Review of “The Applicability of the Integral Method with Variable Limit in Solving the Governing Equations for Temperature and Salinity in an Ocean Circulation Model”

This paper applies the IMVL method to the ocean tracer advection-diffusion equation, and implements it in POM. It is useful for ocean modelers to see new discretization methods tested in ocean settings. The authors provide sufficient details of the formulation for it to be reproduced by others. I think this paper is novel and interesting, and the topic is appropriate for publication in GMD.

My main criticism is that convergence tests against exact solutions are required for new methods of spatial discretization. This will verify the order of the method, and needs to be conducted for individual operators and for the full equation with time-stepping. See details in point 1. This will add a new section to the paper, and in my view is a requirement for publication.

Major items

1. A major item missing from this paper is convergence plots with rates of convergence for each operator and for the whole tracer equation together with time-stepping. This is required and basic for the introduction of all new numerical methods. Luckily there are numerous examples of such verification tests in the literature for advection-diffusion operators. See Bishnu et al 2024 for operator convergence tests and the method of manufactured solutions. This paper is for the shallow water equations, but includes examples in the supplementary material for advection-diffusion equations. The most elementary test for advection is a Gaussian distribution of tracer which advects back to the initial condition (the exact solution), on both a periodic plane and a sphere. See Section 2a in Skamarock and. Gassmann, 2011. Likewise, it is elementary to construct an exact solution for just a diffusion equation in time to measure the order of convergence of your method.
2. If the paper is too long with the inclusion of the new material recommended in point 1, some of th plots in the later sections could be moved to supplementary material or removed without loss of important content for the paper. For example, Fig. 6 and 7 could be merged and just one time slice chosen.
3. The authors appear to be careless in their writing, with simple mistakes such as wrong variables and missing spaces. See small comments below. I ask the authors to proofread their papers more carefully in the future.
4. I find equation 8 very non-intuitive. The IMLV idea seems to be based on this definition as a clever way to solve integral equations with specific, highly-accurate stencils. I can appreciate that, but it would be nice to have some physical intuition about equation 8. I read the previous two papers by Luo to understand the methods in this paper. Those also do not provide any explanation about the direction or reasoning behind these derivations, which was frustrating to me. For example, equation 8 integrates in x three times, but the outer two integrals are about the limits of the inner-most integral. So the units are [f x^3]. I suspect that there is a nice way to represent this spatially in a diagrammatic figure as a triple integral between epsilon_1 and epsilon_2, which would be nice to add to your paper. If the authors have some interpretation of eqn 8 that lends

some spatial or physical intuition to this method, please add it. If not, then explain that this is simply a mathematical formulation that allows us to solve the tracer equation with integrals rather than derivatives.

5. On equation (8), some readers are more used to the integral sign (integrand) dx notation with the dx at the end. In this case, that would put the dx dx_a dx_b at the end. You don't need to change it, as your notation is also used in the literature and follows the Luo papers. However, it would be good to comment at line 155 that (8) is a triple integral of f(x) in dx dx_a dx_b.
6. Under section 2, the authors need to provide context on the full use of the model for this method. Are the numerical methods for the solution of the momentum equation left unchanged? Are the layer thicknesses fixed (z-level), and what is the surface (rigid lid or free surface?). How do these equations interact with the new methods introduced in this paper? Why did the authors decide to only alter the tracer equations and not momentum?
7. Similarly, in the conclusion please discuss the potential for this method to be used for momentum and thickness/free surface equations. In the conclusion, please conjecture if the IMVL is appropriate for other types of models (B-Grid, unstructured, finite volume, sigma-coordinate, etc) or if there are any fundamental limitations that require that IMVL only be used on B-grid finite difference models.
8. Please comment in the conclusion if these methods may be used on a spherical domain in the current I-POM formulation. If alterations are needed, what are they?

Small items

Author's name Li appears in lower case "l" on author list.

3 a integral -> an integral (check throughout)

30 The Parallel Ocean Program (POP, Maltrud and McClean 2005) is a major finite difference ocean model missing from the list.

40 ICON (Korn 2019, Korn et al 2022) and MPAS-Ocean (Ringer et al 2913, Petersen et al 2019) are major finite volume codes missing from the review. They are unstructured grid models, like FVCOM.

eqn 1 first d/dT should be time, d/dt. vertical diffusion derivative shown at dT/dT should be dT/d\sigma.

eqn 4: same comment as eqn 1 but for S.

119 watch for spacing after commas: Similarly,the

160 scriptkis -> subscript k is (missing spaces)

295 Settings -> settings

342 should be: In Figures 6 and 7, the

Fig 6 and 7, captions should say what the rows are.

361 should be: Figures 8, 9, and 10 demonstrate that (spaces are missing)

Fig 10. Missing spaces in caption.

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