1 General comments

The manuscript is devoted to the study of tidally-driven numerical mixing in the z-layer SYMPHONIE ocean model. Spurious mixing is not computed as a diagnostic quantity. A rather practical approach is followed which consists in an extensive validation of the tidal and non-tidal runs against several temperature and salinity datasets. The comparison shows that, for a third order upwind advection scheme, the non-tidal simulation reproduces unrealistic sharper profiles while the tidal simulation is overly diffusive. An filtered advection scheme is introduced to cure the problem and reduce spurious mixing in tidal simulation. The manuscript is interesting and clear in both the analysis and the numerical experiments. It shows it is possible to perform tidal and internal tide simulations with a $z-$layer model, at the price of using very high order advection schemes. It shows also how different processes may balance in ocean models in an unpredictable way (in non-tidal simulation, spurious mixing compensates for the absent tidally-driven mixing and the more diffusive scheme outperforms the filtered one). The comparison with the state-of-the-art approaches that solve the problem of tidally-driven spurious mixing in other ways (smarter choice of the vertical coordinate, for example) could have been pushed further. As an aside, in some secondary discussions, I have found the style a little bit intricated.

2 Specific comments

1/ Line 6. Unclear sentence "This papers provides a clear illustration of this phenomenon in the context of simulation of the South East Asian Seas is provided"

2/ Line 12. I found the sentence "Simultaneously, an improvement of this advection scheme to make it more suitable for use on the vertical is provided" a little bit twisted style.

3/ Line 20. Is it more correct to talk about "diffusion" instead of "dissipation"?

4/ Line 53. "Considering the eulerian nature of the problem", what problem? Maybe rephrase: "Considering the Eulerian nature of geopotential coordinates".

5/ Line 53. I could agree with you that the adaptive or the $z-$tilde coordinates remain a research topic and their implementation into already existing numerical codes requires considerable effort. But, if the aim is to have accurate internal tide simulations, I would mention the z-star, which belongs to Arbitrary-Lagrangian-Eulerian coordinates. It has been around for over 20 years, easy to code in z-layers models and it is implemented in many operational models nowadays. Since the seminal work of [Adcroft and Camping, 2003, Ocean Modelling] it has demonstrated to outperform fixed $z-$ layers for internal tide simulations, reducing tidally induced numerical mixing. I know that it is beyond the scope of the paper but it would be very interesting to add a 5th run with the current advection scheme (UP3) and $z-$star. It is mandatory to comment on $z-$star in the introduction, it is not mandatory to add the comparison.

6/ Line 211. Symbol R (real part?) is undefined.

7/ Line 230. "and finally its limited stencil size does not increase the computational cost of the scheme too much". I kindly suggest to make this sentence more precise by computing the number of points in the stencil for both the scheme that update the tracer with UP3 (7 points?) and with the filtered version (9 points?).

8/ Which is the order of accuracy, order of truncation error of the filtered scheme? It would be very illustrative to report the curves of the damping coefficient in term of θ for UP3, the filtered scheme and a fifth-order finite difference scheme.

9/ Does the filtered scheme run stable with the same timestep of the UP3?

10/ Line 109. Could you please add the range of the vertical resolution (e.g "60 levels with varying resolution $1\,m - 100\,m$"). Spurious mixing, as shown in your analysis is strongly influenced by Δz and the reader may be able to check the values you used.

11/ Line 131. "even though horizontal advection will also be discussed". From this sentence it seems that horizontal advection is included into the analysis but it is not. Please remove or move this sentence.

12/ Line 168. "Such non-linear schemes are however computationally costly and still exhibit a sensible amount of spurious numerical diffusion, comparable to the physical one (see e.g. Megann, 2018), with the additional drawback that it cannot be explicitly diagnosed." I find this sentence quite unclear. The first part is at least debatable. Why should vertical TVD schemes be costly (if only the horizontal grid is partitioned there is no issue in terms of parallelization and i don't think we have to worry about a few more operation to compute the slope limiter in a complex ocean model and on modern HPC architecture). Sorry I have not read your reference but with respect to what would the slope or flux limiters increase numerical diffusion? The TVD scheme has to be compared with your (4) which uses a blending between a low-order scheme and a high-order one. Why should your blended scheme (4) outperform TVD schemes? Finally why numerical mixing cannot be diagnosed? I would remove the whole sentence which is not directly related to the manuscript topic.

13/ Section 2.3.2. For sake of clarity could you please add the explicit formula for the UP3 flux, something like:

$$F^{UP3}_{j+1/2} = W_{j+1/2}\frac{T_{j+1} + T_j}{2} + ... \qquad (1)$$

Then it can be easily coded by someone else.

14/ I am curious about the physical (not numerical) mixing induced by the tide in you model. This seems to be crucial in the correct reproduction of water masses, in fact, the non-tidal simulations cannot reproduce this mixing and compute very sharp tracer profiles. But I believe the hydrostatic models cannot compute explicitly this physical phenomenon. How it is recovered? Could you comment more on this in the introduction?

Thank you very much