Response to reviewer2 (egusphere-2024-1477 manuscript)

Thank you for your careful review. We appreciate your thoughtful comments to improve our paper. We copied your comments in the blue text and have provided our responses in the black text. We have revised the manuscript according to your suggestions. Our point-by-point responses to the reviewers' comments are provided below. We hope that these improvements satisfactorily address the issues pointed out by you.

This paper presents a global nonhydrostatic dry atmospheric dynamical core discretised using high order discontinous Galerkin methods. The motivation for using high order methods comes from the authors' previous work which showed that when using an LES turbulence model, the order of accuracy of the spatial discretisation needs to be sufficiently high. This also motivates the use of DG methods as they avoid the large computational stencil required for high order grid point methods. The new model is tested using a range of well known 3D problems, with some changes made to avoid the development of small scales in order to make convergence analysis possible.

The work presented here is of interest and has been carefully done, however it is not clear exactly what is new about this model and how it relates to previous DG / high-order discretisations. In some places additional clarifications and/or references are required and the language needs to be more precise - e.g. words like "attempt" or "about" should be avoided. In many places the results are qualified with "about" and I wonder if it is possible to be more precise, or more confident in what is described. I have highlighted these places below.

I recommend numbering all the equations - this makes it much easier when others discuss your paper!

Thank you for the valuable suggestion. In the revised manuscript, we have numbered all the equations.

In the results section, the information on the number of elements, polynomial orders and resulting equatorial resolution is hard to read
- could this information be summarized in a table for each test?

We have summarized the number of elements, the polynomial order, and the resulting equatorial resolution in Table 5 of the revised manuscript. In addition, we have simplified the description of spatial resolution in the main text.

Many of the comments below require only minor changes to clarify the text. However, I have selected "major revisions" because I have requested a lot of clarifications and in particular, I think it is very important that the novelty of the method is clarified with reference to other DG dycore publications.

As for the novelty, please see our response to your comment for line 58 (Comment 11).

Introduction:

1. line 18: "inertia subrange" should be "inertial subrange"

We have modified it in line 20 of the revised manuscript.

2. lines 26-28: This sentence is confusing and the `e-folding time' is not defined or referred to again. As the use of high order methods is motivated by this study, I think it is worth adding another sentence or two here to adequately explain this previous result.

Thank you for your suggestion. As in the introduction of Kawai and Tomita (2023, MWR), we agree with you to explain our numerical criteria more adequately. In lines 30-34 of the revised manuscript, we have modified the corresponding statements as

“In particular, the study derived two ratios associated with numerical diffusion and numerical dispersion: the ratio of decay time with the SGS terms to that of the numerical diffusion error terms and that of phase speed due to the error in advection terms to that of the SGS terms. Moreover, we pointed out that the advection scheme requires at least seventh- or eighth-order accuracy to ensure that both ratios are less than 10^{-1} at wavelengths longer than eight grid lengths for grid spacing simulations of O(10) m.”

3. line 30: "which is recognized as a local spectral method" - what do you mean by "local spectral method" or can you cite something here?

Our meaning of the term “local spectral method” is that if we see the formulation of nodal DGM in the elementwise level, the discretization strategy looks like a spectral collocation method. For example, Chen et al. (2013) referred the spectral element method and DGM used in Giraldo and Restelli (2008) as a local spectral method. However, in the revised manuscript, we have removed this sentence because we do not consider it to be essential.

[References]

- Chen, X., N. Andronova, B. Van Leer, J. E. Penner, J. P. Boyd, C. Jablonowski, & S. Lin (2013): A Control-Volume Model of the Compressible Euler Equations with a Vertical Lagrangian Coordinate. *Monthly Weather Review.*, 141, 2526–2544. https://doi.org/10.1175/MWR-D-12-00129.1
- Giraldo, F. X., & M. Restelli (2008): A study of spectral element and discontinuous Galerkin methods

for the Navier–Stokes equations in nonhydrostatic mesoscale atmospheric modeling: Equation sets and test cases. *Journal of Computational Physics.*, **227**, 3849–3877. https://doi.org/10.1016/j.jcp.2007.12.009

4. line 35: Again this description of the previous result needs either simplifying or clarifying - as it is written it provokes questions: What was special about the case with upwinded numerical flux and sufficiently high order modal filter? What is "sufficiently high order" in this case? What happens more generally?

Thank you for your comment. The upwind numerical flux and modal filters are used to ensure numerical stability. The reason why we mentioned “sufficiently high order” is because the low-order filter can contaminate the flow structure at the wavelength range longer than the eight grid lengths even for a small decay coefficient of the filter. Based on your comment, we have modified the corresponding sentence as

“…when the upwind numerical fluxes and sufficiently scale-selective modal filters are used to ensure numerical stability.”

in lines 41-42 of the revised manuscript.

5. line 44: Clarify what you mean by "effective resolution significantly apart from the grid spacing". What is your definition of "effective resolution".

