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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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We thank the referee for the helpful comments. Our reply is given below.

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.

The abstract, introduction and conclusions of the paper are revised in response to comments from the reviewers and the editor.

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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 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).

We appreciate the referee's remark about the insufficient description of the advection/transport algorithm. The following information is added to the revised manuscript:

A group of upwind, conservative, flux-form semi-Lagrangian transport algorithms are implemented for tracer transport in the ICON triangular models. Options for horizontal advection include the first-order upwind scheme, and a triangular version of the "swept-area" approach following Miura (2007). The latter algorithm is second-order in time and either second-, third- or fourth-order in space, depending on the choice of reconstruction polynomial (linear, quadratic or cubic). The polynomial coefficients are estimated based on a conservative weighted least squares reconstruction method (Ollivier-Gooch and van Altena, 2002). Transport in the vertical is calculated with the Piecewise Parabolic Method (PPM, third-order) of Colella and Woodward (1984). The optional limiters employed include semi-monotonic and monotonic slope limiters (Barth and Jespersen, 1989) as well as the flux corrected transport (FCT) approach (Zalezak, 1979). Consistency between tracer transport and the discrete continuity equation can be achieved when the dynamical core uses a two-time-level time stepping scheme. These transport algorithms in the ICON models have gone through comprehensive testing, e.g., following the proposal of Lauritzen et al. (2012). Some of the evaluation results were presented by D. Reinert in http://www.cgd.ucar.edu/cms/pel/transport-

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workshop/2011/16-Reinert.pdf. More detailed results of the second-order (linear) horizontal transport can be found in Lauritzen et al., (2013).

In the aqua-planet simulations presented in the paper, the horizontal transport of water vapor, cloud liquid and cloud ice was computed using the second-order Miura (2007) scheme with monotonic FCT, while the PPM scheme with semi-monotonic slope limiter is used for the large-scale vertical transport. Temperature advection in the hydrostatic dynamical core, on the other hand, does not yet use these advanced transport schemes. Rather, the mass and heat fluxes at triangle edges are computed by first averaging the layer thickness and temperature from cell centers to edge midpoints using a distance-based linear interpolation and then multiplying by the normal velocity. Because of the rather simple flux calculation and the inherent property of the divergence operator as discussed in the paper, the horizontal temperature advection is only first-order accurate. In the ICON nonhydrostatic model, the edge-based potential temperature and density values are estimated with the second-order Miura (2007) for the discretization of the horizontal advection in the thermodynamic equation and the continuity equation. The details are given in Zängl et al. (2013, manuscript in preparation).

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.

We have revised the introduction and conclusions of the paper in response to the referees' comments. As explained in the revised manuscript, triangular C-grids were chosen for the ICON models for the flexibility in implementing mass conserving local zooming and multi-resolution approaches. In principle we also prefer numerical schemes that need, or inherently produce, as little damping as possible. However, in the earlier days, the grid-scale noise issue on the triangular C-grid was not recognized either by us or in the dynamical core development community. In the paper we have been attempting to present an evaluation of the capabilities of the present discretizations without the intention to hide the potential issues. We state clearly in the paper that rather strong damping is applied for noise control, and that its impact on long-term real-world simulations should be assessed at resolutions higher than presented in the paper. In the revised conclusions, we state again that the truncation error of the divergence operator is a major issue one needs to address in terms of both algorithm development and model evaluation.

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.

As pointed out in Section 6.1.2 of the paper, we believe the lack of convergence of the R2B5 results in Fig. 8 is caused by the phase error of the baroclinic wave, attributable to the low-order discretization of temperature advection. Skamarock and Gassmann (2010) have found similar errors in their hexagonal models, and demonstrated that the simulations can be improved by applying higher-order advection schemes. Clarifications are added to Sections 5.5, 5.11, 6.1.2, as well as to the conclusions of the revised manuscript.

More worrying is the noise which increases with resolution evident in Fig. 9.

