Geosci. Model Dev. Discuss., 6, C2459–C2463, 2014
www.geosci-model-dev-discuss.net/6/C2459/2014/

Geoscientific Model Development Discussions Open Access

Interactive comment on "Non-orthogonal version of the arbitrary polygonal C-grid and a new diamond grid" *by* H. Weller

H. Weller

h.weller@reading.ac.uk

Received and published: 28 January 2014

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.

Response: 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.

Response: 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.

Response: 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.

Response: 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?

Response: 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.

Response: 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.

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

Interactive comment on Geosci. Model Dev. Discuss., 6, 6035, 2013.