

Review of the paper untitled :

“Towards a regional ocean forecasting system for IBI (Iberia-Biscay-Ireland area): developments and improvements within the ECOOP project framework”

Answer to anonymous referee #2

Dear Referee,

Thank you for your comments. We tried to improve our paper as the best as we could by following your advice.

The paper describes the evolution of the Iberia-Biscay-Ireland Ocean Operational System (IBIROOS) from the early version flagged as V0 to the most recent, developed in the framework of the ECOOP project, V2. Although the description of the systems includes regional and sub-regional models, the actual focus is on the regional model only. The paper is organized with a clear logical flux: first the overview of the evolution of the system (Sections 2, 3 and 4), then an intercomparison and validation of the two most recent versions (V1 and V2) of the regional model (Section 5). The overview provides relevant info, although some details regarding numerics, physics and methods are not always accompanied with proper references. The validation is a little bit limited however it provides a first useful guess of the performance of the systems. This draft certainly fits the special issue devoted to ECOOP but the quality of the draft could and should be improved.

Just a comment about the validation:

A system is never assessed enough, especially in a large regional system as IBI which gathers a lot of local physical processes and even different physical regimes between shelves and abyssal plain areas. We haven't wanted to make this paper as a validation report, so we've selected any main thermodynamical features and that we assessed in qualitative and quantitative way by using the few available observation data. The objective of this paper hasn't consisted in an exhaustive validation, as another more quantitative paper that we submitted (Maraldi et al. 2011). We have rather focus on some the most obvious improvements between the two system versions V1 and V2.

* Maraldi C., B. Levier, N. Ayoub, J. Chanut, P. De Mey, F. Lyard, G. Reffray, S. Cailleau, M. Drévilion, E. A. Fanjul, M. Garcia Sotillo, P. Marsaleix and the Mercator Team, 2011: NEMO on the shelf: assessment of an Iberia-Biscay-Ireland configuration. Submitted.

General comments

English is poor. There are several grammar errors. Some statements don't make sense. Some work on this is mandatory.

We tried to improve this: we partially rewrote abstract and conclusion. But we are sorry for our English style, we aren't bilingual ... We wrote this paper as best as we could with our knowledge of English.

Tons of local geographical names are used, but not shown anywhere in the figures. This is a problem for readers not familiar with the region. Please try to include most of them in your figures.

All areas mentioned in the validation part are shown in the figure 7. The reader can report to this figure.

Labels in the figures are generally too small and hard to read.

Font size has been increased in order to make labels more readable.

The validation using the PELGAS09 dataset would benefit from additional analysis, e.g. the RMSE along vertical profiles, in support to the assessment/improvements of the vertical mixing.

The diagnostics of the difference of the temperature and salinity between surface and bottom provide a first idea about the stratification obtained by the system but we added and commented mean T and S profiles vs PELGAS data to complete these diagnostics. Data are not timely and spatially homogeneous. So we can't calculate significant statistics from PELGAS data such as RMSE or show the winter stratification situation and so compare the seasonal mixed layer.

In my opinion, you should discuss, at least briefly, what is the impact of the different vertical resolution (6 m V1 vs. 1 m V2) on the assessment involving summertime SST.

It's difficult to isolate the impact of the vertical resolution between V1 and V2 since the physics of these models are different: V2 has a physics more adapted to regional and coastal processes compared to V1 which was a basin model. Preliminary tests had been carried on other Mercator Ocean models with both vertical resolution and it had been shown that the Eckman layer as well as the diurnal cycle of SST have been better resolved. Besides *Bernie et al (2005) showed that 1m vertical resolution at the surface was the minimum resolution to correctly resolve the diurnal cycle. As V2 is forced by high frequency atmospheric forcings (3-hourly) compared to V1 forced by daily ones, it has been necessary to increase the vertical resolution in order to take into account high frequency processes.

Besides, in Summer, the stratification is very strong over the shelf when the thermocline can reach only few meters. So this also can justify a one vertical meter resolution.

And finally, as *Burchard, 2001 have shown, the parameterization of the wave breaking effect in the mixing in the superficial layers depends on the vertical resolution. This effect also needs high resolution to be taken into account.

We added these details in the description of V2

* Bernie D. J., Woolnough S. J., Slingo J. M., Guilyardi E., 2005. Modelling diurnal and intraseasonal variability of the ocean mixed layer. *Journal of Climate*, 18(8), 1190-1202.

