Geosci. Model Dev. Discuss., 6, C2239–C2241, 2013
www.geosci-model-dev-discuss.net/6/C2239/2013/

Geoscientific Model Development Discussions
Open Access

GMDD
6, C2239–C2241, 2013

Interactive Comment

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

A. Gaßmann (Referee)

gassmann@iap-kborn.de

Received and published: 19 December 2013

This is rather a friendly letter than a review...

I appreciate the intent to formulate a model on quadrilaterals, because it does not have additional constraints that have to be satisfied. The diamond grid is really an excellent idea and it is striking that the results are better than with cubed sphere.

From experience with the ICON model development I want to bring the following topics to the fore:

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.

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. 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. 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.

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

GMDD
6, C2239–C2241, 2013

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?

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.

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

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