

[Reply on RC2]

General comment

5 This article presents a set of developments for spectral non-hydrostatic models using two-timelevel 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
10 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 nonhydrostatic 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 nonhydrostatic spectral models. In particular, the ability to remain stable over very
15 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,
20 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.

[Response]

25 We are very grateful to Referee 2 for the positive assessment of our study and the many useful comments, which have helped us improve the paper. We respond to the specific comments below.

[Note]

30 During the revision, we changed $\mathcal{Q}$ to $\mathcal{P}$ in Eq. (27e) to make it consistent with Eq. (C24). We found the corresponding bug at one location in the source code of the 2D x-z model and corrected the bug. The results remained almost unchanged after the bug was corrected. Clearly visible differences occur only in Figs. 3c-d for case S1, where the small oscillations present both before and after the correction make the differences more noticeable. We have updated Figs. 1-5 for the test cases with mountains. Figures 6-9 have not been updated because the bug does not affect the test cases without mountains where the no-recalculation method is used. Despite these corrections, the overall results and conclusions of the paper remain unchanged.

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.

[Response]

Thank you for the important suggestion. The cost of spectral transforms is important because it is a major part of the computational cost in very high-resolution three-dimensional spectral models. In the non-constant coefficient semi-implicit scheme, 13, 19, and $19 + 6N_{\text{iter}}$ latitudinal transforms of multilevel variables are required per timestep when using the no-recalculation method, the single-recalculation method, and the preconditioned GCR method, respectively, where N_{iter} is the number of iterations. In the ICI scheme, $13 + 13N_{\text{iter}}$ latitudinal transforms are required. Thus, the number of spectral transforms for one recalculation or iteration in the non-constant coefficient semi-implicit scheme (6 transforms) is smaller than that in the ICI scheme (13 transforms). We have added the following detailed assessment of the spectral-transform cost in Sect. 2.7:

"2.7. Computational cost of spectral transforms

In spectral models, computational costs for spectral transforms and associated MPI communication become large, especially at high resolutions. To estimate the computational cost of spectral transforms per timestep in a three-dimensional model, the number of latitudinal spectral transforms, such as Legendre transforms, is counted as in Ritchie et al. (1995). ..."

65

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 B  nard 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:

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

which can be rearranged to:

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

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.

[Response]

Thank you for the useful suggestion. As the referee indicated, F is the same in both Eqs. (22) and (23) and plays the role of the full right-hand side. The linear terms are removed from and inserted back into F, with the parameter β applied to L_C , as in Ritchie et al. (1995). We have added the following sentence to Sect. 2.4.

"The parameter β applied to L_C is similar to that used in the second-order three-time-level semi-implicit semi-Lagrangian scheme of Ritchie et al. (1995)."

Following the suggestion, we have added the following sentences and equations to Sect. 2.4:

"Eq. (23a) can be rewritten as follows:

$$\frac{X^+ - X_D^0}{\Delta t} = \frac{1}{2} [N_D^{(+)} + N^0] + \frac{1}{2} [L_{C,D}^0 + L_C^+] + \frac{1}{2} [L_{N,D}^0 + L_N^+] + \frac{\xi}{2} [-(L_{C,D}^{(+)} + L_C^0) + (L_{C,D}^0 + L_C^+)]. \quad (23a')$$

$$N \equiv F - L_C - L_N \quad (23c)$$

$$\xi \equiv \beta - 1 \quad (23d)$$

When $\xi = 0$, Eq. (23a') becomes the centered second-order semi-implicit semi-Lagrangian formulation with SETTLS extrapolation applied to the nonlinear residual term N at the departure point. In this formulation, the linear terms include not only L_C but also L_N ."

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 $(1 + \text{eps})/2$, 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.

[Response]

Thank you for the useful comment. Following the suggestion, we have introduced the decentering parameter $\xi \equiv \beta - 1$ as described in our response to major comment 2. We have added the following sentences and an equation to Sect. 2.4 to provide the references where formulations using the same temporal weighting as our constant-coefficient linear terms are shown to retain second-order temporal accuracy:

"When no distinction is made between the departure and arrival points in order to examine temporal accuracy, Eq. (23a') can be rewritten using Eq. (23b) as follows:

$$\frac{X^+ - X^0}{\Delta t} = \frac{1}{2} [N^{(+)} + N^0] + \frac{1}{2} [L_N^0 + L_N^+] + \frac{1}{2} [(1 + \xi)L_C^+ + (1 - 2\xi)L_C^0 + \xi L_C^-]. \quad (23e)$$

The temporal weighting of the constant-coefficient linear terms in Eq. (23e) is the same as that used in the second-order decentering formulations presented in Eqs. (12) and (14) of Rivest et al. (1994) and Eq. (38) of Simmons and Temperton (1997)."

