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Geoscientific Model Development Discussions

Interactive comment on "Icosahedral Shallow Water Model (ICOSWM): results of shallow water test cases and sensitivity to model parameters" *by* P. Rípodas et al.

Anonymous Referee #1

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Icosahedral Shallow Water Model (ICOSWM): results of shallow water test cases and sensitivity to model parameters by P. Ripodas et al.

An enormous amount of effort goes into developing an operational weather or climate model and it is valuable to document the performance of the model or, as in this case, a simplified model that forms part of the development path. With a few clarifications (listed below under "comments that need to be addressed") and typographical corrections, this paper would serve that purpose and I would be happy to see it published.

However, the performance of the ICOSWM is not hugely impressive, though apparently it is better than the shallow water version of the operational GME model. Moreover, the paper demonstrates little understanding of the causes of some of the problems. If I were one of the authors I would want to make more progress in understanding and improving the behaviour of the numerical schemes before publishing, bearing in mind their own scientific reputations and those of their host institutions. I make some further comments for the authors' consideration to encourage them in this direction.

Comments that need to be addressed

P586, l24; P592, l25. A tangential velocity component is needed at the edges of the triangular cells for the Coriolis terms. How is this reconstructed?

P587. The description of the Radial Basis Function reconstruction method was not clear enough for me to follow. Why is the RBF matrix-valued? I would expect the scale factor to vary with the model resolution, but the text implies it does not - is that correct?

P587-588: The description of the time scheme would be clearer if the paper simply stated the relevant equations. As it is written, I cannot be sure what exactly has been done.

P588, last two sentences of section 2.3. Perhaps make it clear that Bonaventura and Ringler show how to specify the cell edge values of potential vorticity in order to conserve potential enstrophy (not enstrophy); but there is still some freedom remaining in how to choose the cell edge tangential velocity, and that freedom is explored in the current paper.

P589. Equation (4) comes from a particular discretization of the Laplacian operator on a square grid. Where does equation (5) come from? Presumably it is exact for a uniform grid of equilateral triangles but is an approximation for the grid actually used? What discrete approximation to the Laplacian is used in the model (bearing in mind that only the normal wind components are predicted)? And why is the numerical diffusion applied only to wind and not to mass? Why use equation (6) to estimate the diffusion coefficients when you have all the information needed to use equation (5)? Since (5)

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is used when running the model, can't the values simply be output at the first step? Presumably the numerical diffusion is evaluated at time level n-1 for stability?

Top P591. This section does not make sense because the relationship between the spherical coordinate system and the grid has not been described. In test case 2 the axis of the flow always coincides with the Earth's rotation axis (otherwise it would not be steady), so the flow is always 'zonal'. Presumably you mean there is an angle \alpha between one of the dual pentagons and the Earth's rotation axis (but you only discuss the case \alpha = 0)?

P595, l12. The reference to Gill 1982 does not seem relevant.

P598, l18. The RH wave is actually unstable as a solution of the barotropic vorticity equation too. It is an exact solution, but if you perturb it then it breaks down.

P599, l19. The l_\infty height errors definitely converge slower than second order.

P600, l7. Both potential enstrophy and energy have large 'reservoirs' that do not really take part in the dynamics. Perhaps the change in energy should be compared with the available energy.

Fig 15. The 'conservation' of global vorticity is really just a check that the vorticity has been diagnosed in a consistent way from the wind field, so that the global integral of a discrete curl vanishes.

P604, 605. I am unconvinced by some of the interpretation of the kinetic energy spectra. It is true that part of the spectrum shows an n^{-3} behaviour. The text invites a comparison with observed atmospheric kinetic energy spectra. However, there is currently no convincing theoretical explanation for the observed atmospheric spectra. Moreover, the observed atmospheric flow is not a wavenumber 4 Rossby-Haurwitz wave, so why should the test case 6 model spectrum agree with observations? The text goes on to note that the spectrum starts to deviate from n^{-3} around wavenumber 160, and claims that this is due to the numerical diffusion, and that wavenumber 160 therefore represents the effective resolution of the model. This may well be correct, but more evidence is needed to establish whether it is in fact true. No evidence is given (either theory or high resolution reference numerical solution) that the spectrum should follow n^{-3} in the absence of diffusion. As the authors correctly note, the T511 STSWM solution itself is affected by numerical diffusion. Finally, the same argument would lead to a different conclusion if applied to Fig 21.

