Editor
Dear authors,
Both reviewers made some comments to further improve the manuscript. I agree with their main points. Please take their comments into account to revise the manuscript.
Best wishes
Qiang Wang
Dear Editor,
Following the recommendations of the two reviewers, we modified and shortened by 15% our manuscript in order to highlight its relevance. We hope these modifications make the manuscript significantly clearer and thus worth publishing.
Best regards,
Sébastien Petton

Report 1:
The revised manuscript has addressed some of issues mentioned in my first review. Added material to show the parallel computing cost is quite useful and clarifying new development reported in this manuscript is also helpful. Although the authors have provided some arguments in their responses to my first review comments, some issues remain unsolved.

1. The manuscript still looks too long for a journal paper. As the main development is adding the two-way nesting, it should concentrate on this point and minimize other technique details to shorten it. Two-way nesting is not a novel technique. This should be classified as an application to this particular system.
We do concede that the manuscript is quite long compare to the normed. However, it could be explained by the recent addition of the numerical developments realized in MARS and in in order to use full two-ways nesting in MARS model. In addition, we have taken care to properly demonstrate the various improvements. As said in the previous review step, the implementation of AGRIF in a split semi-implicit surface model was never performed to our knowledge. We think it is worth presenting it as long as the Alternate Direction Implicit (ADI *i.e.,* semi-implicit) solver used in MARS3D is quite popular among coastal modelling community. Nevertheless, we shortened the manuscript by 15% to comply with your requirement. We removed any redundant idea, the timescale indicator introduction and the conclusion part.

2. Whether the MARS3D-AGRIF system is a convenient tool for coastal marine application is still a question though the authors maintained their claim. From my understanding, the system is complicated to set up and awkward for post-processing, especially when there are many nested child grids. The authors have claimed that they can build a hierarchy of 4 or even more levels to meet coastal refinement but this will make the system even more complicated as they have explained in section 5 discussion. Adding a child grid requires offline bathymetric adjustment, physical parameter tuning, extra care for vertical level alignment, and boundary setup for overlapping with neighbouring child-grids. Considering that there might be over hundreds of child-grids when more levels are added, the system is doomed to be complicated. The authors argued that file compression may reduce the waste of storing land point values but storing output from over hundreds of child-grids already makes the output processing a huge burden. I think this is not the authors fault but the flaw of the system design. It would be unreasonable to ask the authors to simplify the system in a short time. I recommend that the authors just clarify it to avoid misleading readers.
As you spotted, the building of multi-resolution configuration with hundreds of zoom grids represents a real challenge for next generation of hydrodynamic models. The complicated process of creating this hierarchy (grid generation, overlap area fitting, bathymetric adjustment) is partially achieved at the moment with an external Fortran tool based on AGRIF library. The vertical alignment is automatically realized thanks to the system design. The physical parameter tuning and boundary setup fixing are part of the validation process and cannot be bypassed one way or another, as in any other hydrodynamic model. Upon us, the determining factors are now composed of parallel optimization computation and output files generation with the minimal overlap data. For the latter, we have already two solutions in mind depending on the final study objectives as said in the previous review: the user could choose to get a unique grid per level with all the child grid's data gathered upon weight averaged on overlapping areas and without keeping data where there is a subgrid level; or it will rely on a more classic format like the one used in unstructured model with a set of 1D nodes (longitude, latitude) where only data at the highest resolution will be kept.
This is still a major development and it was mentioned here as future perspectives. Therefore, we decided to remove this last paragraph of the discussion to avoid any misleading information.

Report 2:
The authors have implemented significant restructuring of the manuscript to improve its focus. The paper now flows much better, and presents as a study showing improvements to regional dynamics using 2-way nesting as diagnosed by comparison to observation (tides, temperature, salinity) and consideration of a flushing metric. As mentioned in the previous review, such implementations of 2-way nesting are not new, but here would add weight to the case in favour of using 2-way nesting. It's probably the editor's call to decide if this work significantly adds to the pool of literature on the subject such that it is worthy of publication.

The authors have included considerably more detail about how the semi-implicit method is handled in MARS. However, this doesn't translate to a clear picture of how the barotropic mode is coupled between coarse and fine grids. It seems like the ADI solver is not iterative, and computes updated barotropic velocities in one step (e.g., P4, L194). In terms of the 2-way nesting, then the coarse grid cannot supply barotropic velocities to the fine grid at shorter time-steps (as could be done with explicit split models at the barotropic timestep, or semi-implicit models with matrix inversions at every iteration of the implicit solver). If this is the case, then it should be clearly stated in Section 2.2. Also, some statement should be made on stability and accuracy of not coupling coarse and fine grids at a barotropic step, and perhaps speculate why it's not necessary when many split explicit models do require this for stability.
Yes, you are right, the barotropic and baroclinic velocities are computed in a single time step (see equations system 3). Then the coupling between both modes is straightforward (see L115-119). Nevertheless, the coarse grid forces every sub time step of the fine grid, thanks to the spatio-temporal conservative interpolator P (see L149-151). Doing so, the fine grid follows the trend of the coarse one. After the complete integration of the fine grid, the update procedure (from fine to coarse grid) finalizes the coupling. Consequently, the coupling is done at every (half) time step of the mother grid. Half is for a row-column scan for instance).
In the paper, this chronologic procedure is summarized in equations of system 6:

1. The coarse grid evaluates its dynamic over a half time step.
2. Then the coarse grid supplies boundary conditions (sea surface elevation, barotropic and baroclinic velocities, temperature, salinity) to the child grid at shorter time-steps thanks to the spatio-temporal interpolator P (Sect. 2.2.1).
3. After the complete sub-time steps integration, the coarse solution is updated using the child solution

The same integration is realized afterward for the other half time step of the coarse grid for the calculation of η, v, u (column-row-wise, instead of row-column-wise for the first half time step).
We concede that the barotropic fields, which force the child grid, do not come directly from the coarse grid computation (as for explicit split models). But it is worth noticing that the conservative interpolation allows us to introduce several levels of child grids. Following your remark, we specified on L153 and L163 that the described procedure is valid for each half time step of the coarse grid.

