Review of the GMDD / EGUsphere Manuscript https://doi.org/10.5194/egusphere-2023-1078

Title: Analytical and adaptable initial conditions for moist baroclinic waves in a global hydrostatic model

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Summary:
The manuscript describes a new family of analytical initial conditions for atmospheric General Circulation Models which are suitable for the simulation of idealized baroclinic waves on the surface of the sphere. This research is inspired by existing descriptions of baroclinic wave test cases for the dynamical cores of GCMs, and thereby further extends this line of research. The suggested initial conditions consist of a well-balanced background state with two midlatitudinal zonal jets for either dry or moist model configurations. This balanced initial state can either be used as is as a steady-state test case for a dynamical core or can be overlaid with a midlatitudinal zonal wind perturbation to trigger the generation of a baroclinic wave in the midlatitudes. Various options are suggested for the background state which are determined by the chosen parameters for the width of the zonal jets and their vertical center position. These choices determine the baroclinicity and stability characteristics of the initial conditions which then impacts the growth rates and propagation speeds of the baroclinic waves in case a perturbation is chosen.
The newly-derived analytical initial conditions for the moist configuration are then tested in the ECMWF model OpenIFS to demonstrate the characteristics of the baroclinic waves and their sensitivity to the chosen parameters.

Major comments:
The research is very interesting and, as mentioned above, extends the already available suite of idealized GCM test configurations with respect to baroclinic wave investigations in spherical geometry. However, there are some major aspects that need attention before a publication can be recommended. They are related to the reproducibility of the results, an error in the formulation of the initial moisture field, and the actual implementation of the initial conditions in OpenIFS. The major concerns are:

1) A particular deficiency is that the presented OpenIFS implementation results are irreproducible by other GCMs since the authors decided to show the simulation results for their moist configuration. The latter utilizes a selected suite of OpenIFS physics parameterizations which are not available in other models. Despite the authors' choice of the moist configuration no attention is paid to the actual impact of the moisture on the simulation which could have served as an interesting talking point. For example, no precipitation or cloud patterns are discussed in the manuscript that would take advantage of the moist configuration and physical parameterizations. Therefore, the question arises why the irreproducible (by other models) moist version was picked here.
A revised version of the manuscript should push the majority of the moist results into a new section, the Appendix, and/or the Supplemental material, and focus the proof-of-concept and sensitivity study for the parameters *n* and *b* on the dry configuration. This way, new users of the test configuration can directly compare their dry results to this manuscript, thereby gaining

confidence that their implementation is correct (provided OpenIFS is correct, see point 4). The title should be more inclusive and state … 'for dry and moist' … instead of only 'moist'.

2) In order to make the results reproducible, important pieces of information about the OpenIFS diffusion settings are required. A listing of the diffusion coefficient, decentering parameter (if used), and the Asselin filter coefficient is needed as the growth rates of the modeled baroclinic waves are impacted by these dissipation choices. For example, is the 4^{th}-order horizontal diffusion used as described in Eq. (2.60) in https://www.ecmwf.int/en/elibrary/80319-ifs-documentation-cy43r3-part-iii-dynamics-and-numerical-procedures, e.g. with the specified coefficient? Quote the value for the TL319 resolution.

3) As mentioned in 1) it is left open what the differences between the dry and moist simulations are. A short paragraph/section on the dry/moist differences is desirable. Experiences from the Dynamical Core Model Intercomparison Project (DCMIP) in 2012 and 2016 showed that the presence of idealized precipitation processes intensifies the development of the baroclinic waves in comparison to their dry counterparts. Is this the case here? I recommend adding a time series plot of the minimum mean sea level pressure for both the dry and moist configurations that can display the various growth rates.

4) It would have been beneficial to also see the simulation results for a second non-OpenIFS (dry) dynamical core to gain confidence that the implementation is correct and that two models converge towards a reference solution. This is not a must for the revised version though. However, the relatively slow growth rates for the current moist implementation (and the expected even slower growth rates for a dry implementation) are surprising. The slow growth rates in comparison to other baroclinic wave examples from the literature might be a product of a reduced baroclinicity in this configuration, but this also raises the question whether the OpenIFS configuration works flawlessly.

5) The Zenodo archive distributes the source code for the initial conditions as OpenIFS Fortran code. This means that the initial conditions are not a standalone subroutine that others could just grab and embed into their models. In case the authors would like to promote a wide adoption of the initial conditions by others, they should consider also providing a generic non-OpenIFS version of the initial condition routine.

6) The definition of the saturation vapor pressure (Eq. 13a) is incorrect. This equation (this is the approximation by Bolton (1980)) needs to use the temperature instead of the virtual temperature. All moist simulations will need to be revised after the correction.