P606 reference to Gassmann and Heinze: is this published? Perhaps give a web address.

P610: Table 2 would be much clearer if formatted in separate columns.

P612: Table 4 is just 14 times Table 3, so could be omitted.

Typos, grammatical errors, etc

P582, l26: among which -> among which are

P583, l17: resolutions are considered -> resolutions is considered

P583, l18: that have no -> which have no

P584, l24: control volumes -> control volumes for mass

P584, l26: allows to use -> allows the use of

P588, l27: called e-folding time -> called the e-folding time

P591, l17: 10 days runs -> 10 day runs

P592, l11: have larger errors -> has larger errors

P592, l23: radial basic -> radial basis

P593, l11: Asselin filter -> Asselin filter parameter

P594, l1: define -> defines

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P595, l24: different e-folding -> different diffusion e-folding

P595, l25: All of then -> All of them

P599, l4: convergence test -> convergence tests

P599, l8: All of then -> All of them

P600, l20: difference -> differences

P600, l25: largests -> largest

Additional comments for the authors' consideration

P592-594. Even for the solid body rotation flow, which is smooth and involves no nonlinear cascades, the model generates a significant amount of noise and requires both spatial and temporal smoothing to control the noise and remain stable. Do the authors understand the origin of the noise? Is it related to the velocity reconstruction? Is it related to the relative numbers of mass and velocity degrees of freedom on the triangular C-grid, which imply that some of the mass degrees of freedom cannot be in geostrophic balance (there are not enough velocity degrees of freedom), and which imply that the triangular C-grid dispersion relation includes two spurious 'inertio-gravity' wave branches? Even with 2 hour diffusion there is a distinct flattening of the KE spectrum beyond about wavenumber 250 in fig 21; this may be a related issue.

The Asselin coefficient needed for stability is rather large. What is the nature of the instability that it controls? (The Courant number is comfortably smaller than 1.) Is the instability related to conservation properties of the model? Note that the Asselin filter damps short-timescale transience, so presumably the problem is not a stationary computational mode. It might be informative to look at a 1:-2:1 combination of fields from three successive steps to see the pattern that is damped by the filter. The value of the Asselin parameter needed to maintain stability clearly depends on the test case, as well as other factors such as resolution and perhaps the velocity reconstruction scheme; this is a strong reason for seeking some understanding of why it is needed,

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so that a safe but not excessive value can be used operationally. Note, also, that the use of the Asselin filter makes the time scheme only first order accurate. Since the time step is reduced in proportion to the grid length in the convergence tests, we should expect convergence ultimately to be no better than first order. Could this explain the slowing of convergence at high resolution in some of the tests? Is the Asselin filter responsible for the dissipation of energy in fig. 14?

Does the RBF velocity reconstruction itself become more accurate when a bigger stencil is used (as implied at the bottom of p600)? If it does then presumably some other factor is limiting the overall accuracy of the model.

In the convergence tests the attained convergence rate is often compared with second order. Is there a theoretical basis to expect second order? As noted above, the Asselin filter is only first order in time. Also, the orography is not smooth in test case 5, so even a truly second order accurate scheme would not achieve second order convergence.

The vorticity errors in case 5 (fig 8) are rather large, and they don't appear to come from a simple displacement of the true vorticity field. Are they due to some kind of instability? There is a need to understand where they come from.

P602, l25-27, and P605. You could test the hypothesis that the Heikes-Randall grid optimization is responsible for the improvements over GMESWM, by switching off the optimization. It would be valuable to know the answer.

Interactive comment on Geosci. Model Dev. Discuss., 2, 581, 2009.