Response to reviewer (egusphere-2024-1477 manuscript)

Dear Dr. Valeria Barra

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 reviewer's comments are provided below. We hope that these improvements satisfactorily address the issues pointed out by you.

General comments:

The paper presents a numerical dynamical core (dycore) for global non-hydrostatic atmospheric simulations. The numerical discretization uses the high-order discontinuous Galerkin method (DGM) both horizontally and vertically and targets global atmospheric simulations in the setting of large-eddy simulation (LES), with grid spacing of O(10–100 m). The paper presents several numerical experiments to verify the numerical framework adopted. The problems are scientifically important and the work seems to have been carried out with care. The scientific significance of the work and novelty compared to other DG dycores are the aspects that concern me the most. The grammatical correctness of the English language is also another aspect that would require further revisions.

We are grateful that you noticed the importance of the problems treated in our study. Before answering the specific comments, please allow us to refer to the scientific significance of the work and novelty compared to previous studies that developed DG dynamical cores. As other reviewer pointed out, the spatial discretization strategy used in SCALE-DG follows a nodal DGM (e.g., Hesthaven and Warbuton, 2007), which is mostly similar with that used in 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.

[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

Specific comments:

1. My main concern is with the aim and motivation of the work. Authors state that "Recently developed supercomputers have enabled us to conduct high-resolution global atmospheric simulations using a sub-kilometer horizontal grid spacing", without commenting or expanding on whether this should really be undertaken as a scientific endeavor. Just because we can, it does not mean that we should. The authors do not seem to outweigh the pros and cons of conducting such numerical simulations, especially in light of the carbon footprint and computational costs associated with said sub-kilometer scale simulations.

We agree that it is crucial to consider whether conducting high-resolution global atmospheric simulations with a sub-kilometer horizontal grid spacing has additional scientific value.

The accurate representation of convections is an essential part in global climate simulations. Miyamoto et al. (2013) conducted the global simulation with the horizontal grid spacing of 870 m where individual deep convections are explicitly represented using multiple grids and the cumulus parametrization was not used. In such simulation, it is expected to reduce uncertainties associated with the cumulus parametrization. The simulation achieved by using the K computer not only demonstrated the computational feasibility of global sub-meter resolution simulations using recent supercomputers, but also provided important scientific findings. For example, their grid-refinement experiments gave important knowledge about the numerical convergence of statistical properties of deep convections. In addition, by further analyzing the datasets obtained from the 870 m simulation, Miyamoto et al. (2015) elucidated the convection differences in various atmospheric disturbances including the Madden-Julia oscillations, tropical cyclones, mid-latitudinal low-pressure disturbances, and synoptic-scale front.

However, in the high-resolution simulation of Miyamoto et al. (2013), the spatial resolution is insufficient to explicitly represent the turbulent flows in boundary layers and the low-level clouds such as shallow cumuli and we need to rely on the parametrizations. Although such parametrization is crucial to reproduce the global radiation budget in the realistic Earth's atmosphere, they are a source of uncertainty in climate simulations. To decrease the uncertainty and accurately model the climate system, we cannot avoid increasing the spatial resolutions and replacing the parametrization by a strategy with less uncertain parameters, such as LES compared to RANS. In terms of conducting LES precisely, Kawai and Tomita (2021, 2023) claimed that high-order dynamical cores are needed.

The required computational resources are a serious issue as you pointed out. Although global simulations with horizontal grid spacing of 100~200 m appears feasible in exascale computers, this spatial resolution is still in the gray zone with the turbulence. For a "true" global LES with uniformly O(10 m) grid spacing, we will have to wait for the development of zeta-scale computers. To reduce the computational cost in the future global LES, it is necessary to consider sophisticated techniques such as a dynamically adaptive mesh refinement (e.g., Blaise and St-Cyr, 2012) and a super-parameterization (Grabowski, 2016).

Based on your comment, we have modified the statements in the first paragraph of Sect. 1 as "Recently developed supercomputers have enabled us to conduct high-resolution global atmospheric simulations using a sub-kilometer horizontal grid spacing. For example, Miyamoto et al. (2013) conducted a global simulation at a horizontal grid spacing of 870 m and discussed the numerical convergence of statistical properties of deep moist convections. In the near future, this continuous development of computer technology is expected to enable us to perform global simulations using O(10–100 m) grid spacing (Satoh et al., 2019), which begin to explicitly represent turbulence in the inertial sub-range. Then, large-eddy simulation (LES) is a promising strategy, since in LES, the turbulence in a spatial scale larger than a spatial filter is explicitly calculated, whereas the effect of turbulence in a smaller spatial scale is parameterized using eddy viscosity and diffusion terms. By explicitly representing the large-scale eddies in boundary layers and the low-level clouds such as shallow cumuli, we expect to reduce a source of uncertainty associated with the parameterizations and improve representation of global radiation budget in a realistic Earth's atmosphere."

