1 Reviewer 1

Dear Mark, We thank for the comments and took them into account. We did not have time for a rigorous convergence test proposed in the review, but will return to this question in the nearest future. Instead, we did analysis to get an estimate of the convergence order, see below. Our answers are given in blue, and the original comments in black.

Thank you, it is helpful for the ocean model development community to see your success with a split explicit method. This paper takes great care in the detailed presentation of the numerical methods for the new split explicit solver in FESOM2.

What order of convergence do you expect for your time-stepping method? Please test the convergence rate in time. The test case would need to have both slow and fast dynamics to be a useful test, but you could test them first separately.

As concerns barotropic dynamics, the inclusion of dissipation in the forward-backward method makes the barotropic time stepping first order in time. However, dissipation is small, and one expects a higher order in practice. The baroclinic time step is second order except for viscous and diffusive terms, which are first order. The convergence order of the barotropic part we found from our convergence test, is around 1.6. The test was done on the soufflet channel test case with relaxations turned off. The simulation was run for 1600s with timesteps dt = 10,8,6,2,0.5s similar to the Ange Pacifique Ishimwe paper (2023).

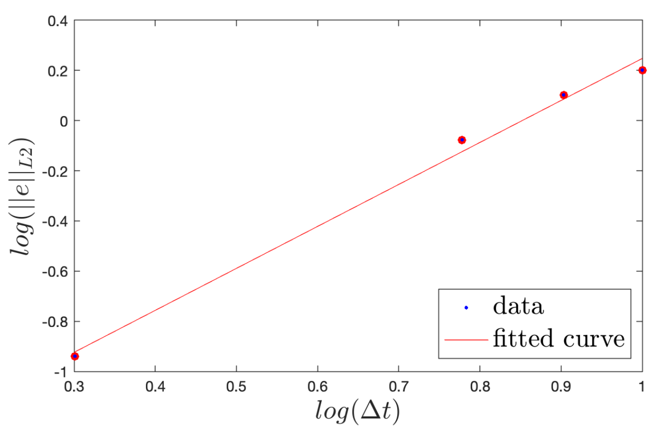

Figure 1: Order of convergence, $P \approx 1.6$

You could test JUST the barotropic part with the Kelvin Wave or Inertial-Gravity wave tests here in Bishnu et al. (2024). Just use redundant layers in the vertical. Then you can compare against an exact solution for the convergence test.

You could then use the baroclinic channel test case from Ilicak et al. (2012) and Petersen et al (2015), refine in time, and compute convergence by comparing to a short-timestep case. The duration of the simulation would need to be short (30 minutes, say) and sufficiently laminar that the results do not differ due to small differences in the turbulent flow. This test case definitely has baroclinic dynamics. It is not designed for barotropic dynamics, but should include some surface gravity waves as the SSH and barotropic eastward flow adjust to a geostrophic balance. Thanks to Ange Ishimwe (Université catholique de Louvain) for pointing out this test case during his talk at AGU Ocean Sciences. He was able to show second-order convergence for his split explicit scheme using this method in his recent paper published in December, in Ishimwe et al. (2023) figure 6. This would be a good paper to reference for comparison in the current work.

We did not do the suggested tests right now, but will try to perform them in future. As a part of revision, we carried out simple estimates of the convergence, as explained above. Also, to better illustrate the reduction in dissipation compared to the current SI scheme of FESOM, we added an additional surface gravity wave test to the paper. In this test, we measure the decay of surface gravity wave energy, which is much faster for the SI solver.

Smaller comments:

You subcycle the external mode to exactly one baroclinic time step, and do not use a filter, as explained in lines 30-34. I think this is important enough to put in the abstract, as it is a potential 2x speed-up for the barotropic stage compared to models that subcycle to n+2

Yes, added. This is indeed a very important advantage of the forward-backward dissipative time stepping proposed by Demange et al.

On equation 8, first line, I believe the sign of gH grad eta should be positive.

Yes, added

The surface flux W was dropped in equation 8. It would be good to just comment that W was added to the baroclinic (eqn 14), and not the barotropic mode, and your reasoning for that. Because you have a function at the top with the delta in equation 14, it makes sense that this is a baroclinic addition.

Made necessary changes

Fig 3 caption m2 needs superscript

Yes, added

I appreciate your description of the reasoning behind your choices. For example, the discussion of whether to add the bottom drag term to the barotropic dynamics at line 95, and the discussion on abandoning filtering at line 110. I have considered these exact issues and it is good to hear your thoughts on these.

Many thanks! Indeed, although the bottom drag and viscosity can be included in the barotropic time step, they require reconciliation with the baroclinic dynamics. Our test simulations are stable without these terms, but in realistic simulations we had to step back and add them for stability.

2 Reviewer 2

Dear Reviewer, many thanks for your feedback. We took them into account and made changes wherever necessary. Please find below in blue, our replies to your comments.

