

Interactive comment on “Skill assessment of global, regional and coastal circulation forecast models: evaluating the benefits of dynamical downscaling in IBI surface waters” by Pablo Lorente et al.

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In this manuscript, titled "Skill assessment of global, regional and coastal circulation forecast models: evaluating the benefits of dynamical downscaling in IBI surface waters", the authors describe an inter-comparison of different oceanographic operational systems in the Iberian shelf and in the Gibraltar Strait. Although the numerical results are deeply investigated showing the different model performance in reproducing surface water properties, to my opinion the manuscript suffers from a lack of identification of the novel aspects of the research. As stated by the authors, the three considered

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operational systems have been already described in many publications. Similarly, the benefits of downscaling (i.e. increasing resolution) in coastal waters have been widely treated in literature.

Major comments:

1. I appreciate the authors for highlighting the need of accurately addresses land-sea, air-sea, and coastal-offshore interactions when dealing with regional operational ocean systems (see also Kourafalou et al. 2015; Wilkin et al. 2017; Ferrarin et al., 2019). However, I think the authors should mention in the introduction that the downscaling from open sea (ocean) to coastal area can be also achieved through the implementation of numerical models based on a unique unstructured grid (Cucco et al. 2012; Ferrarin et al. 2013, 2019; Zhang et al. 2016; Federico et al. 2017; Stanev et al. 2017) able to describe processes at different spatial scales, and not only or through nesting of models. I'd like to point out that unstructured approach is particularly useful for realising a seamless transition between adjoining regional seas connected by narrow straits (i.e. the Gibraltar Strait; Stanev et al. 2017 and Ferrarin et al., 2018).

2. The only novel aspect in this work seems to be the inclusion of the so called “spectral” nudging technique into the operational IBI system. A similar methodology has been recently applied by Ferrarin et al. (2019) in the Tiresias operational system with a relaxation coefficient spatially varying over the Adriatic Sea domain (as a function of the grid resolution) from 2 days in the open sea and increasing, thus diminishing the restoration contribution, toward the coast. The authors should provide more details about the nudging methodology in terms of variables assimilated and 3D spatial weight functions (relaxation time). Moreover, it is not clear what are the benefit of this improvement, since according to the results presented in Fig 2-5, IBI performance is lower to the GLOBAL model on open sea regions (IBISR, ICANA, WSMED), where ideally nudging allows the model state to be reconciled with the assimilated GLOBAL data. Do the differences with the GLOBAL demonstrate that over the sea surface the impact of the air-sea heat fluxes is stronger that the restoration contribution of the GLOBAL

nudging? In this contest, the authors should demonstrate the benefit of the nudging approach analysing the water column properties and not only the sea surface.

3. One of the most important topic described in the manuscript is the model comparison in the Gibraltar Strait. As one would expect, the SAMPA high resolution model performs better in reproducing hydrodynamics in this region having complex morphology and dynamics. However, it is not clear to me if this improvement is only due to the model spatial resolution or to the inclusion of the remote effect of the atmospheric forcing in the barotropic flow through the strait. According to the model description reported at page 7, SAMPA is not directly nested in IBI since it is forced by daily fields (from IBI) and tidal and meteorological-driver barotropic velocities from MOG2D and NIVAR. In this contest the authors should:

3.1. make clear throughout the whole manuscript that SAMPA is not only nested in IBI. Therefore, IBI could not be defined the only parent system of SAMPA.

3.2. explain the reason of this choice, which sound counter-intuitive to me, since IBI already account for tide and meteorological forcing.

3.3. the authors should discuss the results of the inter-model comparison in the light of the different approaches adopted for the forcing and parametrization in the three systems, not only resolution.

Minor comments:

Pg. 2, line 35: Is Herbert et al., 2014 (on Mercator Ocean - Quarterly Newsletter) a peer review publications? Only peer review articles should be cited.

Pg. 3, line 20: Sotillo et al., 2018. Do not include under review article.

Pg. 3, lines 22-29: The part regarding operational products for harbours seems out of topic.