There is indeed noise visible in the 850 hPa relative vorticity field at day 9 near 145W, 60N at resolution R2B7, which is not present in other panels of the same figure. We have looked into the other simulations listed in Table 2 of the paper and did not find similar noise up to R3B6 (down to 23 km grid spacing). Such noise is not present in the ICON nonhydrostatic model either. The origin of the un-smoothness in Fig. 9 at R2B7 is not yet clear. At this point we do not think the high-resolution results are worrying, although we agree that the cause of the noise at R2B7 merit further investigation.

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.

Staniforth and Thuburn (2012) pointed out that “a largely overlooked aspect of the development of dynamical cores is the response of their numerical schemes to important forcings.” In our case, one of the major concerns regarding the numerical properties of the dynamical core is whether the grid-scale noise from the divergence operator can be effectively controlled when moist processes, for example condensational heating, trigger feedbacks between the numerical error and the diabatic forcing. If the aqua-planet results obtained with the triangular C-grid discretization had shown severe contamination by noise, they would have been taken as the argument to reject the viability of the triangular dynamical core. The 140 km simulations shown in the paper indicate that the ICOHAM model can correctly capture the characteristics of the tropical precipitation and their sensitivity to sea surface temperature. In the revised manuscript, following the suggestion of Dr. Almut Gassmann, a discussion is also added on the simulated 250 hPa kinetic energy spectra. We acknowledge that further tests at various resolutions are needed before conclusions can be drawn regarding whether the grid-scale divergence noise can be controlled well enough without introducing unacceptable damages to important features of the model climate. The results included in the paper marks the starting point of this investigation.

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.

In the revised manuscript we summarize the main results from the numerical test but removed the statement that “the new dynamical core behaves well”. We also clearly point out that

- The numerical diffusion employed to control the divergence noise leads to strong damping of the flow and loss of freedom to choose the diffusion coefficient by physical argument.
- Numerical solutions at R2B5 and lower resolutions show a phase error in the baroclinic wave test.
- These issues need to be addressed in terms of both algorithm development and model evaluation.
- In the ICON nonhydrostatic dynamical core (Zängl et al., 2013, manuscript in preparation), the velocity field is filtered with a five-point stencil to achieve nearly second-order accuracy for divergence, while the edge-based potential temperature and density values needed for horizontal advection in the thermodynamic equation and the continuity equation are estimated using the second-order Miura (2007) scheme.

With respect to other developments such as MPAS, we would like to remark that the development of the present approach was essentially complete before or at the same time when the concepts employed in MPAS were proposed (see e.g. Wan, 2009). Therefore, MPAS should certainly be taken into account a posteriori as an alternative (and possibly superior) modeling approach, but it could hardly have been considered as such during the development phase of the present model.

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

The topic of degrees of freedom (DOF) was touched in the discussion on equivalent resolutions between the new dynamical core and the spectral transform core of ECHAM. The point we were trying to make there was that for models employing discretizations of very different orders of accuracy, the equivalent resolutions can not be estimated a priori from the number of DOF but need to be assess a posteriori through numerical experiments.

In response to comments from the reviewers and the editor regarding the equivalent resolutions, we have revised the corresponding section of the paper.

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

Corrected.

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 ....".

Corrected.

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)?

Experiences with the ECHAM model family have confirmed that the isothermal reference state does not cause problem in the middle and upper atmosphere. High-lid versions of ECHAM, namely MAECHAM (Manzini et al., 2006; Giorgetta et al., 2006) and HAMMONIA (Schmidt et al., 2006a,b) in which the model top is located at 80 km and 250 km, respectively, employ the same time integration method and reference

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state. These two models require smaller time steps because the horizontal winds in the middle and upper atmosphere can be more than twice stronger than those in the troposphere. More sophisticated semi-implicit integration schemes that do not depend on a reference state are available, but require the solution of a more complex Helmholtz equation. They have not been introduced to either the ECHAM family or the ICON models.

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