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Geoscientific Model Development Discussions

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 and suggestions. Our reply is given below.

1. The introductory part, as written, lacks in reality motivation and mixes several things together. It is true, that mass conservation is desirable, but one can do it with spectral methods too (the other question is whether this is practical). Further, mass conservation is intrinsic in many existing finite-volume or finite-element solutions, so why a new development? Then, one learn about pole singularity, then about the need for local zooming, and the need of developing atmo-

sphere and ocean in the same framework (but it is not explained what is implied here). I think that the goals should be formulated more precisely, and given the goals, the approach followed by ICON should be motivated. Also, this should be done, perhaps, not solely on the background of spectral models, but many other efforts that have already implemented icosahedral approaches (beginning from GME to MPAS).

In response to the referee's comments, we have rewritten the introduction of the paper to provide a clearer idea about the goals of the ICON model development. There we also try to explain what the motivations are behind the proposed choice, and why we believe that it has some advantages over other approaches.

A comparison with GME was carried out by Rípodas et al. (2009) in the context of validating the ICON shallow water model. The conclusion was that the present approach is more accurate than that implemented in GME. 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.

References:

Rípodas, P., Gassmann, A., Forstner, J., Majewski, D., Giorgetta, M., Korn, P., Kornblueh, L., Wan, H., Zängl, G., Bonaventura, L., and Heinze, T.: Icosahedral Shallow Water Model (ICOSWM): results of shallow water test cases and sensitivity to model parameters, Geosci. Model Dev., 2, 231–251, doi:10.5194/gmd-2-231-2009, 2009.

Wan, H.: Developing and testing a hydrostatic atmospheric dynamical core on triangular grids, Reports on Earth System Science 65, PhD thesis, Max Planck Institute for Meteorology, Hamburg, Germany, 2009.

2. This eventually leads us to the question of why triangular C-grid is selected instead of hexagonal C-grid. Using an approach with a too large scalar space is far from being the accepted way to go.

As explained in the revised introduction, the features of the triangular mesh related to its flexibility in implementing mass-conservative local zooming and multi-resolution approaches make us believe it is an attractive choice. The property of the divergence operator on the triangular-C grid (which causes grid scale noise) was recognized during the course of this development. In the paper we have been attempting to present a balanced evaluation of the capabilities of the triangular C-grid discretizations without hiding the potential issues. 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.

3. I like the explanation of divergence noise proposed in the manuscript, but would like to comment that the velocity field the model operates with is the discrete field produced by model numerics. While the explanation highlights the origin, it not necessarily gives the correct estimate in the end.

We agree that whether the results from the truncation analysis are correct depends on whether the assumptions they are based on are valid in the model. Following the general procedure of studying a complex problem in a simpler but relevant context, the truncation error analysis presented in the paper is performed on a regular planar grid with strict assumptions about the operand. In Section 4.2 we pointed out that although different perspectives can be taken when interpreting the meaning of the operand and the result given by the operator, the first-order accuracy of the divergence operator revealed by the truncation error analysis is relevant. In the actual model, the operands of the divergence and Laplace operators are the discrete fields produced by model numerics, as pointed out by the reviewer; Additionally, the spherical geometry and the unavoidable grid irregularity also introduce more terms to the truncation error. However, the key features of the triangular C-grid that cause the grid-scale noise in the

divergence operator, namely the asymmetric shape and the upward- and downward-pointing directions, stay unchanged. The numerical tests we have carried out with the dynamical core showed that the magnitude of the numerical diffusion, chosen according to the truncation error analysis, is both necessary and effective in suppressing the grid-scale noise. This indicates that the truncation error analysis, albeit idealized, provides relevant and useful information.

In response to the referee's comment, a discussion is added to Section 4.2 (Truncation error analysis) in the revised manuscript.

4. The proposed magnitude of biharmonic diffusivity (scaled with time step) would effectively imply more dissipation as the resolution is refined. I do not think it is justifiable. Also it basically implies that flows at grid scale are experiencing e-fold damping per time step.