[References]

- Blaise, S., & St-Cyr, A. (2012): A dynamic hp-adaptive discontinuous Galerkin method for shallow-water flows on the sphere with application to a global tsunami simulation. *Monthly Weather Review*, *140*(3), 978-996. https://doi.org/10.1175/MWR-D-11-00038.1
- Grabowski, W. W. (2016): Towards global large eddy simulation: Super-parameterization revisited. *Journal of the Meteorological Society of Japan. Ser. II*, *94*(4), 327-344. https://doi.org/10.2151/jmsj.2016-017
- Miyamoto, Y., Kajikawa, Y., Yoshida, R., Yamaura, T., Yashiro, H., & Tomita, H. (2013): Deep moist atmospheric convection in a subkilometer global simulation. *Geophysical Research Letters*, 40(18), 4922-4926. https://doi.org/10.1002/grl.50944
- Miyamoto, Y., Yoshida, R., Yamaura, T., Yashiro, H., Tomita, H., & Kajikawa, Y. (2015): Does convection vary in different cloud disturbances?. *Atmospheric Science Letters*, *16*(3), 305-309. https://doi.org/10.1002/asl2.558

2. In the literature review, the authors seem to miss to mention the The Nonhydrostatic Unified Model of the Atmosphere (NUMA), which also successfully used DGM

Thank you for your suggestion. We agree that we should refer to the NUMA as a nonhydrostatic global dynamical core based on the high-order element-based method. In lines 68-70, we have added a sentence as

"In the Nonhydrostatic Unified Model of the Atmosphere (NUMA; Kelly and Giraldo, 2012; Giraldo et al., 2013), which is applicable for both limited-area and global atmospheric simulations, the continuous and discontinuous Galerkin methods are adopted for the spatial discretization."

[References]

- Kelly, J. F., & Giraldo, F. X. (2012): Continuous and discontinuous Galerkin methods for a scalable three-dimensional nonhydrostatic atmospheric model: Limited-area mode. *Journal of Computational Physics*, 231(24), 7988-8008. https://doi.org/10.1016/j.jcp.2012.04.042
- Giraldo, F. X., Kelly, J. F., & Constantinescu, E. M. (2013): Implicit-explicit formulations of a three-dimensional nonhydrostatic unified model of the atmosphere (NUMA). *SIAM Journal on Scientific Computing*, *35*(5), B1162-B1194. https://doi.org/10.1137/120876034

3. The authors mention multiple times that they conduct classical numerical experiments to validate their numerical model. However, they seem to confuse Validation with Verification. In Numerical Analysis, the concepts of Verification and Validation (V&V) can be oversimplified in a succinct manner by saying that "verification is solving the equations right" (verifying the numerics) and "validation is solving the right equations" (verifying the physics - often done by comparing model results with actual data given by observations).

Thank you for pointing out our confusion between "validation" and "verification" and explaining the difference in the context of the numerical analysis. To obtain a correct understanding, we also checked the meaning of "V&V" in several Japanese documents. We think that "verification" is appropriate for representing the investigation of model performance using test cases for the dynamical cores. In the revised manuscript, we have replaced "validation" by "verification".

4. The authors seem to have chosen favorable examples/results and have not sufficiently provided explanations on reasons behind some degraded results, such as when a less than theoretical convergence rate was achieved. Also, the use of filters to overcome numerical

instabilities was not always comprehensively justified and their effect on the quality of the results was not extensively elaborated on.

