Point by point reply to all reviers' comments on "Non-orthogonal version of the arbitrary polygonal C-grid and a new diamond grid" GMD-2013-145

by Hilary Weller

7th March 2014

Responses to Reviewer 2

Comment The above calls for an analysis of the accuracy of the asymmetric and symmetric H operators in isolation. This would help interpreting the numerical results obtained for the whole shallow-water scheme, especially the differences observed when only the operator H differs between two experiments.

Reply I realised that previously I was using the symmetric H on grids without centroidal duals. On these grids the symmetric H is not guaranteed to be even first-order accurate. And indeed I found it to be zeroth order accurate. I have therefore modified all of the grids to have centroidal duals when using the symmetric H. I now compare centroidal grids and grids with centroidal duals when using the asymmetric H and I compare the symmetric and asymmetric H when using grids with centroidal duals. There are benefits to using a centroidal grid but, if you are using a grid with a centroidal dual, it doesn't make much difference if you use the symmetric or asymmetric H.

Comment Ideally one would also like to have some indications of the truncation error of both H for nearly-orthogonal quadrilateral meshes, for instance a planar mesh made of identical parallelograms. This would shed light on the role of the off-diagonal terms of H.

Reply Based on the new results on meshes with centroidal duals, the off-diagonal terms don't have much of an effect.

Comment 3.2.1 The test flow (solid-body rotation) is solenoidal, it may be useful to do a similar convergence study (Fig. 5) with an irrotational flow, e.g. ulat = cos(φ), ulon = 0.

Reply I did this but found that the results were pretty much the same as for the solenoidal flow. So the results are not included.

Comment It should be mentioned that the symmetric H does not become diagonal when used on an orthogonal triangular/hexagonal C-grid but does become diagonal when used with an orthogonal Cartesian C-grid.

Reply Done

Comment Is (14-15) Perot's reconstruction?

Reply No, it is slightly more complicated than Perot's reconstruction. I have not compared this reconstruction and Perot's reconstruction but I think they have some of the same asymptotic properties. I would rather not go into this as I think it would add too much length.

Comment Section 4 : I understand the linearized (unnumbered) equations are integrated over one time step using (21-22) and the eigenvalues μ of this operator are computed, stability being indicated by $|\mu| \leq 1$. It may be useful to express the same information in terms of log $|\mu|$ or log $|\mu|$/dt especially for $|\mu|$. In fact since the time stepping is slightly dissipative it would be nice to see directly the (real part of) eigenvalues of the linearized operator without the time integration (e.g. eigen-values of L where (du/dt, dh/dt) = L.(u, h)). Although in practice what matters is the stability of the whole spatial + temporal scheme it would be interesting to know whether L really has only eigenvalues on the left of the imaginary axis (despite the lack of energy conservation) or maybe a few have a slightly positive real part.

Reply I have changed the parameters in the SWEs in order to better separate the eigen values and I also now plot the real and imaginary parts rather than the amplitude and frequency. This is quite illuminating. The stability is not dependent on the H operator but it is dependent on the centroidality of the dual.

Comment What happens to this linear stability analysis if the TRiSK perp is replaced by the consistent perp (Eq. 16) ?

Reply: I have tried replacing the TRiSK perp by a consistent perp in the shallow water model and it runs stably but gives much worse numerical results due to the lack of stationary geostrophic modes and lack of energy conservation.

The consistent perp does not conserve energy. So the eigen values will no longer have unit magnitude.

I would rather not go into this in this paper.

Comment Section 6 : Since much of the discussion is about the relative merits of symmetric vs asymmetric H it would be useful to have a direct assessment of their accuracy, e.g. something similar to Fig. 5 with H instead of perp. Error patterns would be interesting as well since, as mentioned before, the symmetric and asymmetric H should yield very similar errors in the center of a cubed sphere face due to the mesh being nearly Cartesian and orthogonal there.

Reply Convergence study of the 2 versions of H done. The error patterns are not very illuminating and have not been included.

Comment Fig.8 - 'normalized energy change' : Please specify how energy change is normalized ? I would suggest normalizing by initial available energy, i.e. kinetic+potential minus the potential energy of the flow at rest with the same mass (averaged height) (see e.g. Ringler et al. (2011) Eqs 16-18) Same remark for enstrophy. Also is kinetic energy defined by (10) ? With a symmetric H the kinetic contribution to the conserved energy is Ue Ve (Thuburn & Cotter, 2012, eq. 2.27).

