Analytical and adaptable initial conditions for dry and moist baroclinic waves in the global hydrostatic model OpenIFS (CY43R3)

Addressed Comments for Publication to

Geoscientific Model Development

by

Clément Bouvier, Daan van den Broek, Madeleine Ekblom and Victoria Sinclair

Dear Dr. Travis O'Brien,

Please find enclosed the revised version of our previous submission entitled "Analytical and adaptable initial conditions for dry and moist baroclinic waves in the global hydrostatic model OpenIFS (CY43R3)" with manuscript number egusphere-2023-1078. We would like to thank you and the reviewers for the valuable comments which helped improving the quality of our manuscript. In this revision, we have carefully addressed the reviewers' comments. A summary of main modifications and a detailed point-by-point response to the comments from Reviewers 1 and 2 (following the reviewers' order in the decision letter) are given below.

Sincerely,

Clément Bouvier, Daan van den Broek, Madeleine Ekblom and Victoria Sinclair

Note: To enhance the legibility of this response letter, all the editor's and reviewers' comments are typeset in boxes. Rephrased or added sentences are typeset in color. The respective parts in the manuscript are highlighted to indicate changes.

Authors' Response to the Editor

General Comments. In particular, please note that for your paper, the following requirements have not been met in the Discussions paper:

1. "The main paper must give the model name and version number (or other unique identifier) in the title."

2. "If the model development relates to a single model then the model name and the version number must be included in the title of the paper. If the main intention of an article is to make a general (i.e. model independent) statement about the usefulness of a new development, but the usefulness is shown with the help of one specific model,the model name and version number must be stated in the title. The title could have a form such as, "Title outlining amazing generic advance: a case study with Model XXX (version Y)"."

Therefore please replace the "a global hydrostatic model" in the title of you manuscript by " the global hydrostatic model OpenIFS (CY43R3)".

Response: We appreciate your handling of the review process and apologize for the inconsistency with the guidelines.

We changed the title as suggested:

> Analytical and adaptable initial conditions for dry and moist baroclinic waves in a global hydrostatic model OpenIFS (CY43R3)

> **General Comments.** Additionally, I want to challenge your statement in the code availability section, that the output can be easily reproduced. Exactly because the OpenIFS model is also not completely open accessible, outputs can not be easily reproduced by every interested reader and as you do not provide your plotting scripts, it can also not be directly be tested if it is infact exactly the same result. Please archive the most important data shown in your paper and provide the plotting scripts for your figures.

Response: We want to thank you for this important comment, which will increase the reproductibility of the paper.

We added to the zenodo repository our raw data and cyclone tracked data used to produce the figures and results presented in the preprint. Additionally, we included all plotting scripts used to produce all the figures (https://doi.org/10.5281/zenodo.7890586). We changed the "Code and data availability" section accordingly.

Authors' Response to Reviewer 1

General Comments. This is a very good manuscript which requires only some very minor edits before publication. It is clear and well written describing analytical formulae for initial conditions for a moist baroclinic instability test case to be used in testing dynamical circulation models. My suggestions for editing are as follows:

Response: Thank you for your feedback.

We have carefully addressed all the issues item by item as follows.

Comment 1

Line 5 The statement implies a baroclinic wave will ONLY develop if an unbalanced perturbation is added. This is not so since baroclinic instability is essentially a balanced flow instability , a balanced perturbation can also excite wave development.

Response: Thank you for the comment.

We agree with the reviewer's comment and deleted the word "only" in the specified sentence.

Comment 2

Line 22 'system' should be 'systems'

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the word accordingly.

> ### Comment 3
>
> Line 24 'pattern' should be 'patterns'

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the word accordingly.

> ### Comment 4
>
> Line 34 'on $f - planes$' should be 'on an $f - plane$' and '$\beta - planes$' should be 'a '$\beta - plane$'

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the words accordingly.

> ### Comment 5
>
> Line 36 'used approximation' should be' approximation that is used'

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the expression accordingly.

> ### Comment 6
>
> Line 94 The zonal wind and gradient and hydrostatic balance, being a balance that is possible in a non-hydrostatic system SHOULD be a valid solution to the full equations of motion, not just the hydrostatic equations.

Response: Thank you for the comment.