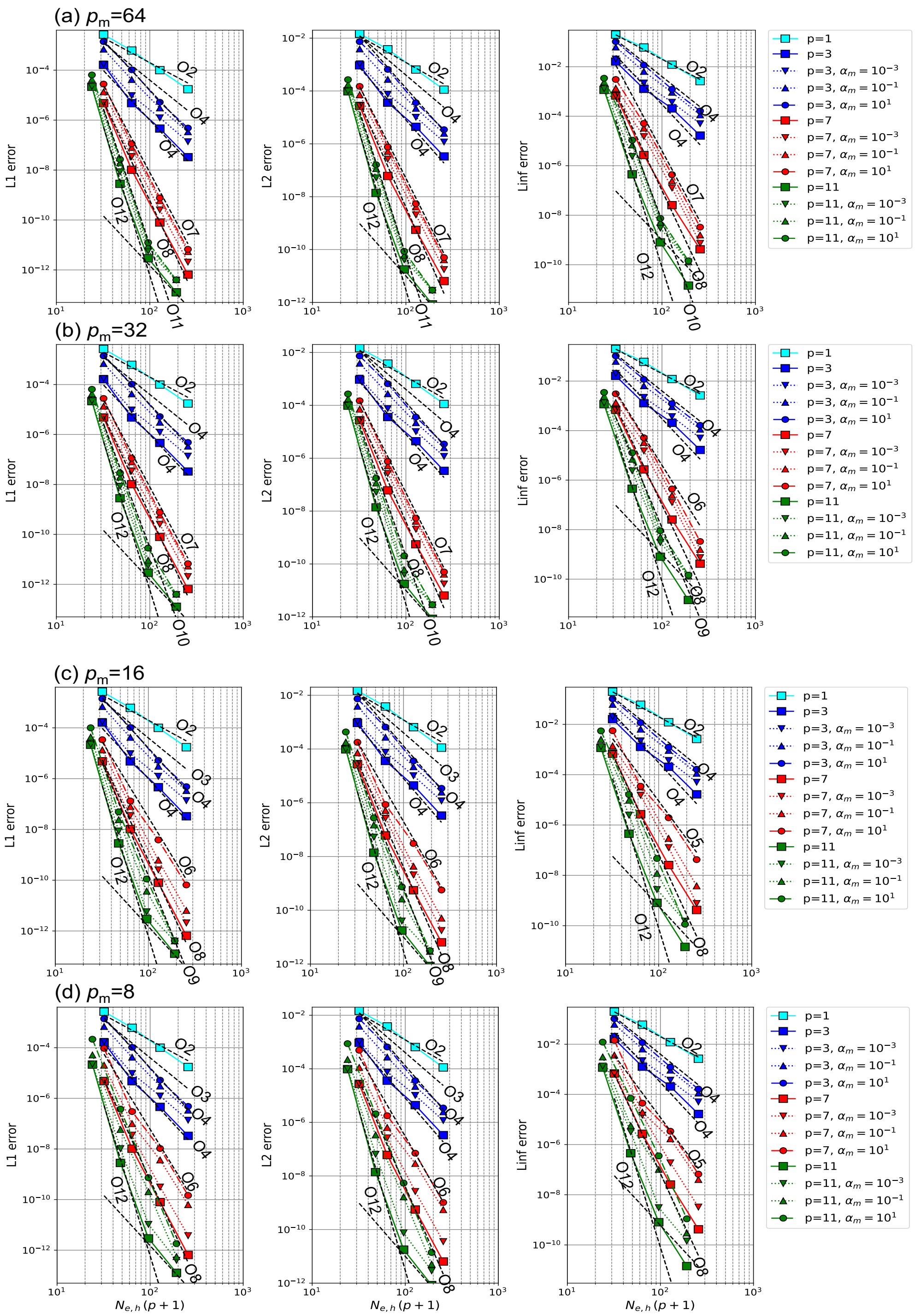

Fig. R1: Impact of the order p_m and the coefficient α_m in the modal filter on the numerical convergence in a linear advection test: (a) p_m=64, (b) p_m=32, (c) p_m=16, and (d) p_m=8. In each p_m, we changed α_m as 0 (without the filter), 10^{-3}, 10^{-1}, and 10^{1}. Please note that the results for α_m=0 are identical to those obtained for φ_0=0 in Fig. 1 of our paper.

Through several deterministic tests including the linear advection, gravity wave, and mountain wave tests, our purpose was to check the numerical convergence associated with high-order DGM. Thus, we focused on showing the convergence rate higher than that achieved by the second-order accuracy in the standard test cases. On the other hand, we agree that it is fair to present a situation where there is less benefit of high-order methods. Based on Comment 6, we have added the results for an advection test where a discontinuous tracer profile is advected by a deformation flow. Please see our response to Comment 6.

As for the explanation of the degraded results, it is preferable to elaborate the impact of modal filters on the convergence rate, as suggested by you. We had investigated how much the modal filters contaminate the eddy viscosity with the turbulent model and the energy spectra in Kawai and Tomita (2023). Here, we focused on the issue regarding the low convergence rate. We conducted a linear advection test described in Sect. 3.1 using the modal filters defined in Eq. (41) of the revised manuscript. Figure R1 shows the impact of order (p_m) and the decay coefficient (α_m) in the modal filter on the numerical convergence. The results indicate that the filters can degrade the original convergence rate and increase the numerical errors because the filter diminishes several high modes in the polynomial expansion. If we use high-order modal filters such as $p_m \geq 32$, the degradation of convergence rate was in the range of 1~3 for p=7, 11 even when we set sufficiently large values of α_m such that the highest mode was immediately decayed after one timestep. For p=3, the degradation of convergence rate appeared less obvious. However, we note that the errors without the modal filter were much larger compared to those observed in the case of p=7, 11. Thus, for p=3, the effect of the increased error due to the filters may be more pronounced in the representation of the flow fields. We have mentioned this investigation into the lines 406-415 and have added Fig. R1 as Fig. 2 in the revised manuscript.

5. Section 3, line 331: Can the authors elaborate a little more on why they consider "difficult" to directly evaluate the numerical convergence in those cases?

We would like to mean that the chaotic behavior of the nonlinear systems can diverge the numerical solutions in the long-termed climatological tests such as the Held-Suarez test. In turbulent simulations such as the boundary layer turbulence, smaller-scale structure of the flows appears as the grid spacing decreases (until the spatial resolution reaches the dissipation scale associated with the kinematic viscosity). Thus, it is difficult to evaluate the quality of the numerical solutions using the L_1, L_2, and L_{inf} error norms, which were appropriate for the deterministic tests such as the linear advection, gravity wave, and mountain wave tests.

In lines 371-375 of the revised manuscript, we have modified the corresponding statement as "… it is difficult to directly evaluate the numerical convergence using the error norms defined in Eq. (42). In the long-termed integration, the chaotic behavior of the nonlinear systems can diverge the numerical solutions. In the turbulent flow simulations, a smaller scale structure becomes more apparent as the grid spacing decreases until the spatial resolution reaches the physical dissipation scale."

6. In Sec 3.1 for the Linear Advection experiment, I wonder if the authors also verified their solver using a slotted cylinder example. This is often used in the literature because the sharp features of the geometry would particularly challenge the solver. I would appreciate if the authors would conduct such numerical experiments and would compare their convergence rates with the results reported in the literature, e.g., Guba et al. "Optimization-based limiters for the spectral element method" (2014) https://doi.org/10.1016/j.jcp.2014.02.029 looking at the results without limiters. In the same section, regarding the numerical results in Figure 1, the authors have not sufficiently explained why the case with $\alpha = 0$, i.e., no singularity in the coordinates on the cubed-sphere corners, in almost all cases presents larger numerical errors.

