

## ***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 variations 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 exist that relate submesoscale energy to mixed layer depth (among other things), so the present qualitative finding is entirely expected. It is stated that their

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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 submesoscale 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.

Detailed comments:

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

(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 towards 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?

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

(4,15) It would be helpful to indicate the annual mean values for the model and obser-

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variations on the figure.

(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.

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.

(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.

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

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

(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.

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

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

(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.

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

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

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

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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.

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