We agree with the reviewer. We changed line 94 from

> As the geopotential and virtual temperature anomaly fields are derived from hydrostatic equations, the solutions only apply to hydrostatic models.

to

> The geopotential and virtual temperature anomaly fields are derived from hydrostatic equations, and the derived initial states apply to both hydrostatic and non-hydrostatic models.

Comment 7

Equation (16) seems to be missing the terms proportional to F_2 and F_4

Response: Thank you for the comment.

At the surface level ($\eta = 1.0$), $u_\eta = 0.0$ in the Equation (11), which means there is no proportional term to F_2 and F_4 in the Equation (16). To ease the understanding of this fact, we added:

> The Sea Surface Temperature (SST) is zonally uniform and is specified to equal the temperature field at $\eta = 1$ (see Eq. (11) and (15)), which means negative temperatures are allowed and the zonal wind is equal to 0.0 ms^{-1}.

Comment 8

Line 243 Since the factorials have been replace by Gamma functions which are DEFINED in terms of factorials, the factorials have not REALLY disappeared .

Response: Thank you for the comment.

Effectively, the factorials did not disappear as stated but were combined to Gamma functions, which in turn were combined to obtain binomial coefficients. We corrected this assertion as follow:

> In order to avoid the costly use of factorials, F_3 was expressed as a binomial coefficient fraction and all the binomial coefficients were computed once with the multiplicative method, since $\binom{z}{k+1} = \frac{z-k}{k+1}\binom{z}{k}$ with z and k being integers.

and,

> the Gamma function can be replaced by binomial coefficients in F_3 as follows

Comment 9

Line 356 'increase' should be 'increases'

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the word accordingly.

Comment 10

Line 421 'use' should be 'use it'

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the expression accordingly.

Comment 11

Lines 570 and 571 A factor of u_η is missing

Response: Thank you for the comment.

Thank you for noting the missing u_η on lines 570 and 571. The new version of the manuscript was revised to include the missing u_η.

Authors' Response to Reviewer 2

> **General 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.

Response: Thank you for your feedback.

We have carefully addressed all the issues item by item as follows.

Response: Thank you for the comment.

Our primary motivation for designing these experiments was to allow for a large range of realistic extra-tropical cyclones and hence baroclinic waves, to be simulated so that the dynamics of extra-tropical cyclones in the current and in future climates could be studied. This is why the focus was originally on moist simulations. However this main motivation was not expressed in the introduction because we are aware that this new formation could also be used to test the stability of dynamical cores and we wanted to appeal to both audiences. However, we agree with the reviewer, in that for this new set up to be useful for the comparison and evaluation of dynamical cores, we need to present

the results of the dry case with no physics. Therefore, we now include a new sub section showing the evolution of the dry case (Section 5.3.1). Furthermore, as the moist case simulations can be used to study cyclone dynamics, we also expand the results of this section to show the precipitation patterns. Lastly, we changed the last paragraph of the Introduction to explicit better our motivations.

Moreover, the title have been changed to:

> Analytical and adaptable initial conditions for dry and moist baroclinic waves in the global hydrostatic model OpenIFS (CY43R3)

Comment 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 4th-order horizontal diffusion used as described in Eq. (2.60) in OIF [a], e.g. with the specified coefficient? Quote the value for the TL319 resolution.

Response: Thank you for the comment.

We agree with the reviewer in that it is important to include all details of the numerical set up, including details on the diffusion. We used the default options in OpenIFS, which are the same as the default options in CY43R3 of the full IFS. In this version there is no decentering nor Asselin filter. The spectral diffusion used by default is of 4th order (with the exponent of the wavenumber dependency REXPDH=4) and is set to be rather weak, the strength of which is related to the used model timestep. The coefficients for T_L319 are 2100.0 seconds (vorticity (HDIRVOR), divergence (HDIRDIV), temperature (HDIRT), humidity (HDIRQ) diffusions) and the other are set to zero. This information

was added in section 3.3.

> ### Comment 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.

Response: Thank you for the comment.

This is a good suggestion and in the revised version of the manuscript we include a subsection describing the evolution of the dry case, then the moist case and highlight the main differences. We added Figure 8 showing the precipitation evolution in the moist case.

Moreover, we added Figure 11 comparing the evolution of the 850 hPa vorticities for different n and between the moist and dry cases. As explained in section 5.3, cyclones developing in the dry cases which are run with no physics, reached higher vorticity levels but slower than their moist counterparts which are run with physics. Also note that friction which is also included in our simulations, acts to weaken the cyclones [Boutle et al., 2010].

