Review of

WAVETRISK-2.1: an adaptive dynamical core for ocean modelling

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This paper provides an overview of the ocean version of the WAVETRISK model (sections 1 and 2). It then describes, in section 3, a number of developments that have been made to the model specifically for the ocean; a split between barotropic and baroclinic steps; an elliptic (Helmholz) solver; a TKE vertical diffusion scheme. Section 3 also briefly describes the penalisation method for lateral boundaries and provides a helpful outline of the model's algorithm. Section 4 describes results obtained for 3 "standard" test cases.

The paper is well written and the developments described are impressive and well worth publication. I learnt a lot quite quickly reading it! I have made quite a number of comments that are mainly intended to help the authors to present their work accurately and more clearly. There is quite a body of literature here that was new to me. Several other readers will be in a similar position. So I hope my comments will help the authors to make their paper more accessible to such readers.

At the end of the review I have asked some questions about the dynamical formulation that the authors are exploring. Publication should not depend on the answers to these questions but it would be interesting to hear the authors views on these questions and perhaps some of these points could be mentioned in the concluding section as areas where detailed exploration would be valuable.

I haven't noted minor typos / grammatical errors as I am sure the journal editors will correct these.

Abstract

This is informative though the adaptation for the sphere is not mentioned and results from test cases are not summarised; it is difficult to summarise them briefly so I think that is OK.

Lines 10-12: innovative feature – is this mentioned in the main text? If not it could be mentioned in the introduction and/or conclusions.

WAVETRISK – it might be helpful to explain the origin of this name at some point. I think it is a short-hand for wavelet and TRisk but this is not very explicit.

Introduction

Lines 31-33: I think vertical columns are a good idea. It's not just a matter of simplifying development. But the authors might prefer to keep this statement as it is.

Line 34: "final stage in foundational set". This is a somewhat subjective statement. For example a non-linear equation of state might be viewed as part of the foundational set and isopycnal diffusion is also a very important component of ocean models.

Lines 39-40: Ripa (1993) introduced n-IL^0 for a reduced gravity ocean (no pressure variations at the bottom).

Line 42: Ripa emphasised that potential vorticity is not conserved (tracers are); it is advected with a source term. I think it is worth mentioning the Hamiltonian structure (as well as the Casimirs) because this can be very helpful for developing discretisations with good conservation properties. There is a nice paper by Salmon illustrating that point.

Line 42-43: "ensures a good approximation of the horizontal pressure gradient". I don't think Ripa discussed that. Could you please provide a reference to support this statement? It might be better to discuss this point in section 4.1 when discussing the sea-mount test case.

Line 44: I think it is worth emphasising that Dubos et al (2015) derive their equations of motion from the discrete Hamiltonian somewhere near here.

Line 66-67: Are the details provided in section 3 new (not previously documented)? I think they are but this should be made explicit.

Section 2

Lines 72 – 83: The complexities of the equation of state are an important issue in ocean modelling. I think it is one of the main difficulties in the formulation of HLPEs. Postponing the treatment of these issues is understandable but will deter readers interested in practical applications. I wonder whether some of the issues for HLPEs might be less serious for ILPEs. A proper discussion of this point might well require a dedicated paper. A sentence or two referring to papers that discuss the issue for HLPEs might be a good "solution".

Line 89: is *v* the vertex of a triangle?

Figure 1: I do prefer figures which indicate how the hexagonal and triangular elements relate to each other. Just showing the triangular elements does not help readers unfamiliar with TRisk.

Equations (4) and (5): It would seem more natural to me to write $\overline{\theta_{ik}}^{e}$ than $\overline{\theta_{k}}^{e}$ but your equations appear to be consistent in dropping the *i* subscript. It might be worth mentioning your convention as I found it quite puzzling at first.

Line 94: q_{ek}: I think potential vorticity is defined at the points marked by red triangles in Figure 1 rather than at the edges. I'm not sure what subscript is used to denote these vorticity points.

Line 95-96: $\overline{(.)}$: the Coriolis term involves quantities at circulation and node points, so the inputs to the reconstruction in some cases are not just node quantities.

