Reply to Reviewer 1

Dear Prof. Bonaventura,

Thank you for considering the idea of the manuscript relevant for the scope of the journal. We are very grateful for all your remarks. We will try to answer to the individual points raised in a separate document that follows.

Sincerely,

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

Specific comments

1) Starting with equations 3-4 the authors introduce an undefined quantity h that seems to play the role of vertical spacing. This quantity appears alongside the quantity h_α , which instead is defined on page 4. Later on, quantities l_α are introduced and employed without being defined. Whether these have the same meaning usually implied in the multilayer literature is never stated. This does not allow the reader to understand exactly how the proposed numerical method is formulated and makes the results described in the preprint impossible to reproduce. A mandatory revision is to define properly all quantities that are being introduced and explain clearly their relationship (if any) to analogous quantities introduced in the literature on multi-layer models. Furtermore, in the appendix (line 386) the authors seem to imply that in models for which $h_\alpha = l_\alpha H$ the mass transfer terms across the layers have to be zero. However, this is not true for the (Audusse et al. 2011) paper which apparently constitutes the reference for the present paper nor for the related discretization approach introduced in Fernandez-Nieto, E. D., Kone, E. H., Chacon Rebollo, T. (2014). A multilayer method for the hydrostatic Navier-Stokes equations: a particular weak solution. Journal of Scientific Computing, 60, 408-437. and employed in (Bonaventura et al. 2018). The authors should clearly specify to which multi-layer formula.

As you remarked, we intended $hu_\alpha = h_\alpha u_\alpha$ with u_α the average value defined in (2). We will change such unclear notation to the standard one. Moreover the total water depth $H = \zeta + b$ has never been defined and this may have generated confusion. In the appendix we have analysed a very idealised case: a barotropic tide with no bottom friction ($u_\alpha(x,t) = u(x,t)$ $\alpha = 1, ..., N$) over flat bathymetry. In this case the mass-transfer at the layer interfaces for the $\sigma-$coordinate case (or $z-$star) collapses to the depth integrated mass conservation $G_{\alpha-1/2} = (\partial_t H + \partial_x(Hu)) \sum_{\beta=N}^{\alpha} l_\beta = 0$, and it is thus zero. That sentence was not referred to the general case where the mass-transfer across the layers is not zero. As you suggested, the appendix will be removed.

2) The authors devote a significant effort to the important issue of proving that what they call the 'tracer constancy condition'. They also refer to this condition as 'Geometric Conservation Laws', but no reference is given for either denomination. However, since the seminal paper Lin, S. J., Rood, R. B. (1996). Multidimensional flux form semi- Lagrangian transport schemes. Monthly Weather Review, 124(9), 2046-2070 it has become customary to describe this condition as 'consistency with continuity' or 'compatibility with continuity', see e.g.Gross, E. S., Bonaventura, L., Rosatti, G. (2002). Consistency with continuity in conservative advection schemes for freesurface models. In- ternational Journal for Numerical Methods in Fluids, 38(4), 307-327. Fringer, O. B., Gerritsen, M., Street, R. L. (2006). An unstructured-grid, finite-volume, nonhydrostatic, parallel coastal ocean simulator. Ocean modelling, 14(3-4), 139-173. Kuhnlein, C., Smolarkiewicz, P. K., Dornbrack, A. (2012). Modelling atmospheric flows with adaptive moving meshes. Journal of Computa- tional Physics, 231(7), 2741-2763. The authors might consider using (also) this terminology in the revised version. More importantly, as shown in (Gross et al. 2002), this consistency/compatibility must be guaranteed also for the time discretiza- tions of the tracer and continuity equations. Apparently, this aspect is not discussed in the preprint, so that the consistency proof provided by the authors cannot be considered complete. This is especially important for semi-implicit discretizations, since using advecting velocities at different time levels might easily occur in this context. Completing the discussion on this aspect would definitely increase the value of the preprint. Furthermore, in the numerical experiments this property is only checked in a case with flat bottom, while a numerical check also for more complex bathymetry is necessary. A mandatory revision is to include similar checks of the preservation of constants also for the sloping channel and Venice lagoon benchmarks.

Thank you for the references. We will improve the bibliography, adding a paragraph in the introduction with the references. We will switch the condition name too, taking out the ambiguous sentence on the GCL.