> **Comment 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.

Response: Thank you for the comment.

Unfortunately it is beyond the scope of this study to produce the simulations with another dynamical core as this would entail a huge amount of work.

The slower growth rates of the cyclones in our new case are very likely due to the reduced baroclinicity in our set up compared to the Jablonowski and Williamson [2006] case (a gradient between the poles and equator of 36°C in our case against more than 80°C in the Jablonowski and Williamson [2006] case). One motivation for the development of this new background state was to produce a background state and subsequently cyclones which are much more flexible with more degrees of freedom than in the Jablonowski case Jablonowski and Williamson [2006]. Moreover, the slower growth rates observed in the paper can be explained by the friction induced by the Charnock value (see 3 and section 5.4 of the corrected version).

Response: Thank you for the comment.

We agree with reviewer's comment and included a standalone version in the Zenodo archive. This standalone have been compiled with GFortran version 8.5.0 and tested on Red Hat 8.5.0-10 (`https://docs.csc.fi/`). This standalone is divided in two parts: (1) a main program setting all the variables to compute the zonal fields and (2) a subroutine computing the zonal fields detailed in sections 2.1 and 2.2 of the manuscript.

Response: Thank you for the comment.

Yes, equation 13a in the manuscript is incorrect and T_v should be T in this equation. This has now been corrected and we now also state in the revised manuscript that this equation is the Bolton approximation.

> The specific humidity field $q(\lambda, \phi, \eta)$ is then computed to ensure concordance with the proposed virtual temperature and jet structure by assuming $T = T_v$.

The specific humidity field is derived from the relative humidity ($RH(\eta)$), the saturation vapour pressure (e_s) and the saturation mixing ratio (w_s) using the Bolton approximation for the saturation vapour pressure [Bolton, 1980, Yau and Rogers, 1996] as presented in the following equations

$$e_s(\lambda, \phi, \eta) = 611.21 \exp \frac{17.67(T(\lambda, \phi, \eta) - 273.15)}{T(\lambda, \phi, \eta) - 29.65} \text{ and} \qquad (1a)$$

$$w_s(\lambda, \phi, \eta) = 0.622 \frac{e_s(\lambda, \phi, \eta)}{p(\lambda, \phi, \eta) - e_s(\lambda, \phi, \eta)}, \qquad (1b)$$

where $T(\lambda, \phi, \eta)$ is the temperature field (K), $e_s(\lambda, \phi, \eta)$ is the saturation vapour pressure (Pa) and p is the pressure (Pa).

With only T_v and RH it is not possible to compute T and hence q. Therefore we needed to implement an iterative solution to obtain T (see Figure 2 in the revised manuscript). To obtain the final T, we implemented an iterative scheme. On the first iteration, we assume that $T = T_v$ in the Bolton approximation. Then, the estimated T is used again in the Bolton approximation to estimate a new T. This cycle is repeated 10 times to obtain the final T and q.

Comment 7

The authors never define the value for the surface pressure ps, 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 h 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 $p = a + bp_s$ 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:

$$p = \eta p_s = (a/p_0 + b)p_s = ap_s/p_0 + bp_s \tag{2}$$

instead of the OpenIFS definition:

$$p = a + bp_s \tag{3}$$

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 OIF [a] (e.g. see Eqs. (3.8) and (3.14), also the list of coefficients on the page OIF [b])

Response: Thank you for the comment.

We did set the surface pressure to 1000 hPa in these experiments, which as the reviewer states, is not fully consistent with the definition of the vertical coordinate. We have re-run the simulations with a surface pressure of 1013.25 hPa and changed the text accordingly. As predicted by the reviewer, the RMSE computed from the new simulations with the corrected surface pressure (the revised Figure 2, now Figure 3) does not present any wavy pattern.

Comment 8

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).

Response: Thank you for the comment.

We have carefully proofread the revised manuscript and have hopefully now corrected these minor issues.

Comment 9

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.

Response: Thank you for the comment.

We agree with the reviewer's comment and deleted the word "only" in the specified sentence.

Comment 10

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`

Response: Thank you for the comment.

We agree with the reviewer's comment and included the references in the Introduction.

