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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.

Anonymous Referee #2

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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 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 de-

scribed 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.

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.

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 al least if notations are used, they should be clarified.

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?

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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.

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

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.

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.

Comments on English:

1. More attention should be given to articles.

2. p. 4262: Revise "... coordinate system (ξ, η) are shown in Fig.", "...the governing equations is rewritten..."

3. p. 4264: Revise "...we solving ..."

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4. p. 4267: Revise "The conservation errors of total energy and enstrophy are interest for atmospheric modelling..."

5. p. 4268: Revise "Two kinds of setup of this test are usually checked in literatures..."

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