General comment

This article presents a set of developments for spectral non-hydrostatic models using two-time-level semi-implicit semi-Lagrangian (SISL) time stepping. The proposed formulation combines a non-constant-coefficient semi-implicit scheme, improved free-slip boundary conditions, and a vertically conservative semi-Lagrangian method. These developments are shown to substantially improve numerical stability in idealised domains containing very steep orography. A further potential advantage is improved computational efficiency relative to the classic ICI (Iterative Centred Implicit) schemes, such as those of Bénard et al. (2010), which are commonly used in this class of models. The ideas are evaluated in a two-dimensional vertical-slice model, used both as a test bed and as an intermediate step towards a full three-dimensional non-hydrostatic spectral model.

The manuscript contains detailed mathematical derivations and an adequate set of numerical experiments demonstrating the stability benefits of the proposed approach. The methodology appears sound, and the work is potentially valuable for the further development of non-hydrostatic spectral models. In particular, the ability to remain stable over very steep slopes while preserving the original pressure-based vertical coordinate system is an important result as it is a known major limitation in spectral SI models which traditionally use a constant coefficient direct Helmholtz solver. However, I believe that the present manuscript would benefit from a more transparent presentation of the time stepping scheme, including a concise and general algorithmic form, in addition to the equation-by-equation derivation. Moreover, the claimed computational advantage relative to classic ICI schemes is not yet supported by a clear cost comparison or analysis, especially given the cost implications that spectral transforms at high non-hydrostatic resolution simulations have. For these reasons, although I consider the article very interesting and publishable, I find that it needs revision. The main issues that should be addressed are listed below.

Detailed comments

1. The manuscript should provide a clearer assessment of the computational cost of the proposed method. On page 2, it is stated that the cost per iteration is lower because only the non-constant coefficient terms are recalculated at each iteration. This raises the question of where the dominant cost lies. Does the saving mainly concern the evaluation of right-hand-side terms in grid-point space? In spectral-transform models, a major part of the cost at very high resolutions typically comes from transforming variables between spectral and grid-point space. As described in the paper, each iteration appears to require such transformations. Therefore, I suggest that the authors quantify how many spectral transforms are required for k iterations in the classic ICI scheme and in the proposed non-constant coefficient scheme. If the number of transforms is the same, then the saving obtained by splitting constant and non-constant coefficient terms may be rather limited in a three-dimensional model. It would also be useful to state how much

more expensive one recalculation is relative to the basic direct solve, and to quantify the additional cost of the GCR solver.

2. The system of equations (22), dX/dt = F, is discretized in (23). The latter is written in a way in which the symbol F resembles a nonlinear residual formulation, which is a standard technique in SI time stepping for spectral models. However, after examining the equations more carefully and doing some algebra, I was convinced that in both (22) and (23), F is the same thing which I believe it agrees with the intention of the author: it plays the role of the full right-hand side, and that the linear terms are removed from and inserted back into F. I suggest that the author uses the commonly used expansion of F by Bénard et al., suitably adapted to reflect the split into constant and non-constant coefficient linear terms, i.e. F = N + L_C + L_N, where N is the nonlinear residual. Inserting this into (23a) gives the right-hand side:

$$\frac{1}{2}\left[N^{(+)} + (1-\beta)\,L_C^{(+)} + \beta L_C^0 + \beta L_N^0\right]_D + \frac{1}{2}\left[N^0 + \beta L_C^{(+)} + (1-\beta)\,L_C^0 + L_N^+\right]$$

which can be rearranged to:

$$\frac{1}{2}\left[N_D^{(+)} + N^0\right] + \frac{1}{2}\left[(1-\beta)\,L_{C,D}^{(+)} + \beta L_{C,D}^0 + \beta L_C^{(+)} + (1-\beta)\,L_C^0\right] + \frac{1}{2}\left[L_{N,D}^0 + L_N^+\right]$$

For beta = 1, we recover the centred second-order SISL formulation, with SETTLS extrapolation applied to the slowly varying non-linear residual and constant-coefficient linear terms while the non-constant-coefficient linear terms are kept in centred implicit form. I think the article would benefit from explaining this point, rather than leaving the reader to deduce it.

3. The treatment of second-order decentring requires further clarification. Although Eq. (23) appears related to the pseudo-second-order decentring of Simmons and Temperton, it is not obvious from the formulae presented that 2nd order temporal accuracy is retained when this decentring is combined with SETTLS and with the split L=L_C+L_N. I suggest that the author provides the exact reference where this formulation is analysed and 2nd order is demonstrated or to derive the scheme from the centred 2nd order SI–SETTLS formulation and demonstrate its formal order of accuracy. If this cannot be done and decentering does in fact formally reduce the order of accuracy, this would also be acceptable provided that the magnitude of the decentering factor remains small and the implications are stated. I also suggest introducing the decentering parameter as (1 + eps)/2, which would make the connection with a small perturbation from the centred scheme clearer. Finally, it would be useful to report whether the scheme has been tested without decentering, and whether any instability or numerical noise was found in that case.

4. The presentation of the time-stepping algorithm should be made more concise so that readers can understand the "big picture" without having to work through low level implementation details. The current equation-by-equation description is detailed, but it is difficult to see the overall structure of the method from it. The verbal description on page 9, from line 225 onwards, would benefit from a high-level algorithmic summary of the time step, explicitly identifying the main stages of the calculation. Introducing the

iteration number in this description, as in Bénard et al. (QJRMS, 2011), would help the reader distinguish what is evaluated once, what is updated at each iteration, and where the constant- and non-constant-coefficient terms enter the solve.

5. The manuscript would benefit from a clearer discussion of when the different stabilising options are expected to be required. A major weakness of constant-coefficient SI approaches is their loss of stability once the terrain slope exceeds a certain limit. It should be possible to indicate, at least approximately, the global resolution or slopes in which additional iterations and the GCR options become necessary. This would help the reader assess whether the method is likely to remain competitive with other grid-point, non-spectral, non-hydrostatic approaches in realistic three-dimensional applications.
6. The two-dimensional experiments are useful and appropriate for developing and analysing the numerical method. A short discussion of the additional challenges expected in the global implementation, if any anticipated, would also help the conclusions.

Minor comments

1. On page 2, please add the recent non-hydrostatic IFS reference by Vivoda et al. (QJRMS, 2026), https://doi.org/10.1002/qj.70144. This paper describes the latest state of the non-hydrostatic IFS, includes relevant discussion of ICI, and also addresses second-order decentering in the context of ICI, which is also relevant to major comment 3.
2. I suggest using "spectral space" consistently throughout the paper, rather than alternating between "wave space" and "spectral space".
3. Please check and correct the second paragraph on page 12. It states that "Figures 3c–d are the same as Figs 3a–b except that the initial condition is given by …". However, the corresponding Figure 3 caption on page 40 states that they differ in temporal and spatial resolution rather than in the initial condition.
4. The vertically conservative semi-Lagrangian approach is a useful addition. However, the manuscript should clarify whether the splitting used in this formulation introduces an additional splitting error and whether the 2nd order accuracy is preserved.
5. In E10, page 30, the equation K=U^{-1}TU looks like an eigenvalue decomposition rather than Singular Value Decomposition (SVD). Please check this and clarify.
6. In the Figure 5 caption on page 42, the term SATEM is used in some figures instead of SAETM.