Geosci. Model Dev. Discuss., 6, C2758–C2763, 2014
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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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Received and published: 4 March 2014

We thank the reviewer for their positive and constructive comments on the manuscript. Below is a point-by-point response to the comments.

1. *Special handling on the cube edges...* We use no special handling near the cube edges, or near the pentagons on the hexagonal-icosahedral grid. (We will emphasize this in the revised manuscipt, at the end of section 2.) The scheme presented is essentially an unstructured grid scheme; part of the purpose of the work is to investigate the intrinsic performance of the scheme on different grids with no special handling of

particular grid regions.

2. *The necessary globally-implicit operation would then be very inefficient in parallel...* It has been widely feared that the solution of global elliptic problems needed for implicit time integration would be inefficient and scale poorly on massively parallel computers, and this has led most recent model development efforts to opt for a HEVI (horizontally explicit vertically implicit) approach. However, there is growing evidence (e.g., Heikes et al. 2013, Müller and Scheichl 2014) that elliptic problems can be solved efficiently and scalably in parallel. We will mention this in the revised manuscript.

3. *Are there any plans for some sort of monotonicity or positivity enforcement?* There exist limiters, guaranteeing local boundedness of advected tracer mixing ratio, that could be used with the advection scheme presented in section 5 and that would be straightforward to implement (e.g. Thuburn 1996 and improvements by Miura 2013). We have not used them here simply because there was no evidence that they were necessary in these tests. Nevertheless, we will add these two references to section 5.7.

...is there any explicit artificial dissipation,...? The only dissipation mechanism in the model is the inherent dissipation in the upwind advection scheme; in section 2 we have referred the reader to work by Kent and colleagues for a discussion of the effects of this dissipation on energy and potential enstrophy cascades. Apart from this, no other dissipation mechanism (either explicit or inherent in the numerics) is needed to maintain stability or to control dispersion errors or other numerical sources of noise. This will be emphasized in section 2 of the revised manuscript.

Minor comments

4. *Many figures are hard to read...* Apologies for this. This is partly due to the change in format between the preprint and the online version. Where possible we will improve the figures in the revised manuscript.

5. *(Specification of grid resolution)* This is largely a matter of what you're accustomed

to. Some of us find the “C90” notation for the cubed sphere non-intuitive! The important point is that Table 1 allows the reader to quickly covert between number of cells, degrees of freedom, and grid spacing.

6. *Why is the KE term treated as a backwards term?* Apart from the advective fluxes, the scheme is essentially a Crank-Nicolson scheme. Thus, the KE term is treated in a centred time-averaged way (not really backwards). In particular, the scheme is actually quite different in detail from the Lin-Rood (1997) scheme, though it shares the important property of computing the nonlinear Coriolis term as a vorticity or potential vorticity flux.

7. *Why build the stencils iteratively?* One could write down the stencil explicitly by hand for particular cases. However, for higher degree polynomial fits (requiring large stencils) near pentagons or cube corners, there are many cases that would need to be considered, and the process would become tedious (and error prone!) The iterative scheme is very general and easily automated, and appears to produce reasonable stencils in all cases tested. Note that the stencils are generated once at the start of a run, so cost is not an issue.

8. *Have you done the test of a spatially-uniform tracer field in a non-divergent flow?* Yes, we have done the test. An initially uniform tracer density remains uniform in a non-divergent flow, and an initially uniform tracer mixing ratio remains uniform in any flow, on both the primal and dual grids. In the revised manuscript (section 5.8) we will mention that these two design requirements are verified in tests.

9. *Tables 6 and 7, ...convergence rate...* Qualitative rough estimates of the convergence rate are already given in the text for both cases. The resolutions tested are not high enough to obtain clean estimates of the asymptotic convergence rate.

10. *Could a couple of different resolution be shown?* Table 6 already shows how various error norms depend on resolution. Note that grid imprinting errors are nothing but numerical truncation errors whose pattern happens to reflect the underlying grid

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structure.

11. *Angle of the flow in test case 2.* The default grid orientation (as noted in section 1.2) is used; i.e., pentagons at the poles on the hexagonal grid, and cube corners at latitude $\pm\pi/4$ on the cubed sphere, and the flow parallel to the equator.

