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Interactive comment on "Development and evaluation of a hydrostatic dynamical core using the spectral element/discontinuous Galerkin methods" *by* S.-J. Choi and F. X. Giraldo

Anonymous Referee #3

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This manuscript describes the development of two hydrostatic atmospheric dynamical cores based on high-order continuous Galerkin (CG) and discontinuous Galerkin (DG) methods, using a single modeling framework. Authors have made a close comparison between CG and DG formulations using several test cases. Nodal formulation of CG and DG methods (combined with inexact integration) results in efficient spatial discretization, and minimizes the difference between these two methods. In such a case, the only difference is how the fluxes are handled at the element edges. DG scheme uses upwind fluxes via approximate Riemann solver such as the Rusanov (or LLF) flux formula, which resolves the inter-element discontinuity, however, the CG scheme relies on simple averaging at the element edges to maintain the C^0 continuity through an operation known as DSS. The manuscript is reasonably well written and clear.

There are many hydrostatic dynamical cores based on CG method, including the one developed by the second author. Since the two methods (CG and DG) as implemented by the authors share many properties, one would naturally expect the results are very close, as demonstrated through the baroclinic experiments in the manuscript. If there exists some difference between the solutions produced by CG and DG, one might infer that is due to the treatment of fluxes at the element edges. In other words, there are no interesting results provided in the manuscript, and conclusions are obvious. Authors do however boast their DG model can simulate Held-Suarez test, is that anything particularly challenging or exciting to warrant publication in GMD? Any run-of-the-mill dynamical core can do this job. Recent research shows that the DG methods are capable of handling far more complex tasks in atmospheric modeling. In my opinion, this manuscript lacks novelty and requires a major revision. A way to improve the manuscript is to perform additional challenging experiments, which may serve as a reference for the model developers interested in high-order methods. The authors should address the following suggestions.

Major Comments:

(1) The spectral-element (CG) based dynamical cores employing cubed-sphere geometry are in use over a decade, however, the authors chose only one reference (GR04) to indicate earlier research in this field. The introduction should be extended to include the following research work:

Thomas S and Loft R (2005) The NCAR spectral element climate dynamical core: Semi-implicit Eulerian formulation. J Sci Comput 25: 307–322.

Fournier A, Taylor M and Tribbia J (2004). The Spectral Element Atmosphere Model (SEAM): High-resolution parallel comptation and localized resolution of regional dynamics. Mon Wea Rev 132: 726–748.

GMDD

7, C1262–C1265, 2014

Interactive Comment

Wang H, Tribbia J. J, Baer F, Fournier A and Taylor MA (2007) A spectral element version of CAM2. Mon Wea Rev 135: 3825–3840.

(2) **Additional New Tests:**

For the Held-Suarez test case, the results are zonally and temporally averaged after running the model for 1200 days at a low resolution. One downside of this experiment is that it can mask the fine details of the dynamics. Since the only difference between CG and DG implementation is the flux operations (i.e., DSS vs. Rusanov flux as indicated on Page 4136, line 5-10), it is important to show how this subtle difference influences the wave propagation in the vertical X-Z plane, and also in the presence of orography. Jablonowski et al. (2008) proposed a set of benchmark tests with varying complexity including the baroclinic instability test. Authors should do the following experiments proposed by Jablonowski et al., detailed in the NCAR Tech report on ASP test suite 2008.

(2a) The propagation of gravity waves in the spherical domain with and without earth's rotation (Expt. 6-1-0 and 6-2-0). The simulated results should be produced at 96 h (see Fig.24 and 25 in the tech. report) using DG and CG versions with same configuration as recommended in the test.

(2b) The mountain induced Rossby wave test, where an idealized mountain triggers the evolution of a Rossby wave train over the course of several days. This test should be performed with both CG and DG variants and results should be compared at day 25. See the Fig.19 and 20 for the details.

(3) **More technical details should be provided.**

(3a) On page 4128 (Eq.20), a third-order FD formula for the vertical differencing is provided, but no details are given how this is used on or near the top/bottom boundaries.

(3b) Give the explicit form of the $\lambda_{\max}$, provide more details on how it is used in the discretization. Did you consider entire vertical column to find $\lambda_{\max}$ in σ-direction?

GMDD

7, C1262–C1265, 2014

(3c) Details about the diffusion and dissipative mechanisms used for the dynamical cores are missing in the manuscript. The diffusion operators for DG and CG work quite differently, authors should discuss it with proper references. Did you consider using hyper-viscosity?

(3d) The explicit SSP-RK3 is used as the time integrator, please mention what is the time step size in seconds, for each experiment with DG and CG.

(3e) CAM-SE uses CG method with a relatively low-order polynomial $N = 3$, employing 4×4 GLL quadrature points on each element. They found it is cost effective for practical applications. At least one simulation should be performed with $N = 3$, you may replace Fig.7 by new low-order results with a comparable horizontal resolution. This will give an opportunity compare your results with CAM-SE simulations.

(4) **Conclusions:** Authors blame Rusanov flux for the diffusive DG solution. However, for various shallow-water (SW) and non-hydrostatic models based on DG method combined with the Rusanov numerical flux works really well, and produces high quality results even better than that of the CG variant. The 3D hydrostatic model may be viewed as a stacked up SW models and vertically coupled by hydrostatic balance, therefore, it is not clear that the Rusanov flux is the culprit. It will be useful for the DG modelers if the authors could substantiate their observation about the Rusanov flux, by replacing it with other flux recipe such as the HLL, HLLC or Roe etc., in their DG model. This is an option left to the authors and not a requirement, though.

Interactive comment on Geosci. Model Dev. Discuss., 7, 4119, 2014.