

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.

Anonymous Referee #3

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This paper is an overview of the formulation and performance of the ICON baseline version dynamical core. Generally it is well-written with mostly sufficient detail needed for an overview. Further details on ICON are referenced adequately. I have few comments on details within the paper but rather more concerns regarding the conclusions.

It is not clear to me what the properties are of the advection scheme for pure advection. Is it upwinding, conservative, monotone for tracers? How diffusive and how accurate is it? Currently the best performing NWP models use semi-Lagrangian advection, the accuracy of which has been a significant factor in the increase in NWP skill over the last 20 years. There is a reference to potentially using a higher-order scheme for the

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temperature advection (on page 83). However, since the phase error in the baroclinic wave test diminished at higher resolution it appears that a higher order scheme will not be used in future. It would probably be wise to look more carefully at pure advection tests (e.g. test case 1 of Williamson et al).

In the description of the numerical scheme the aspect of noise control is dealt with adequately but the conclusions do not highlight this sufficiently as more than a cause for concern. Recent work by Lauritzen (2007) and Whithead et al (2011) suggests that divergence damping as a means of noise control requires a lot of care and perhaps is best avoided. I believe that another significant factor in the increase in NWP skill has been the reduction in damping or diffusion needed, by reducing the effect of or eliminating computational noise. The leading NWP models can run the test cases described later without additional diffusion or damping. The reason additional damping or diffusion is best avoided is that it (wrongly) weakens gradients. This effect diminishes at (very) high resolution.

In both the discussion of tests and in the conclusions the performance of the dynamical core appears overstated. The baroclinic wave results are the most worrying. The authors correctly highlight the poor performance at low resolution but even R2B05 (70km) lacks skill according to Fig. 8. More worrying is the noise which increases with resolution evident in Fig.9. I am not sure whether the inclusion of the aquaplanet tests reveals very much. As the authors state, further tests at various resolutions are needed before meaningful conclusions can be drawn.

Given the above remarks, I find it difficult to believe statements such as "On the whole the new dynamical core behaves well in the evaluation." (page 90, line 22) and "We must conclude that the ICOHDC can serve as a good basis for further development of a global model for climate research." (top of page 91). Work elsewhere (e.g. MPAS) makes a more convincing case for quadrilateral C-grid. Even if the lowest resolution configurations are not recommended, full Earth system modelling for centuries will probably only be able to use the more modest resolutions for some time to come.

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Given the vagueness in its definition, I did not find the discussion on degrees of freedom helpful or illuminating.

On page 84, third line, "It is interesting to note that, ... " is better English than "It is interesting to notice that, ... ".

On page 95 I think there is an "as" missing after "seen" in the sentence "Equations (B15-B18) can be seen a simplified version of". In this paragraph there is also a reference to using an isothermal reference state. Would this be sufficient for high lids (e.g. above 70km or 0.1hPa)?

References.

Lauritzen, P.H. 2007 A stability analysis of finite-volume advection schemes permitting long time steps. *Mon. Weather Rev.*, 135, 2658-2673

Whitehead, J.P., Jablonowski, C., Rood, R.B. and Lauritzen, P.H. 2011 A stability analysis of divergence damping on a latitude-longitude grid. *Mon. Weather Rev.*, 139, 2976-2993

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