7) The authors never define the value for the surface pressure p_s, but from the Figs. 5 and 6 as well as the hard-coded value 100000 in the (Zenodo) Fortan code for the moisture initialization, it seems that p_s = 1000 hPa is intended and was used for the implementation of the initial conditions in OpenIFS. The information about p_s needs to be provided. Unfortunately, the choice of p_s = 1000 hPa leads to an inconsistency between the OpenIFS hybrid vertical coordinate η design and the normalized pressure variable defined for the test case $\eta = p/p_s$. This is due to the choice of the reference pressure p_0 = 1013.25 hPa in OpenIFS instead of 1000 hPa. OpenIFS defines the eta coordinate as $\eta = a/p_0 + b$ where a (in Pa) and b (unitless) are the hybrid coefficients of the 137 vertical layers. This means that the pressure in OpenIFS is computed as $p = a + b\, p_s$ which corresponds to $p/p_s = a/p_s + b$.
However, using the normalized pressure for the baroclinic wave from the manuscript $\eta = p/p_s$ and plugging in the OpenIFS definition of η we get

$$p = \eta p_s = \left(\frac{a}{p_0} + b\right) p_s = a\frac{p_s}{p_0} + bp_s$$

instead of the OpenIFS definition:

$$p = a + bp_s$$

Since $p_s \neq p_0$ this means that the implementation of the current initial conditions in OpenIFS is slightly imbalanced once the first time step is conducted. This can be remedied by either selecting p_s = 1013.25 hPa for this test case or rescaling/redefining the OpenIFS hybrid coefficients 'a' to correct this inconsistency. The latter might be preferred as p_0 = 1000 hPa is a popular choice for other models. I suspect that the wavy behavior shown in Fig. 2 for the steady-state condition might actually be caused by this inconsistency (or at least it contributes). In any case, all simulation results will need to be rerun after the correction.

The OpenIFS hybrid coordinate is described in https://www.ecmwf.int/en/elibrary/80319-ifs-documentation-cy43r3-part-iii-dynamics-and-numerical-procedures (e.g. see Eqs. (3.8) and (3.14), also the list of coefficients on the page https://confluence.ecmwf.int/display/OIFS/4.4+OpenIFS%3A+Vertical+Resolution+and+Configurations)

Minor comments:

1) There are many small English grammar mistakes or missing words (like 'the' or others) throughout the manuscript. The authors should work with native speakers or professionals to correct these (too many to list them here).
2) Line 5: the statement that a baroclinic wave can only develop if an unbalanced perturbation is used is strictly speaking incorrect. This is typically only true for models on lat-lon grids. If other grids are used, the grid itself is a perturbation and acts as a (slow) trigger for waves. Please rephrase.
3) Line 43-44: The introduction lacks depth/references when it comes to describing the current suite of baroclinic wave test cases for spherical geometry. I suggest adding:
   https://journals.ametsoc.org/view/journals/mwre/132/11/mwr2788.1.xml
   https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.2241
   The QJ (2006) version of the NCAR Technical Report:
   https://rmets.onlinelibrary.wiley.com/doi/10.1256/qj.06.12
   There is also a new moist and dry variant of the Ullrich et al. (2014) test case with topography as the trigger of the baroclinic wave instead of an overlaid perturbation:
   Hughes, O. K. and C. Jablonowski (2023), A Mountain-Induced Moist Baroclinic Wave Test Case for the Dynamical Cores of Atmospheric General Circulation Models, EGUSphere and Geosci. Model Dev. Discuss., https://egusphere.copernicus.org/preprints/2023/egusphere-2023-376/, in press

   It might also be worth including information about steady-state initial conditions on the sphere like:
   https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.122

