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Geoscientific Model Development Discussions

Interactive comment on "A fully consistent and conservative vertically adaptive coordinate system for SLIM 3D v0.4, a DG finite element hydrodynamic model, with an application to the thermocline oscillations of Lake Tanganyika" *by* Philippe Delandmeter et al.

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We would like to thank the anonymous referee for her/his careful reading and constructive comments. Please find our replies below.

On behalf of all the authors,

Philippe Delandmeter

***GENERAL REMARKS** The paper presents a new version of 3D hydrodynamic code SLIM 3D v0.4 intended for broad range of marine and limnological applications. The main new feature of the model is an algorithm for vertical grid adaptation to tackle hydrodynamic processes at sharp density gradients. The new model is tested in both idealized scenarios of wind forcing and lake stratification, and realistic simulation of Lake Tanganyika. An impressive correspondence between the model simulations and analytical solution for the steady-state thermocline tilt is achieved, as well as very good preservation of sharp density gradient at very coarse (6 vertical levels) resolution. Simulations of Lake Tanganyika demonstrate promising capabilities of the model for future studies of the lake circulation and thermal regime. The paper is well structured, the conclusions are clear and enough supported by results presented. Despite the overall high quality of the paper, I see space for improvement, especially in representation of the material and in clarifying some methodological issues.*

Thank you!

SPECIFIC COMMENTS

1. The title seems too long. Also consider substituting abbreviation "DG" by the full term.

Indeed, the title was quite long. To make it shorter, we removed from the title the description of what is SLIM 3D, which is given in the abstract.

2. p.4, line 13. When you first refer to "consistency", could you provide defini-

tion?

Yes. The term was defined later in the paper. We now define consistency the first time we refer to it.

3. There is no general information on the model equation set and boundary conditions

Indeed, this paper does not modify the governing equations that were set in Kärnä et al. (2013). But as you suggested, it is easier for the reader to have the full set of equations in the same manuscript: we added them in a new sub-section at the beginning of the Methods section.

4. Could you provide a clear definition of what is "fixed domain" and what is "moving domain"

The governing equations are originally written on a continuously moving domain, since the water volume boundary moves following the free surface. Consequently, the equations cannot be discretised directly, since the mesh does not move continuously but discretely in time. Let's consider a fixed domain, in which the free surface does not move. In such domain the equations can be discretised, but governing equations are not written in this fixed domain. It is why we introduce a mapping to reformulate the original equations from the moving to the fixed domain. This is not new, and it is why we refer also to Formaggia and Nobile (2004).

A better introduction to fixed and moving domains was given in the "Moving mesh and ALE formulation" sub-section.

5. p.6, line 1. "equations" → "equation"

Corrected, thank you!

6. p.6, line 9: please provide explanation to "P1"

The P1 shape functions are first order polynomial functions. They are bi-linear in case of an extruded triangle mesh and tri-linear for an extruded quad mesh. Since the bi-linear/tri-linear explanation is already given in the next sentence, we simply replaced "P1" by "polynomial" to keep it simple.

7. p.6, line 16: could you explain what is "lateral and horizontal interfaces"?

Following your comment 11, a new Figure (Fig. 3) was added. In this figure, we also illustrate what are the lateral and horizontal interfaces.

8. I could see no information on the order of approximation of the model scheme.

In SLIM 3D, the equations are approximated using discontinuous piecewise bi-linear/tri-linear functions. This was explained for the moving mesh algorithm, but as you wrote, it was not explained that the entire model follows this approximation. This is now written explicitly in the beginning of the numerical modelling sub-section.

9. p.7, line 7. Does the zero vertical velocity at the bottom fits the simulations of Lake Tanganyika with uneven bottom shape? Rather, normal component of velocity should be zero.

Indeed, the impermeability boundary condition requires that the normal component of the water velocity is zero at the bottom. The line you refer says that the vertical mesh velocity (w_m) is zero, not the water velocity.

10. Not all symbols in equations are explained. I recommend to add a List of symbols.

The different symbols defined in this paper as well as the variables were listed in two tables (Tables 1,2).