* Burchard, Hans, 2001: Simulating the Wave-Enhanced Layer under Breaking Surface Waves with Two-Equation Turbulence Models. *J. Phys. Oceanogr.*, 31, 3133-3145.

Specific comments

Check carefully the references. For example, Griffies and Pacanowski (2001) is not listed. Canuto 2001 is maybe Canuto et al. 2001? Gaspar 90 should be Gaspar et al 1990. Chanut et al, 2008 is missing. Jones and Launde 1972, Burchard et al., 1988, Leonard 1991 from table 3 are not listed in the reference list. I have the feeling also that some references listed are not actually cited in the paper.

The references has been checked:

- Canuto et al. 2001
- Gaspar et al. 1990
- S.M. Griffies, R.C., Pacanowski, M. Schmidt, and V. Balaji , 2001. Tracer Conservation with an Explicit Free Surface Method for Z-coordinate Ocean Models. *Monthly Weather Review*, 129, 1081-1098.
- Chanut, J., B. Barnier, W. Large, L. Debreu, T. Penduff, J-M. Molines, P. Mathiot, 2008. Mesoscale Eddies in the Labrador Sea and Their Contribution to Convection and Restratification. *Journal of Physical Oceanography* 38:8, 1617-1643
- "Jones and Launder 1972" has been removed.
- "Burchard et al., 1988" is an error and has been replaced by Umlauf and Burchard, 2003.
- Leonard, B. P., 1979. A stable and accurate convective modelling procedure based on quadratic upstream interpolation. *Comp. Meth. in Appl. Mech. and Eng.*, 19, 59-98.
- Indeed, some listed references weren't cited. So they have been removed.

Intro, l23: Iberia-Irish-Ireland?

Iberia-Biscay-Ireland: modified in the paper.

Something is wrong with figure 1 y-axis label. The Med Sea is not at 24 Latitude N, nor the Gulf of Mexico is south of 12 Latitude N. Should this be because you have a rotated domain or something, at least state it clearly in the figure caption. The same holds for figures 2 and 5.

Indeed there is a problem with axes. These figures have just given an idea of the domain of each system, the accurate longitude and latitude are provided by the configuration table of V0, V1 and V2, so the axes in the figures 1, 2 and 5 have been removed.

Reference missing for several datasets used (e.g., MEDATLAS, Reynaud Climatology, ODYSSEA, WOA05).

The references for these datasets have been added in the paper:

- MEDATLAS: <http://www.ifremer.fr/medar>

- Reynaud Climatology: Reynaud, T., P. Legrand, H. Mercier, and B. Barnier, A new analysis of hydrographic data in the Atlantic and its application to an inverse modeling study, *International WOCE Newsletter*, 32, 29-32, 1998. 40.
- ODYSSEA:
http://cersat.ifremer.fr/data/discovery/by_parameter/sea_surface_temperature/odyssea_global_sst_analysis
- WOA05 (World Ocean Atlas 2005):
http://www.nodc.noaa.gov/OC5/WOA05/pr_woa05.html

If available, provide a reference for the OI data assimilation method used in V1. Spell out and add reference for the SEEK filter for V2. SLA is an acronym and it should be spelled out the first time it is used (as well as GFO). Assimilated T/S vertical profiles from which network?

Acronyms have been spelled out and referenced:

- SEEK (Singular Evolutive Extended Kalman filter (*Pham et al., 1998) is a reduced filter deduced from the extended Kalman filter.
- SLA = Sea Level Anomaly.
- GFO (GEOSAT Follow On, US Navy) is a family of radar altimeter satellite.

V0 and V1 assimilate T/S profiles from CORIOLIS network (<http://www.coriolis.eu.org>) which gathers several buoy and ship data sets.

*Pham, D. T., Verron, J., and Roubaud, M. C., 1998. A singular evolutive extended Kalman filter for data assimilation in oceanography. *J. Mar Sys.*, 16 :323-340.

Figure 1 vs. Section 2 first para: in figure 1 is reported MANGA by IFREMER, not GDGE by PREVIMER.

It is GDGE by PREVIMER and it has been corrected in Fig.1

Figure 2 vs. Section 3 first para: now is written MANGA by PREVIMER but in figure 2 is MANGA by IFREMER. The same in figure 5.