For dynamical cores that do not have strong inherent diffusion associated with the discretization itself, it is generally necessary to apply additional diffusion to remove numerical noise at scales near the truncation limit. To our knowledge, it is common for atmospheric models to use damping timescales that decrease with increasing resolution (e.g., Boville, 1991; Roeckner, 2003; Williamson, 2008b; Lauritzen et al., 2012; Rausher et al., 2012). Takahashi et al. (2006) carried out a series of simulations with the spectral model AFES to empirically determine the appropriate relationship between the magnitude of hyper-diffusion and model resolution, aiming at correctly capturing the shape of the kinetic energy spectrum in both the inertial regime and the mesoscale regime. Their results suggested a scaling of $n_0^{-3.22}$ (or $\Delta x^{3.22}$, where n_0 is the truncation wavenumber and Δx the grid spacing) for the diffusion coefficient. In the ICOHDC, the choice of a 4th order diffusion with damping time equal to time step implies a scaling of Δx^3 according to Eq. (20) of the discussion paper, meaning the decrease of diffusion coefficient with increasing resolution is close to but slightly slower than suggested by Takahashi et al. (2006).

We agree with the referee that the choice of diffusion as described in the paper implies that flow at grid scale experiences strong damping. We have clearly stated in the paper that this is an undesirable feature that warrants special attention in further development. Its impact will be further evaluated in the future by analyzing features of idealized and/or climate simulations that are sensitive to diffusion.

In the revised manuscript, a figure and two paragraphs are added to Section 7 (First results from the aqua-planet experiments) to present the simulated kinetic energy spectra and discuss the impact of horizontal diffusion.

References:

Boville, B. A., (1991): Sensitivity of simulated climate to model resolution. Journal of Climate, 4, 469–486. Roeckner, et al. (2003): The Atmospheric General Circulation Model ECHAM5. PART I: Model Description, Technical Report 349, Max Planck Institute for Meteorology.

Williamson, D. L. (2008b): Convergence of aqua-planet simulations with increasing resolution in the Community Atmospheric Model, Version 3, Tellus, 60A, 848–862.

Lauritzen, P. H., Mirin, A. A, Truesdalea, J., Raederc, K., Andersonc, J. L., Bacmeistera, J. and Neale, R. B. (2012): Implementation of new diffusion/filtering operators in the CAM-FV dynamical core, International Journal of High Performance Computing Applications, 26, 63-73

Rauscher, S., T. Ringler, W. Skamarock, and A. Mirin, (2012): Exploring a Global Multi-Resolution Modeling Approach Using Aquaplanet Simulations. J. Climate, in press.

Takahashi, Y. O., K. Hamilton, and W. Ohfuchi (2006), Explicit global simulation of the mesoscale spectrum of atmospheric motions, Geophys. Res. Lett., 33, L12812.

Minor comments

Abstract: "and show a clear trend of convergence as the horizontal resolution

increases" Did you have any doubts? Is it an achievement a reader should learn about?

Convergence is required and expected for a reasonably behaving dynamical core, but nevertheless needs to be confirmed a posteriori by numerical tests. In the revised manuscript we've rewritten the abstract such that it gives more detailed information about the model's properties and it's performance in the idealized tests. We also explicitly point out the aspects where improvements are needed.

Introduction: "adiabatic fluid dynamics equations that govern the atmospheric motions" – there always are sources and sinks.

By using "adiabatic" we meant to exclude model components that represent parameterized processes like radiative transfer, turbulence, cumulus convection, etc., which are often referred to as the "diabatic physics". The term "dynamical core", although widely used in NWP and climate model development, does not have a standard definition. Staniforth and Thuburn (2012) pointed out "there are some subtleties, such as whether scale-selective dissipation terms should be considered part of the dynamical core or a parameterisation of subgrid-scale processes". To avoid this terminology issue, we replace "adiabatic" in the above-quoted sentence by "resolved-scale" in the revised manuscript.