11. p.8, lines 15-20. I suggest that you provide a 3D picture of the model grid, as this text is somewhat difficult to follow.

A new figure (Fig. 3) was added to illustrate this paragraph using a very simple 3D mesh. Examples of horizontal and vertical interfaces (see comment 7) were also highlighted on the mesh.

12. eq. (15). Is this constant in time or in depth?

Thank you for highlighting this confusing term. It is a constant only in depth. This was fixed in the revised manuscript by removing the “constant” term:

$$h_{i+1/2}\, e_{i+1/2} = h_{j+1/2}\, e_{j+1/2}, \quad \forall i, j = 0 \,...\, n-1.$$

13. p.9, line 2 : “adaptivity” → “adaptation”

Changed, thank you!

14. p.9, lines 8-9: the definition is difficult to understand, please consider rephrasing

You are right: the explanation is not straightforward. The upper and lower DG values of a mesh node were illustrated on new Fig. 3 for a better understanding.

15. Figure 4: please explain what do the numbers mean

The numbers mean the level depth. It was added in the figure (It is Fig. 5 now) caption.

16. p.10, line 21. At this stage, not clear which “model” is meant

Indeed, details about the COSMO-CLM^2 come later. This sentence introduces the section about the two dataset. We rephrased the sentence to remove this unclear “model” word.

17. p.11, line 5: COSMO-CLM2, “2” looks like footnote

Indeed, this confusion is understandable. However, the model is called “COSMO-CLM^2” in the existing literature and we have to keep it this way. Fortunately, the model name is used twice in the same paragraph, so the reader should see that the “2” is part of the model name.

18. How does modeled stress at Figure 6 compare to measured at Figure 5?

As you pointed out, one figure shows modelled wind stress while the other shows measured wind stress at one location. We cannot compare them directly, they do not refer to the same calendar year. But beyond those considerations, they both have the same magnitudes during both dry and wet seasons, and we observe on both similar patterns in variability. We didn’t compare them directly, but use both of them, the measured (uniform) data for a simple modelling maintaining the sharp interface, and the spatial-varying map for the more realistic simulation.

19. Eq. (19) Is there any special reason for parameterizing heat flux by this

crude scheme, rather than to apply standard surface flux schemes, based on Monin-Obukhov similarity?

A relaxation flux is not unusual (see e.g. Kamenkovich and Sarachik (Journal of Physical Oceanography, 2004) and Barnier et al. (Journal of Marine Systems, 1995)) and is a simple alternative choice depending on the availability of forcing data. More complex surface fluxes, which require more input data, are currently considered for a better modelling of the surface heat fluxes, but this is not done in the study described here, which focuses on the adaptive moving mesh.

20. Figure 8. What are the black lines? Could you depict the grid levels, at least in the inset?

The black lines are precisely the grid levels that you ask to add to the figure. This was highlighted in the caption.

21. I found no details on which computing system has been used. Was the model parallelized, what number of cores has been utilized?

SLIM 3D runs on parallel computers. For the Tanganyika simulation, the mesh is rather coarse ($\sim$ 13000 elements) and the simulation was run on 8 CPUs. We also added information about the time step, which was missing.

22. Figure 14. There is larger vertical diffusion of heat in observations, than in the model. What could be the reason?

As it is pointed out in the discussion section, the surface temperature used to compute the heat flux (from COSMO-CLM^2) does not match accurately with the surface temperature in the two validation locations. Temperature are generally higher in COSMO-CLM^2 data than in observations. This induces higher temperature in SLIM 3D

surface results, which increases the stratification. This is most likely a reason of the larger stratification in SLIM 3D. Since stratification is larger, then the water column is more stable and the vertical diffusion is smaller than in observations. But it is noteworthy that this difference in stratification between model and observations isn't very large, and it is relevant to use it to evaluate the quality of the simulation which uses the best available input data. We have improved the paragraph referring to this point in the discussion section.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-221/gmd-2017-221-AC2-supplement.pdf

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