Our definition of “effective resolution” is the shortest wavelength fully resolved by discretization methods based on the dependence of numerical diffusion and numerical dispersion errors on the wavelength (e.g., Walters (2000); Kent et al. (2014)). Based on your comment, we have modified the corresponding statement as

“… shortest wavelength fully resolved by discretization methods, so called effective resolution, ...”.

in line 52 of the revised manuscript.

[References]

- Walters, M. K. 2000: Comments on “The differentiation between grid spacing and resolution and their application to numerical modeling”. *Bulletin of the American Meteorological Society*, *81*(10), 2475-2477. 10.1175/1520-0477(2000)081<2475:CAACOT>2.3.CO;2
- Kent, J., Whitehead, J. P., Jablonowski, C., & Rood, R. B. (2014). Determining the effective resolution of advection schemes. Part I: Dispersion analysis. *Journal of Computational Physics*, 278, 485-496. https://doi.org/10.1016/j.jcp.2014.01.043

6. line 46: "eight grid spacing" might be clearer as "eight grid lengths". Do you have a reference

for this claim?

Thank you for your suggestion. We have cited Kent et al. (2014). In lines 53-54 of the revised manuscript, we have modified the statement as

"The low-order spatial scheme typically leads to significant discretization errors at wavelengths shorter than eight grid lengths (Kent et al., 2014)."

[References]

Kent, J., Whitehead, J. P., Jablonowski, & C., Rood, R. B. (2014). Determining the effective resolution of advection schemes. Part I: Dispersion analysis. *Journal of Computational Physics*, 278, 485-496. https://doi.org/10.1016/j.jcp.2014.01.043

7. line 49: "than that in plane domains" the "that" is not needed

In the revised manuscript, we have removed "that".

8. line 50: "archive" - I think you mean "achieve"

We have modified it in line 58 of the revised manuscript.

9. line 52: "the pole problem" - add a short description of this for those not familiar with the issue.

In lines 60-61 of the revised manuscript, we have modified this statement as

"the problem of restrictive timestep near the poles due to the convergence of meridians".

10. line 53: "we can suffer..." the "we" here is confusing as I don't think you mean the work you are describing in the paper. I suggest rephrasing as "However, significantly high resolution global simulations can suffer from..."

Thank you for your suggestion. We have modified the statement as "However, high-resolution global simulations can suffer from.." in line 61 of the revised manuscript.

11. 1ine 58: "The Climate Machine..." What order is used in this model? How is what you have done different? This section describes a some other DG dycores but it is not clear how the work presented here differs from each one, especially as line 70-71 says "This study includes several progresses from previous studies..." Firstly, "progresses" is not quite the right word here. You could say "This study build on progress from previous studies..." or "This model includes several algorithmic features from previous studies..." but more importantly it needs to be clearer what "progresses" or "features" and what previous studies.

Although Sridhar et al. (2022) showed the results using the polynomial order p=4, we expect that arbitrary polynomial orders are available in the ClimateMachine based on their codes in https://github.com/CliMA/ClimateMachine.jl.

The spatial discretization used in SCALE-DG follows a nodal DGM (e.g., Hesthaven and Warbuton, 2007) and the discretization is mostly similar with that in previous studies with DG dynamical cores such as ClimateMachine and NUMA. Although this study has little novelty in the context of numerical methods of DGM, we consider that the following points to be our unique contributions:

1) Introduction of a turbulent model to a global DG dynamical core on cubed-sphere coordinates:

   To construct a global LES model, we formulated SGS eddy viscous and diffusion terms with a Smagorinsky-Lilly type turbulent model on cubed-sphere geometry in the DGM framework. Several previous studies (Ullrich, 2014; Guba et al., 2014) presented strategies for the vector Laplacian operator in element-based global shallow water models on the cubed-sphere coordinates. For our purpose of introducing the turbulent model, we treated the Laplacian operator acting on the component of vector fields in the cubed-sphere coordinates and the eddy viscosity dependent on local flow fields. Furthermore, we introduced the turbulence model to our DG dynamical core and verified its behavior by conducting an LES experiment of idealized planetary boundary layer turbulence.

2) Modification of test cases for high-order dynamical core:

   We modified existing test cases to investigate the numerical convergence associated with high-order dynamical cores. When using the totally second-order dynamical cores, due to relatively large discretization errors, the problem of ill-posed experimental setting might not be essential. However, we modified the experimental setup to evaluate numerical features, such as the convergence rate, of high-order dynamical cores.

3) Evaluation of numerical convergence with global dynamical core based on DGM:

   By conducting several standard tests, we quantitatively evaluated the numerical convergence of a global nonhydrostatic dynamical core based on DGM and indicated the high-order convergence rate. Although such investigations for regional DG dynamical cores can be found (e.g., Giraldo and Restelli, 2008; Bardar et al., 2013; Blaise et al., 2016), few studies are available on global DG dynamical cores.

Based on your comment, we have modified the corresponding statements in lines 86-88 of the revised manuscript as

"By building on progresses from the previous studies showing the applicability of the element-based methods to atmospheric flow simulations, this study attempted to develop a high-order global dynamical core using a nodal DGM both horizontally and vertically for future global atmospheric simulations with O(10–100 m) grid spacing."