Reply The normalisation is just the standard Williamson et al 1992 normalisation. I would rather stick with this as it has been so widely used.Text clarified. Yes, the KE is defined by 10. Clarified. Yes, the KE contribution to an edge is UeVe. But in 10 I partition this KE into cells differently which makes no difference to the total. Again, clarified.

Comment Lack of second-order convergence with the symmetric H on the HR grids seem consistent with its non-diagonal character. Conversely second-order convergence on the the cubed and diamond grids suggests a superconvergence of the symmetric and asymmetric H (formally first-order accurate only).

Reply Also, the symmetric H is closer to diagonal on the grids of quadrilaterals. Yes, superconvergence is in effect.

Responses to Almut Gassmann

Comment Grids: Regarding the hexagonal grid, we did a lot of investigations concerning suitable grid optimizations. Finally, we found that it is the "smoothness" of the grid which improves the results the most. Smoothness means that the lengths of arcs and areas of grid cells do not change rapidly. The HR grid had quite bad properties in that regard compared to spring dynamics (perhaps the Centroidal Voronoi grid would behave similarly as spring dynamics). We had also a so-called C-grid optimization, which kept orthogonality and replaced great circle arc by small circle arcs (e.g. latitude circle arcs are small circle arcs) in order to put the edge point in the center between cell midpoints and the center between vertices. But this optimization was not helpful at all and was discarded. Therefore - as you show, I can imagine that orthogonality is not the whole story and could be relaxed.

Reply My own investigations with orthogonal grid optimization confirms the results of Heikes, Randall and Konor (Dec 2013, MWR 141:4450-4469) that the Heikes and Randall (1995b, MWR 123:1881-1887) tweaking leads to better convergence than spring dynamics or a centroidal Voronoi grid. However these small differences are only important for simple, well resolved test cases. I don't think that we need to resolve this issue for the sake of this paper. I agree that it would be interesting to investigate non-orthogonal relaxations in more detail. But I think that this is beyond the scope of this paper.

Comment Two/three dimensions: Our experience with ICON was that a lot of things were working in two dimensions, but not in three dimensions. Apart from the fact that the triangles were problematic, the hexagonal model was also not so much better in the beginning. It took me some while until I realized that it was the Hollingsworth instability which was occuring here. I have a thorough explanation of this instability in my paper in QJRMS (2013, DOI: 10.1002/qj.1960) in the appendix. I would really encourage (or even urge) you to consider it before deciding finally.

Reply Your work on this instability is of great interest and concern to me and to other UK model developers. I will put in a reference to this paper in a separate paragraph in the introduction:

Hollingsworth (1983) described an instability that can grow when solving the primitive equations in 3D using the vector-invariant form of the momentum equation, conserving energy and enstrophy but not momentum. Gassmann (2013) found that this mode could grow when solving the fully compressible Euler equations on a hexagonal-icosahedral grid of the sphere using a C-grid discretisation and described how it can be controlled. It is possible that this mode grows more quickly when it interacts with the computational modes of the hexagonal C-grid but this is not proved and has not been demonstrated. If the discretisations described on various grids of quadrilaterals were extended to 3D, the behaviour of the Hollingsworth instability could be compared on hexagonal and quadrilateral grids. Gassmann (2013) found that this mode is triggered at the pentagons of the icosahedral grids. The cube corners of the cubed sphere grid have larger distortions that the pentagons of the icosahedral grid. Therefore it seems likely that this mode would also grow on a cubed-sphere grid.

Comment This instability does not occur at all in two dimensions. It comes to the fore in baroclinic zones with strong vertical wind shear. I would say that the TRISK method only by chance had less problems (they were there, but less pronounced) with it than my original idea (which was never published but pointed me to the problem). I guess that the occurrence of the Hollingsworth instability cannot be avoided by upwinding PV, by CLUST, or by APVM. I guess that the problem gets even worse with those methods.

Reply Why do you guess that upwinding PV makes the problem worse? APVM is just Lax-Wendroff so not a bounded advection scheme. My guess would be that a more bounded advection scheme for PV would control the instability more.

Comment The point is that the vorticity equation and the divergence equation, both, have to work correctly. A focus primarily on the vorticity equation is not sufficient. Here I want to stress that the issue of the Hollingsworth instability may occur for any shape of the grid. It was first described in the context of quadrilateral grids.

