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, however it needs a lot of revisions to make it more a scientific paper than a technical report.
The English could be improved and detailed suggestions have been given but the reviewer is not mother tongue, thus take them carefully. The description of the methodology, results and conclusions appears sometime superficial and needs to be improved to be more complete and precise to allow their reproduction by fellow scientists. The figures must show always the same models (NW-Med, MED and upscaled). Labels, legends must be enlarged and captions improved. The RMSD could always be provided (MED-NW-Med and Upscaled-NWMed), and eventually be summarized in a table for the 5 metrics. The models' nomenclature should be consistent throughout the manuscript. Some references are missing.
The underlying assumption that the child model has a better performance is only stated in the conclusions, while is should be clearly stated in the abstract or at least in the introduction, since this is not always true due to possible phase errors (in space and time) in the higher resolution models. Moreover the upscaling technique to be more powerful could weight pseudo-obs according to their misfit with real observations assuring that the upscaling is stronger when and where the child model is closer to reality.

I recommend to accept the paper for further publication after a major revision.

Specific Comments

Abstract
Line 5: *Therfore* instead of therefor and I would take out "*in practice*"
Line 6: …" to replace the missing model feedback…" I would insert "*child model or high resolution model*".
Line 10: I suggest to rephrase something like:
"A basin scale model simulation is compared to one simulation…, and another model analysis which applies the upscaling technique…"

Introduction
Line 15: "reanalyses, analyses and forecasts"
Line 16: " …by different institutes within the regional monitoring and forecasting centers.."
Line 24: could you insert a reference for this?
Line 6 page 2: I would take out "in this artice"

Line 12 page 2: I would substitute *basin-scale* with *regional*
Line 13 page 2: "…in the basin-scale model, … is to obtain"
Line 14 page 2: "(along with…)"
Please rephrase the entire sentence, it seems too informal to me
Line 16 page 2: I suggest *"...to the child model will progressively gain consistency with the child model solution within its domain, being beneficial for the child model over time.*"
Line 20 page 2: do you have references for this?
Line 21 page 2: Is this pertinent? I do not see the connection, please explain.
Line 26 page 2: Other re-initialization techniques have been used blending, through optimal interpolation, coarse resolution operational analyses and coastal observations in so called Rapid Environmental Assessment experiments. Please give a look at *Simoncelli et al.(2011)*, they show improvements in the nested coastal model performance using observations.
Line 35 page 2: I am not sure that the syntax is correct please check the English.
Line 3 page 3: "Therefore acknowledging that operational…, Schulz-Stellenfleth and Stanev (2016) strongly…"

2.1 Hydrodynamic Model
Line 13: I would substitute *tried out* with *has been implemented*
Line 15: *"created by the junction of the Eastern and Western Corsican Currents"* I would cite *Pinardi et al (2015)*
Line 17: I would add some recent references *Pinardi et al (2015), Somot et al. (2016), Simoncelli and Pinardi in von Schuckman et al. (2018).*
Line 21: The resolution of MFS is $1/24^{th}$ (Clementi et al. 2017, https://doi.org/10.25423/cmcc/medsea_analysis_forecast_phy_006_013) of a degree since October 2017. The reanalysis (https://doi.org/10.25423/medsea_reanalysis_phys_006_004 Simoncelli et al., 2014, 2016) is still at $1/16^{th}$. Please check the CMEMS product catalogue for the correct acknowledgment. The system at $1/16^{th}$ (https://doi.org/10.25423/medsea_analysis_forecast_phys_006_001)is still operational at INGV http://medforecast.bo.ingv.it/mfs-copernicus/
Line 30: please specify for reproducibility issues which analyses has been used, I guess the 1/16? Or the reanalyses? Please clarify and insert the reference.
Line 32: ERA Interim is not at a resolution of $1/8^{th}$ of degree, it is 0.75 degrees!!! You might have re interpolated it from $1/8^{th}$ to $1/16^{th}$. Please explain it and add the Dee et al () reference of ERA Interim.

Line 1 page 4: Which literature?
Line 2: 5 rivers' data comes from?
Line 4: Please consider the CMEMS has 39 rivers, thus it is not very much coherent. Please look at the CMEMS products descriptions and may be cite the reanalysis instead. Rivers are described in the Simoncelli et al 2016.

Figure 1: Please increase the axis font, not readable now. I would show the two models' salinity fields to emphasize the differences due to the daily river outflow, instead of the difference. In fact, you describe the different plumes in the manuscript.

Line 10: please describe more in detail Fig1b eventually, isn't it the difference among the two models' salinity after 1 month of simulation? Please improve also the caption.

2.2
Line 16: I would re-phrase something like "*In order to assimilate* …, different set ups could be implemented (adopted, applied) depending on …"

Line 21: please improve the description of the settings, it contains repetitions.
Line 23-25: Start a new phrase please and please say something more about the statement that the T and S pseudo-obs are considered independent. You mean that you assume that even if it is not the case. You assimilate the full resolution 3D T and S fields? None thinning? Please motivate a bit this part. The word *also* could be neglected and substitute ";".
It looks like you wrote this in a rush without much care.

Line 31: random not randon

Line 7 Page 5: Why did you select 1 month of spin up time? Please start a new phrase and integrate a bit on that. What you use in the evaluation is the ensemble mean of the 100 members?