Lines 96-97: I think the discrete operators used are dimensionless. In other words strictly speaking they are difference operators. Calling them gradients or divergences is helpful as long as their difference operator nature is also made clear. This is important for (7) – (9) to be dimensionally correct.

Lines 99-100: This sentence when read together with the previous sentence is obscure. Does the additional N+1 vertical layer include depth integrated velocities v as well as the free surface height? If so, saying that would make the text much clearer.

Line 103: $\delta\theta_k$: I think this should be $\delta_i\theta_{ik}$. The horizontal derivative must be within a layer (rather than horizontal in x,y,z space). It might be worth mentioning that.

Lines 104-105: Ripa (1993) used vector invariant form (but also reduced gravity as mentioned earlier).

Equations (6) – (9): I haven't had time to check these. Unfortunately "horizontal" diffusion does not correspond to isopycnal diffusion.

Lines 119-120: "for better accuracy and stability" I imagine this separation does not need to be exact and is just intended to reduce the precision with which variables need to be stored. Is the mean independent of horizontal position?

Line 142: I found Dubos & Kevlahan (2013) to be a good starting point for understanding the wavelet scheme for the TRisk grid. It would be helpful to mention that.

Line 160: Typical internal gravity wave speeds are between 2 and 3 m/s not 0.05 m/s. I'm not sure how that error has crept into the text.

Lines 162 – 165: I mis-read this sentence at first despite being fully aware of what it is trying to say. I think the punctuation is difficult to follow. Please try to separate the 3 alternatives more clearly.

Line 173: "vortical": do you mean "baroclinic vertical modes" ??

Lines 175-176: for clarity please say explicitly that it would not represent barotropic tides accurately.

Lines 181-185: There is repetition of points about RK3 and RK4 that should be removed.

Lines 187-230: In general this is well explained, but I'm not completely clear about two things. First I think (please confirm) that (19) is solved on each of the three RK3 sub-steps. Second does the vertical diffusion use the fields on the backward time-step? I think this is fairly clear but the text could be tidied up a little bit. More specifically, the point in the sentence on lines 188-189 could be moved to follow eqn (21).

Line 196: insert a comma after "position"

Lines 239-245 and Figure 2: is the Coriolis parameter equal to zero in these tests or are the results independent of it?

Lines 253-266 and Table 1: This would be easier to read if the sentence in lines 260-261 was brought forward to the start of line 255; I felt quite puzzled after reading lines 259 and Table 1. The computational core of ROMS has two tracers and might do some extra calculations (e.g. isopycnal diffusion) so the comparison is not very precise but it is useful and quite impressive in my view. The performance has only been shown for < 5 nodes. Are issues anticipated with configurations using 100 or more nodes?

Section 3.2: I did not notice any errors but I'm not very familiar with the TKE scheme.

Line 313: "baroclinic and vortical modes" again

Section 3.3: I was not previously aware of the SRJ method so found this section very interesting.

Section 3.5: I haven't understood how the adaptation of the horizontal grid differs from the wavelet transform algorithm (algorithm 4) which seems to involve adapting the points / zero wavelets. Algorithm 4 is called at a low level and hence very frequently whereas the adaptation of the horizontal grid is said to occur infrequently (once every 10 baroclinic steps if I recall correctly).

Section 4.1

I believe this seamount test case, as it has been set up, is an extremely rigorous test of this model in the sense that the horizontal inhomogeneity is an extreme case.

For this case is there a similar issue in your equations to that found in standard terrain following coordinates in the calculation of the hpg, i.e. that two large terms are nearly equal and opposite? I imagine there is. Some discussion of this point would be helpful to the reader.

I don't know what vertical discretisation you are using. Could you please direct the reader to the paper that describes this or provide at least a brief description of it.

It seems to me that this test case is less well defined / documented than it could be. I don't know how the results depend on the details of the vertical grid (number of levels and stretching). This complicates the comparisons.

SMcW (2003) use only 10 levels and the domain average kinetic energy of their best method appears to be smaller. Which figure in SMcW (2003) did you use to obtain the numbers you quote for their errors?

Beckmann & Haidvogel have a very different (actually quite strange) specification of the background density gradient and the density gradient that is included in the hpg calculation. It is quite difficult to compare your results with theirs.