We would like to inform you that the purpose in Sect. 3.1 is to test the spatial discretization with the cubed-sphere geometry and check the convergence rate with high-order DGM in the smooth solution. We agree you that the linear advection tests with a discontinuous profile provide important information about the robustness of numerical schemes. For readers with such interests, we have conducted an advection test using the Gaussian hills and slotted cylinder profiles in a deformation flow, which is presented as Case 4 in Nair and Lauritzen (2010). In contrast to the case of the solid body rotation flow, the tracer profile is significantly deformed during one period.

[Results of Case 4 in Nair and Lauritzen (2010) as suggested by you]

Figure. R2 shows the dependence of error norms on the horizontal resolution when the Gaussian hills were given as the initial condition of the tracer. To compare the errors of Guba et al. (2014), hereinafter referred to as G2014, we normalized the errors following Appendix C of Nair and Lauritzen (2010). Because of the infinitely smooth profile, we obtained (p+1)-order accuracy for p=1, 3, 7, and 11 using the upwind numerical flux without the modal filter. The behavior of p+1-order numerical convergence and the magnitude of errors are comparable to G2014 (see the values of l_1, l_2, l_∞ for "Gauss." with the hyperviscosity (but without the limiter) in Tables 1-4 and Fig. 4 of G2014). In our results, the L_{inf} error was larger than the unity for p=11 in the coarsest spatial resolution. This reflects a numerical instability with the aliasing errors near the static stagnation point of the deformation flow. It occurred when the static stagnation

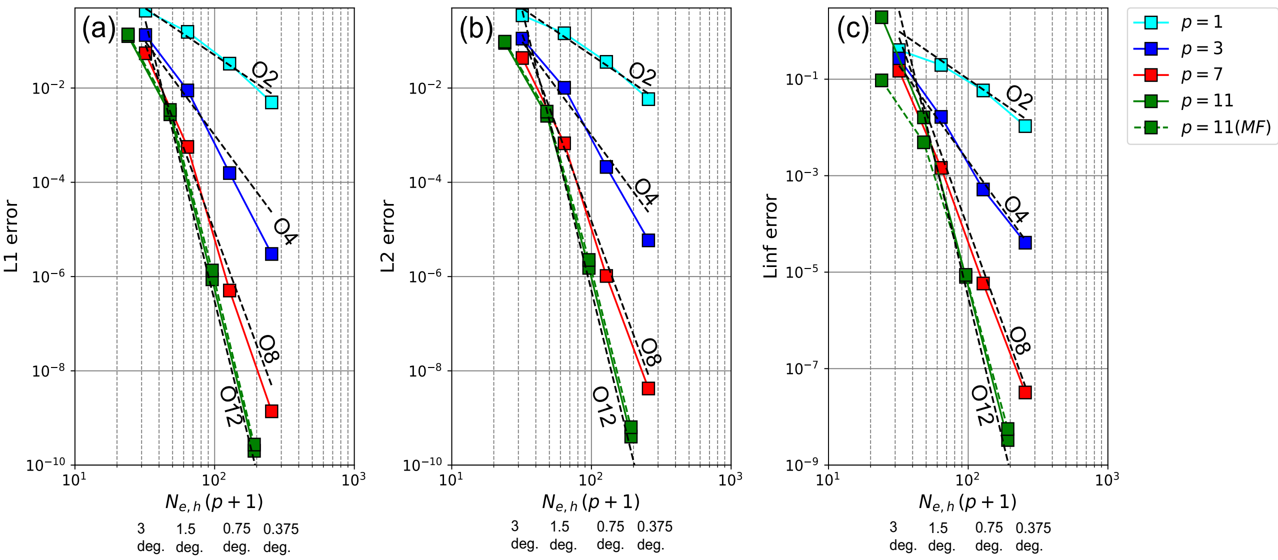

Fig. R2: Dependence of (a) L_1, (b) L_2, and (c) L_{inf} errors after t=12 days on the horizontal resolution in the Case 4 in Nair and Lauritzen (2010) with the Gaussian hills. The green dashed line represents the case of applying a modal filter for *p*=11.

