c) and have updated the manuscript accordingly. While the spread is large, the dots mostly scatter around the 1:1 line. The model is not able to capture the observed instances of very high zooplankton biomass, though these are outliers where also the observational data quality may be an issue. Observational mesozooplankton data have a very large error bar because of differences in sampling methods, mesh sizes, seasonal sampling bias, and diurnal vertical migration (O'Brien, 2007). This is why we expect simulated zooplankton biomass not to match observational estimates as well as physical variables do.

L279: why "must" export overcompensate? Please rephrase, the entire sentence is unclear to me.

R: Export efficiency is calculated as the ratio of export to primary production. For export efficiency showing the same seasonality as export (numerator) and primary production (denominator), namely high values in summer and low values in winter, the term in the numerator (export) has to show a larger seasonal variation then the denominator (primary production).

L294: *phytoplankton was diluted*

R: Done.

L335: Messié and Chavez (2015) also mention the effect of nearshore eddies, which transport upwelled nutrients offshore and downward. This effect has been evidenced in several works (e.g. Gruber et al., 2011, Lathuilière et al., 2010) and should be discussed in section 4.2.

R: We added the sentence (L335-336):
"Eddies have been found to favour offshore export and subduction of phytoplankton and nutrients (Lathuiliere et al.,2010; Gruber et al., 2011; Messie and Chavez, 2015)."

L347: Mesozooplankton is ...

R: Done.

L380: "we argue...": Could you explain more clearly what are the open questions about the interactions behind the mixed layer and upwelling dynamics and food web processes? I find the last sentences of this paragraph very vague and I do not see how they will motivate future studies.

R: We rephrased L380-381 and specified:
Given that changes in MLD are correlated to many ecosystem components related to plankton ecosystem functioning, we argue for a more thorough understanding of the impact of the seasonal paradox on the ecosystem. In particular effects on the trophic transfer of energy through the plankton food web to higher trophic levels such as fish will determine ecosystem functions like trophic transfer efficiency, fish production, and ultimately potentially fisheries yields.

Legend of Figure B2: *it would be nice to cite a publication to have details on the two cruises.*

R: Thank you for pointing out the missing reference. We have added the reference provided at PANGAEA with the data set, Thomsen et al., 2016.

L440 and Figure B3: It would be useful to explain how the model mixed layer was computed and whether the method differs from the one used in the ARGO and De Boyer Montegut data sets. Please explain also how you obtained the error bars. The model mixed layer tends to be shallower than ARGO data in june, july, august, september, november. ARGO is also much shallower in Boyer Montegut data set. This need to be commented. You may also be interested to cite a very recent paper which shows that the mixed layer depth bias can reduced when the chlorophyll shading effect is taken into account (Echevin et al., 2021).

R: We have added in the text how we computed the mixed layer depth. To be consistent with the observational data, we calculated the mixed layer depth

offline as the depth with a 0.2°C temperature difference to the surface. We are also more clear in the legend now that the error bar indicates the standard deviation of all data points in the focus area in the respective month. A more detailed comparison between ARGO data, the de Boyer Montégut data, and the simulated mixed layer depth is added in the text. We also included the Echevin et al., 2021 paper, with the argument that the chlorophyll shading effect may partially explain the deep bias of the modelled MLD.

Figure B4: to be consistent with the previous figure error bars (standard deviation) should be shown for MODIS and CROCO.

R: Figure B4 has been updated accordingly.

L451: *It is nice of the authors to have made the effort of comparing zooplankton data with their model results. However I am not very convinced by the comparison. The large scale meridional gradient (low zoo in the south, more zoo in the north) seems to be roughly reproduced, but the large-scale zonal gradient and cross-shore gradient near the coast do not seem well reproduced to me. There tends to be a higher zooplankton biomass to the north west of the white box. A model vs data scatterplot would be also useful. Are the data annual averages? Does the seasonality of zoo in the box fit with the model's seasonality?*

R: Next to adding a scatterplot of observed versus modelled zooplankton biomass as suggested by the reviewer in a comment above, we now show the spatial pattern of modelled and observed zooplankton in two separate maps instead of one to highlight the large-scale pattern that we feel is well represented by the model. We have added also a more detailed discussion of the spatial pattern, and the agreements and disagreements of the modelled zooplankton biomass versus observed estimates, such as the offshore high bias of the model. We suggest that the offshore zooplankton high bias is likely related to the high bias of offshore phytoplankton biomass, which in turn presumably results from a lack of iron limitation in the model. Furthermore, the observational estimates of zooplankton biomass are based on a wide range of methods and accordingly have a large uncertainty that is difficult to quantify. An agreement of model and observations in magnitude and large scale pattern is therefore a meaningful result.

The observational estimates are temporally-averaged because they are very sparse, and there is a summer bias in the observational sampling, with the observational data only covering half of the year. We do call for more extensive observations that cover in particular the full seasonal cycle, we have added this latter point to the manuscript.