Furthermore, we have modified the statement before mentioning our progresses in lines 89-92 of the revised manuscript as

"Although the numerical methods used in our dynamical core are similar to those used in previous studies that developed global DG dynamical cores such as NUMA and ClimateMachine, we consider that the following points are the unique contributions of the current study: 1)…"

Because we consider that the most important contribution is the development of a high-order global dynamical core for LES, we have mentioned it first in the three points.

[References]

- Blaise, S., Lambrechts, J., & Deleersnijder, E. (2016): A stabilization for three-dimensional discontinuous Galerkin discretizations applied to nonhydrostatic atmospheric simulations. *International Journal for Numerical Methods in Fluids*, *81*(9), 558-585. https://doi.org/10.1002/fld.4197
- Brdar, S., Baldauf, M., Dedner, A., & Klöfkorn, R. (2013): Comparison of dynamical cores for NWP models: comparison of COSMO and Dune. *Theoretical and Computational Fluid Dynamics*, *27*, 453-472. https://doi.org/10.1007/s00162-012-0264-z
- Giraldo, F. X., & Restelli, M. (2008): A study of spectral element and discontinuous Galerkin methods for the Navier–Stokes equations in nonhydrostatic mesoscale atmospheric modeling: Equation sets and test cases. *Journal of Computational Physics*, *227*(8), 3849-3877. https://doi.org/10.1016/j.jcp.2007.12.009
- Guba, O., Taylor, M. A., Ullrich, P. A., Overfelt, J. R., & Levy, M. N. (2014). The spectral element method (SEM) on variable-resolution grids: Evaluating grid sensitivity and resolution-aware numerical viscosity. *Geoscientific Model Development*, *7*(6), 2803-2816. https://doi.org/10.5194/gmd-7-2803-2014
- Ullrich, P. A. (2014): A global finite-element shallow-water model supporting continuous and discontinuous elements. *Geoscientific Model Development*, *7*(6), 3017-3035. https://doi.org/10.5194/gmd-7-3017-2014

12. line 76: "provide a chance to modify the experimental setup" - Make it clear that the standard

tests were modified for a particular reason related to your aims and the features of your method.

Based on your comment, we have modified the corresponding statements as

"We modified experimental settings of idealized test cases to demonstrate the numerical convergence with high-order dynamical cores."

in lines 106-107 of the revised manuscript.

13. line 79: "Even when the aim of this study..." I am not sure what this sentence means.

We would like to mean that an evaluation framework using high-order dynamical cores would be beneficial even when research interests are not in the dynamics. In line 110 of the revised manuscript, we have modified the statement as

"Even when research interests do not include the dynamics, …".

14. line 86: should be "cost required for numerical stabilization" (the "for" is missing)

We have added "for" it in line 96 of the revised manuscript.

15. line 88: "semi-discretization" should be "semi-discretized"

We have modified it in line 103 of the revised manuscript.

16. line 89: "which is an extension of..." - more detail would clarify the novelty, e.g. "which extends... by..."

Thank you for your comment. To clarify the novelty, we have modified the statement as

"In particular, we extended a numerical experiment of idealized planetary boundary turbulence used in regional plane models (KT2021 and KT2023) to spherical geometry by slightly changing the initial condition."

in lines 105-107 of the revised manuscript.

<u>Model description:</u>

17. line 101: "the Jacobian are denoted" - "are" should be "is". On line 104 you give the expression for the Jacobian of the vertical coordinate transformation - you could do the same here for the horizontal coordinate transformation, for consistency and clarity.

We have replaced "are" by "is" in line 130 of the revised manuscript. Moreover, as suggested, we have presented the expression for the Jacobian and metric tensor of the horizontal coordinate transformation as

"The horizontal Jacobian is defined as $\sqrt{G_h} = |G_h^{ij}|^{-1/2}$."

18. line 109: This clarification of the different notation for the coordinate variables is distracting - can you move it to where you actually use this different notation for the first time (I think in the next section (2.2), line 180 onwards)?

Thank you for your suggestion. However, because the notation (ξ^m) is used in the horizontal pressure gradient terms (in Eq. (8) of the revised manuscript), we think that it would be better to position the clarification near the statements that introduce the coordinates.

19. equation 1: should S_{SGS} also depend on grad(q) (as in line 137)?

Thank you very much for pointing out ∇q should be added as a dependent variable of S_{SGS}. We have corrected it in the revised manuscript.

20. line 151: "where $¥delta_S$ is an index..." I think it is a switch rather than an index.

Thank you for suggesting a better word. We have replaced “an index” by “a switch” in line 177 of the revised manuscript.

21. line 208: "effective horizontal grid spacing" - how is this related to the "effective resolution" you talk about elsewhere? Or is it just the spacing between the nodes? In which case, is "effective" the right word?

We agree with you that the term “effective grid spacing” is possible to confuse the term “effective resolution”. We are referring to a representative grid spacing which is equivalent to that in the grid-point methods. In lines 235-236 of the revised manuscript, we have modified the statement as

“We defined a representative grid spacing at the equator which approximately corresponds to that in the grid-point methods as …”

22. line 253: should the j be a second subscript of s? it looks like it isn't.