https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/asl.349

4) Line 59: acronyms Z1, Z2, Z3 and LC1, LC2 and LC3 need some context/explanations
5) Line 75: misleading wording, v is not constant over time, just state that the initial v is set to 0
6) Line 97: p_s is used but never defined, correct, also make sure to state that the topography (surface geopotential) is zero for this initial data set.
7) Line 103: define R_d as the gas constant for dry air
8) Line 161: quote the units for RH (percent). This is also true for Eq. (14): 100 needs to be 100%. I saw in the Fortran implementation that RH is handled as a fraction (between 0-1), therefore the units avoid any confusion here.
9) Line 166: State that Eq. (13a) is the Bolton (1980) approximation. The use of T_v is incorrect. Here, T needs to be used. Specify the units of the numbers in the Bolton equation.
10) Fig.1 is incorrect. There is a direct linear correspondence between $n = p/p_s$ (left axis) and pressure (right axis). The current graph uses a logarithmic relationship which is incorrect. Maybe the authors wanted to show the height along the left axis? Revise the figure.
11) Line 176, revise: negative temperatures in Celsius are allowed.
12) Line 214-215: The original OpenIFS implementation contains the dry and moist variant of the Jablonowski and Williamson (2006) baroclinic wave. The moist variant was used during DCMIP 2012 event (described in
The DCMIP-2012 test case document: Ullrich, P. A., C. Jablonowski, J. Kent, P. H. Lauritzen, R. Nair, M. A. Taylor (2012): Dynamical Core Model Intercomparison Project (DCMIP) Test Case Document, Technical Report, version 1.7 from Jan/13/2013).
The current OpenIFS implementation seems to overwrite the original implementation with the test case numbers 41 and 42, thereby reusing the existing test case infrastructure. Is this correct? I recommend mentioning this.
13) Line 220: What is the relevance of the -1.8C freezing point for water here? The authors define SSTs that are about -20C or lower, therefore it is unclear why -1.8C is emphasized. It is not the actual value of the SST that matters for the surface fluxes and stability, but the jump between the conditions at the surface and the lowest atmospheric layer.
14) Line 231: do not use ‘complex’ since it alludes to complex number theory (which is not used).
15) Line 236-243: factorials are not actually removed, they are just hidden in the binomials now, revise line 232
16) Lines 244-253 and Table 1 are OpenIFS-specific and better suited as an Appendix with specific OpenIFS implementation details. Consider moving this information. ‘namespace’ is not the correct phrase, it is called ‘namelist’. Line 252 states N3DINI=3, but Table 1 lists N3DINI=2 (contradiction), correct
17) Line 259, use: ‘Gaussian hill zonal wind perturbation’
18) Table 1: why is LAPE and LAQUA are set to true in the dry case (as shown in the Zenodo archive), explain the meaning of all namelist settings, the acronyms are too OpenIFS specific to be understood as is by the general audience
19) Table 2: Remove ‘Maximum’, u_0 is not the actual maximum, correct the units of the lapse rate (K/m), use ‘Amplitude of the zonal wind pertubation’
20) Provide more insight into the actual resolution. It is stated that TL319 is used which should correspond to a linear grid with 320x640 grid points (in case of the full grid) with a grid spacing of about 62 km. Is the reduced Gaussian grid (N320) or the full grid (F320) used?

However, the Zenodo fort.4 files list the input values
&NAMFPD
NLAT=640,
NLON=1280,
which do not correspond to the N320 but N640 (31 km grid spacing). Please clarify what the actual resolution for the simulations was.

21) Line 270: Provide a reference for L137 level setup (e.g. online page), list the position of the model top
22) Line 279: The symbol w is not the location of the cell interfaces, it is the weight at these locations (typically used as the cos(phi) as a weight that takes the convergence of the meridians into account). The OpenIFS w is the 'Gaussian' weight.
23) Lines 191-294: 'geostrophic' is mentioned here. When plotting the stability parameters in Fig. 3 are indeed the geostrophic definitions use, or the generic ones (without the geostrophic approximation)? Please clarify.
24) Line 310: was a decentering parameter used in OpenIFS? Without decentering, the NCAR CAM SLD T170 RMSE error is only 0.02 m/s at day 15, thereby comparable to OpenIFS. With the decentering activated (the parameter was 0.2) the SLD errors were higher as shown in the referenced NCAR Technical Report.
25) Line 360, use: … vertical temperature gradient in the tropics …
26) Line 363: what is meant by the phrase 'moist T' in the supplement? Is it T_v or T from the moist simulation? Does 'dry T' refer to the temperature in the dry simulation? This needs to be clarified.
27) Line 393: It is not explained whether Fig. 6 shows the moist or dry simulation. I guess it is the moist one. Caption also needs to state this (also true for the Fig. 5 and Fig. 7 captions)
28) Fig.5 and 6: the colors are too dense and hard to distinguish, thin out by a factor of 2 (4C spacing). Does it rain in these simulations? When does the rain start? Comment in the text.
29) Section5: Add (multi-panel) figure to show the time evolution of the minimum surface pressure (for the various n options). No tracker is needed for this. Also expose the evolution of the dry configurations versus the moist one. The growth rates should be different.
30) Line 435: I strongly recommend adding a standalone initialization routine to the Zenodo archive to promote the use of this baroclinic wave configuration
31) Line 449: Explain how c_p is modified when moisture is used.
32) Line 466: correct formatting problem, provide value of p_s.
33) Reference: many references are incomplete (see GMD formatting guidelines and correct). Use unique names/acronyms for the journal names, currently it is a mix (e.g. MWR, MON WEATHER REV)