A 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*“
by F. Qiao et al.

This study introduces the use of a method referred to as IMVL (Integral Method with Variable Limit) for the discretization of horizontal advection as well as horizontal and vertical diffusion of active tracers within ocean models. Section 2 is devoted to presenting the temporal discretization of the temperature and salinity equations in the POM model (a standard Leapfrog scheme with a Robert-Asselin filter) and to introducing a spatial discretization method referred to as IMVL. In Section 3, IMVL is successively applied to the discretization of 1D horizontal advection, horizontal diffusion, and vertical diffusion. The proposed discretization method systematically requires the inversion of a tridiagonal matrix to estimate the corresponding terms. At the end of this section, the numerical stability of IMVL for linear 1D advection is analyzed considering a Leapfrog-type time discretization. The proposed discretization schemes have been implemented in the POM model. In section 5, they are evaluated for a set of idealized test cases, in comparison with the schemes usually employed in POM (although their exact nature is not clearly specified). Based on these numerical simulations, the authors conclude that IMVL provides superior results in terms of accuracy and stability compared to the standard discretization used in the POM model.

In addition to containing fundamental issues, which I highlight below, the level of clarity, rigor, and pedagogical quality of this manuscript is well below the standards required for publication in GMD. Significant portions of the manuscript are difficult (if not impossible) to follow because of a combination of undefined notations and erroneous equations. The study is limited by an insufficient consideration of the overall formulation of ocean models and by a vague mathematical presentation of the proposed method (there is an incalculable number of equations that are wrong). I now outline the arguments supporting my recommendation against the publication of this study.

Requirements before manuscript submission for publication   It may be worth reminding the authors that reviewers are not expected to correct typographical errors in the manuscript, particularly when such errors lead to equations that are nonsensical. In a manuscript with seven co-authors, one would reasonably expect such errors to have been identified before submission. This remark also applies to the presentation of the paper, in which many notations are introduced without any explanation. The two central equations of the study (Equations (1) and (3)) appear to contain significant errors concerning the variables with respect to which differentiation is carried out. Furthermore, the title and text of the manuscript discuss the *integral method with variable limits*, whereas the terminology used in the cited references is 'Integral Method with Variational Limit'. Considering that this discretization method is a niche approach used in only a very limited number of publications, it further contributes to the confusion. In addition to the fact

that the reference paper Luo et al. (2017) supposedly introducing IMVL is not available online as open access. I regret to say that Section 2.3 appears to be either a compilation of excerpts from unrelated studies or possibly generated by an inadequately trained AI. This contributes to making the paper extremely poor from a pedagogical standpoint.

I now present my scientific arguments:

- **Discrete local and global conservation :** PDE solvers, including but not limited to oceanic models, are formulated by discretizing the flux-divergence (conservative) form of the governing equations, thus ensuring discrete local and global conservation of the relevant physical quantities. In this study, instead of approximating fluxes at the interfaces, the operators are directly evaluated at the cell centers, as in a conventional finite difference scheme. Specifically, $\partial_x(DUT)$ is discretized at the cell centers rather than DUT at the interfaces. The same approach is applied to the horizontal diffusion operator, where $\partial_x(HA_H)\partial_x T + (HA_H)\partial_x^2 T$ is discretized at the cell centers, rather than discretizing $HA_H\partial_x T$ at the interfaces. The consequence of this is that, although the proposed numerical scheme may still approximate the correct continuous operator, the conservation properties of the continuous problem are lost at the discrete level. Such a lack of conservation is unacceptable for realistic simulations over long timescales.
- **Constancy preservation:** as emphasized by Shchepetkin and McWilliams (2005), an important constraint in the design of an ocean model is the constancy preservation for tracers (i.e., if the tracer is initialized as a spatially uniform field, it stays uniform regardless of the velocity field). Constancy preservation is essential to ensure that the compressibility or divergence of the flow will not change the scalar quantity that is advected. To satisfy this constraint, there is a consistency required at a discrete level between the continuity and the tracer equation. Setting $T \equiv 1$ in the temperature advection operator should provide an equation discretely equivalent to the continuity equation. First, the manuscript does not address how the continuity equation is discretized in POM. Second, I am skeptical that the discretization of the continuity equation has been appropriately adapted between O-POM and I-POM. In I-POM, a discretization scheme equivalent to (14) but for $e = D$ should be used to provide $\partial_x(Du)$ in the continuity equation. Constancy preservation is a fundamentally essential property for any ocean model.
- **Equivalence with compact schemes:** even if the scientific procedure used to obtain them is obscure, the discrete schemes you obtain appear to me to be identical to classical fourth-order compact schemes, which have been known for years. If you open Lele (1992) and consider his equation (2.1), applying the constraints to achieve fourth-order accuracy, you obtain (for $p = \partial_x e$):

$$\frac{1}{6}p_{i-1} + \frac{2}{3}p_i + \frac{1}{6}p_{i+1} = \frac{1}{2\Delta x}\left(e_{i+1} - e_{i-1}\right)$$

which is your equation (17). Then, considering equation (2.2) from Lele (1992) and applying the constraints to achieve fourth-order accuracy, you obtain (for $q = \partial_x^2 e$):

$$\frac{1}{12}q_{i-1} + \frac{10}{12}q_i + \frac{1}{12}q_{i+1} = \frac{1}{\Delta x^2}\left(e_{i+1} - 2e_i + e_{i-1}\right)$$

which is your equation (20) (Setting aside the fact that your equation is wrong, as it should be q rather than p, $\delta_2(e_i)$ is also wrong $2e_i \rightarrow -2e_i$). Therefore, this study does not demonstrate the benefit of the IMVL method compared to standard compact schemes. For a reason unknown to me, the authors do not consider the discretization of vertical advection. It is worth noting that compact schemes (sometimes interpreted as a reconstruction with parabolic splines) have been used for years in the context of ocean models (e.g., within the ROMS, CROCO, and NEMO models), see Shchepetkin (2015) (eqn 2.5), or

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Lemarié et al. (2015) (e.g. their App. B). Except that in this context, the compact schemes are used to approximate the interface fluxes rather than the cell-centered operators. Unless explained in a scientifically irrefutable manner, the relevance of the IMVL discretization is far from obvious. It would be interesting to elaborate on how this method differs from compact schemes that have been available for several decades (seminal work by Orszag and Israeli in 1974 and Lele (1992) has nearly 6000 citations).