Thank you very much for suggesting our mistake. The second index of s should be an index of the element face associated with the gradient operator in the $\tilde{x}^j$-direction, i.e., f'. We have modified it in line 282 of the revised manuscript.

23. line 270: "severely restrict to the timestep" should be "severely restrict the timestep"

We have removed “to” in the revised manuscript.

24. line 319: Should ¥alpha_m be ¥alpha_i where i is the index of the highest mode? Or is m the index of the highest mode?

We apologize that we used inappropriate symbol for the decay coefficient for the modal filter and confused the readers. We should denote the symbol as α_m because we assume it is independent on the mode. In Eq. (41) of the revised manuscript, α_i has been replaced by α_m. We appreciate you pointing it out

We would like to inform you that the amplification factor with the cutoff matrix for the highest mode ($i = p$) is $\sigma_p = \exp(-\alpha_m)$ during one timestep. Thus, we described the corresponding decay time scale for the highest mode as $\Delta t/\alpha_m$ approximately.

Validation of dynamical core:

25. line 332: as "for this test case" after "Thus" to clarify. Also, do you mean "by focussing on the energy spectra"? This would be clearer if you define what you mean by "effective resolution" and how it relates to the spectra.

Thank you for the valuable suggestion. As mentioned in one of previous replies, our definition of the effective resolution is the shortest wavelength fully resolved by discretization methods. In the energy spectra, we investigated the shortest wavelength at which the spectra begin to separate from that in the reference experiment with the highest spatial resolution. In lines 375-376 of the revised manuscript, we have modified the statement as

"Thus, for the test cases, we mainly investigated the impact of the polynomial order on the shortest wavelength at which the energy spectra began to separate from that in the reference solution."

26. line 348: "we set to D=..." should be "we set D=..."

We have removed "to" in line 391 of the revised manuscript.

27. line 355: "Figure 1 shows the numerical errors..." from this I was expecting a spatial plot of the error - the figure shows the dependence of the errors at 12 days on horizontal resolution (as described in the caption) - this description should be in the text too. I'm not sure that the qualifying "about" is necessary in the next sentence.

In line 396 of the revised manuscript, we have modified the statement as

"Figure 1 shows the dependence of the L_1, L_2, and L_{inf} errors at 12 days on the horizontal spatial resolution."

In addition, we have removed "about" by modifying the corresponding sentence as

"…, we obtained p+1-order spatial accuracy for p=1, 3, and 7. For p=11, …"

28. line 360: "there is less difference between the angles" - less difference in what?

Our intended meaning is that the numerical errors for p=1 are almost independent of the angle

of the rotation axis. We have modified the statement in lines 401-402 of the revised manuscript as
“the numerical errors were almost independent of the angle of the rotation axis.”

29. line 367: "This study considered..." should be "This study considers..." or even "This study presents..."

Thank you for your suggestion. In line 421 of the revised manuscript, we have modified the statement as “This study presents...”.

30. line 375: "effective grid spacing" - is this the same as N_{e, h} as described in the previous test setup? Make sure you are consistent.

Please note that $N_{e,h}$ is the number of finite elements in the horizontal direction mentioned in Sect. 2.3. We agree that the description of spatial resolution should be consistent in Sect. 3. Thus, in the revised manuscript, we have removed the extra information that explained the number of DOF in the one-dimensional direction on one cubed-sphere panel.

31. line 378: "we set the Courant number against the..." What does this mean? "against" doesn't make sense here - do you mean the acoustic Courant number? Can you define C_{rh, cs} with a formula?

Thank you for your suggestion. We were referring to the acoustic Courant number associated with the horizontally propagating sound waves.

We agree to present the formula for defining the Courant number. We have added the definition in Sect. 2.4 as follows:

“We introduce two types of Courant number, which are used to explain the timestep setting in Sect. 3. For the horizontal advection test, the advective Courant number associated with the horizontal wind is defined as $C_{r,adv} = U_0 \Delta t / \Delta_{h,eq}$ where U_0 is the representative wind speed. For other numerical experiments, the acoustic Courant number associated with the sound wave propagation is defined as $C_{r,c_s} = c_s \Delta t / \Delta$, where Δ is the effective grid spacing; In particular, for the HEVI approach, $\Delta = \Delta_{h,eq}$.”
in lines 297-301 of the revised manuscript. Then, using this definition, we have modified the statement in Sect. 3.2 as
“For the HEVI scheme, we set the timestep such that $C_{r,c_s} = 1.34 \times 10^{-1}$ for p=1,3,7 and …”
in lines 431-432 of the revised manuscript.

32. line 402: "is well known..." Do you have any citations here?

Thank you for your suggestion. In line 456 of the revised manuscript, we have cited Zängl (2012) as a reference which mentioned the problem of the pressure gradient terms when using the basic terrain-following coordinate and the influence of discretization errors. In particular, the introduction section of Zängl (2012) has summarized the problem.

