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Interactive comment on "A global finite-element shallow-water model supporting continuous and discontinuous elements" *by* P. A. Ullrich

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This paper introduces the techniques of flux-reconstruction, previously mostly only applied in aeronautical applications to compressible flow, to the rotating shallow-water equations with a view towards their use in numerical weather prediction.

I think that this application is novel, and the paper communicates these ideas to the NWP community very well and is generally very well-written, and should be published.

I have a few minor comments which the authors should take a look at:

p5143: "This reduced communication requirement implies better overall scalability on large-scale parallel systems." I worry about remarks like this because once they appear in the literature they tend to get cited out of context. Since a scaling factor is applied

to the number of communications, the scaling itself won't be affected, but the limit to strong scaling might be. However, DG will hit the wall in strong scaling almost as soon as CG does.

p5145: I don't think that Thuburn (2008) actually says anywhere that the non-conservative form is better for conserving potential enstrophy and angular momentum. Potential enstrophy can be exactly conserved if one specifically uses the vector-invariant form of the equations together with a careful treatment of the discretisation, but I don't think this would necessarily be a generic property of non-conservative formulations. Angular momentum can only be exactly preserved if the grid has rotational symmetries.

p5148: please provide more clues as to why this discretisation produces identical results to nodal spectral element for CG spaces i.e. with direct stiffness summation.

p5150: "A stabilization operator is necessary for finite element methods to avoid dispersive errors associated with spectral ringing." Again, I'm worried about this being cited out of context. In the compatible finite element setting we have produced finite element discretisations that are stabilised purely by the stable vorticity advection operator, with no need for an explicit stabilization operator. We don't need any viscosity or hyperviscosity. It is also the case that DG methods are stabilised purely from the upwinding. Please have a go at narrowing down the language here.

p5151: The viscosity operator here feels a bit like you are mixing your drinks in that the non-diffusive part doesn't rely on any test functions, but the viscosity part does. I think this just needs a bit more careful explanation to explain how you are obtaining the operator by dividing by the (diagonal) mass matrix. Is this viscosity operator actually a stable discretisation of the Laplacian i.e. does it have spurious eigenvalues? The flux reconstruction people normally have to resort to LDG/CDG-style operators for this.

Please provide a bit more detail on how you obtained the timestep sizes for your numerical calculations, I think this is important as it is the main assessment we can make of computational cost here. It would also be good if you could make some remarks about the relative computational time for one timestep between CG and DG for the same polynomial degree (I know this is tricky since implementation details vary). It's also good to remind the reader how CG and DG DOFs scale with number of elements as a function of polynomial degree (unless I missed this somewhere?).

Interactive comment on Geosci. Model Dev. Discuss., 7, 5141, 2014.