These, and the points below, are minor additions to the manuscript to strengthen its case, and I recommend minor revision.

P1, L13. 'As for structured grids', suggest 'for structured grids'.
Ok

P1, L16. 'coastal environmental researches and studies', suggest 'coastal environmental research and studies'.
Ok

P3, L121. 'a regional modelized configuration', suggest 'a regional model configuration'. 'a focus coastal' suggest 'a focussed coastal'.
Ok

P3, Eq. 1. Perhaps state u and v are depth averaged velocities. These are written with an overbar in Fig 2?
Thank you for this remark. For the sake of clarity, we had an overbar in each concerned equation. We also corrected Eq. 4 for the estimation of f_{wet} coefficient.

P6, L. 176. It seems that tracers on the boundary are computed using a radiation boundary condition (should this be characteristics – e.g., upstream advection?), rather than using interpolated coarse grid fluxes to update tracers via divergence. This gives global, rather than local, conservation, which is far less useful. Perhaps acknowledge here that local conservation is not achieved at the boundary.
You are right, the tracers are forced thanks to a radiation boundary condition not based on divergence of heat/salt fluxes. We explicitly specified that local conservation is not achieved at the boundary due to the interpolation. The same remark is valid for the velocities. However, it is not so penalizing for these fields because the momentum equations are not written in flux form. Moreover, at the open boundaries, the velocities are only used in Coriolis and Non-linear terms. Concerning the tracers, the misfit is however reduced thanks to the high coherence between the fine and coarse grids and the conservative interpolation for entering/exiting fields (Piecewise Parabolic Method in the normal direction to the boundary).

Section 2.2.3, first para. It's not clear over which part of the coarse grid the restriction operator is applied. Some 2-way nesting applies the restriction operator to the full coarse grid domain, others only to the interface separation (area of overlap). The last sentence of this paragraph says split explicit models apply R to 'a limited updated area next to the boundary of overlapping', whereas MARS uses R on 'the full area of overlapping'. This isn't clear; does the former mean the interface separation plus some extra area beyond the overlapping, and the latter only overlapping? Please clarify.
This note about split explicit models was in fact too general. For such models, the restriction operator could only be applied to a few meshes on the inner contour of the interface between mother and child grids. For MARS model, the restriction must be applied to the whole overlapping area. We clarify this point by only mentioning what is realized in our case.

P7, L197. ‘has be obtained’ suggest ‘has been obtained’.
Done

P8, L217. Here it’s acknowledged that global mass conservation is achieved. Perhaps also acknowledge that the more stringent requirement of local mass conservation is not.
We explicitly wrote in the paper that the update procedure is fully conservative (thanks to the procedure itself and the perfect match between the child and mother grid bathymetries) and that only the global conservation is achieved (due to potential misfits from the interpolation). The non-local conservation is specified in the previous section (L171).

P8, L221. ‘at same hierarchical grid level’, suggest ‘at the same hierarchical grid levels’.
Done

P8, L225. It’s not clear how the weights are computed – is it the distance to its own open boundary, or the boundary of the child grid it overlaps?
Actually, it is a mix of both criteria you cited: The weights are defined on the overlapping area according to an order of priority with respect to a limit of mother cells inside the child grid at its interface (three times the spatial refinement factor). In this limit, the child grid does not have priority, the weights are decreasing reaching 0 at its boundaries. Outside this limit, the daughter grid takes priority and the weights are defined according to the position of the border of the other overlapping child grid. We clarified this point from L220 to L226.

P9, L255. Change ‘nesting ang’ to ‘nesting and’.
Ok

P11, L306. ‘A zero gradient condition is applied to currents’. Is this both normal and tangential currents. What’s the boundary condition for elevation (Sommerfeld radiation, Flather etc)?
Yes, it is both for normal and tangential currents. A clamped boundary condition is set for the elevation with FES model. We added these specifications to the manuscript. L290-291

P14, L365. ‘depth is only of 8 m’, suggest ‘depth is only 8 m’.
Ok

P22, L898. ‘relies more on the accuracy of available bathymetric data than technical issues’ – is this really true? How much effort is required to optimize restriction operators, interface separation and sponge zones in order to achieve a viable solution? Is the system really insensitive to these operators?
Technically, creating an additional zoom is a process that only requires three steps (creation of zoom grid with respect to mother, interpolation of bathymetric data and connection with grid hierarchy). In practice, one has also to consider validation step, and of course this requires paying attention to interface dynamics. The key element lies in the geographic extent of the zoom depending also on the bathymetric gradient. As this assumption is only partially true, and to comply with Reviewer #1 we remove the conclusion and this sentence.

P22, L901. ‘that require mass conservation’ – BGC typically requires local mass conservation; i.e. no mass is created or destroyed locally (e.g. at the coarse-fine boundary). Perhaps clarify this.
You are right, we should have been more specific and we clarified this in the paper (see above answers). Regarding the conclusion, we removed it (and thus this sentence) to comply with Reviewer #1.