Debreu et al 2018 do not say which algorithm they are using for the hpg calculation. I'm not sure what vertical grid spacing they use (is it uniform?). The results in your paper do look similar to those of Debreu et al 2108. Perhaps the simplest way to get a clean comparison would be to consult with Debreu and to use the same grid as that paper and give some more details of the scheme that he used.

When higher order reconstructions of fields are used to calculate the hpg, it becomes important to decide whether cell values represent point values or grid-cell means. The specification of the initial state is easier if the cell values are taken to be point values. If they are taken to be grid-cell means 3D volume integrals should be calculated to define the initial state. Could you please state what choice you have made here.

Equation after (29): S is the Burger number (not Burgers).

Line 358: I wonder whether removing the north-south boundaries has any impact on the results.

Figure 3 lower plot: The initial density field should be flat (horizontal isopycnals). So something is wrong in this figure.

Figure 4: It is interesting that there are no damped oscillations (unlike most low order schemes).

Section 4.2

I am not familiar with this test case so cannot offer detailed comments on it.

Line 385: I wonder what the value of \beta is in the centre of this domain. Is the impact of the variable Coriolis parameter on the results negligible?

Table 3: layers 3 and 4 have typos in the decimal place for z and dz.

Lines 395-397: The integration J8 that runs with r_{max} = 0.66 is impressive. I think I have not grasped the point about this only being possible because the grid is adaptive. The text says that J8 is a non-adaptive integration.

Lines 407-408. The differences between these simulations and CROCO are very large. I am not sure what to make of this as a test case.

Figure 6: I suppose that most readers will look at the non-adaptive simulation J8 and compare it with J6J8 to see how closely they compare. J6J8 vertical velocity has more small-scale structure than J8. I think that could be a cause for concern.

Section 4.3

Line 432: Again I wonder what \beta is for this configuration and whether that is relevant to the results.

Figures 8 and 9: There is not much discussion of these. I wonder whether it would be more helpful to present the fields in figure 9 for the smaller & larger tolerances in a single figure. If the fields are "directly" comparable that could be more informative.

Figure 10: Upper panel: Would it be interesting to present results below the surface where k^{-3} power spectrum might be expected?

Figure 10: Lower panel: It is hard to see the detail. Perhaps limiting the section to 100 degrees of longitude would enable the plot to be made larger and the detail to be more visible.

Conclusions

Lines 479-480: this conclusion may well be true (which would be exciting) but the test case is not as standardised as it could be.

Lines 488-489: I didn't pick this up whilst reading section 4.3. Could this be explained more explicitly there?

Lines 491-493: It is also a good test-bed for exploring some important questions about the strengths and weaknesses of the numerical formulations (see the following points).

Some questions about the basic formulations

A well-known difficulty with the TRisk formulation relates to the ratio of the number of degrees of freedom of its velocity and tracer fields. This affects the number of inertia-gravity waves supported by the discretisation. Some of the "additional" inertia-gravity waves can generate computational modes (see e.g. Cotter & Shipton https://doi.org/10.1016/j.jcp.2012.05.020). Is this a serious problem and how is it controlled in the author's model?

Another issue relates to the accuracy/consistency of some of the terms: Peixoto (2016) https://doi.org/10.1016/j.jcp.2015.12.058 discusses this issue and there are more recent papers following up on this point.

Level PE models using the vector invariant form of the momentum equations can suffer from the Hollingsworth instability whilst it seems that homogeneous layer (HLPE) models do not. I wonder whether your ILPEs are similarly not prone to this problem and whether the calculations in Bell et al (2016) **https://doi.org/10.1002/qj.2950** which provide an explanation of the difference between the layer and level models could be repeated using the ILPE equations.

As I understand it homogeneous layers are first order accurate approximations to a continuously stratified fluid. But if the calculations are "centred" they are nonetheless second order accurate. In traditional ocean models (like NEMO) it is not entirely clear whether tracer values represent cell means or spot values. In your layer model this is more explicit. Is your model second order (or more) accurate? Is there any attempt to calculate the hpg using higher order reconstructions of the fields similar to those of Shchepetkin & McWilliams (2003)?