Reply to interactive comment on "Interannual evolutions of (sub)mesoscale dynamics in the Bay of Biscay" by Guillaume Charria et al.

Anonymous Referee #2

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This is a relatively short paper documenting the seasonal variability in a high resolution, regional numerical model of the upper ocean in the Bay of Biscay. It is first demonstrated that the model faithfully reproduces the large space and time scale vari- ations in the region. The authors then go on to describe the level of submesoscale variability through the spectral energy of vorticity and vertical buoyancy flux. A relationship between the depth of winter mixing and the energy in the submesoscale is observed.

This paper is well written and the model seems to provide a useful representation of the meso- to submesoscale variability in this region. However, aside from documenting the realism of this particular model, I do not find anything new or novel in the paper. Theories exists that relate submesoscale energy to mixed layer depth (among other things), so the present qualitative finding is entirely expected. It is stated that their results show the importance of submesoscale activity, but this is really only implied. It could be demonstrated by comparison with an otherwise identical model that did not resolve the submesoscale, but this is not done. First one would have to define what quantity they were interested in. It is possible that the submesoscale is important for some things and not for others. The title is somewhat misleading since the subme- soscale is discussed in only about 1 1/2 pages and the interannual in only one short paragraph.

I see this as an editorial decision. I did not really learn much from reading the paper, but it does fairly clearly document some aspects of the fidelity of this regional model. If that fits the goals of the journal then the paper could probably be suitable for publication with some revisions. However, my own recommendation would be to reject the paper since I do not see any reasonable revisions leading to new insights. That is not to say that the model does not contain new and interesting things that could be explored and understood, it is just that does not seem to be the authors objective for writing this paper.

Following major referee comments, the result/discussion parts have been modified including more emphasized results and discussion on the interannual variability. The relationship between forcing terms and ocean features has been discussed including new details on the process explained. However, this remains more limited than expected as we were facing a technical problem to give a full detailed description of the turbulent fluxes (latent and sensitive fluxes) as they have not been saved for this simulation and we do not have the available computing time and infrastructure to perform the same simulation with more saved variables. Nevertheless, we explored the different possible improvements based on offline diagnostics and we propose more detailed analyses to confirm our proposed assumptions in the discussion.

Compared with the previous manuscript, a discussion has also been added on the interannual evolution of the spectral content based on spectral slope analysis.

We believe that the results presented in this paper are leading to a new understanding of the interannual dynamics in the Bay of Biscay and the associated evolution of (sub)mesoscale.

Detailed comments:

(Page 2, line 8): There really isn't any connection in the paper between submesoscale activity and climate change.

Following referee suggestion, the text has bee modified as:

"... long-term fluctuations related to evolutions in atmospheric conditions. ..."

(3,23) More details are needed on the lateral boundary conditions. Is the sponge layer just a region of high viscosity or are the model prognostic variables are restored to- wards the ORCA12 variables? Are the ORCA12 variables interpolated and imposed on the boundaries? If so, are the tidal components then added? Just velocity or do the tides perturb the density field as well? Is anything done to sea surface height? What is the temporal resolution of the ORCA12 data?

An appendix has been added to the manuscript in order to clarify all those details.

Concerning the lateral boundary conditions, variables are interpolated and imposed on the boundaries to ORCA12. Lateral boundary conditions are prescribed in velocity, temperature, salinity and sea surface height.

The temporal resolution of ORCA12 data is 5 days.

(3,26) Is there no restoring for salinity, just a surface flux boundary condition?

There is no restoring for salinity.

(4,15) It would be helpful to indicate the annual mean values for the model and observations on the figure.

As the time series is giving the mean values for each day, we do not really understand what is the referee suggestion.

(4,21) It would be helpful here and elsewhere to mark on the figures various geographic features described in the text to help orient the reader.

Indeed, we agree with the referee and we added in the text a geographical position close to mentioned places not given on the figures.

Section 3.2: What is the standard deviation of the error, and what is the standard deviation in each the model and observations. Some of this spread is due to eddies and different phasing but we can't tell if the model is getting the statistics of the eddies correct or not.

More statistics have been given in the text. However, the dataset does not have enough profiles to conclude on the quality of the simulation to reproduce eddies.

(5,26) These vectors are very difficult to see. Maybe make the arrowheads bigger. In general the figures need to use larger fonts, they are very difficult to read.

We agree with the referee but after several tries, it appears as the best representation for these fields. More generally, fonts have been enlarged in the figures.

(6,5) The agreement suggests that at least some of this variability is forced, not internal.

Indeed, for this comparison, we are checking the model/observation agreement, which is linked to both forced and internal variability.

(6,23) Can the observations be included here? No one is going to track down that paper.

As suggested by the referee, observations have been included here.

(7,5) It would be helpful to include at least one topographic contour so we can tell where the ocean transitions from shallow to deep.

We agree that the topographic contour can be useful to see the transition. However, the red/blue colormap is not adapted to overplot isobaths. We then referred to Figure 1 and Figure 6 including the detailed view of isobaths.

(7,8) How do you know these features are related to local drivers?

This refers to another study focused on frontal activity over the shelf by O. Yelekci. We analysed this structures and related it to the local drivers (mainly rivers and winds). We add a mention to a personal communication.

(7,23) How does the high winter energy decay into the low spring energy? Is it dissipated locally or radiated away?

This question is very interesting question that we cannot address with our realistic (expensive) simulation. Further simplified modelling are planned to explore these issues.

(7,30) The discussion implies that vertical velocity and mixing are directly related but one can have very large vertical velocities through baroclinic instability and no diapycnal mixing.

We agree with the referee, however, as we consider the vertical velocities on the first 150m-depth, we can assume that it will impact the vertical mixing.

(8,21) It looks like there is some energy in salinity at the seasonal period from Fig. 14.

We agree with the referee that it remains a seasonal signal in salinity related to the evaporation-precipitation effect. The text has been modified to mention it:

" As we consider an area not under direct influence of major river runoffs (far from the slope dynamic barrier), the salinity (Figure 14d) does not exhibit a regular seasonal cycle. Indeed, main sources of freshwater in the Bay of Biscay are coming for river discharges. These discharges follow a seasonal cycle with a maximum flow end of winter not simulated over the analysed domain. Furthermore, the evaporation-precipitation budget (related to the more intense and frequent depression in winter) does not induce large variations at seasonal scales in the region but fluctuates interannually depending the atmospheric conditions. "

(8,32) It is not clear what is meant by instabilities driving potential energy to dissipation.

The sentence has been rephrased, as it was not clear for the reader. The text in the manuscript has been modified as:

" These instabilities drive to a conversion in kinetic energy of the stored potential energy in winter and lead to reinforce the seasonal stratification."

(10,5) The interannual variability was not the focus of the paper. The paper really focusses on validating the model.

In the new manuscript, we managed to strengthen the part and discussion on interannual variability as it remains the main targeted focus of the paper. However, we had to keep an important validation part as it is expected when we infer analyses on realistic simulations.

There were also many minor grammatical errors and unclear phrasing, but given my larger concerns about the direction of the paper I have not detailed these issues here.