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Interactive comment on "A high-order conservative collocation scheme and its application to global shallow water equations" *by* C. Chen et al.

C. Chen et al.

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Thank you for carefully reading the paper. Your constructive comments are extremely helpful for use to improve the manuscript. We have fully taken your comments into account and accordingly modified the paper. Below, please find our point-to-point responses to your comments.

This paper presents a flux reconstruction method for discontinuous elements applied to 2D shallow water equations. While this research would be interesting for the modeling community, I have many comments on the quality of the presentation (given by this paper). The main objection is that the paper does not provide enough explanation on

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spatial discretizations. Also, there are comments on the model and on English usage.

I recommend it for publication in GMD after major revisions.

Comments:

1. In the introduction, the paper claims using the flux reconstruction method as described in Huynh, 2007 and modified in Xiao, 2013. The FR technique is to redistribute flux to all element's nodes, but Eqns. (6), (7) and further only update end points. If the paper uses one of the schemes derived in Xiao, 2013, then it should be stated. Still, in Xiao, 2013 (http://arxiv.org/pdf/1206.4406.pdf) it seems that interior points are modified by the FR process. In summary, much more should be given on the scheme used. Also, describe what (if anything) is different from previously published works.

1. Define the unknowns as the local degrees of freedom, which are the nodal values at the solution points within each cell;
2. Build a high-order spatial reconstruction for flux function which is a consistent approximation to the solution over each cell and satisfies the continuity conditions using the Riemann solver at cell boundaries;
3. Evaluate the derivatives of flux function at the solution points to get the time evolution equations to update the solutions.

The key is step 2, and different constrained conditions can used for flux reconstruction, which results in different numerical schemes.

In Huynh 2007, FR is formulated by two correction functions which assure the continuity at the two cell boundaries and collocate with the so-called primary Lagrange reconstruction at their zero-points. So, the existing nodal type schemes can be recast under the FR framework with different correction functions.

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In Xiao et al. 2013, a more general FR framework was proposed by introducing the multi-moment constrained conditions including nodal values, first-order derivatives and even second-order derivatives to determine the flux reconstruction.

In this study, we present a more straightforward simpler approach to derive the FR formulation using the collocation method, which has not been discussed in either Huynh (2007) or Xiao et al (2013). The resulting scheme, Gauss–Legendre-point based conservative collocation (GLPCC) scheme, is new and has not be reported by anyone else to our knowledge.

We have improved the description of the proposed scheme in revised manuscript.***Basically, the flux reconstruction (FR) includes the following steps:***

1. ***Define the unknowns as the local degrees of freedom, which are the nodal values at the solution points within each cell;***
2. ***Build a high-order spatial reconstruction for flux function which is a consistent approximation to the solution over each cell and satisfies the continuity conditions using the Riemann solver at cell boundaries;***
3. ***Evaluate the derivatives of flux function at the solution points to get the time evolution equations to update the solutions.***

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2. Eqn. (12) and conservation: It seems that conservation is only achieved for uniform meshes, because Eqn. (12) depends on the element length. So, in general, the scheme does not conserve mass if non-uniform grids are used? Also, cube-sphere meshes are only quasi-regular. Is scheme conservative on a sphere? It would be useful to provide a plot for mass conservation from one of the shallow water tests, similar to Figure 14.

We have rewritten Eq.(12) in the revised manuscript to avoid possible misleading. **The proposed scheme is conservative even for non-uniform grids. The total mass within each control volume, i.e.** “$\Delta x_i \overline{q}_i'' isexactlyconserved, whichisinfactcomputedbyafinitevolumeformulationwiththenumericalfluxescalculatedatthe$ $integratedaverage(VIA)itself, theproductofVIAandthevolumeisconserved.$

We have rewritten Eq.(12) in the revised manuscript to avoid possible misleading.