Comment 11

Line 59: acronyms Z1, Z2, Z3 and LC1, LC2 and LC3 need some context/explanations

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the text accordingly:

> Popular zonal jet structures are Zonal jet 1, Zonal jet 2 and Zonal jet 3 (denoted Z1, Z2 and Z3) resulting in, respectively, baroclinic lifecycles 1, 2 and 3 (denoted LC1, LC2 and LC3) [Thorncroft et al., 1993, Agustí-Panareda et al., 2005].

Comment 12

Line 75: misleading wording, v is not constant over time, just state that the initial v is set to 0

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the sentence as follow:

> In other words, the meridional wind speed is set to 0.0 ms^{-1} which leads to a gradient-wind balance.

Comment 13

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.

Response: Thank you for the comment.

We agree with the reviewer that defining p_s is necessary. We have corrected this sentence:

> The vertical levels are pressure levels normalised with respect to the surface pressure, defined as $\eta = p/p_s$, where p is the pressure on the model level and p_s is the surface pressure.

to

In OpenIFS, the vertical η levels are defined as $\eta = p/p_s = a/p_s + b$, where $p_s = 1013.25$ hPa is the pressure at the surface pressure, and a and b are hybrid coefficients defined for each vertical resolution.

We have clarified that no topography is used in this study by adding to line 93:

The derivation of the analytical initial conditions for geopotential and virtual temperature fields starts from the primitive equations for moist adiabatic and frictionless flow in spherical coordinates and normalised pressure levels for a planet with no topography (i.e., surface geopotential is zero).

Comment 14

Line 103: define R_d as the gas constant for dry air

Response: Thank you for the comment.

We agree with the reviewer's comment and changed R_d definition accordingly.

Comment 15

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.

Response: Thank you for the comment.

We agree with the reviewer's comment and changed Equations (12) and (14) to include the percentage units.

Response: Thank you for the comment.

As stated above in response to comment number 6, we have changed the section to include the units.

The specific humidity field $q(\lambda, \phi, \eta)$ is then computed to ensure concordance with the proposed virtual temperature and jet structure by assuming $T = T_v$. The specific humidity field is derived from the relative humidity $(RH(\eta))$, the saturation vapour pressure (e_s) and the saturation mixing ratio (w_s) using the Bolton approximation for the saturation vapour pressure [Bolton, 1980, Yau and Rogers, 1996] as presented in the following equations

$$e_s(\lambda, \phi, \eta) = 611.21 \exp \frac{17.67(T(\lambda, \phi, \eta) - 273.15)}{T(\lambda, \phi, \eta) - 29.65} \text{ and} \tag{4a}$$

$$w_s(\lambda, \phi, \eta) = 0.622 \frac{e_s(\lambda, \phi, \eta)}{p(\lambda, \phi, \eta) - e_s(\lambda, \phi, \eta)}, \tag{4b}$$

where $T(\lambda, \phi, \eta)$ is the temperature field (K), $e_s(\lambda, \phi, \eta)$ is the saturation vapour pressure (Pa) and p is the pressure (Pa).

Response: Thank you for the comment.

We have revised Figure 1 and relative humidity is now plotted as a function of η with a linear relationship between the left-hand y-axis (η) and the right-hand u-axis (p (hPa)). As a result, there is now only one line showing the relative humidity as a function of η and p (with $p_s = 1013.25$ hPa and $p_{top} = 0$).

Comment 18

Line 176, revise: negative temperatures in Celsius are allowed.

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the text accordingly:

> The Sea Surface Temperature (SST) is zonally uniform and is specified to equal the temperature field at $\eta = 1$ (see Eq. (11) and (15)), which means negative temperatures are allowed and the zonal wind is equal to 0.0 ms^{-1}.

Comment 19

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 Dyn, 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.

Response: Thank you for the comment.

We agree with the reviewer, the test case have been overwritten to include the our initial state. We mention in section 3.2 that:

> Originally, this background state was implemented in the full version of the IFS, and hence OpenIFS, to test the dynamical core. It was attributed the NTESTCASE 41 (dry case) and 42 (moist case) for the Dynamical Core Intercomparison Project (DCMIP).

Moreover, we changed section 3.4 to state:

> The dry and moist test case can be computed by setting the NTESTCASE value to 41 or 42 respectively, replacing *de facto* the previous implementation.

Comment 20

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.

Response: Thank you for the comment.