[Reference]

Zängl, G., 2012: Extending the Numerical Stability Limit of Terrain-Following Coordinate Models over Steep Slopes. *Mon. Wea. Rev.*, **140**, 3722–3733. https://doi.org/10.1175/MWR-D-12-00049.1

33. Figure 4: The x axis labels should be the same units in each figure.

We agree that it is better to use the same units in the figures. In Fig. 5(a) of the revised manuscript, we have determined to use [km] as the unit of the lateral coordinate and denoted the longitude using a secondary axis.

34. line 422: "at about z < 15km" - why "about"?

This is because a top elevation of the computational region unaffected by the vertical stretched grid is slightly different depending on the polynomial order and the number of vertical elements. Because it is not essential to our discussion about the numerical convergence and we mentioned the vertical stretching in the explanation of the sponge layer, we have removed this sentence.

35. line 423: "a fully explicit" - do you mean the HEVE scheme described earlier? If so, then refer to it.

Yes, we mean the HEVE scheme described in Sect. 2.4.2. In lines 476-477 of the revised manuscript, we have modified the corresponding statements as

"... RK scheme described in Sect. 2.4.2. For the HEVE scheme, we set the timesteps such that ..."

36. line 424: "against the" - same comment as earlier - do you mean acoustic Courant number?

Yes, we are referring to the acoustic Courant number. In lines 476-477 of the revised manuscript, we have modified the statement as

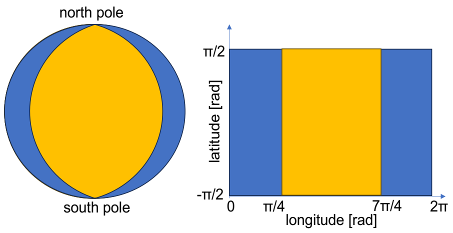
"For the third-order HEVE scheme, we set the timesteps such that $C_{r,c_s} = 2.63 \times 10^{-1}$."

37. line 422: "at about z > 15km" - why "about"?

The reason was described in our response to Comment 34. Because this statement referred to a sponge layer, we reconsider that the lowest altitude at which the decay coefficient is larger than zero is more important information. Thus, in lines 478 of the revised manuscript, we have modified the statement as

"… by introducing a sponge layer at z>15 km where the vertical element size linearly increases with the altitude."

38. line 426: "the 1/4 sector" - what is this?



R1: The distribution of lateral sponge layer which corresponds to the blue region.

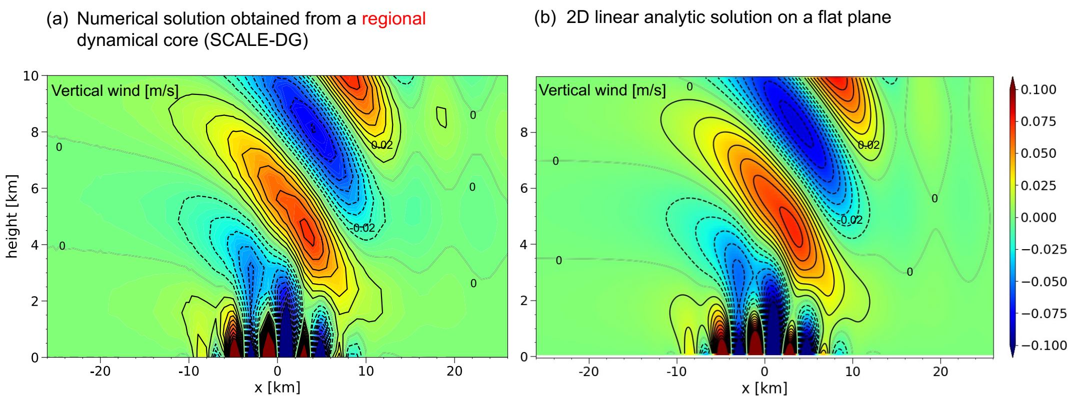
Thank you for pointing out the wording. We wanted to mean a spherical Lune with the dihedral angle of $\pi/2$ radian as shown in the blue region of Fig. R1. In the revised manuscript, we have removed the phrase because the detail of the lateral sponge layer is described in Appendix B.2 of the revised manuscript.

39. line 429: "to ensure the numerical stability" should be "to ensure numerical stability; "which are summarized" should be "which is summarized"

Thank you for your suggestion. We have modified it in lines 481-482 of the revised manuscript.

40. line 437: "We consider..." I'm not sure what this sentence means.

We carefully studied the difference between the numerical solution obtained by our global dynamical core (Fig. 4(a) of the previous manuscript) and the linear analytic solution on a flat plane (Fig. 4(b) of the previous manuscript). For example, the vertical wavelength of the large-scale wave for the global dynamical core is shorter compared to the linear analytic solution on a flat plane.



R2: The spatial distribution of vertical wind from a mountain wave test with a Schär mountain: (a) Numerical solution obtained from a regional dynamical core obtained from (Δ_h,Δ_v)=(625 m, 500 m), (b) Two-dimensional linear analytic solution on a flat plane.

When using our regional dynamical core based on the two-dimensional nonlinear Euler equations with the gravity on a flat plane, the obtained numerical solution well reproduced the linear analytic solution as shown in Fig. R2. Thus, we consider that a cause of the difference in

Fig. 4(a), (b) of the previous manuscript is related to the spherical geometry. If we increase the planetary radius while unchanging the spatial scale of the mountain, the two wave patterns would become closer.