Line 1 Page 6: it is not clear to me "*...and its addition observation localization*", could you please explain?
Line 7: many are the sensitivity experiments with different observation errors, maybe you could insert a table.

3. Metrics

Line 17: Please add something about: .. these metrics have been computed to compare model solutions. What do you want to show? Upscaling model solution (is it an ensemble mean of the 100 members?) with the basin scale simulation?

3.1
Line 23: see previous comment. What do you want to show? Does this metric tell you if the upscaling procedure is driving the upscaled model solution towards the NW-Med? Please integrate a bit.

3.2

Line 28: again, see previous comments. I would integrate at the beginning of the section, and in each sub-section I would state what exactly is expected from each metrics.

3.3
I do not really understand what you are going to present in the results. Plume length/direction from the free and upscaled models? The comparison model-obs using sat chlla images? "Furthermore,…" What does it mean you do it qualitatively, quantitatively, both?

3.4
ok

3.5
Line 15: What do you mean bay "depth is larger than 1000m"? Is it the depth reached by deep convection? Maybe you want to put it at the end of the first phrase at line 13. Here you are describing the WMDW characteristics.
Line 17: Which tail? How could the river see that? Please explain or describe with some detail.\

4. Results
Figure 3: please enlarge the font.
Line 5: The difference of what?
Line 9: Isn't it Fig.1b the difference between unperturbed parent and child model in salinity after 1 month of spin up?
Line 10: Why should I trust the child model more than the parent? You did not provide any model performance. It is not easy to improve significantly the smooth solution of the coarse parent model (phase errors are common).

Figure 4: please include a line to indicate the section location on the map.

Line 16: The scope of Figure 5 is to show that the upscaling technique is bringing the upscaled model close to the NW-Med one. RMS difference could be provided towards the satellite SST to show this. However, this rise the question: " why don't you consider to weight the assimilation of pseudo observations according to the misfit with the observed SST, giving more weight where the pseudo-obs are loser to sat obs? Please keep the same notation to call the models (MED, NW-Med…).

Please switch 4.1 and 4.2 (thus fig. 6 and 7) to be consistent with 3.1 and 3.2.

4.1

Line 21: For which models?

Line 2 Page 9: Why the NW-Med model is not shown in Figure 6? Please include it for consistency in presenting the results, since you want to prove that the upscaled model is driven towards the NW-Med solution. Again please keep the same nomenclature to facilitate the reader. Moreover RMSD could be computed as in 4.2 case.

Lines 9-12: What is this meaning? Is this correct? Is this coherent with NW-Med?

Figure 6: please increase the font in the legend and include NW-Med.

4.2

Figure 7: Please insert NW-Med and the legend.

Lines 4 -6Page 11: I suggest to rephrase *"...this metrics cannot **be** used to compare and validate the models since observations are not available to compute the real NC transport. However, it shows that upscaling of.."*

4.3

As for the other metrics I would include the NW-Med to show its consistency with the upscaled model. At Line 15 you mention the nested (NW-Med) model but it is not shown. RMSD could be computed again as more robust argument of your results.
Line 17: a quantitative estimate of model performance can be provided again by RMSD of MED, and upscaled model towards NW-Med.

Figure 9: This figure presents MED and NW-Med, why not the upscaled model? I suggest to shaw the three salinity fields.

Line 3 Page 12: Is the increase in salinity observed, is it consistent with observations or only with NW-Med? I would include validation with observations, since a lot of them should be available by REP in situ observations fromCMEMS.

4.4

Figure 10 should be mentioned since the beginning of the paragraph to help the reader. Please harmonize the models' nomenclature in the text but also in the legend and caption of figure 10. The validation (actually the only one that is provided with observations) should cover the entire 2014 year and not only two months. The mean RMSE of the three models should be provided as well to support your results. Upscaling should consider the performance of the child model and assimilate only, or give more weight, in those parts of the domain where the child model is close to observations. Without considering models' performance upscaling could force the parent model towards a wrong solution.

Lines 6-8 Page 13: why not? REP in situ temperature and salinity profiles are available from CMEMS. I encourage to look at them and compute some validation to make your paper more robust.

Line 8: "Upscaling is able to bring the differences back to the parent model" or the child model?

4.5

Here you mention also the child-model of the upscaled model and you speculate without any proof about the largest consistency in the nested upscaled models. I recommend to include a figure to show it. Or if you want to use Fig. 11 please give the RMSD of the model couples or include a specific comment to the figure (Line 7-8?)

Line 10: Therefore

5. Conclusions

Lines 4-9 Page 15: "The underlining hypothesis…" You only state it in the conclusions while it should be written clearly both in the abstract and the introduction. In this way the reader is aware

that you only aim to relax the child model solution to the parent model solution, independently from the model performances.

Line 13: please use child instead of nested.

Line 15: Please be more precise and refer to the figures when this is shown.

Lines 1-6 Page 16: This general statement might confuse the reader since your child models never assimilate observations. Please rephrase to underline this.

Line 6: please include a reference since you do not show this in the paper.

Line 7-end: The advantage might be real is the upscaled solution has a better skill towards observations. In the last phrase: If the high resolution model is upscaled into the basin-scale one? Please notice that in CMEMS regional systems are the basin-scale ones.