We agree with the reviewer's comment. -1.8°C is irrelevant for our configuration and we have removed it from the text. Moreover, we clarified the text:

> The original initial state of Jablonowski and Williamson [2006] has a very strong meridional temperature gradient which means that the near-surface temperature reaches -50°C at high latitudes. In the dry case with no physics, the surface heat

fluxes are not computed meaning that the SSTs can be specified to be much warmer (or colder) than the near-surface atmospheric temperatures without causing any problems such as destabilisation of the boundary layer or convection. In contrast, in the moist case with physics on an aquaplanet, exceptionally cold conditions at high latitudes with physically realistic SSTs cause large surface heat fluxes to develop and in the extreme case can result in low pressure centres resembling polar lows developing at high latitudes. Therefore, modifications to the Jablonowski and Williamson [2006] case are needed to enable it to be run with physics and to allow it to be used to investigate cyclone dynamics rather than the numerical accuracy of dynamical cores. Hence, the SST definition presented Section 2.3.

Comment 21

Line 231: do not use 'complex' since it alludes to complex number theory (which is not used).

Response: Thank you for the comment.

We agree with the reviewer and changed the word 'complex' to 'non-trivial'.

Comment 22

Line 236-243: factorials are not actually removed, they are just hidden in the binomials now, revise line 232

Response: Thank you for the comment.

Effectively, this was not clearly explained in the original version of the manuscript which has caused some confusion. The factorials have not been removed as stated but

were combined to Gamma functions, which in turn were combined to obtain binomial coefficients. We corrected this assertion as follow:

> In order to avoid the costly use of factorials, F_3 was expressed as a binomial coefficient fraction and all the binomial coefficients were computed once with the multiplicative method, since $\binom{z}{k+1} = \frac{z-k}{k+1}\binom{z}{k}$ with z, k integers.

and,

> the Gamma function can be replaced by binomial coefficients in F_3 as follows

Comment 23

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

Response: Thank you for the comment.

Table 1 is updated with an additional column "Explanation" that explains the meaning of the different parameters. We have correct the word 'namespace' to 'namelist' in the caption of Table 1. Thank you for noting the contradiction of N3DINI; it is now corrected to 2 on line 252.

Comment 24

Line 259, use: 'Gaussian hill zonal wind perturbation'

Response: Thank you for the comment.

We agree with the reviewer's comments and changed the expression accordingly.

> ## Comment 25
>
> 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

Response: Thank you for the comment.

As stated in 23, we added a new column to disambiguate the parameters presented in Table 1.

> ## Comment 26
>
> 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'

Response: Thank you for the comment.

We agree with the reviewer that u_0 is not the maximum of the wind speed. The amplitude of the zonal wind speed is not defined by u_0 alone but also by the parameter b in the expression for $u(\lambda, \phi, \eta) = -u_0 \ln(\eta) \exp[-(\frac{\ln \eta}{b})^2] \sin^{2n}(2\phi)$. We changed the explanation for u_0 from 'maximum zonal wind speed' to 'Together with b, u_0 adjusts the amplitude of zonal mean wind speed (ms^{-1})'.

We corrected the explanation for u_p to 'Amplitude of the zonal wind perturbation' and the units of the lapse rate to K/m.

> ## Comment 27
>
> 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.

Response: Thank you for the comment.

The model resolution is T_L319 and thus there are 320 by 640 grid points on the full grid. The values in the fort.4 were misleading. This is because in the idealised model set up, OpenIFS does not read these resolution values from the fort.4 files; NLAT and NLON in fort.4 are ignored. Instead, OpenIFS reads the initial condition files and obtains the grid information from them. Note that it is only the grid information from these initialisation files that OpenIFS reads; all other variables in these files are ignored. The inconsistency spotted by the reviewer's comment have been corrected in the fort.4 files in the Zenodo archive.

> ## Comment 28
>
> Line 270: Provide a reference for L137 level setup (e.g. online page), list the position of the model top

Response: Thank you for the comment.

We agree with the reviewer and added after T_L319 L137

and 137 vertical levels with a model top of 0.01 hPa (`https://confluence.ecmwf.int/display/UDOC/L137+model+level+definitions`, accessed: 2023-12-05))

Comment 29

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.

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the text as follow:

where u_{za} is the zonal average of the zonal wind speed, u_{ideal} is the ideal zonal average of the zonal wind speed computed from the analytical expression for the zonal wind (Eq. (3)), w_{ϕ_j} is the weights to correct the convergence of the meridians ϕ_j and $\Delta\eta_i$ is the thickness of the model layer η_i.

Comment 30

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.

