

Interactive comment on “The Matsuno baroclinic wave test case” by Ofer Shamir et al.

Anonymous Referee #1

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The aim of this paper is to present a standardised test case for shallow water models which is derived from analytic solutions of the linearised shallow water equations on the equatorial beta plane published by Matsuno in 1966. Recent work by one of the authors and others has shown that solutions to these equations approximate the solutions of the full shallow water equations on the sphere in the asymptotic limit of small gravity wave speed. The authors propose 2 sets of parameters, one relevant to the atmosphere and one relevant to the first baroclinic mode of the ocean. The wavenumber and mode are chosen to be small (5 and 1 respectively) to ensure the waves are close to the asymptotic limit and so that they can be spatially resolved without requiring too high resolution. The wave amplitude is also necessarily small. For each set of parameters the authors suggest 2 simulations: one of a westward propagating Rossby wave and the other an eastward propagating inertia-gravity wave. To assess the accuracy of the simulation the authors suggest 2 measures: Hovmoeller diagrams and spectral

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analysis. They present these for simulations run using a finite difference model in an equatorial channel in spherical coordinates. This paper is a continuation of the work in Shamir and Paldor (2016) which deals with the case of large gravity wave speeds.

The Rossby-Haurwitz wave described in test case 6 of Williamson et. al. (1992) is known to be problematic. Thuburn and Li (2000) describes these issues and I think that paper should be referenced here, as it was in the previous paper (Shamir and Paldor, 2016). One issue is that the original initial conditions as specified in Williamson et. al. (1992) lead to wrapping up of potential vorticity contours and the associated generation of small scale features and potential enstrophy cascade. A figure showing the potential vorticity at several times throughout the simulations would be appreciated here to show that this does not happen for this test case. The other issue with the original test case is that it is unstable. This is demonstrated numerically in Thuburn and Li (2000) by adding some small noise to the initial conditions after they noticed that the solution they computed using a finite volume model on a grid of hexagons and pentagons (i.e. their only non latitude-longitude model) broke down. The errors related to the structure of the underlying grid triggered the dynamical instability. The solutions in this paper have been computed using a regular latitude-longitude grid so I wonder if a similar issue could occur with this test case. I suggest that the authors could check this by either adding some noise to the initial conditions, as in Thuburn and Li (2000), or by running their code on a rotated grid (i.e. with the poles in the midlatitudes).

Overall I recommend this paper for publication, subject to minimal correction and clarification as described below. It is well written and clearly describes useful diagnostics to be used as error measures. However, I feel that the case for the usefulness of the proposed test case could be made stronger. For example, some papers use the Rossby wave test as a convergence test, using a reference solution from either a higher resolution run or from a different model. Could the analytical solution here be used to test the convergence of a linear shallow water model? This would provide a useful test in between the steady state test case 2 and the other tests that require a reference solution

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from a higher resolution run. Also, if the wave is indeed stable it would be a fantastic replacement for test case 6, especially for unstructured grid models, or models that use adaptive mesh refinement, since truncation errors related to mesh topology will have no dynamic instability to trigger.

Specific Comments:

pg 4, lines 14-15: I am concerned that different pre-factors lead to less stable solutions - it makes me wonder if the version chosen in this paper is indeed stable to differences in grid alignment.

Figure 2: Is there any reason why the Rossby wave with $H=0.5$ is less regular than the other solutions? Why do some of the contour plots have white regions when the values have been normalised so should lie in the range $[-1, 1]$?

Power spectra: Are these at all sensitive to the sampling frequency? My experience is that the spectra can be very sensitive to this but maybe that is for more turbulent simulations.

Supplement: The code provided to compute the initial conditions, while appreciated, could be improved. The authors state that the code will compute the analytic fields on arbitrary latitude-longitude grids but they have assumed that these grids are structured. These codes will not work as written for unstructured meshes, which are becoming more common in the community. The test case is much more likely to be used if these codes could be amended (i.e. they return values given a list of latitude-longitude values). In addition to this, there are some unnecessarily confusing aspects of the code. For example, there is no need to capitalise variable names so the radius of the Earth, which is called a in the paper could be a rather than A in the code. The is especially confusing since there is also an A in the equations described in the paper. It would also make sense to have H as an input parameter, since this can be varied.

Technical Corrections:

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Equation 3b: This is different to that in the code matsuno.py (and I think the code is correct).

Equation 3c: I think this is missing a sqrt around the gH.

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