In lines 489-492 of the revised manuscript, we have modified the corresponding statement as "For example, the vertical wavelength of the large-scale wave in Fig. 5(a) is shorter compared to Fig. 4(b). Based on the consideration using our regional dynamical core, a cause of the difference is related to the spherical experimental setup. Thus, we expect this difference to decrease as the planetary radius increases while the spatial scale of the mountain remains unchanged."

41. line 446: "in mid-latitude" should be "in the mid-latitudes"

We have modified this sentence in line 503 of the revised manuscript.

42. line 447: Remove "are included"

We apologize our carelessness. We have removed the extra words.

43. line 450-1: should be "the adiabatic inviscid primitive equations"

We have modified it in lines 507-508 of the revised manuscript.

44. line 455: "stretch" should be "stretched" and could you please specify what stretching you used?

In the baroclinic wave and Held-Suarez tests, a function form for stretching the vertical element size is similar to that used in Ullrich and Jablonowski (2012, JCP). We calculate the vertical coordinate ζ at the top element boundary of k'-th element as

$$\zeta_{k'+1/2} = z_T \frac{1}{\sqrt{b+1}-1}\left[\sqrt{b\,(\frac{k'}{N_{e,z}})^2+1}-1\right]$$

where b is a positive parameter and $N_{e,z}$ is the number of elements in the vertical direction. As b decreases, the vertical element size near the surface becomes small compared to the upper domain of the model. In this test case, we set b=20 for p=3, 7 and b=5 for p=11. In Sect. 3.4, we have described it briefly as

"We used a stretched vertical grid based on Eq. (102) in Ullrich and Jablonowski (2012). The stretching parameter was set such that the vertical grid spacing near the surface Δ_v took values of …"

in lines 512-514 of the revised manuscript. For the detail on the stretching strategy, we have described it in Appendix C of the revised manuscript.

[Reference]

- Ullrich, P. A., & Jablonowski, C. (2021): MCore: A non-hydrostatic atmospheric dynamical core utilizing high-order finite-volume methods. *Journal of Computational Physics*, 231(15), 5078-5108.. https://doi.org/10.1016/j.jcp.2012.04.024

45. line 456: "about 350m" - why about?

Thank you for your suggestion. In the previous manuscript, we wanted to write "about 350 m" as an averaged vertical grid size near the surface for p=3, 7, 11 (We apologize that there is a slight error in this value). In the revised manuscript, we decided to remove "about". To do so, we have modified the corresponding statement as

"… such that the vertical grid spacing Δ_v near the surface took values of 305 m, 523 m, and 426 m for p = 3, 7, and 11, respectively."

in lines 512-514 of the revised manuscript.

46. line 457: "against the" - same comment as before - is this the acoustic Courant number?

Yes, we mean the acoustic Courant number. In line 515 of the revised manuscript, we have modified the statement as

"… we set the timesteps such that $C_{r,c_s} = 1.68 \times 10^{-1}$ for p=3, 7 and $C_{r,c_s} = 1.26 \times 10^{-1}$ for p=11."

47. line 473: "for" should be "of the"

We have modified it in line 530 of the revised manuscript.

48. line 475: "cancellation of vertical errors" - I don't understand why this happens - is it really that clean? Can you explain more?

This investigation focused on the numerical convergence associated with the horizontal resolution. For this purpose, we left the vertical DOF unchanged while increasing the horizontal resolution for each polynomial order p. Thus, we expected that the vertical discretization errors remain almost identical in the experiments using different horizontal resolutions at the same p. If we regarded the highest resolution experiment using the same p as the reference experiment when evaluating the L_2 error, the contribution of vertical discretization errors would virtually cancel out.

Based on your comment, we think that it is better to move the explanation of this cancellation in the second paragraph of Sect. 3.4 into the description about the L_2 errors. In lines 533-534 of the revised manuscript, we have modified the corresponding statement as

"This is because the vertical spatial errors have similar values among different horizontal resolution cases with the same p and these errors virtually cancel out when the L_2 error is evaluated."

49. line 478: "sufficiently small compared to... for example" - you cite a specific example here so could you also give the magnitude in that specific example so the reader can compare themselves?

We agree with your suggestion. In lines 536-537 of the revised manuscript, we have mentioned the magnitude as

"For example, in the horizontal grid spacing of 50 km (0.5 degrees), the L_2 error was 1×10^{-2} hPa for the FV dynamical core and 5×10^{-3} hPa for Mcore (Ullrich and Jablonowski, 2012b)."

50. Figure 7: Make it clear in the caption that the top plot is the "reference" solution, or order the plots so that the resolution is increasing either moving upwards or downwards - at the moment it is confusing comparing the different figures.

Thank you for your suggestion. In Fig. 8 of the revised manuscript, we have reordered the panels such that the spatial resolution increases as we move downward.

51. line 496: "against the" - same comment as before, do you mean the acoustic Courant number?