Concerning the tracer constancy, in the manuscript we have not considered the time discretization and we focus only on how the variable number of layers may impact the tracer constancy. In SHYFEM the *time-discrete layerwise mass-equation reads:*

$$G_{\alpha-1/2}^{n+1} = G_{\alpha+1/2}^{n+1} + \frac{h_\alpha^{n+1} - h_\alpha^n}{\Delta t} + \frac{\partial}{\partial x} (h_\alpha u_\alpha)^{n+1/2} \tag{1}$$

Then the tracer is updated with: 1/ a θ-method 2/ to answer your question, horizontal transport is computed at $n + 1/2$, while the vertical mass-transfer function is at $n + 1$:

$$\frac{(h_\alpha t_\alpha)^{n+1} - (h_\alpha t_\alpha)^n}{\Delta t} + \frac{\partial}{\partial x} \left((h_\alpha u_\alpha)^{n+1/2} t_\alpha^{n+\theta_h} \right) = [G^{n+1} t^{n+\theta_v}]_{\alpha+1/2}^{\alpha-1/2} \tag{2}$$

We agree that choice 2/ is also important to verify tracer constancy. We will correct/remove "Assuming that the time derivative and the vertical advection terms in (6) and (7) are treated equally, it is enough to verify that the horizontal advection term reduces to the mass-flux term". We will add the time discretization in a proper form, considering also the effect of a variable number of layers and of remaps. Remaps do not destroy the constancy property.

Concerning more complex cases, especially wetting/drying can be tricky. Although, since (1) and (2) are still valid, we do expect to conserve mass and preserve tracer constancy at wet/dry and dry nodes; in the revised manuscript we will show the verification (or not) for all the tests.

3) In many parts of the paper, the authors try to consider different $z-$ coordinate formulations within the same multi-layer framework. While this is definitely a positive thing to do and a potentially important contribution of the preprint, often the way in which the different formulations are handled is confusing, also because of the related lack of specific definition of the l_α coefficients. The authors are strongly suggested to review all the parts of the text in which the different formulations are presented and make sure that all the quantities involved are properly defined and the specific steps to be taken for each formulation are described completely and in detail.

We agree with you that the clarity may be improved by defining properly the interface position $z_{\alpha+1/2}$ the layer thickness h_α for the different vertical coordinates systems. We realized that all these definitions are either at continuous level (like z in (1)) or spread out over different sections. We will rewrite Section 2 in a more structured fashion. First we will present the layerwise Shallow Water model. Then, in the same section, we will close the problem defining the evolution of the interfaces $z_{\alpha+1/2}$ and of h_α, for the different z-systems introduced.

We consider a transformation from a reference domain $x \in [0, L]$, $z \in [0, -b(x)]$ discretized vertically with flat interfaces $Z_{1/2} = 0, Z_{1+1/2}, ...Z_{\alpha+1/2}, ...Z_{N+1/2} = -\max b(x)$ to a physical domain $x \in [0, L]$, $z \in [\zeta(x,t), -b(x)]$ with interfaces $z_{1/2} = \zeta(x,t), z_{1+1/2}, ...z_{\alpha+1/2}(x,t), ...z_{N+1/2} = -\max b(x)$. For $z-$star, the transformation at a discrete level reads:

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

with $S_{\alpha+1/2}(x) = \frac{Z_{\alpha+1/2}}{b(x)}$. The layer thickness can be deduced from the total water depth:

$$h_\alpha(x,t) = z_{\alpha-1/2} - z_{\alpha+1/2} = l_\alpha(x) \left(\zeta(x,t) + b(x)\right) = l_\alpha(x) H(x,t)$$

with $l_\alpha(x) = \frac{Z_{\alpha-1/2} - Z_{\alpha+1/2}}{b(x)} = \frac{\Delta Z_\alpha}{b(x)}$ which is prescribed by the reference grid and satisfy $\sum_{\alpha=1}^{N} l_\alpha(x) = 1$. Except for the fact that $l_\alpha = l_\alpha(x)$, we believe it has the analogous meaning as in the multi-layer literature.

4) In the introduction (line 40) the authors claim that their remeshing strategy solves possible stability problems of approaches proposed earlier in the literature. This claim is repeated later in the preprint (page 9, line 202). However, no stability analysis is provided to support this claim. In the revised version, the authors should either provide a proof of stability for the proposed algorithm or remove/reformulate any claims of superior stability properties.

The sentence will be removed and we can clarify what we wanted to point out. Our approach consists in considering the area swept by the interface 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). Without such an interpretation, one may be tempted for example to perform a removal of a surface layer after the semi-implicit update (no step 3.1, only step 3.2 of the proposed algorithm). Then a surface layer with negative thickness can occur in the semi-implicit update, unless the timestep is somehow limited a-posteriori with an iterative procedure to avoid the appereance of negative layer thickness. While a linear stability analysis would be interesting it seems quite complex (there is a non-linearity in the algorithm because the grid is deformed or not, depending on the free-surface position). We would prefer to focus on the clarification of other points.

5) At the end of section 3.2 (line 175) the authors introduce a pseudo-time quantity τ which is then discretized in steps $\Delta\tau$ to proceed to the remapping of discrete quantities, see equation (15). However, there is no indication on how this pseudo-time step should be chosen and on whether any empirical or theoretical bounds should be respected to maintain stability. Inclusion of some criterion for the choice of $\Delta\tau$ (sufficiently small fraction of Δt?) is mandatory for the revised version.