Response: Thank you for the comment.

As the reviewer stated, we used the generic absolute vorticity and not the geostrophic approximation. We changed the text accordingly:

For the initial state to be stable to horizontal displacements (inertial stability) the absolute vorticity must be positive (negative) in the northern (southern) hemisphere. Situations can exist where the atmosphere is statically and inertially stable, but the atmosphere is unstable to slantwise displacements (symmetric instability). This exists when the potential vorticity is negative.

Comment 31

Line 310: was a decentering parameter used in OpenIFS? Without decentering, the NCAR CAM SLD T170 RMSE error is only $0.02m/s$ 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.

Response: Thank you for the comment.

We did not use a decentering parameter. Moreover, with the correction of the surface pressure and the temperature field, the obtained RMSE for all studied cases is significantly lower than the NCAR CAM SLD T170 RMSE.

Comment 32

Line 360, use: ... vertical temperature gradient in the tropics ...

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the expression accordingly.

> **Comment 33**
>
> 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.

Response: Thank you for the comment.

We agree with the reviewer's comment and changed the caption of Figure S3 accordingly

> Figure S3: Sounding for two cases (n=3 and b=2.0, n=1 and b=1.0) for two latitudes (0° and 45°N). The solid green line represents the dew point, the solid red line the real temperature of the moist case, the solid black line the ideal parcel profile from the surface temperature and the dashed red line the temperature of the dry case (added for reference). The red area represents the convective available potential energy and the blue area the convective inhibition.

> **Comment 34**
>
> 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)

Response: Thank you for the comment.

We agree with the reviewer's comment and updated the captions accordingly.

Comment 35

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.

Response: Thank you for the comment.

We agree with the reviewer's comment and we decreased the spacing in the figures as suggested. Additionally, we also thinned out the spacing of MSLP with a factor of 2, to improve the clarity of the figure.

Moreover, we added a figure with the development of the baroclinic wave which also includes the precipitation (Figure 8 in Section 5.3). We added comments in the text of section 5.3 accordingly.

Comment 36

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.

Response: Thank you for the comment.

As stated above in response to comment number 3, we have added Figure 11 which compares the evolution of the 850-hPa vorticity for different n and both the moist and dry cases. As several baroclinic waves are developing during the simulation (for all values of n), we deemed it important to be able to separate the different waves and study the evolution of the vorticity of the first cyclone. That is the reason why we chose to use a tracking algorithm with the vorticity at 850hPa.

Comment 37

Line 435: I strongly recommend adding a standalone initialization routine to the Zenodo archive to promote the use of this baroclinic wave configuration

Response: Thank you for the comment.

We agree with the reviewer's comment. And as stated in 5, a standalone version is included in the Zenodo archive. Moreover, we changed the Code and Data Availability as follow:

> The licence for using the OpenIFS model can be requested from ECMWF user support (openifs-support@ecmwf.int). The modified subroutines of OpenIFS, a standalone version to compute the zonal fields detailed in Sections 2.1 and 2.2, the submission and plotting scripts, the configuration files and the raw data are available on Zenodo (https://doi.org/10.5281/zenodo.7890586).

Comment 38

Line 449: Explain how c_p is modified when moisture is used.

Response: Thank you for the comment.

Thank you for pointing out the missing explanation on how to modify c_p when adding moisture. We have changed the sentence from

> Please note that the specific heat capacity of air, c_p, in the thermodynamic equation needs to be corrected with a correction factor when moisture is included in the model.

to

Please note that the specific heat capacity of air, c_p, in the thermodynamic equation needs to be corrected with a correction factor δ when moisture is included in the model. The correction factor is defined as

$$\delta = 1 + (c_{p,vap}/c_{p,dry} - 1)q,$$

where $c_{p,vap}/c_{p,dry} = 1860/1004$ (units: J/(kg K)) and q is the specific humidity (units: kg/kg) [ECMWF, 2016, eq. (2.3)].

Comment 39

Line 466: correct formatting problem, provide value of p_s.

Response: Thank you for the comment.

We want to thank the reviewer for noting the formatting problem; it is now corrected. The value of p_s is given in the main text (line: 102) and the derivation of the equations in the Appendix do not use any specific values for p_s.

Comment 40

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)

Response: Thank you for the comment.

Thank you for pointing out the inconsistencies regarding the journal acronyms. The acronyms are now corrected according to the guidelines of GMD.

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