The authors describe the implementation of a split-explicit algorithm for the FESOM2 ocean model, using for time integration of the external mode the modified FB scheme or the modified AB3-AM4 scheme proposed by Demange et al. The implementation is described for the z-star and z-tilde vertical coordinates. Numerical integration tests are carried out for the z-star split-explicit model, and compared with the current semi-implicit algorithm of FESOM2. These tests show numerical solutions that are qualitatively close to those obtained with the semi-implicit version. They also show better scalability, particularly for highly parallelized workflows.

General comments :

The paper addresses the interesting question of how to construct a split-explicit algorithm based on the FESOM time-stepping scheme, which has the peculiarity of staggering the prognostic variables in time. Original questions arise about how to phase the time integration of 3D variables, momentum and tracers, and the time integration of the barotropic mode. However, it seems to me that the paper needs to be clarified on several points and requires major revisions.

Section 2 details the proposed split-explicit algorithm. Some clarifications seem necessary to me concerning (i) the choice made for the order of integration of the variables Ubar and eta with the modified FB scheme and the modified AB3-AM4 scheme and the implications of this choice, (ii) the initialisation of the variable Ubar for the barotropic integration, (iii) the correction of the 3d variables after completion of the barotropic integration.

We added necessary clarifications. (i) The modified FB scheme is first order with $\theta \neq 0$, and second order otherwise. However, since the dissipation is small, the order of convergence seen in our analysis is 1.6. The modified AB3-AM4 scheme should be also moved to the first order as concerns amplitudes because of the added dissipation, but remains higher-order with respect to phase errors. (ii) and (iii): We added additional schematic to the appendix for extra clarity. Briefly, Ubar is initialized by the value at the end of the previous time step (no re-initialization), and 3d velocities are made consistent with Ubar. The correction of 3d velocities is such that vertically integrated velocities agree with the barotropic velocities. The exact procedure depends on the ALE option.

Section 3 details the amplitude and phase errors of the modified FB scheme, the modified AB3-AM4 scheme and the semi-implicit FESOM scheme in the context of the SW equations. This analysis, although largely based on published results by Demange et al., could nicely illustrate the contrast between the errors produced by explicit schemes and the semi-implicit FESOM scheme. The presentation and the figure should however be improved. It seems to me that sections 2 and 3 should then be swapped, so that the SW-based results of the current section 3 introduce/motivate the implementation of the split-explicit

algorithm of current section 2.
We decided to keep the old structure, as it gives a general structure of the algorithm, which can work with different implementations of the barotropic time step. Section 3 then explains why particular selections are made.
Section 4 reports the results of numerical experiments. It is somewhat disturbing that the solutions for the idealised case show little difference between the split-explicit algorithm and the semi-implicit algorithm. This is probably due to the fact that the dynamics of this idealised case is essentially baroclinic and little affected by the representation of the external dynamics. It would have been desirable to consider an idealised case where barotropic dynamics play a more important role (for example, think of an idealised case of internal tide generation on a bathymetry by barotropic current oscillation). However, the results show a certain viability of the split-explicit algorithm and a gain in efficiency compared to the semi-implicit algorithm.

We added an additional surface gravity wave test to the paper. It illustrates more clearly that the APE decays much faster for the SI solver. We also conducted additional channel tests using a reduced bottom drag coefficient. The reduction leads to an increased barotropic component, and one starts to see that SE solver leads to a higher velocity in the deep ocean than the SI solver. We added it to the appendix Specific comments :

l37 : I suppose 'temporal interpolations' should be replaced by 'temporal integrations'?
We made necessary changes

l47 : A schematic describing the time-staggering of variables and the structure of the algorithm could be referred to here, upstream of the equations, to help the reader.
Added schematic to the appendix

l53 : Should the variable h in the expression for the pressure gradient be indexed with k?

Yes. Made necessary changes

l62 : Should the transport U be indexed with k?

Yes. Made necessary changes

l100 : It seems to me that the abbreviation SE isn't very well chosen. It would be clearer to use another which suggests that the basic time-stepping is the modified FB. For the same reason,

It is abbreviated as SE for the reason that we are proposing that as the default choice. If we go for FB, then the other one should be AB3AM4, which is a bit too long. One more option is D and SM, but then there is a dissonance with SI.

l101-102 : Here, with the SE scheme, the choice is made to integrate U_{bar} before eta. With the SESM scheme, the choice will be reversed. What are the reasons for these different choices? What are the implications? In the SE case, this choice is associated with a semi-implicit discretisation of the Coriolis term, which probably requires specific numerical processing ; if so, these should be explained.On the other hand, still in the SE case, this choice leads to the expression of the mean barotropic transport (12), which would be different with a reverse order of integration.

Added necessary clarifications to the text. In reality, everything is by historical reasons. We could start with η in (8) and use θ-representation on the gradient of elevation, to follow SM. The result would be similar. Semi-implicit Coriolis does not create problems, it is algebraic operation.

l103 : The quoted value of theta has been obtained under rather special conditions: it is the value that, for a constant stratification of N=10-2 s-1, a water column of H=4000 m and a splitting ratio of 20, allows the stabilisation of split-explicit algorithms whose stability is limited by large-scale barotropic waves. It is not clear that the algorithm proposed in this paper is constrained by these waves and that this value is relevant here. This is plausible, however, because the FESOM scheme is a variant of the FB scheme, which is used by Demange et al. in their study.

