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Discussions

# *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 paper the authors perform a multi-model comparison in the regions surrounding the Iberian Peninsula. From global models using data-assimilation to local models nested into larger scale ones, the authors explore the differences in surface ocean properties for each approach and provides hypothesis and reasoning for the observed patterns. The paper is in general well written with only a few grammatical errors (see



details below) and easily understandable.

Although I appreciate the approach and the effort to objectively analyze pros and cons of the different models I do have some concerns on the present version of the manuscript. Hopefully, such concerns could be solved through a revision of the text so the manuscript could be made acceptable for publication.

Major concerns:

My first issue could be derived from my own lack of expertise with data-assimilation models but I do find it difficult to completely understand how the CMEMS global model works. It is stated (page 6, line 5) that the system provides 10-days forecasts updated daily. Does this mean that every-day the system assimilate all available information to update its status and then is run for 10 days? Then, the next day the cycle re-start, assimilating data for the new day and re-running the system for another 10 days? If I understand this correctly, the Global model is only left 'free' for one day at a time, am I right?

If the above is correct I wonder how it is possible for the model to present such relatively large deviations with respect to satellite in terms of SST (bias range -2/+2, figure 2). This is particularly shocking for me as satellite SST is part of the data assimilated by the model (as you state in page 6, line 26). I understand the calibration/validation is not a task to be performed by the authors but I would like to know your opinion about the system operation, do you have any thoughts on how to improve this issues? Or maybe I totally miss-understood how the system works?

My second issue comes from your interpretation of the results in the Strait of Gibraltar. The improvement in AJ direction and speed from the global to the regional model is clear, however the reason for such are not that obvious as you seem to propose. I fully agree with you that increasing spatial resolution (global<IBI<SAMPA) is one of the major reason why the direction and speed of the AJ is better reproduced in the regional model. However, the inversion events could not be related with this issue. In fact, a

model with similar resolution to Global (see Macias et al., 2016) was able to reproduce the inversions of the jet. In that work, the remote barotropic effect of the meterological forcing over the Mediterranean Sea was proposed as one of the major players in the regulation of the seasonal cycle of the AJ and of its occasional inversions. As far as I understood, only SAMPA include this type of effects (page 7, lines 43-48) and, in my opinion, this is the main reason why only this model is able to correctly reproduce the inversion events. I would suggest to make this difference clear in the text; increasing resolution helps with the simulation of direction/velocity of the jet; correct atmospheric forcing (remote) is essential to get the flow inversions

I strongly suggest the authors to explore these caveats and to try to address them in a revised version of the manuscript.

Minor details:

Page 2, lines 12-17: I don't think global models are able to 'properly resolve' biogeochemical cycles, not even at large scale. Also, this phrase is too long, please consider breaking it up. Page 3, line 24: consider changing 'lower' with 'less' Page 3, line 36: what does 'poorly controlled information' exactly means? Page 3, line 44: 'researchers' should be 'research' Page 4, line 28: please indicate in caption of Fig. 1c what the white square represents Page 6, section 3.1: as indicated above, I don't fully understand how the assimilation/run/re-start cycle of this model works. Could you please provide a more detailed explanation? Page 6, section 3.2: similarly, the transfer of information from the Global to the IBI system is not fully clear. Does IBI have some data assimilation scheme? Or only information from the parent system is transferred into the model domain? How often the nudging is done? Page 9, lines 32 and 33: the symbol '°' is missing Page 10, line 29: the transect used for evaluation is the black line/white square in Figure 1c? Page 11, line 15: I can't see any clear benefit in using the IBI over the Global model in here.. Page 11, line 23: why the advantages of the SST assimilation into the Global model is not 'propagated' into the IBI? Is it related with the frequency of the nudging? The method? Page 11, lines 43-47: this explanation does not seems fully



justified. The SST anomalies (even in IBI) does not only occur in the coasts, but also many km away where satellite images should not have any issues. Page 12, line 2: could you please explain better how the correlation spatial maps are computed? Page 12, line 12: what do you mean with 'in like fashion'? Page 12, line 19: it is curious that high 'r' are coincident with high 'RMSD' Page 12, lines 29 – 37: as you are mentioning this 3D comparison some numbers (statistics) should be provided (no figures might be needed though) Page 12, line 45: the point-wise comparisons you provide in Figure 4 seems to have lower biases than most of the maps shown above.. isn't it a bit strange? Page 13, lines 10-12: how an intrusion of warmer waters could make the SST to drop? Page 13, line 27: to avoid this bias you could just extract model data from the closet depth to the buoys? Or make an interpolation to the specific depths? Page 13, lines 37 – 38: I am left wondering if NEMO vertical structure (stability) could be partially responsible for the observed differences. As mentioned above, a data-driven model running freely only for a very limited time should not show such large biases in SST. I know for a fact that NEMO has difficulties to simulate the vertical structure of the water column in the Mediterranean Sea and was wondering if something similar could be happening elsewhere? Page 14, line 18: 'accurate' seems a bit subjective.. why not use 'rather accurate' instead? Page 14, line 24: the cooling in the IBI simulation does not only occur along the river plume but also on the NW Iberian coast. Could it be also related with some other process happening a more regional scale? Such as locally-induced upwellings? Page 16, line 34: red line in Figure 6c is quite difficult to interpret because of the continuous changes associated to the tidal cycle. If you use dots (as with the buoy data) it might be more easy to read the figure. Page 15, line 4: it is true that SST decrease on the river plume but, as commented above, in IBI there are other processes bringing up cold waters nearby the coast. Page 16, line 21: the differences between the different models are not just on the downscaling (increasing resolution) but also on the imposition of lateral conditions at the boundaries! Page 16, line 26: positive bias with respect what? Page 17, line 44: the two ways current system you describe in here is not clear from the graphs in Figure 10 Page 19, line

27: the fact that SAMPA and IBI represents the tidal dynamics is not because of the nesting, but because you include this forcing in both models (and not in Global) Page 19, line 35: increasing resolution is not the only reason why SAMPA outperforms the other two models (see general comment above) Page 19, lines 36 -42: where are the metrics you refer here to? Page 19, line 43: the warming in Fig. 12c is less than 7.5 degrees, I would say $\sim$5? Page 20, line 15: the situation of the WAG you describe here is not the typical one. The AJ is entering in a rather meridional direction and the WAG seems to be slightly detached from the NW Alboran. I would say this is already an evolving situation into the inversion episode Page 20, line 20: the coastal eddy in the NW Alboran is almost always there (see situation in previous snapshot and plenty of reports elsewhere), the only difference is how big this structure is (which is linked to the AJ migration and WAG displacement) Page 21, point iii): you acknowledege here the potential effects of barotrophic flows on AJ inversions but is not clear in your discussion above. As suggested in the general comments, I would recommend to make a stronger case for this difference between models, resolution is important for the Strait dyanmics but is not the only element to consider Page 22, lines 14 – 19: I would also recommend to keep on improving the mechanics of the models. Data assimilation is a nice tool but should be developed on parallel with model improvements. Otherwise models would only become very sophisticated data-interpolation tools, losing their potential to fill gaps by doing free-simulations. Page 22, lines 38 – 39: see my comment above about NEMO and vertical stability problems in the Mediterranean. We should think about which model is best to be used depending on its applications

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