We have also added the following sentences to Sect. 2.4 to describe how the combination of SETTLS and second-order decentering enhances numerical stability:

"Although the terms $F^{(+)} + \beta(-L_C^{(+)} + L_C^0) + (-L_N^{(+)} + L_N^0)$ at the departure point in Eq. (23a) contain time-extrapolated values, the combination of these terms is interpolative in time. When $F = L_C + L_N$, that is, $N = 0$, these terms become $(1 - \xi)L_C^0 + \xi L_C^- + L_N^0$. Here, $(1 - \xi)L_C^0 + \xi L_C^-$ represents a temporal interpolation between L_C^0 and L_C^- for $0 \leq \xi \leq 1$; $\xi = \beta - 1 = 0.2$ is used here. Thus, the combination of SETTLS and second-order decentering with $\beta = \xi + 1$, which is a little larger than 1.0, can avoid time extrapolation, a potential source of instability. The use of $\beta > 1$ can also enhance the stabilizing effect of the semi-implicit scheme by increasing the coefficient of L_C^+ . The results in Sect. 3.2 show that numerical stability is reduced when β is changed from 1.2 to 1.0. The factor β is applied only to the constant coefficient linear terms because its application to the non-constant coefficient linear terms does not improve numerical stability in the test cases in this paper."

We have performed additional experiments without decentering and have added the following sentences to Sect. 3.2:

"When second-order decentering is not applied, that is, when β is reduced from 1.2 to 1.0, all six cases, A1–A4, D1, and D2, become unstable. For numerical stability in the constant-coefficient semi-implicit scheme, the constant-coefficient linear terms should be larger than the corresponding nonlinear terms in the original prognostic equations. Therefore, in Eq. (27) for $\mathbf{v}^+$, the coefficient $\beta\bar{T}$ in the constant-coefficient linear term should be larger than the

coefficient T of the corresponding nonlinear term. In Eq. (35) for $\hat{d}^+$, the coefficient $\beta/\bar{T}$ in the constant-coefficient linear term should also be larger than $1/T$. From these conditions, $\bar{T}/\beta < T < \beta\bar{T}$ should be satisfied for the constant-coefficient semi-implicit scheme to provide sufficient stability. Reducing β from 1.2 to 1.0 narrows the temperature range satisfying this condition. Therefore, additional experiments were performed with $\beta = 1.0$, $\bar{T} = 300 \times 1.2$ K, and $\bar{T} = 160/1.2$ K, which retains the original bounds of this temperature range. With this setting, cases A4, D1, and D2 become stable, but cases A1–A3 remain unstable. This result indicates that the above condition alone is not sufficient for numerical stability and that the effects of $\beta > 1.0$ in Eq. (29) for $\ln T^+$, in Eq. (30) for Q^+ , and in Eq. (31) for $\ln \pi_s^+$ also contribute to numerical stability."

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. (QJRM, 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.

[Response]

Thank you for the important suggestion. Following the suggestion, we have added a high-level algorithmic summary of the timestep to Sect. 2.6. The summary also describes what is evaluated once, what is updated at each iteration, and where the constant- and non-constant-coefficient terms appear. The iteration number is introduced for the preconditioned GCR method in Appendix E3. Because this iterative procedure is complex, we keep the explanation in Appendix E3 rather than including it in this high-level summary. Sect. 2.6 is as follows:

"2.6 Time-integration procedure

For time integration, the following seven-step procedure is performed once during each timestep. Step 3 computes the first approximate future values by solving the simultaneous equations with the constant-coefficient linear terms in spectral space. Step 4 updates these values by a single recalculation or by iterations. In each recalculation or iteration, the non-constant-coefficient linear terms are recalculated, and the simultaneous equations with the constant-coefficient linear terms are solved as in Step 3.

Step 1: Explicit time integration and semi-Lagrangian advection in grid space

...

Step 2: Spectral transforms from grid space to spectral space

...

Step 3: Computation of the first approximate future values by solving constant-coefficient linear simultaneous equations in spectral space

...

Step 4: Computation of the final future values in spectral space by recalculating non-constant coefficient linear terms in grid space

...

Step 5: Spectral transforms from spectral space to grid space

...

Step 6: Optional spectral truncation of $\hat{d}^+$ in spectral space

...

Step 7: Computation of future values and application of Rayleigh damping to w^+ in grid space

..."

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.

[Response]

Thank you for the useful suggestion. Following the suggestion, we performed additional experiments to examine which methods are required for different terrain slopes. All cases except cases A4, D2, and S1 remain stable even when the no-recalculation method is used. Notably, even cases A3 and D1 with a 26.5° slope remain stable with the no-recalculation method. Cases A4 and D2 with a 45° slope are stable with the single-recalculation method, and S1 with a 63.4° slope is stable with the preconditioned GCR method with a maximum of four iterations. We have added the following description to Sect. 3.2:

"When the no-recalculation method is used instead of the single-recalculation method, cases A4 and D2, which have an average mountain slope of 45°, become unstable, but cases A1–A3, and D1 remain stable. It is notable that even cases A3 and D1, which have an average mountain slope of 26.5°, remain stable with the no-recalculation method. When $\hat{d}$ is used instead of $\hat{\hat{d}}$ in the no-recalculation method, cases A3, A4, D1, and D2 become unstable, indicating that the use of $\hat{d}$ improves the numerical stability of the no-recalculation method."

