Response to reviewer's comments

6 July 2024

We thank the reviewer for his/her time and professionalism. Below, the comments from the reviewers are in black while our responses are in blue.

REVIEWER 1:

I am satisfied with the answers and the revision made. Below are some minor things and some answers or discussion related to the questions raised previously. The manuscript can be accepted after they are addressed. The manuscript needs not to be sent to me another time.

23 water stress?

As most ocean models nowadays represent the Ekman spiral, there is usually no off-diagonal term associated with the water stress. However, when a non-zero turning angle is used, there is an off-diagonal term. This can be seen in equations A46 and A47.

41 Is not piecewise-constant equivalent to the first-order upwind?

We have clarified in the revised manuscript (lines 40-42) that the upwind scheme in SIS2 is based on a non-directionally split approach while the other options are directionally-split piecewise parabolic, directionally-split piecewise linear, and directionally-split piecewise constant methods.

55 'incremental remapping is much less diffusive...' – it is not so the remapping, but linear reconstruction in cells, which makes the method higher-order. Without it the method would be very similar to upwind.

Good point. We added some text in the revised manuscript (lines 55-57) about the linear reconstruction of scalar fields.

59 Noise is mentioned here for the first time, and your reader is unaware about it, so provide some details.

We agree. We added a paragraph about this in the introduction of the revised manuscript (lines 59-62).

66 'description ...' – I would say about interpretation and elimination.

We have replaced 'description' by 'interpretation' (line 72 in the revised manuscript). We refer to the 'elimination' of noise a few sentences above.

75 Appendices B1-... (if you use plural).

Corrected.

114-115 Keeping E_0 small would require to increase n_{evp} on finer meshes. Instead, one could fix T_d independent of model step, assuming that the convergence to VP will be achieved during several time steps.

We agree this would be a possibility but it would need to be tested. We prefer not to mention this here as this is beyond the scope of this work.

395-398 Do you need this in Conclusions? This is just sanity check, and there is no ground to expect something different.

We agree this is not needed. We have removed the paragraph in the revised manuscript.

Two questions related to the discussion in the first review.

1. Divergence noise: The explanation proposed by the authors can be a part of the story. It, however, contains too many assumptions, such as for example u_0 is considered to be small (and u' is nevertheless kept), and fluctuations of concentration are ignored although they will be appearing with a large factor about 20 in the linearization. Furthermore, the mode it identifies is the mode in divergence, and it is seen immediately. For a Fourier mode, C-grid divergence with respect to T point is $(2/h)(u_E \sin(kh/2) + u_N \sin(lh/2))$. If velocities are interpolated to B-grid, the divergence becomes $(2/h)(u_E \sin(kh/2)\cos(kh/2) + u_N \sin(hl/2)\cos(lh/2))$. (i) It has a mode. (ii) it is in error even if kh is small, but lh is not, or vice versa. While I do not have much against the analysis in the manuscript, it is not really needed. If there is a grid-scale component in velocity, it will be decoupled from the grid-scale component in tracers, which results in non-propagating perturbations.

Event though our interpretation relies on a simplified set of equations we would prefer to keep it. We already mentioned that many assumptions have to be made for our modal analysis. Second, we mention that this is our interpretation of the checkerboard pattern. We also added the following sentence (lines 682-683), in line with the reviewer's comments, in Appendix B: "This

$[1 + \cos(l\Delta y)]$ term characterizes the spurious divergence associated with the interpolation of velocities to the U points."

2. The contributions from metric factors: The authors are right saying that metric factors are everywhere treated as in the manuscript. However, take, spherical geometry. The $r\phi$ and $r\theta$ components will generally appear in strain rate tensor even if velocities are tangent to the surface. Their divergence with respect to ϕ and θ will contribute to tangent vectors. The well-known expression for vector Laplacian in spherical coordinates includes such contributions ($-u/\cos^2(\theta)/R^2$ instead $-u\sin^2(\theta)/\cos^2(\theta)/R^2$ which will be obtained if they are ignored). I do not think further discussion is needed, since now the approximation is mentioned in the manuscript.

Again we would like to thank the reviewer for his/her many useful comments and critiques.

Jean-Francois Lemieux