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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 #2

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General comments The paper discusses numerical results of the shallow water model ICOSWM that serves as a 2D development platform for the forthcoming 3D General Circulation Models at the Max-Planck Institute for Meteorology in Hamburg, Germany, and the German Weather Service (DWD). ICOSWM is built upon an icosahedral computational grid with C-grid staggering and utilizes a finite-difference discretization in space plus a leapfrog time stepping scheme. The paper provides a thorough evaluation of the model performance using three standard shallow water test cases. These are the smooth steady-state geostrophic balance (test case 2), the flow over an isolated mountain (test case 5) and the wavenumber-4 Rossby-Haurwitz wave (test case 6) as proposed by Williamson et al. (1992). ICOSWM thereby assesses the

accuracy of the spatial (horizontal) and temporal discretizations, and numerical stability of the scheme. This is a useful step before concentrating on 3D model developments since the shallow water equations already represent fundamental flow characteristics of an incompressible fluid on the rotating sphere. The paper fits well within the scope of the journal GMD and I recommend publication after some important revisions and improvements.

Overall, the clarity of the model description, some color figures, critical analyses and typos need to be improved. The paper documents the model sensitivities to the choices in the numerical scheme very well. However, the description lacks some critical in-depth discussion about the reasons for the particular model behavior. For example, interesting discussion points are:

- Can we learn more from the fact that the convergence rates get flatter with increased resolution?
- Is this intrinsic, e.g. do all geodesic/icosahedral grids experience this behavior? Can you provide more comparisons and pointers to the literature, e.g. to the Colorado State University (CSU) modeling team, the Japanese modelers, the Duke University model developments (Walko and Avissar (MWR, 2008)) or Stuhne and Peltier (JCP, 1999)?
- Can you think of new ways how to improve the accuracy of the operators and the model further? It seems as if the GME shallow water model and ICOSWM still compare very well (with slight improvements in ICOSWM). Will the accuracy be sufficient for the non-hydrostatic 3D ICON model that is under development?
- Is the C-grid staggering and the dual mesh with grid optimization an optimal choice? Is the apparent phase delay in test case 6 a consequence of the C grid staggering or also present on an A grid in the GME shallow water model?

- Will the computational performance be competitive in comparison to other methods?

In addition, the authors should consider adding a more challenging test case to their test suite to further reveal the characteristics of the numerical scheme. In particular, I suggest assessing the barotropic instability test case by Galewsky et al. (2004) published in Tellus. This is not a strict demand prior to publication but a useful and informative addition to the Williamson et al. (1992) test suite. The barotropic instability test exhibits very strong gradients and sharp frontal zones that are less pronounced or even absent in the three featured (more benign) tests. The suggested general improvements are:

1. The description of the model ICOSWM is insufficient and relies too heavily on other publications and non-peer reviewed literature. I suggest adding enough information so that the reader gets a clear (stand-alone) picture of the model equations and numerical technique. This includes the description of the actual equations that are solved, a brief overview of how the vector reconstructions work and what the main pros and cons of the Raviart-Thomas and Radial Basis Function (RBF) methods in the context of ICOSWM are. The latter is a new aspect of the numerical method and has not been documented in the (peer-reviewed) literature so far. In addition, it would be useful to briefly discuss the *simple* (page 584, line 26) approximations to the gradient, divergence and curl operator despite the fact that they were introduced in Bonaventura and Ringler (2005). Are the operators still identical? Page 588, line 20 states that the model here is “essentially” equivalent to Bonaventura and Ringler (2005). What are the minor differences?

2. The quality of the color figures 4, 6, 8, 11, 13 is inadequate. The authors tried to use too many contour intervals and therefore too many colors that can no longer be distinguished. E.g. the greens and blues blend into each other. I suggest reducing the number of colors or at least selecting a different color scheme so that

all features of the flow can be seen. This is paramount since the most important information about the symmetry of the flows (e.g. in figure 11 for test case 6) is now hidden. Bonaventura and Ringler (2005) clearly showed that test case 6 exhibits asymmetries in the wavenumber 4 pattern which give insight into the numerical scheme. In figure 13 the zero line is hidden by the color scheme. I suspect that an improved version of the figure will show that there is a phase delay in the ICOSWM simulation in comparison to the reference solution. More specific comments can also be found below

Specific comments

1. Abstract: The 3D ICON model is not discussed in this paper. The sentence starting with “In the framework of the ICON project ...” should be removed. This is adequate for the outlook in the last section.

2. Page 582, line 24: It is not entirely clear to me why the model is introduced as a hybrid finite volume and finite difference model. Please clarify the split.

3. Page 583, line 3: can you summarize Bonaventura’s main reasons or key concepts for the new model developments?

4. Page 585, line 15: it is not clear how the tangential velocity components are obtained. The reconstruction chapter mentions a reconstruction for the cell centers of the triangles.