200 We have also revised the following description in Sect. 4 (Conclusions):

Feature 1: The non-constant coefficient semi-implicit scheme is numerically stable and computationally efficient. A single recalculation of non-constant coefficient linear terms (i.e., the single-recalculation method; Appendix E2) is usually sufficient to solve the equations associated with the non-constant coefficient semi-implicit scheme with the help of Features 2–5, even for cases A4 and D2, which have a steep mountain with an average slope of 45° (Sect. 3.2).
205 In case S1, which has an extremely steep mountain with an average slope of 63.4° (Sect. 3.3), several iterations using the preconditioned GCR method (Appendix E3) are necessary to solve the semi-implicit equations, where only non-constant coefficient linear terms are recalculated in each iteration. In the test cases without mountains (Sects. 3.5–3.7) and even in cases A3 and D1, which have a mountain with an average slope of 26.5° (Sect. 3.2), the no-recalculation method (Appendix E1) is sufficient to solve the semi-implicit equations.

210

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.

215

[Response]

Thank you for the useful suggestion. We have added the following sentences to Sect. 4 (Conclusions):

"Although we expect that these improvements can be incorporated into the global model without substantial difficulties, their performance, particularly that of the improved surface boundary treatment, should be evaluated in
220 the three-dimensional global model. Further improvements will be made if necessary."

We have also revised the discussion of the implementation in the global model to avoid making overly definitive statements.

225 Minor comments

1. On page 2, please add the recent non-hydrostatic IFS reference by Vivoda et al. (QJRM, 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.

230

[Response]

Thank you for the useful reference. We have added the paper by Vivoda et al. (2026) as a reference for the non-hydrostatic IFS and for the ICI scheme in Sect. 1 (Introduction), as follows. We have confirmed that the second-order decentering in Vivoda et al. (2026) is different from that in our paper.

235 "The prognostic equations used in the nonhydrostatic option are essentially the same as those in the ALADIN-NH
(Non-Hydrostatic version of Aire Limitée Adaptation Dynamique Développement InterNational) nonhydrostatic
limited-area spectral model (Bubnová et al., 1995; Bénard et al., 2010) and those in the nonhydrostatic version of the
IFS (Integrated Forecasting System) global spectral model (Wedi and Smolarkiewicz, 2009; Dueben et al., 2020;
Wedi et al., 2020; Vivoda et al., 2026). However, there are some differences in time discretization. In the ALADIN-
240 NH and nonhydrostatic IFS, an iterative-centered implicit (ICI) scheme (Bénard et al., 2005; Bénard et al., 2010;
Vivoda et al., 2026) is used to improve model stability, where whole terms are recalculated at each iteration."

2. I suggest using "spectral space" consistently throughout the paper, rather than alternating between "wave space" and
245 "spectral space".

[Response]

Thank you for the useful suggestion. We have replaced "wave space" with "spectral space" throughout the paper.

250

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.

255 [Response]

Thank you for pointing out the inconsistency. The description in the second paragraph on page 12 was correct, but the
Figure 3 caption was incorrect. We have corrected the caption as follows:

"(a) Simulated vertical velocities (contour interval: 2.0 m s^{-1}) and (b) simulated wind vectors. (c) and (d) are the
same as (a) and (b), respectively, except that the initial condition for w is given by $w = \mathbf{v} \cdot \nabla \Phi / g$ instead of $w = 0$."

260

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.

265

[Response]

Thank you for the useful suggestion. We have added the following paragraphs to Appendix B about the second-order accuracy and the additional splitting error associated with the vertically conservative semi-Lagrangian scheme:

"In the vertically conservative semi-Lagrangian scheme, the three-dimensional model is regarded as a vertical stack of two-dimensional horizontal models, and horizontal advection is calculated separately in each layer. Conservative vertical remapping is performed before and after horizontal advection to maintain consistency with the layers defined by the η coordinate. This vertical remapping corresponds to vertical transport of mass. Mass and other variables are advected in a consistent manner.

The effects of horizontal divergence and vertical remapping are calculated with equal weighting before and after horizontal advection. This treatment is consistent with the semi-Lagrangian time discretization, where the tendencies at the departure and arrival points are taken into account with equal weighting. This symmetric treatment is designed to preserve second-order temporal accuracy along the semi-Lagrangian trajectory. Although the splitting between horizontal and vertical transport introduces an additional splitting error, the weak dependence on the timestep size in the test cases examined here suggests that the splitting error is small."

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.

[Response]

Thank you for finding the mistake. The equation represents an eigenvalue decomposition. We have corrected "singular value decomposition (SVD)" to "eigenvalue decomposition (EVD)".

6. In the Figure 5 caption on page 42, the term SATEM is used in some figures instead of SAETM.

[Response]

Thank you for finding the typo in the figures. We have corrected "SATEM" to "SAETM".