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.

I am in general in favor of acceptance to GMD, provided the authors can convincingly respond to the comments.

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.
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
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).
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.
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?
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 α = 0, i.e., no singularity in the coordinates on the cubed-sphere corners, in almost all cases presents larger numerical errors.

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?
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?

Technical corrections (the referee will use italic font for addition to the quoted text where appropriate):

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"
2. Line 16: "In *the* near future"
3. Line 18: "Then, large-eddy simulation (LES) is a promising strategy, *since in* LES"
4. Line 33-34: Rephrase "In the context of DGM, KT2023 investigated the problem with the order of accuracy necessary for LES"
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
6. Line 36-37: Authors mention "2000-2010" but then they survey literature belonging to the following decade
7. Line 64: Please introduce the FDM acronym before using it
8. Line 73: "the impact of high-order DGM on the atmospheric flows". I would rephrase this, similar to the Abstract sentence.
9. Line 71-72: "We focused" and then "We attempt". Please check grammar consistency of temporal tenses throughout the text
10. Line 86: "required" -> "requiring"?
11. Line 155: "angular velocity of *the* planet"
12. Line 156: "In *the* numerical experiments".
13. Line 172: "is essentially *the* same as"
14. Line 175: "In the absence of *a* vertical"
15. Line 183: "D is the divergence of *the* three-dimensional velocity"
16. Line 194: "For further details *on the* turbulence model"
17. Line 227: "For the numerical flux of *the* inviscid terms"
18. Line 229: reword "considered"
19. Line 230: "transformations*, and* is formulated as"

20. Line 270: “restrict the time step” (remove “to”)
21. Line 280: “in *the* case of the diagonally implicit RK scheme”
22. Line 288: “To obtain the solutions of *the* nonlinear equation system”
23. Line 291: “In the case of *the* collocation approach”
24. Line 301: “When using *the* HEVI approach”
25. Line 302: “entries of *the* matrices”
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.”
27. Line 310: Remove “represents” or “is”
28. Line 317: “the order of *the* filter”
29. Line 318: “at the final stage of *the* RK scheme”
30. Line 320. Rename Section 3 “*Verification* of *the* dynamical core”
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.
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.
33. Line 379: “error*s*”
34. Line 381: “*a* modal filter” and “*was* investigated”
35. Line 392: “except *for* the horizontal wind”
36. Line 427: “details *on* the sponge layer”
37. Line 446-447 avoid repetition of “includes” and “included” in the same sentence by using a synonym
38. Line 455: “stretch*ed*”
39. Line 473: “evaluation *of the* horizontal resolution”
40. Line 501: “by using *similar* spatial resolution”
41. Section A3: reword the section title “Investigation on the degradation of the optimal numerical convergence”
42. Figure A2: remove bold text in caption
43. Figure A3: remove bold text in caption
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”