

Upscaling of regional models into basin-wide models

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The paper presents the upscaling technique in a realistic configuration in the Mediterranean Sea domain. A NW-Med model is nested into a Mediterranean model (MED) with a downscaling factor of 5 and the aim is to prove that the upscaling technique is driving the parent model (MED) solution towards the child model one (NW-Med). The upscaling consists in assimilating the 3D temperature and salinity child model fields as pseudo-obs in the parent model. The upscaled model solution is thus closer to the child model when compared to the parent model using 5 different metrics.

General Comment

The paper presents the upscaling technique as a relevant scientific question in the operational model community. After the revision the paper improved but there are things that reveal again a superficial approach of the corresponding author (I am sorry to say), which ignored some suggestions. Some corrections were only partially included. Added material (i.e. Tab.1 and the appendix) has not been described or motivated properly in the text.

I provide further suggestions to improve the paper readability and preciseness, with very detailed indications that required once again a lot of efforts.

However after these corrections the paper could be published.

Specific Comments

Abstract

Lines 2-6-20, line 4-5-8-26 page 2,....

I **already** suggested to revise the following nomenclature to be consistent with CMEMS one. In CMEMS (see Simoncelli et al., 2017!!!!) the **regional** models are considered the basin scale, while you use it to indicate the high resolution model. I seriously recommend to harmonize it in all the paper since you are in a CMEMS special issue.

Line 7 → please substitute “forecasts” with simulations, since you are not using the model in forecast mode. Again here you use low-resolution vs high resolution, other time you use parent and child, please harmonize.

Line 10: Being *in some sense* (not very scientific expression) *more realistic* means to have better prediction skills from model validation with observations, this should be specified.

Line 12: here you use “stand alone model” instead of introducing the **MED free model** and you could also specify the **MED upscaled model**. However you are stating that you are going to compare **MED free model** and **MED upscaled model**, but in the text and some figure you refer to NW MED model (i.e. Fig.9 and 10) and the NW upscaled MED model (i.e.Fig.11, Tab.1). This is another point to be harmonized, and that was ignored by the author.

Line 13: Looking at Tab. 1 you have improvements, if you consider improvement=decrease of RMSD on your metrics computed among parent-child models, that goes from 14% cross-shelf transp to 0% for SST. After SST Rhone plume metrics presents the smallest improvement. I expect/ed from the author this kind of evaluation which is totally missing in this second version. Tab. 1 has just been inserted without any explanation. I think that the paper should be full revised accordingly.

Intro

Line 19 page 1: I would take out “*regional and coastal*” referred to the oceanographic centers

Line 21 page 1: I would take out “*increased experience*”

Line 21 page 1: now you use “*local*” models ...

Line 1 page 2: I suggest “*high resolution observations of currents*”

line 4 page 2: Again please use either low vs high resolution or parent vs child

line 7 page 2: Again....please use either low vs high resolution or parent vs child and substitute forecast with simulation.

line 9 page 2: “*This constitutes the baseline hypothesis of the present study: it is desirable to “copy” the results of the nested model into the parent model.*”

Your **assumption** is that the child model performs better than the parent model within the child domain, your **objective** it to “copy/transfer/mimic?” the child model results in the parent one.

Line 11 and 17 page 2 → I would substitute “*forecasts*” with “*data*”

Line 12 page 3: I suggest to erase *“(only the horizontal grid is different). This could influence the conclusion compared to a set-up with 2 different model codes. However, this is not expected to be a fundamental limitation of the method. Concerning the vertical grid, in the usual case of assimilating real observations such as vertical profiles, the observations and 15 model forecasts have different vertical resolutions. Similarly, if the child model were on a different vertical grid than the parent model, it would still contain useful information, worth to be assimilated in the parent model. A limitation could be that some of the observations may be lost, e.g. the lowest child model layer may be out-of-grid in the parent model.”*

The text is confusing but the content is obvious and does not justify 7 lines of text.

Line 12 page 3: I would substitute *“forecasts”* with *“data”*

Line 18 page 3: *“If different model codes were used, the models could represent different processes.”*

The reviewer considers the text lines 12-29 superficial. Independently from the models' set up, you use child model data as synthetic observations in your data assimilation scheme. As for any other type of observation the assimilation approach is tuned accordingly. Most important would be the model data thinning or weighting as function of child model skill, but the author highly underestimated this aspect, preferring a pure assimilation exercise approach.

Line 1 page 4: I would substitute *“forecasts”* with *“data”*

Line 1 page 4: *“It should be noted that some high-resolution processes, resolved by the nested model but not by the parent model, could have large phase errors in the nested model. In this case, the baseline hypothesis would be violated, and the nested model could actually have higher errors than the former. This aspect is not considered in the paper.”*

Your SST metrics prove that you are in this case thus I would avoid the last phrase and I would ameliorate your results description and Conclusions accordingly.