5. Page 586, line 5: the Gassmann and Heinze reference is an oral presentation, maybe change to private communication

6. Page 587, line 8: is the Ruppert (2007) reference avaliable online? If yes, provide URL, since it the reference is probably hard to find and is be central component of the ICOSWM model.

7. Section 2: show the model equations and operators
8. Page 588, line 18: from the discussion so far it is not clear why the numerical method conserves mass. Do you compute mass fluxes at the edges of the triangles after the application of the Gauss' divergence theorem?
9. Page 588, line 24: is the explicit diffusion optional or needed for stability reasons, especially in the more challenging test cases 5 and 6?
10. Page 589, Eq. 4: how is Eq. (4) derived?
11. Page 590: What is the maximum allowable CFL number?
12. Page 593: Either some of the scale factors are typos or the labels in the convergence plots are wrong. I suspect they are typos and should read 0.2, 0.5 instead of 2 and 5. Introduce the abbreviations used in the plots here.
13. Page 593, line 16, 17: I wonder whether the test is just too smooth and therefore does not show a lot of differences.
14. Page 595, line 12: the Gill reference does not seem to be adequate
15. Page 595, line 19: you could have avoided some of the interpolations of the reference solutions. The spectral coefficients of the reference solution can be directly evaluated at any grid point position.
16. Page 596, line 21: how do you compute the vorticity in NCAR's model? Do you use the native prognostic variable or do you re-derive the vorticity from the velocity components? If the latter, do you use a spectral method or finite-difference approximations?
17. Page 598, line 24: Picking day 10 for test case 6 is an unfortunate choice. It means that the ICOSWM cannot be compared to other published results that

most often show day 14 as recommended by Williamson et al. (1992). It almost looks as if ICOSWM will no longer be stable (broken wavenumber 4) at day 14. Is this correct? If yes, picking day 10 hides this effect which would be interesting to learn about. I also suspect that there is a phase delay and asymmetries in the wave-4 pattern as shown in Bonaventura and Ringler (2005) which is not mentioned here. Can you also plot the reference solution in Fig. 11 with the aforementioned improvements of the color scheme? Please comment further on the characteristics of test 6. If day 14 is still stable, I suggest showing day 14 instead of day 10 in Fig. 11.

18. Page 604: in test case 6 with a dominant wavenumber 4 signal there is no good reason for a n^{-3} tail of the spectrum. You suggest that test case 6 compares well to observed spectra. Why do you connect the two?
19. Page 605, line 2,3: The sentence does not make sense. Is ICOSWM a typo and should read NCAR STSWM?
20. Page 605: The conclusions are weak. How about testing rather than speculating whether ICOSWM without the grid optimization still performs better than GMESWM? Then you will have a better idea what the role of the C-grid staggering is which will contribute to the future developments.
21. Fig. 4: are there error spikes in the top plot that justify the range of the contours. I cannot see the spikes but they might be too small/ Please comment on this. Provide units in the caption.
22. Fig. 11: the contour spacing of 130 m is unusual and make it even harder to compare to other models (together with the fact that day 10 is not normally shown in other publications).

Technical corrections

1. Page 584, line 8: typo, insert “the” in “of the Delaunay”
2. Page 584, line 24: control volumes for *mass*
3. Page 584, line 26, correct: approximations to the gradient, divergence and curl operators
4. Page 585, line 9: typo, it’s Fig. 2
5. Page 585, line 28: typo, “... number of mass points”
6. Page 589, line 4: ΔX is the grid spacing, better expression than grid mesh
7. Page 590, line 10: dash is missing between Heikes-Randall
8. Page 590, line 20: normalization factor $1/(4\pi)$ is missing in front of the integral
9. Page 591, line 1: rephrase “rotation with a balanced geostrophic height field”
10. Page 591, line 8: rephrase
    Convergence results for different sets of model parameters after 10 days are presented below.”
11. Page 591, line 17: omit “runs”
12. Page 591, line 22: omit “for clearness”
13. Page 593, line 22: “not adequate” is “inadequate”
14. Page 594, line 1: typo “defines” instead of define
15. Page 594, line 5 and many other locations: it should read 15-points stencil or 9-point stencil, avoid the “s” in “points”
16. Page 595, line 18 and many other locations: it should read 15-day runs

17. Page 595, line 25 and page 599, line 8: typo, correct: all of “them”
18. Page 601, line 1: “use of a 15-point stencil”
19. Page 602, line 11: it should read “integer parameter”
20. Page 603, Eq. 11: explain that the star symbol means the complex conjugate
21. Page 604, line 5 and 8: use “are” instead of “is due to”
22. Page 604, line 15: use “remain” instead of “keep”
23. Fig. 16, caption: “relative” instead of “relatives”

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