Made additional clarifications in the paper. The suggestion of Demange paper is just a parameter working in some situations. In the case of FESOM, it is a tunable parameter, but we see that the suggested value works well.

l110 : It might be clearer to change 'at the end of the barotropic step' into 'during the barotropic step'.

Agree. Made necessary changes

l114 : The reference to Shchepetkin and McWilliams on the line 112 could be moved to just after the 'traditional notation' line 114.

Made necessary changes

l120 : It is mentioned that it is not clear how to initialise Ubar (due, I suppose, to the time-staggering between U and Ubar), and it is noted that "We return to this topic below". But it's not clear to me where in the paper this is explained.

It is the paragraph starting at line 151 of the revised manuscript.

l125 : In this expression we see the correction by the mean barotropic transport <> of the 3D transports $U_k^{n+1/2}$, which is then used to transport tracers. There doesn't seem to be any place in your implementation for the other transport correction that classically appears in split-explicit algorithms based on synchronous schemes, i.e the correction by the barotropic transport of the 3D transports U_k^{n+1} (see for example p 384 in Shchepetkin and McWilliams). I'm a little embarrassed by the lack of an equivalent to this correction. Isn't there a cog missing to ensure consistency between the 3D and 2D integrations?

Some differences are due to asynchnonous time stepping. What is done on page 384 of Shchepetkin and McWilliams paper, is also done in our equation 12 and 13. Nothing is missing. We do not need to correct 3D U_k^{n+1}, as our time-stepping is staggered. We only need $U_k^{n+1/2}$ which follows the same correction as described by Shchepetkin and McWilliams on page 384. There is an issue of synchronization with the barotropic transport at $n+1/2$, as we discuss in line 151, where we retrim $U_k^{n+1/2}$ after advecting the tracers to the barotropic velocity at $n+1/2$. However, in practice this trimming was found to be unnecessary.

l156 : Should 'temporal interpolations' be replaced by 'temporal integrations'?

Made necessary changes

l157-159 : It seems to me that this sentence is confusing. What is done in this section is not really the analysis of the external mode in the context of split-explicit algorithms (which is done, for example, in Demange et al. section 4.2). Rather, it is the analysis of the modified FB, modified AB3-AM4 and semi-implicit FESOM schemes in the context of SW equations.

Made necessary changes for clarity. We are not analyzing the external mode. We analyze the prototype SW equations to learn about the performance of SE, SESM and SI.

l190 : The solutions of the continuous problem are e^{+ic} and e^{-ic}.

Made additional changes for clarity. The e^{-ic} solution corresponds to a wave propagating in different direction. It is a physical solution, but it will lead to the same result. We wanted to deal with one physical solution that corresponds to a wave propagating in negative direction.

l 214 : The sentence 'This CFL number ... mesh size.' is unnecessary here because the analysis is exact in space and does not depend on Delta x.

Made additional changes for clarity

fig. 1: For the two panels on the left, why not show the amplitude error of the explicit schemes over larger ranges on each of the two axes? This would show the damping for large values of c, and the loss of stability of the schemes. This would make it a little easier to compare the amplitude error of the explicit schemes with that of the semi-implicit scheme.

For the manuscript, in practice we are interested in a limited range of CFLs (for τ=600 s, c_p = 200 m/s and maximum $k = \pi/\Delta x$ for Δx = 10 km we will have maximum CFL=20). Within this range, SE and SESM show much smaller errors than SI, which can be further reduced if M is increased. If for fixed M we go to the stability limit, SE and SESM can also show high dissipation, but we generally will avoid going there. However, for your reference, please find the expanded plots below

For the two panels on the right, the phase error could be plotted as the ratio

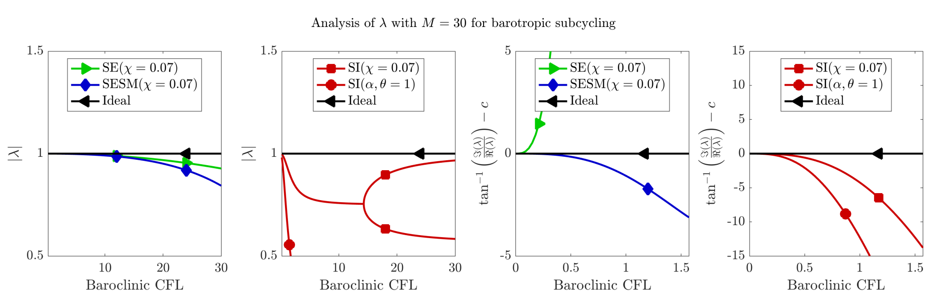

Figure 2: expanded plots

of the discrete phase velocity to the exact phase velocity. why not use the same horizontal axis as for the left panels, with c values varying between 0 and 30? This would make the figure easier to read.

We don't do till cfl 30 because it's already clear that even before that, we are approaching big errors in phase. Therefore, the solution for the semi-implicit case already needs to be highly damped. Anything beyond this wont provide us with any more meaningful information.

fig. 3 : the caption refers to diffusivities that are not traced.

Yes. Made necessary changes