Pg. 5, line 8: Please provide a reference for this statement.

Pg. 6, lines 10-24: Provide more details about the nudging approach.

Pg. 8, lines 13-14 and 38-41: see major comment 3.

Pg. 10: Section Temperature should be shortened.

Pg. 12, line 6: "GLOBAL performance was more accurate from July April to December as indicated by the lower RMSD".

Pg. 12, line 28-29: This is not true, since divergence between IBI and GLOBAL forecast are found also in the open sea.

Pg. 16, line 44: "... midpoint of the selected transect (white square in Fig. 1c)."

Reference: authors should limit self referencing (29/76) and include only peer reviewed articles without considering under review works (Sotillo et al., 2018), conference abstract (Hernández-Lasheras et al., 2018) or poster (Lorente et al., 2017).

Pg. 24, line 19: provide doi for "in press" article.

Pg. 24, line 32: provide the full list of authors.

Pg. 26, line 28: the journal for this reference is missing.

Figure 1: please indicate the SAMPA model domain; specify the meaning of the black line and white square in panel C. Table 2: Open boundary conditions for SAMPA should also include MOG2D-ARG.

Suggested references:

Cucco A, Sinerchia M, Ribotti A, Olita A, Fazioli L, Perilli A, Sorgente B, Borghini M, Schroeder K, Sorgente R. 2012. A high-resolution real-time forecasting system for predicting the fate of oil spills in the Strait of Bonifacio (western Mediterranean Sea). *Mar Pollut Bull.* 64(6):1186–1200.

Federico I, Pinardi N, Coppini G, Oddo P, Lecci R, Mossa M. 2017. Coastal ocean forecasting with an unstructured grid model in the southern Adriatic and northern Ionian

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seas. *Nat Hazards Earth Syst Sci.* 17(1):45–59.

Ferrarin C, Roland A, Bajo M, Umgiesser G, Cucco A, Davolio S, Buzzi A, Malguzzi P, Drofa O. 2013. Tide-surge-wave modelling and forecasting in the Mediterranean Sea with focus on the Italian coast. *Ocean Model.* 61:38–48.

Ferrarin C, Bellafigliore D, Sannino G, Bajo M, Umgiesser G. 2018. Tidal dynamics in the inter-connected Mediterranean, Marmara, Black and Azov seas. *Prog Oceanogr.* 161:102–115.

Ferrarin C., Davolio S., Bellafigliore D., Ghezzi M., Maicu F., Mc Kiver W., Drofa O, Umgiesser G., Bajo M., De Pascalis F., Malguzzi P., Zaggia L., Lorenzetti G. & Manfè G. 2019. Crossscale operational oceanography in the Adriatic Sea, *J Oper Oceanogr.*, DOI:10.1080/1755876X.2019.1576275

Kourafalou V, Mey PD, Staneva J, Ayoub N, Barth A, Chao Y, Cirano M, Fiechter J, Herzfeld M, Kurapov A, et al. 2015. Coastal ocean forecasting: science foundation and user benefits. *J Oper Oceanogr.* 8(sup1):s147–s167.

Stanev EV, Grashorn S, Zhang YJ. 2017. Cascading ocean basins: numerical simulations of the circulation and interbasin exchange in the Azov - Black - Marmara - Mediterranean Seas system. *Ocean Dyn.* 67:1003–1025.

Wilkin J, Rosenfeld L, Allen A, Baltés R, Baptista A, He R, Hogan P, Kurapov A, Mehra A, Quintrell J, et al. 2017. Advancing coastal ocean modelling, analysis, and prediction for the US Integrated Ocean Observing System. *J Oper Oceanogr.* 10(2):115–126.

Zhang YJ, Stanev E, Grashorn S. 2016. Unstructured-grid model for the North Sea and Baltic Sea: validation against observations. *Ocean Model.* 97:91–108.

Interactive comment on *Ocean Sci. Discuss.*, <https://doi.org/10.5194/os-2018-168>, 2019.