12. *... measures of the error ... may have little meaning, ...* All the evidence (from our models and others in the literature) implies that the solution *does* converge with finer space and time resolution, (and to the same solution for different models), implying that the error measures *are* meaningful.

13. *... is the maintenance of sharp filaments and gradients truly a consequence of mimetic properties?* First and foremost, these properties come from the advection scheme; as you note, such behaviour is typical of finite volume schemes. (The mimetic properties do help to ensure that when we insert the PV fluxes in the momentum equation, the PV we diagnose does indeed evolve in a way consistent with those fluxes.)

14. *Does the mimetic scheme conserve energy better than (say) ENDGame?* The results for available energy and potential enstrophy are very similar in ENDGame (for interest, see figure 1 of this reply). The inherent dissipation due to the semi-Lagrangian advection scheme used in ENDGame is expected to be similar in its magnitude and scale dependence to that due to the finite volume advection scheme: both are (quasi-) third-order upwind schemes. This will be mentioned in the revised manuscript.

15. *Use same contour interval in all panels* We will replot using the same contour interval in all panels. Yes, at this resolution full height fields look identical in all three models; that is why we have shown error fields. We will look at whether overplotting the position of the mountain makes the plots clearer.

16. *I would be interested in seeing the Rossby-Haurwitz wave,...* In fact the mimetic properties do not really help the model to hang on to a steady (or steadily propagating) but dynamically unstable solution such as the Rossby-Haurwitz wave. The perturbation

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introduced by grid imprinting is enough to trigger the dynamical instability. A similar thing happens in the Galewsky test case, so we decided against showing results from the Rossby-Haurwitz wave. In the revised manuscript we will expand the discussion of the Galewsky test (section 6.7) and mention that similar grid-triggering of instability happens for the Rossby-Haurwitz wave.

17. *... initializing with a fully-backward method ... yields a better result...* We must be careful with our interpretation here. The test case as those authors defined it does include large amplitude gravity waves (which are deterministic and reproducible at high enough resolution) generated by the initial condition. The correct result therefore includes those gravity waves. However, here we want to know whether there is any spurious generation of gravity waves by the numerics. To see this, we must modify the test case to damp the initial condition gravity waves, which would otherwise mask any numerically generated ones. We do agree that it is important to understand the degree of initial balance or imbalance, even in such idealized test cases; we have seen the consequences of imbalance both in this test and in the isolated mountain test case!

Heikes, R. P.; Randall, D. A.; Konor, C. S., 2013: Optimized Icosahedral Grids: Performance of Finite-Difference Operators and Multigrid Solver. Mon. Weather Rev., 141, Pages: 4450-4469

Miura, H., 2013: An upwind-biased conservative transport scheme for multistage temporal integrations on spherical icosahedral grids. Mon. Weather Rev., 141, 4049–4068.

Müller, E. H. and Scheichl, R., 2014: Massively parallel solvers for elliptic PDEs in Numerical Weather- and Climate Prediction. Submitted to Quart. J. Roy. Meteorol. Soc. Available from: `http://arxiv.org/abs/1307.2036`

Thuburn, J. 1996: Multidimensional flux-limited advection schemes. J. Comput. Phys., 123, 74–83.

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

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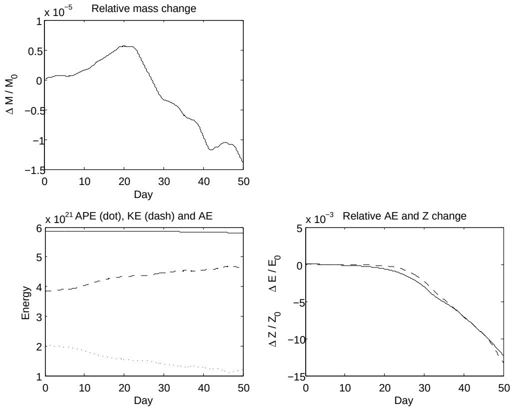

Fig. 1. As in Figure 8 of the paper, but for the ENDGame shallow water model at 320x160 resolution.

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