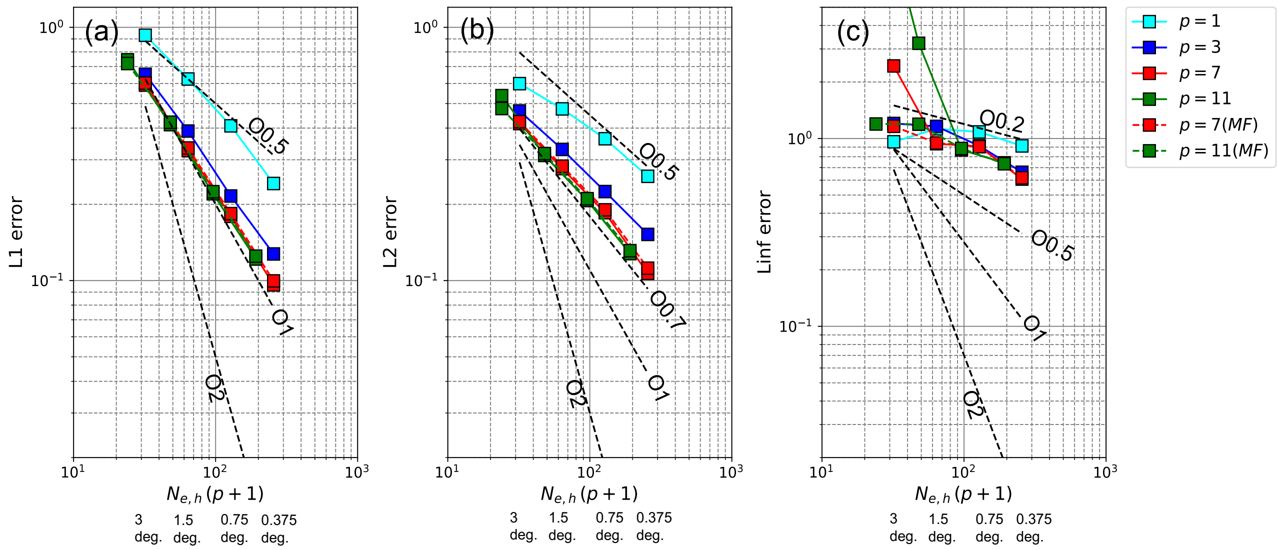

Fig. R3: Dependence of (a) L_1, (b) L_2, and (c) L_{inf} errors after *t*=12 days on the horizontal resolution in the Case 4 in Nair and Lauritzen (2010) with the slotted cylinders. The green dashed line represents the case of applying a modal filter for *p*=11.

point was located at the element boundaries and numerical dissipation was not sufficient. By introducing a very weak modal filter with $\alpha_{m,h} = 2.5 \times 10^{-2}$, $p_{m,h} = 64$, we can control the numerical instability (see the green dashed line in the L_{inf} error of Fig. R2).

Figure R3 shows the dependence of error norms on the horizontal resolution when the slotted cylinders were given as the initial tracer profile. Because of the C^0 discontinuous field, we cannot expect the convergence rate to be higher than that in the case of first-order accuracy.

Even when using the high polynomial orders (p>3), we obtained at most the first order for the L_1 error norm. For the $L_{\rm inf}$ error, the convergence rate was near the zero order. The behavior of slow numerical convergence and the magnitude of numerical errors were similar to those reported in G2014 (see the error norms for "Cyl." with the hyperviscosity (but without the limiter) in Tables 1-4 of G2014). As seen in the case of Gaussian hills, due to the numerical instability near the static stagnation point, we found very large $L_{\rm inf}$ errors for the cases of p=7, 11 with the coarse spatial resolution. By adopting the modal filter used in the case of Gaussian hills, the numerical instability was suppressed. The corresponding results are represented by the dashed lines in Fig. R3.

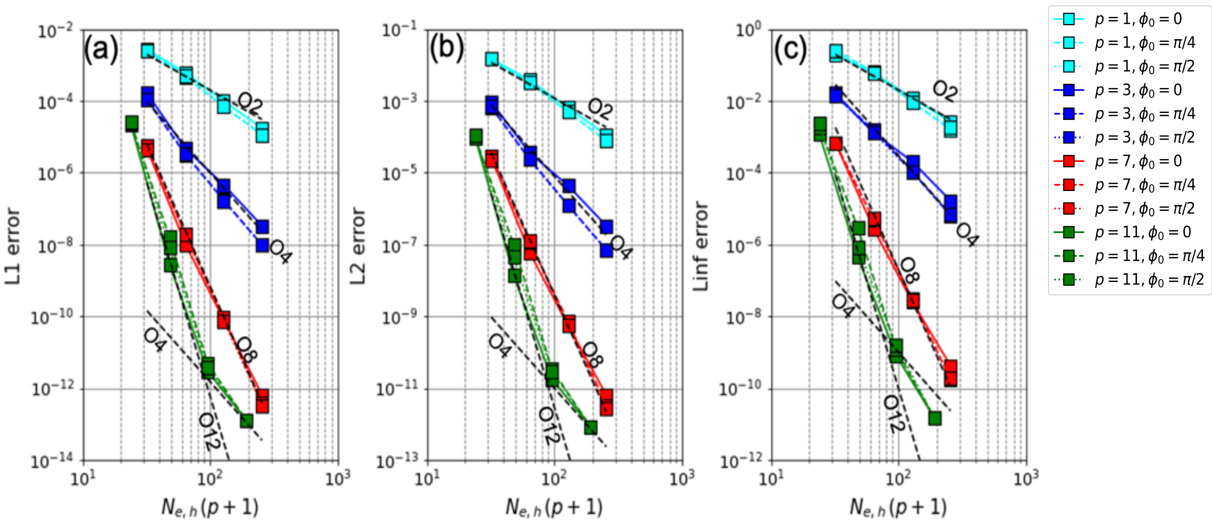

Fig. R4: Fig.1 in the previous manuscript which represented the dependence of (a) L_1, (b) L_2, and (c) $L_{\rm inf}$ errors after t=12 days on the horizontal resolution in a two-dimensional linear advection. However, we incorrectly set the axis angles with the solid-body rotation flow for the cases of φ_0=π/4, π/2.