2.1 Hydrodynamic Model

Line 12 page 4: Please refer to figure 2. I would also use *“The region is characterized...”*

Line 14 page 4: introduce the acronym at line 11 please.

Line 20 page 4: Please specify the resolution of the child model and correct the parent model resolution. 6 or 8 km? (See table in the appendix).

I do not agree on the choice of having an appendix with a table of identical columns. If you want to keep the table just mention it as Tab.1 here. Child and Parent model differences are the horizontal resolution 8km (not 6?), a highest resolution topography and bathymetry of the child model and the Rhone river discharge data.

Line 16 page 4: Please correct the reference in the bibliography to properly cite a specific section in the CMEMS OSR.

Simoncelli, S., Pinardi, N., Claudia Fratianni, Dubois, C., Notarstefano, G. 2018. Water mass formation processes in the Mediterranean Sea over the past30 years. In: Copernicus Marine Service Ocean State Report, Issue 2, Journal of Operational Oceanography, 11:sup1, s13–s16, DOI: 10.1080/1755876X.2018.1489208

Line 21 page 4

Please insert the references as suggested previously.

Med analyses at 1/16th

Clementi E., Pistoia J., Fratianni C., Delrosso D., Grandi A., Drudi M., Coppini G., Lecci R., Pinardi N. (2017). *Mediterranean Sea Analysis and Forecast (CMEMS MED-Currents 2013-2017)*. [Data set]. doi: https://doi.org/10.25423/MEDSEA_ANALYSIS_FORECAST_PHYS_006_001.

Paper describing the reanalysis set up

Simoncelli S., Masina S., Axell L., Liu Y., Salon S., Cossarini G., Bertino L., Xie J., Samuelsen A., Levier B., et al. (2017). *MyOcean regional reanalyses: overview of reanalyses systems and main results*. *Mercator Ocean J.* 54. *Special Issue on Main Outcomes of the MyOcean2 and MyOcean Follow-on projects*. https://www.mercator-ocean.fr/wp-content/uploads/2017/04/Mercator-Ocean-newsletter-2015_54.pdf

Reanalysis data set

Simoncelli S., Fratianni C., Pinardi N., Grandi A., Drudi M., Oddo P., Dobricic S. 2014. *Mediterranean Sea physical reanalysis (MEDREA 1987-2015) [dataset]*. Copernicus Monitoring Environment Marine Service (CMEMS). doi:10.25423/medsea_reanalysis_phys_006_004.

Line 24-27 page 4: this is redundant, it's already written in the introduction.

Line 30 page 4: How do you interpolate MED reanalysis data onto your parent and child model grid? Or is it only for the child model? Which kind of extrapolation did you apply where model topographies mismatch? i.e. Coastal strip, or bottom layers deeper than MED reanalysis ones.

Line 11 page 5: I suggest "...showing the surface salinity difference using climatological or daily data in child model (NW MED model) simulations after 1 month of spin up"

2.2

Line 24 Page 5: none detail is in the annex about the data assimilation. Modify accordingly.

Line 25: whole or thinned?

Line 26 isn't is a super-obs approach?

You are mixing the description of data assimilation and initial condition, I recommend to start from the upscaling experiment description (absent now), then IC and then DA. Moreover in Tab 1 you refer to 2 nested systems, thus you should explain both experiments.

Are the perturbed IC applied to both child and parent models or only the child, this is not specified.

3. Metrics

Please introduce Tab 1 and its interpretation either here or in **4. Results**. Now you mention it in the last line of section 4.5. Insert its reference also in all metrics discussions in 4.*.

3.4 *"This metric is the root mean square (rms) difference between the model and observed SST. For the latter, the L3 images are used. Furthermore, in order to examine the position of features such as fronts and eddies, the rms difference of the norm of the spatial gradient of the SST is also computed."*

This paragraph could be improved, among which models? What is in Tab.1? How is it computed? I suggest also to remove the second phrase, you are not talking about this afterwards.

3.5 I thank the author for the explanation however the text has not been modified. I suggest to do so, without mentioning the tail of the diagram. The reader would thank you.

4. Results

Figure 4: please increase the size of the red arrow

Figure 5: avoid to use forecast (plot titles), use consistent nomenclature in the caption.

4.2 second line, I would use *child* instead of *nested* (same in caption of Figure 3) as in the rest of the paper to harmonize and facilitate the reader. (Already suggested)

4.3

First Line: Why do you say that? Why don't you use Tab.1 to argument your statement?

Lines 3-4: The interpretation of Fig.8 is confusing. The upper panel shows the **MED free model**, please change the title in agreement with figures 6, 7. The bottom panel shows the **MED upscaled model**, please change the title in the plot accordingly. Why don't you comment the **MED upscaled model** instead of the nested model? You do it at line 2 of page 13

Moreover the arrows are pointing North-West or South West, is it correct? Could you better explain and interpret the figure for the reader?