Yes, we mean the acoustic Courant number. In lines 554-555 of the revised manuscript, we have modified the statement as

"… the acoustic Courant number of $C_{r,c_s} = 1.3 \times 10^{-1}$ for p=3,7 and $C_{r,c_s} = 7.56 \times 10^{-1}$ for p=11."

52. line 500: say that these are averaged fields.

We have added "zonally and temporally averaged" in line 558 of the revised manuscript.

53. line 501: "using nearly spatial resolution" - do you mean "nearly the same spatial resolution"? Please clarify.

Thank you for suggesting a better representation. We would like to say that the spatial distribution show in Fig. 9 is similar to that obtained from the numerical experiments with the horizontal grid spacing of ~200 km in the previous studies.

We have modified this sentence based on your suggestion as "nearly the same horizontal spatial resolution." in lines 559-560 of the revised manuscript.

54. line 509 and 510: "resolutions" should be "resolution" in both cases

We have corrected it in lines 568 and 569 of the revised manuscript.

55. line 510: "about 50km" - why about?

This is because we worried about the convergence behavior in the eddy heat flux and eddy momentum flux. However, since we specified the convergence for eddy temperature variance and eddy kinetic energy in this statement, we think the use of "about" is unnecessary. Thus, we have removed it in the revised manuscript.

56. line 513: "by" should be "using"

We have modified it in line 572 of the revised manuscript.

57. Figure 9: "averaging" should be "averaged"

We have replaced "averaging" by "averaged" in the caption of Fig. 10 of the revised manuscript.

58. line 523: "about 10-20 grids" - what does this mean? grid cells rather than grids?

We would like to mean the spectra for p=3 overlap with that of the reference experiment at a wavelength range longer than $10\Delta_{h,eq}$~$20\Delta_{h,eq}$. Using your suggesting term "grid lengths" before, we have modified the sentence as

"... at a wavelength range longer than 10~20 grid length."

in lines 582-583 of the revised manuscript.

59. line 524: "Thus, ..." Could you clarify this sentence? If you replaced "in relatively small polynomial order" with "when using lower polynomial order", would that say what you mean?

Thank you for your suggestion. We would like to mention that the strength of modal filters should be carefully set in $p \leq 3$ based on the energy spectra obtained from the Held-Suarez test. In line 585 of the revised manuscript, we have replaced the corresponding sentence by "when using $p \leq 3$."

60. line 540: "with 200" should be "of 200"

We have replaced "with" by "of" in line 599 of the revised manuscript.

61. Figure 12: What is the polynomial order and resolution for these results?

Thank you for pointing out that we should mention the polynomial order and spatial resolution in the caption. The figure was drawn using the results for $\Delta_{h,eq}$= 10 m using p=7. In Fig 13 of the revised manuscript, we have added the following statement:

"in the LES of an idealized planetary boundary layer turbulence for the case of $\Delta_{h,\mathrm{eq}}$= 10 m using p=7".

62. line 542: What is the form of the sponge layer? Refer to appendix A2 again.

We used a sponge layer based on a half-cosine function. In the sponge layer, the vertical wind was decayed by the Rayleigh damping with an e-folding time of 10 s at the model top. Because Appendix A.2 of the previous manuscript described the sponge layer used in the mountain wave, we have added a new statement in lines 602-603 of the revised manuscript as

"… using a sponge layer, where the vertical wind was decayed by the Rayleigh damping. The *e*-folding time varied as the half cosine function from zero at z = 2km to 10 s at the model top."

63. line 547: Do you mean the acoustic Courant number? Why "about" 0.438?

Yes, we are referring to the acoustic Courant number. By using the Courant number defined in Sect. 2.4 of the revised manuscript, we can remove "about".

In line 608 of the revised manuscript, we have slightly modified the corresponding statement as "We set the timesteps such that $C_{r,c_s} = 4.38 \times 10^{-1}$."

64. line 558: "eight grids" do you mean "eight grid cells"?

In line 619 of the revised manuscript, we have modified it as "eight grid lengths" to be consistent to our response of your comment for the line 523 of the original manuscript.

65. line 560: "required polynomial order is p>3" - required for what?; "which is true for results obtained in this study" - make it clear you have shown this by describing the difference for p=3.

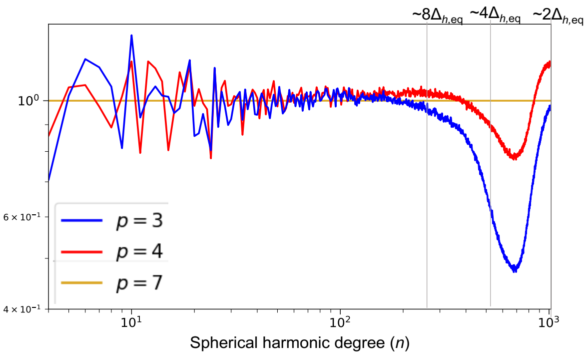


Fig. R3: Kinetic energy spectra normalized by the result of p=7 in LES of planetary boundary layer turbulence.