It is MANGA by PREVIMER and it has been corrected in Fig.2 and 5.

pg1942, l7: what is an ORCA grid?

ORCA grid is a global tripolar horizontal ocean grid used by NEMO code.

pg1944. l10. Craig-Banner should read Craig and Banner.

It has been modified.

pg 1945 last para: In the text you use D0 while in figure 6 you use J0

It has been modified.

para 5.1.1 what is IBI-V2? IBI36? Can you come up with a clear naming? Sometimes IBI means the whole system IBIROOS (several models), sometimes only the Regional Mercator (PSY2Vx), and now

you introduced IBI36 but then you use IBI-V2. And later you use IBI without “-Vx” nor “36”... Sometimes PSY2V3 and sometimes IBI-V1 ... This is confusing.

This issue has been clarified in the text.

5.1.2, first para. which climatology? Please specify WOA 2005 (even if cited in the figure caption), spell out the acronym and add a reference.

WOA05 (World Ocean Atlas 2005): http://www.nodc.noaa.gov/OC5/WOA05/pr_woa05.html

p1949, last para. The discussion of the Navidad current seems to jump in the paper suddenly from nowhere. Try introducing it a little bit better.

It has been better introduced.

5.2, first para: between Iceland and the Faroe Islands?!? these are outside the domain of fig.15

Indeed, Iceland and Feroe are out of the domain shown in the figure 15, it's an error, so we corrected the description.

5.2, second para: when listing the moorings locations, point the reader to Fig.7

Ok

Conclusion sections

p1951, l25: It sounds reasonable, but you didn't show that using k-epsilon improved the vertical mixing, since you are comparing a version (V1) without tides and another (V2) with tides. Your statement is therefore unjustified.

Indeed, the improvement in the vertical mixing particularly due to k-epsilon vs TKE scheme hasn't been proved in this paper. But k-epsilon belongs to the state of the art regarding vertical mixing scheme in coastal and shelf modeling. Some preliminary tests have given the same conclusions than Holt et al. (2008). They have shown that bottom layer and tidal fronts have been better resolved. Anyway, the aim of this paper is less focused on the improvement due to each change between V1 and V2 than the improvements due to the ensemble of modifications. So finally we've preferred to remove this sentence about k-epsilon from the conclusion.

p1952, l2. You didn't show that the model (thanks to the resolution), was able to resolve the submesoscale. Unjustified statement.

1/36 corresponds to 2-3km of resolution in this area. This resolution is enough to resolve sub-mesoscale at these latitudes but I agree we didn't show this. So the sentence p1952, l2 has been modified.

p1952, l14: to me, the depth of the Med water, according to fig11, is not “right”, while shallower.

The Med water is between

- 1100m and 1300m, for the climatology,
- 700m and 900m, for V1,

- And 900m and 1100m, for V2.

Even if the Med water remains shallower for V2 vs climatology, it's better than V1.

The text has been modified consequently.

p1952, l20: do you know now the actual runoff of the rivers in that period? can you guess how close was the true runoff compared to the climatology. How the model takes rivers into account is not detailed. Maybe add a reference if available or provide more details?

Monthly climatology of fresh water transports has been applied to the system at each model grid point corresponding to the river mouth. It works like an open boundary condition but for one point, where we just specify the river transport since we haven't any available data about temperature and salinity from the rivers (we suppose salinity=0). Now, in the framework of MyOcean Project, we're updating the IBI system by using a merge of daily hindcast, forecast and observation runoff data. Hindcasts and forecasts are provided by the E-HYPE hydrological model from SMHI and observations are provided by PREVIMER. We added this short explanation in the part of model description in the paper.

p1953, l7: "wave components"? what do you exactly mean? unclear

We mean that we plan to force the next IBI system with the wind stress processed by the MetNo and integrating wave effect. We usually force directly the system by the total air-sea momentum flux which can be split into two terms: one term acting on the mean currents (e.g. flat plate drag) and the other acting on the waves and responsible for wave growing. For the momentum flux that should be applied in the ocean model, there are two sources: the direct effect of the wind and the forcing by dissipating waves. Whenever there is an imbalance between wave growth and dissipation, the mean ocean surface stress will differ from the total air-sea flux.

This explanation has been included in prospects part of the paper.