

Interactive comment on “The ICON-1.2 hydrostatic atmospheric dynamical core on triangular grids – Part 1: Formulation and performance of the baseline version” by H. Wan et al.

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The reviewers describe this paper as useful and well written documentation on ICON and as editor I agree with these descriptions. I would strongly encourage the authors to submit a revised manuscript, making the clarifications requested by the reviewers.

I appreciate the honesty of the paper. The problems of the triangular C-grid are clearly described and the truncation error analysis is simple and illuminating. I also appreciate the honesty with which the authors describe the over-all performance of the model. For example: "... there is a danger of overly strong diffusivity in our triangular ICOHDC. This is in fact one of the major concerns we have regarding the viability of this dynamical

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core in long-term climate simulations".

A couple of the reviewers mention your description of equivalent resolution. I agree with them that it is too long and not sufficiently clear. I would recommend using something simple like that recommended in Gassmann's review. It may be unfair to simply use degrees of freedom in a low-order model, but this is widely understood. Gauging the equivalent resolution by the results is not useful.

Another review was submitted to me after the deadline and so the authors are invited also to respond to this short review:

"I would essentially ask for details and rationale about the semi-implicit time stepping since the linear solve, which is straightforward to implement in a spectral model, is less so on an unstructured mesh : dimension of Krylov space, convergence rate, number of iterations, amount of work spent in the solver vs explicit computations."

Finally, could details of how to get hold of the model code be given in the revised manuscript.

Many thanks and I look forward to a revised manuscript.

Interactive comment on Geosci. Model Dev. Discuss., 6, 59, 2013.