I suggest to revise the paragraph and adopt the same nomenclature in figures/captions/text. This suggestion was not handled by the author.

Figure 9: This figure presents MED and NW-Med, why not the **MED upscaled model**? I suggest to show the three salinity fields. The author just skipped this suggestion, however the reader is confused since you always change approach in presenting the results. The nomenclature in Fig. 9 is not consistent, please change the titles to match **MED free model** and **MED upscaled model**. The author replied that the **MED upscaled model** is *indistinguishable from the nested model but the scope here should be to show that the MED upscaled model* is close to the nested/child model and not that the nested model is closer to the satellite image. From my point of view the author's answer is very superficial.

Line 10-13 Page 13: Considering that you do not care about what observations indicate, you say that upscaling is changing in-depth salinity in the ECC and WCC. This phrase should start a new line because not related to the Rhone plume, otherwise please explain what is the connection and motivate why upscaling is behaving in the right direction.

4.4

Line 3 Page 14: *Level 3 images are used for computing the metric* → already said in 3.4 (*a level 4 image shown in Fig. 5 is used only for visual comparison*) → This should not go here but in the Fig. 5 caption and specified at line 26 page 9.

Line 4 Page 14: "Results are shown in Fig. 10." What is the plot? What do you want to show? You say it at line 4 Page 15: "Fig. 10 shows the RMS error during the first 2 months of simulation." → of what, which models???

My suggestion is to revise the entire paragraph.

Line 4 Page 14: I do not agree that the **MED free model** is in very good agreement with SST, at least you do motivate it, including some reference to support it. What is the CMEMS skill in this region/period? <http://cmems-resources.cls.fr/documents/QUID/CMEMS-MED-QUID-006-013.pdf>
In fact in the paragraph You say that the error is relatively large in some days, that during summer is around 3 degrees C, that all the models are not resolving some coastal processes.

Line 5 Page 14: *“Usually, the nested model is better still in some areas (e.g. coastal waters), and the upscaling procedure brings back these local improvements to the parent model.”* Please rephrase, this statement is vague. You assume that the nested model is performing better in coastal waters, thus your technique should modify the parent model and increase its performance accordingly, right?

Line 5 Page 15: ~~A similar plot for the whole of 2014 shows that~~ *The situation worsens during summer when the computed RMS errors are of 3_C, both for parent and child model (not shown).*
It goes at the beginning of the paragraph.

“The difference in between models is hidden by the temporal variability of the error. In any case, the upscaled model is still very close, and slightly better, than both the (free) parent and the nested models.”

From my point of view, there are not differences among the **MED free model, MED upscaled model and NW-MED model (Tab.1 prove it)**. Please provide the average RMS computed over the considered time period, if you want to say that **MED upscaled model is slightly better than MED free and NW-MED**.

Line 8-13: They are about the model temperature in depth and should not go in this paragraph, eventually in the general discussion of results or in the summary.

“The model temperature in depth can be only punctually evaluated against observations (when e.g. drifter observations are available). In any case, the goal of the current study is to check whether upscaling is able to bring the parent model closer to the nested model, under the hypothesis that the latter is “better” in some sense. (not needed here it’s a repetition). Differences between the parent and the nested model are locally important, e.g. on the bottom of the Gulf of Lions, or in the Eastern Corsican and Northern Current cores (with differences of up to 0.3_C), and upscaling is able to push the temperature field in the parent model toward the nested model solution.” (not pertinent here and not shown!).

4.5

Again, what are you showing in Figure 10? I suggest also to modify:

“The total amount of Western Mediterranean Deep Water in the free model (blue curve in Fig. 11) and the nested model (green curve) is periodically important (103 km³), ~~and both~~ but the models do not ~~appear to~~ converge during the simulation. ~~On the contrary, a period of large discrepancy, as it appears during most of the second half of the year.”~~

Line 4 Page 16: I would use the reference to Tab. 1 here instead of line 9.

5. Conclusions

Lines 10 Page 17: You should say that for SST the upscaling did not produce any improvement, as shown also by Tab.1

In fact, you say in 4.4.

*“The ~~area-wide~~ **spatial** RMS error is not influenced ~~very much~~ by upscaling (**please refer to Tab.1**), as large areas are essentially unmodified (parent and child models use the same atmospheric forcing fields). Some days, some processes ~~appear to be missed~~ **are not resolved** by the models (both parent and nested), so that the RMS error is relatively large. In this case **again (?)**, upscaling does not influence the RMS error of the parent model ~~very much~~ (**the RMS in tab1 is identical**), as the nested model is not representing these processes any better than the parent model.”*

This suggest that without considering the skill of the child model, your upscaling might not improve the parent model solution, but just bring the child solution closer to the parent one blindly. It could also degrade the parent model performance. Obviously if you do not validate the models with observations, you do not know.