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Geoscientific Model Development Discussions

Interactive comment on "A mimetic, semi-implicit, forward-in-time, finite volume shallow water model: comparison of hexagonal–icosahedral and cubed sphere grids" *by* J. Thuburn et al.

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We are grateful to Dr Gaßmann for her comments on the manuscript.

We agree that the extension of the proposed approach to three dimensions is far from easy (though this is probably true of all suitable numerical methods). Dr Gaßmann highlights two particularly interesting and challenging aspects.

1. We are aware of Dr Gaßmann's very interesting work on the Hollingsworth instability (reference below for others following the discussion). The instability can affect schemes, like ours, that are based on the vector invariant form of the equations. In

particular, our scheme has much in common with the TRSK scheme that manifests the instability presented in her paper. However, as she says, unfortunately the instability does not arise in the shallow water case; at the same time, a complete and convincing analysis on paper remains elusive. Thus, the issue is hard to explore except through numerical experimentation with a three-dimensional dynamical core.

We are currently working on extending our approach to 3D, and we will certainly be looking out for signs of this instability. It is possible that ensuring accurate PV advection will eliminate or minimize the problem. Alternatively, it is possible that the mimetic finite element approach (which has much in common with the scheme we describe here, but improved accuracy) will eliminate or minimize the problem. However, if the instability does arise we will certainly explore the modifications suggested by Gaßmann (2013).

2. The second topic raised is extremely difficult and complex. At least conceptually, we can distinguish between (a) dissipation mechanisms needed to keep a model stable, and (b) dissipation mechanisms intended to represent real physical processes on subgrid scales. (In practice it may not be so easy to separate these two.) Regarding (a), our use of a linearly-energy-conserving spatial discretization combined with a Crank-Nicolson-based time scheme gives us stability without the need for additional ad hoc dissipation.

Issue (b) can be further dissected into the related questions of (i) what is an appropriate form of the subgrid model, and (ii) whether and how to conserve total energy.

On (i), the results of Kent et al. indicate that upwind advection of (potential) vorticity gives us least a partial implicit sugbrid model in the vortex-dominated enstrophy cascade regime, and it is plausible that this will remain true in 3D; this is the approach we have used. Smagorinsky-type schemes are widely used and accepted for LES of 3D turbulence such as that in the planetary boundary layer. Although they are also used on larger scales in atmospheric modelling, their justification is less clear; they might not be sufficiently scale-selective, and they might not capture processes such as gravity wave energy cascade or frontal collapse, which might be relevant in the $k^{-5/3}$ energy spectrum range. We think the choice of suitable subgrid models in these regimes is an open research question. Nevertheless, some form of eddy momentum flux tensor τ is a plausible approach. If τ is to depend on the rate of strain tensor then the referee makes the valid point that, on the hexagonal C-grid (and other C-grids in general), although the divergence and vorticity have natural, simple and compact, approximations in terms of the velocity, the other components of the rate of strain tensor do not; then the most suitable form for use in estimating τ is not obvious.

On (ii), as the referee says, some climate modellers consider it highly desirable to be able to close the energy budget. To do this, we must fully include all energy source and sink terms in our governing equations (the $-\mathbf{v} \cdot \nabla\tau$ and $-\tau \cdot \cdot \nabla\mathbf{v}$ terms mentioned by the referee). We must also either use fully energy conserving numerics (which may come at a price, e.g. in terms of advective dispersion errors), or keep track of all energy dissipated by the numerics (which is difficult in practice) and restore it somehow. This is complicated by the fact that numerical dissipation (and also dissipation by subgrid models) is often excessive and at too large scale, raising the question of in what form to restore the lost energy (internal or kinetic, rotational or divergent ...?) and at what scales.

Moreover, energy is a nonlinear function of the prognostic variables usually used, and so has unresolved as well as resolved contributions. The equations mentioned in the referee's comment ($\rho\partial_t K = \ldots - \mathbf{v} \cdot \nabla\tau$, $\rho\partial_t E_{int} = \ldots - \tau \cdot \cdot \nabla\mathbf{v}$) assume that sources and sinks of the unresolved contribution are instantaneously in balance; a more complete treatment would carry a prognostic equation for the unresolved contribution with sources and sinks that need not balance instantaneously.

The referee suggests that we might mention possible 3D development of the scheme in the manuscript, particularly the Smagorinsky diffusion term. However, as is clear from the above (and we have barely scratched the surface!), we could not possibly do justice to such a vast and complex topic. We therefore prefer to leave such discussion

to another place where it can be treated more completely.

Gassmann, A., 2013: A global hexagonal C-grid non-hydrostatic dynamical core (ICON-IAP) designed for energetic consistency. Quart. J. Roy. Meteorol. Soc., 139, 152-175.

Interactive comment on Geosci. Model Dev. Discuss., 6, 6867, 2013.