- **Stability and multidimensional aspects:** Regarding the stability analysis presented in Section 3.4, it ignores the Asselin filter, which affects stability. Moreover, the result you obtain has already been given in Lemarié et al. (2015) (see their Table 2) in the more complete case that includes an Asselin filter. The present analysis yields a CFL number of $1/\sqrt{3} = 0.57735$, and when including an Asselin filter (with $\alpha = 0.05$) Lemarié et al. (2015) find a CFL number around 0.522. Moreover, the impact of multidimensional aspects on the stability of advection is not addressed. It would have been interesting to verify that applying your scheme in 2D independently in the x and y directions leads to a stable scheme when a Leapfrog method is used.

- **Practical implementation:** A main reason why compact schemes are not used in the horizontal direction in ocean models is the need to solve a tridiagonal problem in a direction where the grid is decomposed into MPI subdomains, and where the presence of coastlines (and the associated boundary conditions) complicates the structure of the matrix to be inverted. This issue is completely overlooked in this study, in addition to the fact that horizontal resolution is generally not constant in models, which would require introducing scaling factors to account for this in the proposed discretization scheme.

Detailed comments

- "To address limitations in traditional discretization methods for ocean numerical modeling", what are those limitations?
- Equations (1) and (3): the errors are obvious at first glance.
- Equation (1): R is not defined
- l. 118: U and V should not be interpreted as "flow rates", these quantities should be expressed as velocities in m s^{-1}; otherwise, Equations (1) and (3) would not be dimensionally consistent.
- l. 124: what is meant by "analytical treatment" ? The remainder of the paper relies on numerical methods; no analytical treatment is provided.
- l. 130: the classical Arakawa grids are defined in the horizontal plane. It is misleading to refer to the vertical coordinates as an 'Arakawa C-grid'; this is not meaningful. The staggering in the vertical is either referred to as Lorenz grid or Charney-Phillips grid.
- Fig. 1 : "POM brochure" ?
- Fig. 1 : $Z(k)$, $ZZ(K)$ and $Z(K+1)$ are not defined
- Fig. 1 : K_M is not defined
- The sentence "The first stage (...) for the horizontal advection and diffusion terms" is inconsistent with Equation (5), which also accounts for vertical advection...
- Equation (6): same error as in equation (1) and R is still not defined
- l. 140: "format" ? The paper alternates between the terms 'format' and 'equation'; it is unclear whether 'format' is standard terminology.
- l. 142-144: the sentence "The POM ocean model uses a weak filter (Asselin, 1972) to filter out the computational modes brought by the time-splitting algorithm" is highly imprecise. Firstly, a Leapfrog-type scheme exhibits only a single computational mode. The nonlinear terms introduce a coupling between the physical and computational modes which may amplify the computational mode. This is the reason why filtering is necessary; it has nothing to do with the time-splitting (it is not even clear which time-splitting method is being referred to here...). Finally, there is no justification for assuming

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that this filtering is necessarily 'weak'.

- Sec. 2.3: This section is intended to be central to the rest of the study, yet it is practically incomprehensible, as none of the introduced notations are defined. It is not even possible to understand what the authors' objective is, or what f, a_k, and b_k represent. The notations are ambiguous and nonstandard (what does $\int_{xx}$ mean?).
- Lemma 1: how does this lemma provide an approximation method? What is K ? What is R (certainly not the same as in Equation (1)) !
- The three equations between line 170 and line 175 are dimensionally inconsistent (Δx^3 and Δx^2 do not match the dimensions of the left-hand sides of the corresponding equations, i.e. $e(x)$, $p(x)$ and $u(x)$).
- The equation $u(x) = \ldots$ for the 2-point Lagrange interpolation is incorrect, it shoud be

$$u(x) = \frac{1}{\Delta x}\left(u_{i+1}\left(x - x_i + \frac{\Delta x}{2}\right) - u_i\left(x - x_i - \frac{\Delta x}{2}\right)\right)$$

such that $u(x_i + \Delta x/2) = u_{i+1}$ and $u(x_i - \Delta x/2) = u_i$ (which is consistent with figure 2) and the Δx^2 has nothing to do in this equation.
- Equation (12) is incorrect, Δx^2 does not have the same units as the other terms.
- Remark 1 is correct, but it would have been helpful to note that solving a tridiagonal system is required, which is challenging in the horizontal direction because of the MPI domain decomposition employed by all ocean models. For this reason, compact schemes are never applied horizontally in these models.
- First equation of section 3.2: what is the rationale for rewriting this operator from divergence form into non-divergence form? This kind of thing should be avoided at all costs, as it breaks the global conservation properties of the quantity being diffused.
- Equation (18): you define q but you never use it !
- Equation (20): again this equation is incorrect, the left hand side and the right hand side do not have the same units !
- Définition of α_0 following equation (24): d_z is not defined.
- l. 266: "the other parts continue to follow the POM model?", clarifying the numerical options of O-POM would have been valuable, because, as presented, it is not even clear which reference your schemes are being compared to.

References

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