3. Eqn. (19) and spectral analysis: How was eqn. (19) derived and what are the coefficients? Also, it seems that the spectral problem is formulated globally because neighbor values are included. I found more details in paper Xiao, 2013

(http://arxiv.org/pdf/1206.4406.pdf) but at least if notations are used, they should be clarified.

We have revised this part of in revised manuscript with more details. The spectral analysis adopted here follows the procedure in Huynh (2007).

4. Super convergence: The authors mention super convergence a few times. I believe they mean that their method is an h-p method with corresponding convergence properties. It would be desirable to clarify terminology. Also, the authors state (p. 4253) that "The Fourier analysis and numerical tests show that the present scheme has the super convergence property same as the DG method." First, they did not show this numerically because there are no tests for the p refinement. Second, which DG method are they referring to? Third, how exactly the Fourier analysis can be used for exponential convergence?

The discussion on the super convergence follows the context in Huynh (2007) where a scheme using K solution points is said to be super-convergent if its order of accuracy is higher than K. As shown in Huynh (2007) that the nodal DG scheme has a convergence rate of 2K-1, we demonstrate that the present three-point GLPCC scheme has 5th order convergence rate by the Fourier analysis used in Huynh(2007). Here, the DG scheme is referred to the nodal type DG defined in Huynh (2007) which uses the Radau polynomial as the correction function. A relevant theoretical work can be found in Guo et al. (W.Guo, X. Zhong and J. Qiu, J. Comput. Phys. Vol. 235, 458–485 (2013)).

5. "The parameter a in Eq. (24) is determined by the contravariant velocity component and the water depth, which are exactly same on two adjacent patches." A continuous velocity field in contravariant coordinates on an edge has two components, and one of them, corresponding to a basis vector perpendicular to the edge, is the same (up to the sign) for adjacent elements.

It has been clarified in the revised manuscript.

6. Figure 15 needs labels ((a), (b) ...) and captions for them.

Thank you for suggestion. We added the figure labels in the revised manuscript.

7. p. 4263: “The expression of metric tensor can be found in Chen and Xiao (2008)”. I believe (correct this if I am wrong) that this paper largely uses transformations and formulations from earlier papers (Nair, R. D., S. J. Thomas and R. D. Loft, 2005: A discontinuous Galerkin transport scheme on the cubed sphere. Monthly Weather Review,Vol. 133, pp 814-828) and (Nair, R. D., S. J. Thomas and R. D. Loft, 2005: A discontinuous Galerkin global shallow water model. Monthly Weather Review, Vol. 133, pp 876-888). The reference should be corrected then.

Thank you for your comments. We revised the sentence in the manuscript.

8. The paper does not cover diffusive properties of the proposed method and possible applications of artificial diffusion. I believe this is a valid point for discussion. Shallow water models are often considered as preliminary studies for 3D models. In 3D models, diffusion mechanisms cannot be ignored.

Based on the Riemann solver at cell interfaces, the proposed scheme is essentially an upwind type method. As a result, the inherent numerical dissipation is included and stabilized the numerical solutions. We did not use any extra artificial viscosity in the shallow water model for the numerical tests presented in the paper. We agree with you that additional dissipation or limiter projection might be necessary in other cases in 3D. Because of the algorithmic similarity, the existing works on high-order limiting projection and artificial dissipation devised for DG or spectral element methods should be applicable to GLPCC without substantial difficulty. Some comments have been included in the revised version.

Comments on English: 1. More attention should be given to articles. 2. p. 4262: Revise “... coordinate system **(**$\xi;\eta) are shown in Fig.$", “$...the governing equations is rewritten..$"$3.p.4264$:

Revise“...wesolving...”4.p.4267 : Revise“Theconservationerrorsoftotalenergyandenstrophyareinterestforatmosphericm
Revise“Twokindsofsetupofthistestareusuallycheckedinliteratures..”

We have made a thorough linguistic check. All your comments are reflected in the revised manuscript. Thank you.

Interactive comment on Geosci. Model Dev. Discuss., 7, 4251, 2014.