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}. In the implementation of eq. (15) we made the simplest choice: set a grid velocity that conserves the volume ($\sigma_{top} = \Delta z/\Delta t$), upwind flux and Explicit Euler. In the case of layer 1 removal (fig.2 "layer collapse" of the manuscript) we have the interface $2 - 1/2$ which goes towards $1 - 1/2$, thus moving upward; eq. (15) for the layer 2 reduces to (we neglect superscript $n + 1$):

$$
\begin{aligned}
\widetilde{h_2 u_2} &= h_2 u_2 + \Delta\tau[\sigma_{top} u]_{2+1/2}^{2-1/2} \\
&= h_2 u_2 + \Delta\tau \left(\frac{\Delta z_{2-1/2}}{\Delta t} u_{2-1/2} \right) \\
&= h_2 u_2 + \frac{\Delta\tau}{\Delta t} h_1 u_1
\end{aligned}
$$

with $\Delta\tau = \Delta t$, $\widetilde{u}_2 = (h_2/\widetilde{h_2})u_2 + (h_1/\widetilde{h_2})u_1$. Since $\widetilde{h_2} = h_1 + h_2$ we have that $\min(u_1, u_2) \leq \widetilde{u}_2 \leq \max(u_1, u_2)$. Sorry for the lack of clarity, we will correct this part.

6) The truncation error analysis presented in the appendix uses in an essential way the linearized equation

$$\partial_t \zeta + H_0 \partial_x u = 0$$

This form is consistent with the assumption that the linearization has been performed around the constant state $U = 0$, $H = H_0$ and that the velocity field u is a first order perturbation. This seems however inconsistent with the assumption of an $O(1)$ tidal amplitude A. The correct linearized equation would be in this case

$$\partial_t \zeta + U \partial_x \zeta + H_0 \partial_x u = 0$$

with $U = O(1)$. As a consequence, the upper bound on the divergence would also depend on the free surface gradients, which would seem physically reasonable. The whole derivation in the appendix would have to be reformulated taking into account the correct linearized equation. Furthermore, the following numerical experiments seem to consider a constant laminar diffusivity, which is rather different from the turbulent profiles that would typically arise in a realistic situation, making the whole discussion somewhat academic. Either the authors find a way to address this major shortcomings of the analysis presented in the appendix, or they would be strongly suggested to remove this analysis which is only marginally related to the main topic of the paper.

We will remove the appedix with the analysis. We wanted to understand the differences between the $z-$star and the z simulations in coastal environments with stratification and tide. We agree that 1) it is not relevant to the implementation with insertion and removal of layers 2) being too simplified at the end it was not really helpful to guide us in the interppratation of the results. Just for our knowledge, the tidal amplitude is $O(1)$ but the depth is still $H_0 = 50\,m$, $\epsilon = A/H_0 \leq 0.05$. Probably was this value too high for the linearization to be valid? Thank you.

7) The model equations are written in dimensional form, so measure units should be introduced for all the quantities reported when describing the numerical experiments (they are missing in section 5.1)

Yes, thank you. We will add them.

Technical corrections

1) line 13: replace 'that follow the materials' with 'that are material surfaces

2) line 21: the reference to (Cheng et al. 1993) could be complemented with a reference to the unstructured UNTRIM-3D model, such as e.g. Casulli, V., Walters, R. A. (2000). An unstructured grid, threedimensional model based on the shallow water equations. International jour- nal for numerical methods in fluids, 32(3), 331-348.

11) line 221: change 'sophisticate' into 'complicate'

12) line 240: change 'kernel' into 'basis function'

16) line 333: replace 'summerized' with 'summarized'

18) general comment: personally I think it is graphically better to write $z-$ coordinate than z-coordinate; this is not a required change but I think that it would be appropriate

Thank you we will correct the typos, sentences and the references.

3) line 87: the explicit definition of the term IPG_α should be introduced

For the internal pressure term SHYFEM use the density Jacobian form:

$$IPG_\alpha = h_\alpha g b(\zeta) \frac{\partial \zeta}{\partial x} + g \int_{z_{\alpha+1/2}}^{z_{\alpha-1/2}} \int_z^\zeta J(b, z') dz'\, dz$$

with $J(b, z) = \frac{\partial b}{\partial x}\big|_s - \frac{\partial b}{\partial z}\frac{\partial z}{\partial x}\big|_s$ *the density Jacobian (*$b = \frac{\rho_0 - \rho}{\rho_0}$ *the buoyancy). The integral is performed by interface:*

$$\int_{z_{\alpha+1/2}}^{z_{\alpha-1/2}} \int_{z_\alpha}^\zeta J(b, z') dz' = h_\alpha \sum_{\beta=1}^{\alpha} J(b_{\beta-1/2}, z_{\beta-1/2}) h_{\beta-1/2}$$

which means evaluating the Jacobian at the interface location, with a standard formula that can be found in [Shchepetkin and McWilliams, Journal of Geophysical Research, 2003 formula 2.3] or [Klingbeil et al., Ocean Modelling, 2018 formula 7.5]. We will add these formulas with the details in the text.