Reply Agreed. See above

Comment Vorticity on hexagonal grid: I disagree that vorticity is defined on triangles. It should be on a set of 3 rhombi. Perhaps the reason for the different behaviour of enstrophy in Fig 8 is that it is the "wrong enstrophy" which is shown here. And the comparison is a bit weak if CLUST is taken for the hexagons and not for the cubed sphere (or did I misinterpret lines 10-18 on page 6053 ?). In DOI: 10.1002/qj.1960 I have explained how I would interpret the vorticity (page 159 left, last paragraph (sorry, the first three lines in that paragraph were reformulated by somebody during the printing process, but I am sure, that you still understand the content)). But I have to admit that I never measured enstrophy conservation with the latest version of the generalized Coriolis term in a SW model. On the other hand, I am not sure what enstrophy conservation should mean in the context of a three-dimensional model. Is it then Ertel's PV? Or is it only meaningful if the vertical layers are isentropes?

Reply In 2D, the vorticity and hence enstrophy on triangles are good diagnostics for the presence of the hexagonal C-grid computational mode. PV and enstrophy conservation properties can be derived considering the vorticity to be defined on triangles. But from the perspective of the momentum equation, the vorticity is defined on an upwind biased stencil of triangles. I do not argue against the definition on a set of 3 rhombi. But for the TRiSK discretisation that I am using, vorticity has been defined on triangles. I do not state that this is better than defining it on rhombi. But I am not proposing to change that aspect of TRiSK.

In Fig 8, CLUST is used for all grids. I will amend the manuscript to clarify. My conjecture is that the improved enstrophy conservation of the grids of quadrilaterals is due to the lack of computational modes. The growth of the computational mode modifies the enstrophy. This will be clarified in the revised manuscript.

Comment Resolution/DOFs: On page 6051 you mention that "Resolution is measured by the total degrees of freedom (number of cells plus number of edges)." I would not agree that this is a correct measure of DOFs. It should the number of unslaved velocity components divided by 2 (and perhaps multiplied by 3, if you wish). On a hexagonal (or triangular grid) the velocity components are linear dependent, hence one out of three is represented by the two others. Perhaps you ask John Thuburn about his opinion. If interpreting other DOFs, the comparison between the different grids may give other results. It would be interesting to see how the measures in some of the Figures would then change.

Reply Thanks for pointing out the error in saying "Resolution is measured by the total degrees of freedom (number of cells plus number of edges)." However I would like to stick with total DOFS as a combined, approximate measure of computational cost and resolution. John Thuburn has also been using total DOFs recently. Using total DOFs does of course give an immediate advantage to methods with the correct ratio of DOFs. But having slaved DOFs leads to additional cost in calculating the additional DOFs, slaving them and dealing with the problems that they create. I am also going to stick with DOFs as this has been agreed with the Met Office in the Gung Ho project to be the measure that is of interest to them. However I will add a Comment of how total DOFs can be related to effective resolution.

Comment A little remark concerning the last sentence: Hexagonal or triangular codes can also be cast with structured grids, as the example of GME demonstrates.

Reply Many thanks for reminding me of this. I will amend accordingly.

Responses to John Thuburn

Comment P6041 and Table 1. Not only is the ratio Delta x_max / Delta x_min a bit worse for the diamondized grid, it seems to be getting worse (more rapidly) with increasing resolution. So, is the new grid really quasi-uniform? I.e. does this ratio tend to some limit as resolution is refined ? (Maybe the author could do some geometry to check ? Otherwise it might just be worth checking the ratio on one or two finer grids.)

Reply Many thanks for this comment. I've done the geometry. In the limit of an infinite sphere or infinite resolution, the diamond grid should be orthogonal, have no skewness and the ratio Delta x_max / Delta x_min should be sqrt(3). In this limit, the diamond grid becomes a grid of rectangles each with aspect ratio sqrt(3) apart from at the edges where there are kite shaped cells which also have the max/min ratio of sqrt(3). However this limit only holds for particular dual vertex locations. Which I am not using. I am placing the dual vertices to make the primal cell centroidal (of vice-verca). So the theoretical limit is not reached. The theoretical ratio of sqrt 3 is the min to max edge length whereas for each grid, I calculate Delta x based on the cell centre to cell centre distance. For each grid, I have made either the primal or dual grids centroidal which involves moving vertices and consequently cell centres. This brings cell centres together along the edges and at the corners. I now comment on this.