We would like to mean that p>3 is necessary to ensure that the effect of numerical diffusion term is sufficiently small compared to that of the SGS eddy viscosity term at the wavelength longer than eight grid lengths. We agree that the difference among p=3, 4, 7 needs to be shown more clearly. Accordingly, we determined the energy spectra normalized by the result of p=7 as Fig. R3. In the revised manuscript, we have showed such figures in Figs. 15(b) and C3(b) instead of the spectra normalized by the -5/3 power law. In addition, in lines 621-623 of the revised manuscript, we have modified the corresponding statements as

"… p>3 is required that the effect of numerical diffusion term is sufficiently small compared to that of the SGS eddy viscosity term at the wavelength longer than eight grid length. This is true for global LES as shown in Fig. 15(b)."

66. Figure 13: "averaging" should be "averaged"; "variable" should be "variance"

Thank you for pointing out our mistake. We have corrected it in the revised manuscript.

Conclusions:

67. line 563: "our previous studies" - cite specifically which ones.

In line 625 of the revised manuscript, we have cited Kawai and Tomita (2021, MWR).

68. line 567: "considering advantages" - what does this mean? do you mean that the DGM method has advantages? I don't think "considering" is the right word.

Thank you for pointing out the unclear statement. As you indicated, we wanted to mean that we focus on DGM because it has several advantages including a simple strategy for high-order discretization and higher computational efficiency in parallel computers. In line 629 of the revised manuscript, we have modified the statement as

"… because DGM has several advantages over grid-point methods, including …"

69. lines 593, 594 and 600: "grids" should, I think, be "grid cells"

Thank you for your suggestion. In lines 655, 656, and 663 of the revised manuscript, we have changed it as "grid lengths" for consistency with the corresponding modification.

Code and data availability:

70. "crate" should be create

We have corrected it in line 685 of the revised manuscript.

71. Could the data go on GitHub LFS?

We appreciate your suggestion. We understand that it is better that the output data would be in a public repository. However, the file size significantly exceeds the storage limit of GitHub LFS which is 1 GiB for a free account.

Appendix A:

We would like to inform you that Appendix A has become Appendix B in the revised manuscript because new appendices were added.

72. line 627: Can you cite the previous studies that did this?

In line 717 of the revised manuscript, we have cited two papers, Durran (1986) and Sachsperger et al. (2016).

[Reference]

Durran, D. R., 1986: Another Look at Downslope Windstorms. Part I: The Development of Analogs to Supercritical Flow in an Infinitely Deep, Continuously Stratified Fluid. *Journal of the Atmospheric Sciences*, **43**, 2527–2543, https://doi.org/10.1175/1520-0469(1986)043<2527:ALADWP>2.0.CO;2.

Sachsperger, J., Serafin, S. & Grubišić, V. (2016), Dynamics of rotor formation in uniformly stratified two-dimensional flow over a mountain. *Quarterly Journal of the Royal Meteorological Society*, **142**: 1201-1212. https://doi.org/10.1002/qj.2746

73. line 638: Say that alpha is given below.

In line 730 of the revised manuscript, I have added the sentence as "… which is provided in this subsection."

74. line 670: why does this equation go on to the next line and what is the dot at the start of line 671 for?

In the original manuscript, we broke the line in the middle of the equation to make it easier to distinguish between the longitudinal and latitudinal dependence of the coefficient $\alpha_{s,h}$. The dot means the sign of multiplication. Because we felt that the unnecessary line break could cause confusion, we have removed the line break in the revised manuscript.

Appendix B:

We would like to inform you that Appendix B has become Appendix D in the revised manuscript because new appendices were added.

75. line 730: "inertia subrange" should be "inertial subrange"

We have modified it in line 826 of the revised manuscript.

References:

76. Check formatting - some titles are in all caps.

In the revised manuscript, we have modified the titles in all capital letters.

Further notable modifications made in the revised manuscript

- In the linear advection test in Sect 3.1 of the previous manuscript, the axis angles with the solid-body rotation flow φ_0 were incorrectly set for the cases of φ_0=π/4, π/2. Using the results obtained from the modified experiment, we have replaced Fig. 1 in the revised manuscript. No qualitative changes were observed although the numerical errors for φ_0=π/2 become identical to that for φ_0=0. Thus, we have left most descriptions of the results unchanged.

- We added a new figure, Fig. 2 in the revised manuscript, which shows the impact of the modal filters on the numerical convergence in the linear advection test in Sect 3.1.
- In the Held-Suarez test, we adjusted the vertical element size for the case of $\Delta_{h,eq}$=280 km using p=7 for the vertical stretching to be consistent to all cases using p=7 and corrected the vertical grids for the case of $\Delta_{h,eq}$=52 km using p=3. In addition, we extended the temporal integration of the highest resolution case to 1000 days based on the suggestion of a reviewer. In Figs. 10, 11, and 12 of the revised manuscript, we have presented the results of the modified experiments. Fortunately, because no significant changes were observed, the claims made in the previous manuscript were left unchanged.
- We added a new appendix, Appendix A. Here, following the suggestions of a reviewer, we have described the results of a linear advection test, Case 4 presented in Nair and Lauritzen (2010).