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Ocean Science Discussions

Interactive comment on "An operational model for the West Iberian coast: products and services" *by* M. Mateus et al.

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Dear anonymous referee,

Thank you very much for your feedback. I will try to answer to each of your comments. General comments: Overall you are correct regarding the place of the operational system within MyOcean: the operational system was greatly improved within the MyOcean context, both in infrastructure (computing resources, data servers, etc. . .) and in design (offline coupling of local operational models based on window extractions with more fine temporal outputs to accurately reproduce the tide signal in the water elevation and in the barotropic velocity). And really the paper should focus on the impact that products of MyOcean had on this operational system. We will rewrite paragraph on p.1651 line 10, in the introduction, for that matter.

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As for your general comment that the paper is trying to be all things to all people, we, the authors, must clarify that the emphasis on the paper was to review the potential uses of an operational system that feeds local models. This is in line with our role within the MyOcean project (intermediate users). However, we felt obliged to provide an accurate, but synthetic, description of the model (the MOHID configuration) and of its validation (based on MyOcean products). For a more accurate description we refer to the Leitão et al 2005 paper. When read in-depth, it contains all the details required to configure such system. Describing them extensively (for other to reproduce the exact same configuration with MOHID or with another model, like ROMS) is beyond the scope of this article.

Specific comments: P 1653 line 10: answered in the general comments reply.

P 1656 line 18: Initially the model was only Cartesian (Riflet et al 2008). However it failed to represent adequately the gradient of the river plumes near the surface due to a lack of vertical resolution near the shallower regions, and computational resources made inefficient the increase of cartesian layers. Thus, a 10 layer topography-following vertical coordinate, such as sigma coordinate, for the first 10 meters near the surface was a cost-effective choice as it preserves 10-layer resolution even in 1 meter to 2 meter depth shallow areas. I remind that Sigma alone is inadequate to model the whole water column in a domain with a steep slope such as the abyssal plain/continental shelf region and that a hybrid coordinate system, such as double sigma or such as the one used in this work, is always required.

P 1656 line 28: The methodology is described in the Leitão et al paper. The article does not present a mathematical formula, but a linear superposition means a simple "sum" at the boundary between the tide signal and the reference baroclinic model (the water levels are summed and the velocities are summed – so it is SSH and currents –). As for the frequency, this isn't mentioned in the Leitão et al paper so a paragraph will be added in this article. But basically, at every time step, the model computes the sum of a barotropic and baroclinic external solution. And both external solutions con-

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sist in a linear interpolation in time between two consecutive outputs of the respective barotropic (180s time step) and baroclinic models (1 day time step).

P 1657, line 21: It is indeed a ramped increase in forcing. The Leitao et al formulas with the ramp functions will be included in an updated version of this article. Section 3.1: The validation methodology will be further detailed in the revised version of the manuscript. Currently we are comparing the best guess, but so far we have not made a comparison between best guess and forecast. The added value of this regional model compared to other forecasting systems for the region is the tide and river discharges being implemented, both very important drivers in the dynamics of the ROFI.

P 1661, line 20: The permanent eddy shown in SW region of the map is not present in the baroclinic reference solution and is not present in remote-sensing imagery; it is therefore spurious. This type of trapped spurious eddies may occur due to ill parameterized boundary conditions or due to inconsistent forcing. For example, the atmospheric forcing in the baroclinic reference model and in the MOHID model comes from different models. Eddies generated inside the domain when travelling across the open boundary may be trapped near the boundary. Indeed, the OBC (Open boundary condition) is a clamped solution for velocities, salinity and temperature. Arguably, a better job can be done at the open boundary. However, the critical aspect is the quality of the flow over the continental shelf and near the river regions of influence.

P 1663, line 20: Indeed, this short paragraph uses many different concepts which may yield some confusion. There is the skin temperature, which we regard as the temperature of the model that should be compared with remote-sensing SST imagery; skin temperature is not a direct output of the model and should be computed in post-processing via some algorithm. Then, there is the bulk temperature of the model, which is the average temperature of the layer. This bulk temperature is a direct output of the model and should be post-processed to generate a skin temperature of the model. Currently we only compare bulk temperature of the surface layer with remote-sensing SST imagery and don't compute a skin temperature i.e. our surface layer bulk

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temperature is our proxy skin temperature.

Then there is the mixed layer, which is a surface layer that is present in the Ocean and in lakes and that is well mixed i.e. that has homogeneous temperature. The mixed layer undergoes seasonal variations and the processes involved in generating it are predominantly wind, waves and water column light absorption. Thus, during summer months (more incident solar radiation increasing the thermal stratification) there usually is a thinner mixed layer than when compared to winter months (more windy) which presents a thicker mixed layer. Thus, during summer, where the mixed layer is thinner, the model vertical resolution required to accurately reproduce it is higher than during winter. So, in the article we argue that there is a higher error in the model during summer, when modeling the mixed layer at the surface. Therefore, a higher difference is expectable during summer between the model top layer bulk temperature and the satellite SST.

P 1667, line 3: Thanks for pointing this out. We will include the notion in the next update of the article.

P 1670, line 5: indeed, atmospheric forcing used in the model is not a product of MyOcean, but rather a product from Meteo-IST of Lisbon, Portugal.

Fig 2 and 3: The horizontal lines correspond to near-shore dry/wet cells that do not have their values changed when they are dry. They should have been filtered out of the set of data.

P 1652, line 8: “type of results” means the categorization of the results via their type: SST model vs satellite comparisons, in-situ ARGO profiles data vs model comparison, etc... The whole paragraph should be rewritten in the next update of the article.

P 1653, line 21: MOHID stands for the Portuguese term ’modelo hidrodinâmico’ (hydrodynamic model). It possesses a Portuguese acronym.

P 1653, line 23: “based at MARETEC” seems appropriate to describe the meaning. To

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include in the next version of the article.

P 1653, line 25: “which uses the hydrostatic approximation” is adequate. To include in the next version of the article.

Every other technical correction will be taken into consideration. Thank you very much for your time and

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Interactive comment on Ocean Sci. Discuss., 9, 1651, 2012.