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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.

P.A.U. Ullrich (Referee)

paullrich@ucdavis.edu

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The manuscript "A mimetic, semi-implicit, forward-in-time finite volume shallow water model: comparison of hexagonal-icosahedral and cubed sphere grids" by J. Thuburn, C.J. Cotter and T. Dubos presents an exciting and novel approach for modeling the shallow water equations on the sphere while conserving the highly desirable mimetic properties of the underlying method. The mathematics that underlies the derivation of this method is presented clearly and concisely, and the usual suite of Williamson et al. shallow water tests have been leveraged to validate the correctness of the method. Consequently, I am happy to recommend this article for publication, although I had three minor points for the authors' consideration.

1) More of an aside than an issue that needs addressing: I noticed that an earlier referee comment already pointed out my concern with the scalability of the numerical method, considering the horizontal implicitness of the method. Although Heikes et al. (2013) did show excellent scalability at high resolutions, perfect scalability was only shown for approximately 4096 elements per processor. On a petascale machine with order 100k processors, this would mean that the grid resolution would need to be less than 1 kilometer to maintain scalability of the method. Even higher resolutions would be needed for exascale systems. That being said, these calculations do not take into account physics, which is presumably embarrassingly parallel, and may be sufficient to recover the scalability at coarser grid resolutions. Further, such arguments over scalability make certain assumptions about the underlying hardware paradigm that are unlikely to hold going forward.

2) The authors observe an instability in the method for Courant numbers between 0.75 and 1 (section 6.1). Is there any suggestion as to what may be causing this instability?

3) Although the cosine bell test has been used for testing since the dawn of modern advection scheme development, I think it can be fairly universally acknowledged that it is largely insufficient for really pushing these methods. The divergent flow test of Nair and Lauritzen (2010) presents an alternative that is more effective at pushing the scheme. However, I would personally really like the authors to run the test of Ullrich et al. (2013) "Some considerations for high-order 'incremental-remap'-based transport schemes: edges, reconstructions, and area integration," already cited in the manuscript. That is, simply run the barotropic instability test with an initial tracer field that matches the zonal velocity profile. For the cubed sphere grid, I can almost guarantee that the scheme described in this paper will generate an obnoxious pattern driven largely by grid-imprinting over the duration of the integration. If it does not, that is an exciting result on its own. I further suspect that under this test the icosahedral mesh will produce a much better looking solution, so it will be yet another argument for the

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benefits of the hexagonal-icosahedral mesh when using first- and second-order methods.

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