

Interactive comment on “Interannual evolutions of (sub)mesoscale dynamics in the Bay of Biscay” by Guillaume Charria et al.

L. Brannigan (Referee)

liam.brannigan@misu.su.se

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This manuscript by Charria et al examines the mesoscale and submesoscale dynamics that emerge from a 1 km resolution simulation of the Bay of Biscay. The authors investigate the extent to which the model provides a realistic representation of the region by comparison with observations. They then consider some metrics of submesoscale dynamics through the seasonal cycle. Finally, they look at the interannual variability of these metrics.

Overall, I think the simulation carried out is an interesting one. The description of the simulation is missing numerous details such as the bottom and lateral boundary conditions - in general the authors should include everything that would be needed for someone else trying to reproduce their experiment. There are various forms of anal-

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ysis carried out. However, the description of the analyses are also generally lacking important details. The authors should describe any analysis carried out and set out the equations used to do the analysis. See the line comments below for specific examples of these points.

In my view the analysis in Section 4.1 should be the key result of the paper, but I think that the analysis is relatively limited in this section and the discussion. Much of the previous results on the seasonal cycle are simply reproductions of results found in previous papers that cover other regions and this interannual aspect is really the potentially exciting and novel part of this simulation - as reflected in the title of the manuscript. The authors have shown that there is significant interannual variability in the vorticity, vertical velocity and mixed layer depth fields and that this may be related to surface fluxes. I think they should examine and present figures (probably scatter plots) in the revised draft showing the relationships between the vorticity, vertical velocity and mixed layer depth metrics and the forcing terms. Perhaps simple linear relationships could be investigated between averaged values of buoyancy forcing, wind stress magnitude, down-front winds (see Thomas 2005, <http://journals.ametsoc.org/doi/abs/10.1175/JPO2830.1> or Brannigan et al (2015) for an example <http://www.sciencedirect.com/science/article/pii/S1463500315000803>), as the independent variables with vorticity and vertical velocity as the dependent variables. If the authors want to maintain the length of the paper then some of the more qualitative discussion around Figures 10bc, 12 bc could be removed.

Line-by-line Comments

ÅÑL22 " The first high resolution" - could be "The first submesoscale-permitting"

L24 - "potentially"

L29 "Our understanding of the general..."

L10 " (adjustment, for example)" - I'm not sure what you're referring to here.

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L13 "decreases to values around 5-8 km" - reference for this?

L36 - "the two-dimensional barotropic system is resolved" - I'm not sure what you mean by resolved here.

The equation of state for the simulation, along with bottom and side boundary conditions have not been stated. A comment of how long the simulation takes to spin-up its annual cycle would be useful. Is the model hydrostatic? Given that the model used is not that familiar to many (or me at least) it may be better to set out the continuous form of the model equations.

" θ and b are surface and bottom control parameters)" - I'm not really sure what these parameters do from this description. Does theta have units? b seems to have units of metres, but they are not mentioned.

L9 "Digital Terrain Model (DTM)"

L13 " are interpolated on the grid and merged" - it's not evident from Figure 1 that the grid resolution is close to 1 km. Perhaps you want to include a second panel that shows a zoomed-in area that highlights the complex topography which the model allows?

L15 - what is h ?

L24 - are these horizontal "viscosity/diffusivity values"?

L26 - "is provided"

L29 - "components"

L34 "December 2014"

L37 - are the outputs daily averages or snapshots?

L7 - "over 2010" - to emphasise that the averaging is carried out over the full year.

L9 - "shows a good agreement" - what metric are you using to define what makes for a good agreement?

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L14 "Figure 2b"

L14 "the temporal variation of the spatially averaged bias and the associated standard deviation" - this calculation should be given explicitly

L14 "and on average" - average over what?

L18 "The largest"

L23 "In front of the Brittany" - you can't expect all readers to know where Brittany is. A latitude and longitude is much more helpful. This point can be repeated in general for many of the references made to figures.

L26 "and 34.8" - missing units. Indeed salinity units are missing throughout the manuscript.

L31 "The elongated freshwater filaments extending to the Southwest in the Southern" - again, specifying the position of these filaments would be helpful.

L30-35 - this whole paragraph (and the associated figure, see comments below) feels like it has not been developed and does not seem to go anywhere. There is talk of producing some filaments and export, but little or no evidence has been presented for the various statements. If this point on capturing freshwater export is genuinely important then it should be developed, otherwise I'd delete this paragraph.

L37-40 - these statements seem premature given that the vertical structure and flow hasn't been considered yet.

L9 - "Figure 5" L10 "with a small average misfit of 0.015°C " - I'm not sure that the average is the key metric here, because there are lots of offsetting positive and negative errors. The RMS error is also required.

L15 " average misfit is very small at surface" - if we assume the haline coefficient is about 4 times the thermal expansion coefficient, then this misfit is equivalent to a temperature difference of 0.1 degree C. Yet the 0.015 degree thermal difference is

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referred to as small, but the effectively 6 times larger salinity difference is referred to as "very small". This is inconsistent.