4) line 107: in formula (8), the argument of the flux limiter should be specified; this should also be done in all the other points where this quantity is introduced, most of the time without specification of the argument and of the location at which it is computed; also in formula (9), a ϕ_α appears that is not defined anywhere

We will specify both the argument and the location of the limiter $\phi(r_{\alpha-1/2})$. Formula (9) will disappear with the appendix.

5) line 114: in formula (9), the second derivatives on the right hand side have no reason to be positive, so that this kind of upper bound should be performed considering only absolute values of the quantities involved

Thank you for the correction, it is a mistake. We will remove the appendix and formula (9) with it, see point 6 of Specific comments.

6) line 135-139: what the authors denote as the space discrete and fully discrete variables, respectively, are indeed (see e.g. formula (17)) the P^1 finite element approximation of the solution, which is a piecewise polynomial continuous function; the text should be changed to avoid this confusion; even though the notation $u_h(x)$ is customary in the finite element literature, it is a bit confusing in a context where h has a different meaning (the finite element 'h' would correspond to what in the preprint is called Δx_E.)

We think the confusion comes from the fact that we wanted to describe the $z-$ surface-adaptive coordinate regardless of the horizontal spatial discretization (finite volume, finite element). And then some definitions conflict with the finite element notation introduced for the tracer constancy verification. We can discuss only the staggered finite element case. To avoid $(\cdot)_\mathrm{h}$ we can change it to $(\cdot)_{\Delta x}$ or we can omit the subscript, with an abuse of notation. e.g. for formula (18) "We seek an approximation, still denoted by ζ with an abuse of notation, which belongs to the finite dimensional space .... ". We will think how to clarify the notation.

7) line 146: the first sentence of section 3.1 is superfluous, any time discretization method will update the free surface based on equation (3)...the sentence should either be removed or reformulated if something else was meant

We will remove the sentence.

8) line 158: coefficients $l_{\alpha,i}$ are introduced without having been previously defined; this is related to point 1) in the specific comments above; clear definition of these quantities is essential, since otherwise the proposed methods are not completely defined nor reproducible

See answer to Specific comment 3)

9) line 188: it is unclear what do the authors mean by 'z-layer depth at rest Δz^0_α ; if this is the depth at the initial time, it is better to say so because what 'at rest' means in a hydrodynamical simulation is very unclear

With "at rest" we meant the reference configuration (see above, specific comment 3) which for us is always the one with $\zeta(x) = 0$ ("at rest"). Actually, it's true that the notation with Δz^0_α is not clear because it seems to suggest the initial time. In the revised manuscript we will use instead capital letters for the reference configuration $\rightarrow \Delta Z_\alpha$ (see always specific comment 3).

10) line 217: the expression 'hanging interfaces' is probably derived from the 'hanging nodes' used in the literature on numerical methods for non conformal meshes; however, while hanging nodes makes sense (the quadrature nodes on one side do not have a counterpart on the other side and numerical fluxes or mortar procedures must be employed), hanging interfaces does not make much sense in my opinion, since the interface is a perfectly well defined geometrical object; the authors are strongly suggested to modify this terminology and use instead e.g. non-conformal boxes, as they do in the following

We agree that, in the literature, "hanging interface" never appears. We will modify this expression.

13) line 244: the formula below this line is obtained according to the authors by integration by parts, but contains no boundary terms, the authors should explain whether these terms are zero and why or correct the formula

The boundary term has been neglected. We will add a comment on boundary conditions.

14) line 286: the rule used to define the mesh size should be explicitly reported, e.g. h_K as the maximum length of the triangle sides

We will be more precise about the mesh size.

16) in the caption of Figure 8, the quantities T, T_0 used to define the relative tracer conservation error are not defined; if they are meant to be the total tracer mass at the end and at the beginning of the simulation, then $|T - T_0|$ (absolute value is missing in the text!) is the absolute error, not the relative error; the authors are suggested to display values of $|T - T_0|/|T_0|$

We confirm that the values shown are not relative but absolute. There is an error in the caption of the figure. We will show the relative errors with a modified caption.

17) line 437: replace 'its' with 'her' !!! it would also be appropriate to specify better the direct or indirect contribution of Dr. Bellafiore to this work

We are really sorry, this is a bad mistake due to english deficiency