Response to topic editor (egusphere-2024-1477 manuscript)

Dear Dr. Sylwester Arabas,

Thank you for addressing reviewers' remarks and congratulations for receiving "Excellent" marks for Scientific quality from both Referees.
I am accepting the paper requesting the following technical corrections:
- please follow Referee #1's comments,
- please correct seven occurrences of bogus "https://doi.org/https://doi.org" URLs in references.

Thank you for providing your thoughtful comments. We also appreciate the Referee#1's review for improving our paper. Based on Referee#1's comments, we have modified the manuscript as mentioned below. In addition, we have corrected seven URLs in references.

Referee#1's comments

General comments:

The authors have significantly improved the manuscript and addressed most of the reviewers' comments. However, the scientific significance of the work and novelty compared to other DG dycores, in my opinion, remain the weak points of this paper.

We are grateful to notice the improvements compared to the previous manuscript. In the sight of numerical aspect of atmospheric dynamical cores toward global LES, we consider that completing our model description paper of DG dynamical core is an important step following global simulations with sub-kilometer horizontal spacing, such as Miyamoto et al. (2013, GRL). By building on progresses from previous studies with the element-based methods, we have constructed a global dynamical core based on DGM for LES. By quantitatively investigating numerical performance through various test cases, we have advanced the fundamental understanding of numerical behavior of global nonhydrostatic dynamic cores using conventional DGM for both horizontal and vertical discretization. Based on the findings and using a set of simulation codes developed here, we will attempt to improve DGM to optimize for atmospheric flows also considering dynamics-physics coupling strategies. We would like to enhance the uniqueness of our model by such research activities.

Specific comments:

The authors have acknowledged and cited previous studies in their revised manuscript's Introduction and listed what they believe are their unique contributions (lines 92-116) in three key points. However, none of the three points is a real breakthrough. They mentioned other studies who have followed the same approaches before, without really emphasizing why their proposed approach should be better.

Although we appreciate and understand most of reviewer#1's opinions, we believe there are certain contributions as we mentioned above.

We focus on the first key point, which mentioned numerical approaches following several previous studies, since the second and third key points in lines 107-116 of the previous manuscript mentioned the modification of experimental setup to evaluate high-order numerical convergence and the investigation of numerical performance of global DG dynamical core, respectively. We agree that it is better to add information on why the approaches are necessary when introducing a turbulent model to our global DG dynamical core. In lines 100-104 of the revised manuscript, we have modified the corresponding statements as

"However, we did not directly use these approaches to the vector Laplacian when introducing the turbulent model. This is because we need to treat eddy viscous and diffusion coefficients dependent on local wind shear and stratification. In addition, we consider that the vector Laplacian operator applied to the vector component on the cubed-sphere coordinates can be convenient for straightforward distinction between horizontal and vertical directions. Using tensor analysis, …".

Also, it would be nice if the authors mentioned which configuration in terms of numerical settings and choice of dissipation mechanisms they plan to use in the final "operational" version of SCALE-DG, even if subject to change in the future.

We agree that it is better to mention possible numerical setting and choice of dissipation mechanism in operational runs. To stabilize realistic simulations, we may need to increase the strength of modal filters, as discussed in our response to other reviewer's comment. However, it is difficult to generalize the strength of dissipation mechanisms because it depends on the extent of smoothing the topography and the dissipation with turbulent models. Thus, in lines 685-687 of the revised manuscript, we have added statements as

"For actual operational runs including physical processes and other factors, such as realistic topography, the degree of numerical filters depends on situations and cannot be generalized now. It is an important issue to produce a kind of criterion for the numerical stabilization."

Technical comments:

Line 100: Change "For introducing the turbulent model" with "To introduce the turbulent model"

Because we made the modification mentioned above, we have changed the corresponding statement as “when introducing the turbulent model”.