Comment P6045 '...without serious oscillations in pv.' Some ripples are visible in the vorticity field in the Galewsky test, and the author does point them out on p6056.

Reply My implication is that these ripples are not "serious". I have re-worded: "without grid-scale noise in pv".

Comment P6047. The H given by (13) would be diagonal if d_e and d_e' were orthogonal for all the e' in the stencil. So is the point that a grid can tend towards primal and dual edges being orthogonal but without the above property holding?

Reply The off-diagonal terms of H do not vanish on an orthogonal grid of triangles. Only for an orthogonal grid of quadrilaterals. This is now clarified.

Comment I got confused on P6054 in the discussion of using the asymmetric H on the orthogonal HR grid. Surely the original Ringer et al H is diagonal on this grid, and any diagonal H is symmetric. This led me back to sections 3.2.9 / 10 / 11 where I realized it was not clear to me, after all, which H's had been used with which grid. Could this be made clearer?

Reply Yes, I can see that this would be confusing. The point of the asymmetric H is that it tends towards diagonal as the grid tends towards orthogonal. So on an orthogonal grid the asymmetric H is diagonal (and orthogonal). The symmetric H does not tend towards orthogonal for the icosahedral grid. This is now clarified.

Comment P6051 L13-14. If you take more iterations or reduce the time step does the amplification factor get closer to 1 ? (Just a sanity check.)

Reply No. I have not modified the time step but the outer iterations are converged after just 2. I have changed the SWE parameters in order to separate the eigen values. Interestingly, on the grids with centroidal duals, the eigen values do all have magintude 1. On the grids without centoidal duals they do not quite. I am not sure why. I have tried initialising the linear model with one of the unstable modes and it does not appear to be unstable.

Comment P6053 L10. The symmetric H does indeed have better energy conservation, but the non-conservation with the asymmetric H is rather weak.

Reply This is now clarified. In fact I point out that the non-conservation of the asymmetric H is the same as the non-conservation of the original TRiSK on the orthogonal grid

Comment A few places you use the phrase 'more orthogonal'. Being pedantic, edges are either orthogonal or not. How about 'more nearly orthogonal'?

Reply Many thanks. Fixed.

Comment P6037 L3. These authors certainly weren't the first to consider a hexagonal C-grid, but what they did was to figure out what to do with the Coriolis terms to get steady geostrophic modes.

Reply I have changed the wording to "The hexagonal C-grid has become popular since Thuburn ... worked out how to calculate the Coriolis term so as to get steady geostrophic modes."

Comment P6073 L11. Use \citep instead of \citet to get the parentheses in the right place.

Reply Done

Comment P6043 L18. to calculate

Reply Thanks. Done.

Comment P6049 L4. the we -> then we

Reply Thanks. Fixed

Comment P6057 L12-13. The first time I read this it seemed like a non sequitur. Perhaps add half a sentence to say that damping of the computational modes by the advection scheme is what leads to the enstrophy loss.

Reply I have now inserted "(Growth of the computational mode can lead to enstrophy increase whereas control of the computational mode can lead to enstrophy decrease.)"

Comment Fig.4 . Caption: 'Amplitudes' here means amplitudes of the amplification factor, not amplitudes of the normal modes.

Reply Thanks. Fixed

Responses to Nigel Wood

Comment i) I think (as Andrew [Staniforth] pointed out to me originally) that a further advantage of this grid is that the velocity components are not held at the edges where their components change discontinuously.

Reply I am not sure that this is a separate advantage. I am thinking in terms of arbitrarily structured grids rather than having different coordinate systems on every panel. I do not know how to measure the problem that you describe in terms of arbitrarily structured grids. I would rather not mention this advantage.

Comment: ii) Your number (1.8) for the ratio of min to max lengths for the equi-angular cubed sphere does not tally with the asymptotic 1.3 given by Staniforth and Thuburn, citing Rancic et al (1996)? And in that vein, it would be interesting to know what the asymptotic value is for the diamond grid (the resolution you go to is not enough to be able to say it has converged yet).

Reply See reply to John Thuburn on this topic

Comment: iii) Figure 10 shows a strange mix of experiments with different numbers of d.o.f.'s (and therefore also timesteps) whereas in other figures you have approximately matched the d.o.f.'s?

Reply The dofs and the time steps have been matched better