L27-35 - the kind of qualitative analysis presented here is subject to "cherry-picking" the parts of the simulation that agree with the observations. Could the model ssh field be compared with altimetry instead? Or (more challenging) could a dynamic height field be constructed from hydrographic observations that can be compared with the observations?

L4 "Two dimensional"

L11 - "A general agreement following the current directions and amplitudes is observed" - can some statistical basis for this be given?

L21 "located on the Aquitaine shelf" - I'd say that few people outside of France knew where this is.

L1 "the relative vorticity" - this needs to be defined explicitly. Presumably you are referring just to the vertical component of relative vorticity and this should be made clear.

L7 "in the northern part of the domain spreading from the shelf break" - give a reference location so the reader knows exactly what you are referring to. "small structures related to local drivers" - same point, there is a very large area and I'm not sure what you're talking about exactly.

L14 "shadowed by large-scale vortices" - do you mean "obscured"? "Shadowed" means more like "followed" in English. Also, I don't think it's physically sound to talk of obscuring by large scale vortices - it implies the small-scale vortices are there, but you just can't see them. In reality, I imagine that the small-scale vortices are simply absent in summer.

L16 "The spatial spectral analysis over the domain" - This analysis would be interesting to see. You could perform an analysis as in Brannigan et al. (2015) whereby you

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compute the mean spectral slope over different regions and plot the time series of this slope through the seasonal cycle.

L20 "Based on the spatial average integrated over 150 m depth" - the use of a fixed depth will bias the calculation low in summer time compared to winter. What we're really interested in is the evolution of vorticity in the mixed layer over the year. The calculation should make some effort to track the mean mixed layer through the seasonal cycle, even if it isn't exact.

L22 "The horizontal patterns (Figures 10b and 10c) associated with these average time series confirm the larger range of relative vorticity values related to small scale structures" - you're making a comparison here ("larger") but haven't stated what you're comparing. Using the seasonal spectral slope calculation would allow you to make the seasonal comparison explicitly without having to do this qualitative analysis.

L25 - " surface relative vorticity spectra" - you need to set out explicitly how you did this calculation. You should also justify why you choose to use the vertical component of relative vorticity for this rather than a kinetic energy spectrum. The spatial pattern used to calculate the spectra is also unclear.

L26 " seasonal variation of the energy" - presumably you mean the variance here, as the underlying spectra is of vorticity.

L30 "The relative vorticity fields, related to vortices" - this isn't right, the vorticity comes from a mix of vortices, jets and filaments.

L30 "The relative vorticity fields, related to vortices, can also be explored through vertical motions. " - This sentence doesn't make sense to me. Surely we are investigating the overall model dynamics, and the vorticity and vertical motions are two separate diagnostics for this?

L30 " The role of these structures" - again, this sentence implies that vortices are the only processes of interest, when it is a mix of dynamically distinct processes.

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L6 "The diagnostic"

L15 - this comment on the spin-up period should be in the second section. Some justification needs to be given as to why the model is considered to have spun-up in two years.

L15 - I think it would be more intuitive to divide the values in Figure 14 by 150 m to get mean values, I've no idea how to judge the magnitude of vertical velocity in units of m^2/s^{-1}

L26 "A model simulation...exhibits"

L28 "theatre" change to "location"

L34 " and to the seasonal restratification" - this hasn't been shown in the paper. In any event, the seasonal restratification is likely to be driven primarily by heating at the surface. This is shown by the fact that a 1D model in the vertical produces adequate seasonal restratification without any MLIs.

L36 " Indeed, Soufflet et al (2016)" - this isn't included in the list of references.

A number of figures are missing x, y and colorbar axis labels. The text on the labels is also generally quite small and hard to read. Maybe it could be made into a semi-bold font?

Figure 1 - the non-uniform color scheme here is not a good choice - it strongly highlights the yellow region which may or may not be the region of strongest gradients. A uniform perception scheme is recommended instead.

Figure 2 - the title should be in symbols rather than the computer code format used. This is confusing as it's not clear to me if the $< >$ are being used as mathematical operators here or just part of your text formatting. The temporal averaging that is required to make the calculation in the plot should be made explicit in the text. "The shape of the curves represents the spatial standard deviation" - this seems incorrect,

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the shape of the curves is the seasonal cycles, but it seems like you're actually talking about the shading around the curves.

Figure 4 - why are there two subplots? Why is one of them smaller than the other? Does the second one even get referred to?

Figure 6 - shouldn't these plots show the average over 50 grid points rather than just selecting one grid point out of 50?

Figure 8 - a red-white-blue colorbar should be used instead of a rainbow color schemes. In general rainbow color schemes are known to distort perception of data and should be avoided. The figure titles could mention which is the model and which is the observation.

Figure 9 - it is more helpful to plot the relative vorticity normalised by the local value of f (and other vorticity plots).

Figure 12 - the depth of the plan views needs to be stated.

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