Reference:

Staniforth, A. and Thuburn, J.: Horizontal grids for global weather and climate prediction models: a review, Quarterly Journal of the Royal Meteorological Society, 138, 1–26, doi:10.1002/qj.958, 2012.

"to avoid the polar singularities of global latitude-longitude grids" – true, but how it is related to the motivation above?

The part in the Introduction section that contained the above-quoted phrase has been rewritten and extended in the revised manuscript, to provide a clearer explanation of the motivations for the ICON development and the presented choice of grid and discretization methods.

The main text: Fig. 1 p and phi are appearing at full and half levels

Legends in the figure are modified in the revised manuscript.

"For example the divergence operator per construction makes it straightforward to achieve mass conservation", — but it is so in any finite volume or finite elements, so what is the point?

We meant to point out the mass conservation property which is not always guaranteed or considered essential, for example, in climate models that use spectral transform cores and in many weather forecast models. In the revised manuscript the sentence is changed into "The divergence operator, essentially a finite-volume discretization, makes it straightforward to achieve mass conservation".

"The divergence and gradient operators are mimetic in the sense that the rule of integration by parts has a counterpart in the discrete model (cf. Eqns. (9) and (10) in BR05), a desirable property for achieving conservation properties. – again it is maintained by each properly designed model – there is no way in obtaining correct transfers between the kinetic and available potential energy if this consistency is violated.

It is not clear to us what the referee means by "properly designed". Many discretizations that are correct and convergent from a purely mathematical viewpoint do not have this kind of property. Just to mention an example, this is not true for spectral transform discretizations whose accuracy relies rather on their small truncation error. Therefore, we feel that the property we are pointing out for the discretization in our model is not obvious to every reader, and has several advantages, although we do not imply that it automatically makes the discretization better than other ones.

Section 4.3: grid scale noise is seen in p_s when it is too late. It is typically seen

in the horizontal divergence and leads to problems very gradually.

The corresponding sentence is changed into "Grid scale noise in the divergence operator typically causes noise in the divergence field and in temperature,..."

"vector Laplacian (15)" – vector biharmonic operator

This is corrected in the revised manuscript.

What is the advantage of RBFs over the Perot reconstruction?

RBF reconstructions have the nice property that they allow for straightforward extension to larger stencils and higher order approximations, as shown, e.g., by Bonaventura et al. (2011). On the other hand, if a small stencil is employed, all reconstruction algorithms have essentially equivalent first-order accuracy. In that case, the choice of the reconstruction algorithm may rather depend on other properties, such as the mimetic features of the reconstruction proposed by Perot (2000) and Thuburn et al. (2009).

References:

Bonaventura, L., Iske, A., and Miglio, E.: Kernel-based vector field reconstruction in computational fluid dynamic models, Int. J. Numer. Meth. Fl., 66, 714–729, 2011.

Perot, B.: Conservation properties of unstructured staggered mesh schemes., J. Comput. Phys, 159, 58–89, doi:10.1006/jcph.2000.6424, 2000.

Thuburn, J., Ringler, T. D., Klemp, J. B., and Skamarock, W. C.: Numerical representation of geostrophic modes on arbitrarily structured C-grids, J. Comput. Phys., 228, 8321–8335, 2009.

Fig. 6 Is the left panel for day 6 correct?

Yes, the panel is correct. The referee's question might have been caused by the fact that a different color scale is used for the day 4 and day 6 panels in the left column.

This is clarified in the revised manuscript.

"The simulated flow does not appear noisy (cf., e.g., Fig. 9)." – This should be judged by looking at the horizontal divergence or 'vertical velocity'

We have indeed checked the simulated divergence fields and do no see clear grid-scale noise. In the manuscript, the paragraph containing the above-quoted sentence is removed during the revision.

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