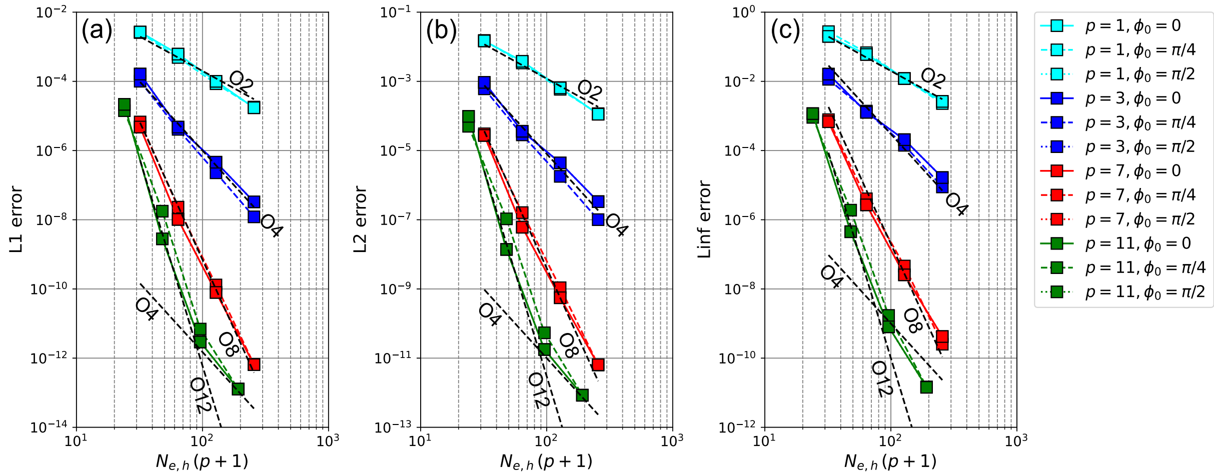

Fig. R5: The modified version of Fig. R4 where we correctly set the axis angle with the solid-body rotation flow.

Based on your suggestion, we presented the above mentioned results in Appendix A of the revised manuscript.

[Results of linear advection experiment discussed in Sect. 3.1]

Next, we refer to your comment regarding Fig. 1, which is presented as Fig. R4 in this document. First, we apologize that the axis angles with the solid-body rotation flow were incorrectly set for the cases of $\varphi_0=\pi/4$, $\pi/2$. The results obtained from the modified experiment are shown in Fig. R5. The numerical errors for $\varphi_0=\pi/2$ were identical to those obtained for $\varphi_0=0$. The change is intuitively plausible because, for $\varphi_0=0$, $\pi/2$, there is no essential difference in the movement of advected quantities in the cubed-sphere geometry. For $\varphi_0=0$, $\pi/2$, the numerical errors were still larger compared to $\varphi_0=\pi/4$ when $p=3$ was used. Note that the error norms shown in Fig. R5 are the values at a certain time. Figure R6 shows the temporal series and the moving average of L_2 error norm for the case of $\Delta_{h,eq}=78$ km for $p=1$, 3, and 7 and $\Delta_{h,eq}=104$ km for $p=11$. As you mentioned, the moving average indicates that the numerical errors for $\varphi_0=0$, $\pi/2$ tend to be larger than those obtained for $\varphi_0=\pi/4$. Similar results are seen in previous studies (e.g., see Table 2 of Ullrich et al. (2010)).

Although we investigated the temporal evolution and spatial distribution of the errors, we have not yet confirmed the reason. As one possible reason, the change in the grid aspect ratio on the tracer path is small for $\varphi_0=\pi/4$ compared to $\varphi_0=0$, $\pi/2$ (see the case of gnomonic projection in Fig. 3 of Rančić et al. (2017)). However, because this is a matter of speculation, we would like to keep the description in the manuscript to the fact that the corner singularity with the cubed-sphere coordinates is well handled without any numerical instability in the DGM framework. In lines 402-403 of the revised manuscript, we have modified the corresponding statement as "The errors for $\varphi_0=\pi/4$ radians can be smaller than those observed for $\varphi_0=0$, $\pi/2$ radians (e.g.,

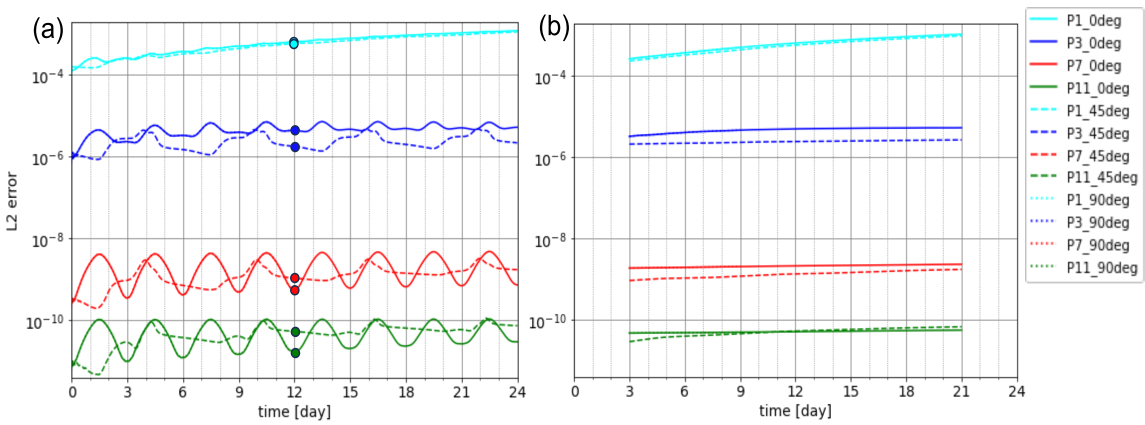

Fig. R6: (a) Time series of the L_2 error obtained from $\Delta_{h,eq}=78$ km for $p=1$,3, and 7 and $\Delta_{h,eq}=104$ km for $p=11$ in a two-dimensional linear advection. (b) the moving average for 6 days. Note that the solid and dashed lines represent the cases of $\varphi_0=0$, $\pi/4$, respectively.

p=3). The reason has not been confirmed, but we have found similar results in previous studies (Ullrich et al., 2010)."

