Reply to Reviewer 2

Dear Dr. Capodaglio,

Thank you for considering the idea of the manuscript relevant for the scope of the journal. We are grateful for your feedback and suggestions. We aim to enhance the presentation's clarity and rigor while incorporating additional tests. We address the individual points raised in a separate document that follows.

Sincerely,

The authors,
Luca Arpaia, C. Ferrarin, M. Bajo and G. Umgiesser

General comments

1) The paper is currently suffering from a lack of mathematical rigor and the presentation is sometimes confused. The notation is often unnecessarily complex and some definitions are missing. Please refer to the comments in the attached PDF document and revise the presentation (especially in Section 2).

In the revised version, Section 2 will be restructured. First we will present the multi-layer (or layerwise) shallow water model, eq.(3),(4) and (7) with improved references and notation. Then, in the same section, we will close the problem defining the generalized vertical transformation (1) in a discrete form which determines the evolution of the interfaces $z_{\alpha+1/2}$ for the different z-coordinates introduced (z and $z-$star).

The main issues in Section 3 are the remap part and paragraph 3.3: both need a clarification. Concerning the first point, our approach consists in considering the area swept by the interface in one time-step as the sum of two contributions: one due to the grid movement with velocity σ_{mov} and one due to the collapse of the element with grid velocity σ_{top}, see fig.2 (in the manuscript unfortunately both the interface velocities have the identical symbol σ, this will be changed). The pseudotime is $\tau = (t - t^n)$. It is introduced because, instead of solving (4),(7) at once with $\sigma = \sigma_{mov} + \sigma_{top}$, we have considered a splitting procedure. First we solve the governing equations on a moving grid, that is the discrete counterpart of eq. (4) or (7) with σ_{mov}. Then we solve equation (15) with σ_{top}. We will review this part. We will also consider the comments provided in Section 3.3.

Concerning your main remarks in the pdf, related to these aspects:

- *Line 65. We will add a sketch for the multilayer notation, similar to the one in the figure attached (which is the same as [E. Audusse et al. A Multilayer Saint-Venant system with mass exchanges for shallow water flows. Derivation and numerical validation. ESAIM, 2011] but with a reversed layer ordering).*
- *Line 65,66,70,71,75. This part could be rewritten in a vertical discrete framework as follow ($\zeta(x,t)$ is the free-surface, $b(x)$ the bathymetry, see again the figure):*
  "Consider a transformation from a reference domain $x \in [0, L]$, $z \in [0, -b(x)]$ discretized vertically with flat interfaces $Z_{1/2} = 0, Z_{1+1/2}, ...Z_{\alpha+1/2}, ...Z_{N-1/2}$ to a physical domain $x \in [0, L]$, $z \in [\zeta(x,t), -b(x)]$ with interfaces $z_{1/2} = \zeta(x,t), z_{1+1/2}, ...z_{\alpha+1/2}(x,t), ...z_{N-1/2}$. For $z-$star, the transformation at a discrete level reads:

  $$z_{\alpha+1/2}(x,t) = \zeta(x,t) + S_{\alpha+1/2}(x)\,(\zeta(x,t) + b(x))$$

  with $S_{\alpha+1/2}(x) = \frac{Z_{\alpha+1/2}}{b(x)}$. For standard $z-$coordinate we can use the following definition for the internal interfaces:

  $$z_{\alpha+1/2} = Z_{\alpha+1/2}$$

  ".
- *missing definitions may help clarify the text and will be added (e.g. square brackets for jumps $[\cdot]$ formula (4), finite element notation $(\cdot)_h$ line 136, simplification of the symbols in formulas line 155, 157).*
- *the notation will be changed when it is confusing (e.g. capital letter for tracers, notation of internal pressure gradient) or not rigorous (line 170).*
- *proper references will be added. For example, the name tracer constancy preservation (line 98 and 227) has been taken from [Shchepetkin and McWilliams, Ocean Modelling, 2005].*
- *we can rephrase unclear word/sentences, being more precise: e.g. Line 150 from "the layers spanned by the free-surface" to "the layers whose top-interface crosses or is above the free-surface", Line 235 from "form" to "basis", Line 240 "kernel" to "basis" etc ...*

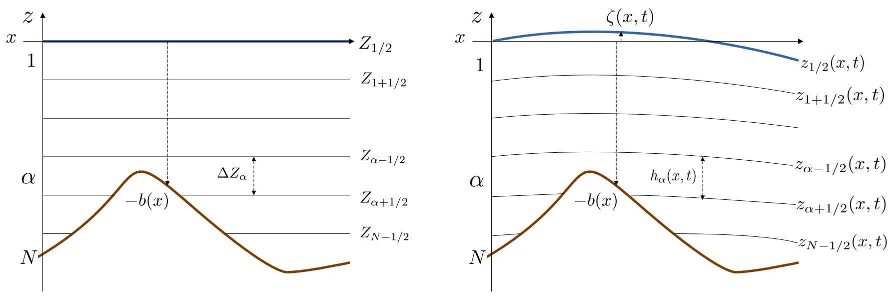

Figure. Sketch of the reference (left) and physical (right) space

2) The authors are doing a good job in the numerical results with showing that the proposed method is well behaved and that it does not perform worse than z-star. Although it is my opinion that additional results are missing to show that the extra hassle of introducing or removing layers is actually beneficial for some application. The authors should include at least one additional test case to show that their method enables simulations that are currently not possible with other existing z-type coordinate models. If this is deemed unfeasible, please show at least that there exists one relevant test case for which your model clearly outperforms existing z-coordinate models.

The reviewer raised a good point. We agree with him that this part is missing in the manuscript. We would have expected some performance differences between $z-$surface-adaptive and $z-$star coordinate, in intertidal flats computations. This is because the vertical discretization of the water column differs significantly in the two cases (with $z-$surface-adaptive one mainly performs runup/rundown with a one-layer shallow water model, with z-star the initial number of layers remains constant during the simulation). For the test #2 (tidal channel) such performance differences are still not very clear to us. We intend to further assess the two methods by evaluating their performance on additional wetting/drying benchmarks. Finally, in light of the limited differences observed in the Venice Lagoon test case, we plan to substitute it with the SHYFEM application to the Po Delta where more significant disparities between the two methods can be observed in reproducing salinity stratification and salt water intrusion.

3) The computational performance of the method is relegated to the last 4 lines of the numerical results section. Parallel performance is clearly of paramount interest for your readers so a more detailed analysis should be included in the paper. Currently, in the serial runs reported, the overhead of the present method is 8% over z-star. Would that get worse in parallel? If so, what are possible avenues for mitigation?

We can add computational performances for all the tests. We have not covered parallel implementation aspects because all the tests have been accomplished with a serial run. Although a recent version of SHYFEM is parallelized with MPI, the branch with the $z-$surface-adaptive developments only supports a partial parallelization with OMP. Unfortunately, this means that we cannot evaluate parallel performances of the $z-$surface-adaptive algorithm in the short term. We can comment that the algorithm (grid movement, insertion/removal) mainly operates on the vertical grid, and the parallel execution of these tasks should not encounter any issues. The stencil of the numerical scheme is not enlarged with respect to the standard method. However some variables have been introduced only for the insertion/removal operations. This is the case of the nodal top layer index (10) which must be exchanged between the domains.