Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-238-RC1, 2016

Geoscientific Model Development Discussions
Open Access

Interactive comment on "Total energy and potential enstrophy conserving schemes for the shallow water equations using Hamiltonian methods: Derivation and Properties (Part 1)" *by* Christopher Eldred and David Randall

Anonymous Referee #1

Received and published: 31 October 2016

Total energy and potential enstrophy conserving schemes for the shallow water equations using Hamiltonian methods: Derivation and properties (Part I) by C. Eldred and D Randall gmd-2016-238

General comments

The manuscript presents two new numerical methods for the rotating shallow water equations: one is an extension of the Arakawa and Lamb (1981) C-grid scheme to arbitrary non-orthogonal polygonal grids; the other is an extension of the Salmon (2007) scheme to arbitrary orthogonal polygonal grids. Their common feature is that they are derived using Hamiltonian methods along with ideas from Discrete Exterior Calculus. This is an impressive piece of work and represents a significant step forward for such approaches. I also particularly enjoyed the Introduction, including Figure 1, which clearly and concisely summarizes the complex and often competing set of requirements that are considered desirable for numerical methods aimed (ultimately) at weather and climate modeling. The work will certainly be publishable after some revision

My main concern is that, especially towards the end, the presentation becomes less clear. Most of my comments are therefore aimed at improving the presentation so that readers can appreciate the substance of the paper.

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Specific comments

My most important point is that throughout sections 3, 4, 5 (and Appendices B, C, D) the notation is awkward and potentially confusing. In the papers cited the notation U, for example (no subscript) is typically used to mean the vector comprising all the velocity degrees of freedom while the notation U_e means one component of that vector, that is, the velocity at edge e. In the present manuscript u_e, for example, seems to have both meanings. This then makes many of the formulas difficult to understand, particularly for readers not intimately familiar with the cited previous work. It looks like it will be a rather tedious job to fix this up, but I think is it is essential for the clarity (and correctness) of the paper.

As the authors note, the principal novelty (and difficulty) of the proposed C-grid scheme is the specification of a suitable Q operator (section 4) given by some coefficients \alpha satisfying (60). (60) is eventually solved by a least squares method which (lines 441-2) ‘has a unique, exact solution’. Could the authors please clarify whether (60) itself is solved exactly? If it is not then the scheme does not quite have the desired properties (though it may do so to a very good approximation). If, on the other hand,

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Discussion paper

(60) is solved exactly, despite being overdetermined, then this suggests that some solvability condition must be satisfied, as Thuburn et al 2009 found in constructing their W operator. (It might even be possible then to write down the solution fo the \alpha's without resorting to a numerical solution?) Either way, there should be something interesting to say.

Line 9, line 677 (perhaps elsewhere). Perhaps make it clear that here 'orthogonal' means that there is a dual grid whose edges are orthogonal to those of the primal grid.

Section 2.2. What is \Omega? Are some boundary conditions assumed in writing (7)?

P6. Define \nabla^T; P7 define \nabla^\perp

Eq (26). There is the potential for some confusion because (26) is not quite the same expression as that below (2).

Line 204-241. Readers not familiar with differential forms and their discrete counterparts might be thrown by this new terminology. Perhaps explain briefly that 1-forms correspond to edge integrals and 2-forms to cell integrals; that should be enough for most readers to follow.

Eqs (37) (38). Explain the subscripts I and H.

Line 268. It is not clear until later that \phi is an interpolation operator.

Eq (50). Is there a factor 1/2 missing?

Section 3.4. From what we understand about the Hollingsworth instability, a Charney-Phillips vertical grid should reduced the instability compared to a Lorenz grid (rather than avoid it), but it could still be an issue at very high vertical resolution. Also, on a square grid one can rigorously derive a reformulation of the KE so as to rule out the Hollingsworth instability. On general grids this does not seem possible (e.g. Gassmann 2013), one can only minimize the non-cancellation that leads to the instabilty. In practice this seems to be sufficient at least for the published results, but we should not take it for granted that the problem is solved.

Eq (84). Is K defined?

The definition of FD in (101) is not quite the same as that below (87), which could cause confusion.

Line 679. Surely any Voronoi tessellation / Delaunay triangulation gives an orthogonal primal-dual pair?

Appendices B and C. It would be helpful to have a brief (one or two sentence) interpretation of what each operator does.

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Minor points, typos, etc

Line 37. typically -> typical

Line 40. Perhaps change ‘realistic’ to ‘practical’?

Line 56. of a -> of

Line 65. posses -> possess

Line 111. \hat{k} is the unit vertical vector? I think standard notation would use bold font.

Line 132. posses -> possesses

Line 228. Use a consistent font for Z.

Line 252. I’m not sure Thuburn and Woollings (2005) is the correct reference here.

Line 253. Format for equation cross-references.

Line 281 and elsewhere. Reference to THESIS.

Line 289. pseudo-energy

Line 291. Coriolis parameter (f is not a force!)

Line 308. This is due.

Line 361. that the proposed

Line 378. which appendix?

Eq (72). Are le and de defined?

Eq (80). Is there a factor 1/2 missing?

Eq (81). Should the first term have a minus sign?

Line 591. Subscript \delta should be \mu ?

Line 655 and elsewhere. Eldred and Randall 20016a,b

Line 701. the this

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