[References]

- Guba, O., Taylor, M., & St-Cyr, A. (2014): Optimization-based limiters for the spectral element method. *Journal of Computational Physics*, 267, 176-195. https://doi.org/10.1016/j.jcp.2014.02.029
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- Ullrich, Paul A., Christiane Jablonowski, & Bram Van Leer. (2010): High-order finite-volume methods for the shallow-water equations on the sphere. *Journal of Computational Physics*, 229, 6104–6134. https://doi.org/10.1016/j.jcp.2010.04.044

7. Sec 3.3, line 443: authors mention the modal filter as one potential reason for the degraded sub-optimal convergence. Shouldn't it help instead? Can they elaborate on this further?

As we described in the reply to Comment 4, the modal filters contribute to prevent the numerical instability, while also contaminating the numerical accuracy in calculating the smooth flow fields. The essential cause is that the filters shave off the high modes in the elementwise polynomial expansion. In Appendix A3 of previous manuscript, we indicated the degraded suboptimal order due to the modal filters. When requiring a convergence rate with a certain order of the accuracy, we need to increase the polynomial order according to the intensity of the filters.

Based on your comment, we have added statements as
"Because the modal filters shave off the high modes in the polynomial expansion, the convergence rate can be degraded. When requiring a convergence rate with a certain order accuracy, we need to increase the polynomial order according to the filter intensity."
in lines 499-501 of the revised manuscript.

8. Section 3.5, Caption of Figure 11: Can the authors explain why they presented numerical results for the highest resolution case with a temporal average over only 300 days as opposed to 1000 days for the other cases? Was it too computationally expensive to perform the highest resolution simulation over 1000 days, or the model presented difficulties over 300 days, such

as it suffered from numerical instabilities/crashes?

The initial reason for the integration time in the highest resolution experiment case shorter than that in the other cases is due to the computational resources available to us. We agree that it is better to show the results based on the simulation over the 1000 days. Thus, we extended the temporal integration of highest resolution experiment case.

Unfortunately, to continue the stable temporal integration, we need to temporarily increase a horizontal coefficient of the modal filter by 20 % for 20 days during the 1000-day integration. In the revised manuscript, the fact has been mentioned in the caption of Table 9. In addition, using the obtained results, we have replaced the corresponding figures (Figs. 11 and 12). Because no qualitative change was observed, we have retained our claim in the manuscript.

Technical corrections:

First, the authors are deeply grateful for many suggestions regarding our grammatical issues.

1. Line 5: “the impact of high-order DGM on atmospheric flows was investigated”. I would rephrase this with another sentence along the lines of: “the impact of high-order DGM on the quality or accuracy of the numerical simulations of atmospheric flows was investigated”

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

   “… the impact of high-order DGM on the accuracy of the numerical simulations of atmospheric flows was investigated.”

   in line 5 of the revised manuscript.

2. Line 16: “In the near future”

   We have modified this in line 18 of the revised manuscript.

3. Line 18: “Then, large-eddy simulation (LES) is a promising strategy, since in LES”

   We have modified this in line 21 of the revised manuscript.

4. Line 33-34: Rephrase “In the context of DGM, KT2023 investigated the problem with the order of accuracy necessary for LES”

   Thank you for pointing out an unclear statement. We have revised this statement as

   “KT2023 extended the discussion presented in KT2021 to the DGM framework and investigated a polynomial order necessary for precisely conducting LES.”

   in lines 40-41 of the revised manuscript.

5. Line 35: Add plural for generic or non specific countable nouns in English, i.e., “modal filters

are used", or add an article if you want to use singular nouns

Considering the intent here, we have modified "filter" to the plural in line 42 of the revised manuscript.

6. Line 36-37: Authors mention "2000-2010" but then they survey literature belonging to the following decade

   We apologize for the error in the wording of the age. We should write it as "2000's-2010's". We have modified it in lines 43-44 of the revised manuscript.

7. Line 64: Please introduce the FDM acronym before using it

   Thank you for pointing it out. The term "finite difference method" appears only once, so we have decided not to use the abbreviation in the revised manuscript.

8. Line 73: "the impact of high-order DGM on the atmospheric flows". I would rephrase this, similar to the Abstract sentence.

   Based on your suggestion, we have revised the corresponding statements as "... the impact of high-order DGM on the numerical accuracy of atmospheric flow simulations" in lines 113-114 of the revised manuscript.

9. Line 71-72: "We focused" and then "We attempt". Please check grammar consistency of temporal tenses throughout the text

   Thank you for pointing out the tense inconsistency in the sentences. The past tense should be used here, and we have modified it in line 112 of the revised manuscript. In addition, we have reconsidered the temporal tense throughout the text.

10. Line 86: "required" -> "requiring"?

    Thank you for your suggestion. We apologized that "for" was missing as other reviewer pointed out. We have corrected it in line 96 of the revised manuscript.

11. Line 155: "angular velocity of the planet"

    We have modified it in line 181 of the revised manuscript.

12. Line 156: "In the numerical experiments".

    We have added an indefinite article it in line 182 of the revised manuscript.

13. Line 172: "is essentially the same as"

We have added an indefinite article in line 198 of the revised manuscript.

14. Line 175: “In the absence of a vertical”

    We have modified it in line 201 of the revised manuscript.

15. Line 183: “D is the divergence of the three-dimensional velocity”

    We have added an indefinite article in line 209 of the revised manuscript.

16. Line 194: “For further details on the turbulence model”

    We have added an indefinite article in line 219 of the revised manuscript.

17. Line 227: “For the numerical flux of the inviscid terms”

    We have added an indefinite article in line 256 of the revised manuscript.

18. Line 229: reword “considered”

    We have replaced this word by “taken into account” in line 259 of the revised manuscript.

19. Line 230: “transformations, and is formulated as”

    Thank you very much for your comment and we have reconsidered this sentence. In lines 258-259 of the revised manuscript, we have modified it as

    “Previous studies (e.g., Li et al., 2020) formulated the Rusanov flux taken into account the horizontal and vertical coordinate transformation as ..”

20. Line 270: “restrict the time step” (remove “to”)

    We have removed to “to” in line 304 of the revised manuscript.

21. Line 280: “in the case of the diagonally implicit RK scheme”

    We have added an indefinite article in line 315 of the revised manuscript.

22. Line 288: “To obtain the solutions of the nonlinear equation system”

    We have added an indefinite article in line 322 of the revised manuscript.

23. Line 291: “In the case of the collocation approach”

    We have added an indefinite article in line 325 of the revised manuscript.

24. Line 301: “When using the HEVI approach”

We have added an indefinite article in line 335 of the revised manuscript.

25. Line 302: "entries of the matrices"

We have added an indefinite article in line 336 of the revised manuscript.

26. Line 304: Rephrase with: "For high-order methods, numerical instability is likely to occur in advection-dominated flows, because the discrete advection operator is oscillatory."

We have reconsidered the statement. In line 339-340 of the revised manuscript, we have modified it as

"For high-order DGM, numerical instability likely to occur in advection-dominated flows because the numerical dissipations with the upwind numerical fluxes weaken."

Although you suggested the statement "because the discrete advection operator is oscillatory", we would like to mention that the inherent numerical dissipations with the upwind numerical fluxes are weak at a wavelength range longer than 2~4 grid lengths when a large polynomial order is used. Thank you very much for your suggestion.

27. Line 310: Remove "represents" or "is"

We apologize the careless mistake. We have modified it in line 345 of the revised manuscript.

28. Line 317: "the order of the filter"

We have added an indefinite article in line 351 of the revised manuscript.

29. Line 318: "at the final stage of the RK scheme"

We have added an indefinite article in line 352 of the revised manuscript.

30. Line 320. Rename Section 3 "Verification of the dynamical core"

We have used "verification" in the section title.

31. Line 322: "we mainly focused on the impact of the polynomial order on the effective". There are several missing articles throughout the text. I stopped correcting all of them after some point. The authors should more carefully proof-read for English correctness.

We have added an indefinite article in line 375 of the revised manuscript. Thank you for suggesting our manuscript need further proof-read for English correctness. In the revised manuscript, we have reconsidered the use of articles. Then, the modifications have been checked by Editage.

32. Line 339: I know α is used in the literature to denote the angle between the axis of the solid body rotation and the North pole. However, the authors should be careful because they also previously used (α, β, ζ) for the local coordinates on the cubed-sphere.

    Thank you for pointing out we should change the symbol for denoting the angle between the axis of the solid body rotation and the North pole. In the revised manuscript, to denote it, we have used φ_0. In addition, we have replaced the symbol of the latitude by φ.

33. Line 379: "errors"

    We have modified it in line 432 of the revised manuscript.

34. Line 381: "a modal filter" and "was investigated"

    We have modified them in line 434 of the revised manuscript.

35. Line 392: "except for the horizontal wind"

    We have added "for" in line 445 of the revised manuscript.

36. Line 427: "details on the sponge layer"

    We have modified it in line 480 of the revised manuscript.

37. Line 446-447 avoid repetition of "includes" and "included" in the same sentence by using a synonym

    Thank you for pointing out the repetition. We have modified the corresponding statement as "It includes small-scale structures such as front and filament formations."
    in lines 503-504 of the revised manuscript.

38. Line 455: "stretched"

    We have modified it in line 512 of the revised manuscript.

39. Line 473: "evaluation of the horizontal resolution"

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

40. Line 501: "by using similar spatial resolution"

    Thank you for your suggestion and other reviewer also commented this statement. We have modified this sentence as "nearly the same horizontal spatial resolution" in lines 559-560 of the revised manuscript.

41. Section A3: reword the section title "Investigation on the degradation of the optimal numerical convergence"

Thank you very much for indicating a better representation. We have modified the section title in the revised manuscript.

42. Figure A2: remove bold text in caption

We have changed it to a normal style in the revised manuscript.

43. Figure A3: remove bold text in caption

We have changed it to a normal style in the revised manuscript.

44. Line 741: I am sorry, but even in the acknowledgement sentence in which the authors thank the company they used for the English editing, there is a grammatical error "We would like to thank Editage for the English language editing"

We apologize the grammatical issues in the manuscript. In the revised manuscript, we have made further effort for English correctness and the proofreading by Editage has been done again. We greatly appreciate your comments.

Further notable modifications made in the revised manuscript

- 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. In Figs. 10, 11, and 12 of the revised manuscript, we have presented the results of the modified experiments. Fortunately, because there were no essential changes, we did not change the claims made in the previous manuscript.
- We added a new appendix, Appendix C. Here, we have described the vertical stretching used in the baroclinic wave and Held-Suarez tests.