1. OVERALL REVIEW

The manuscript is well-written, the derivations are clear, and the arguments are coherent. However, some major issues need to be addressed to verify and demonstrate the properties of the new remapping scheme variants introduced here.

The fundamental mismatch in the derivation presented here occurs due to an incorrect transformation from a spherical coordinate system to a cylindrical projection onto a 2D logical plane. Since the discussions are primarily restricted to RLL grids on a logical plane, these coordinate transformations play a role in computing the actual weights. The author assumes that the position vector $r = [\theta, \phi] = \theta e_\theta + \phi e_\phi$ instead of the J99 assumption of using $r = [\theta, \phi] = \theta e_\theta + cos(\theta)\phi e_\phi$. The author should address these concerns and verify if the conclusions differ.

The author presents variants of the second-order scheme. However, discussions related to accuracy for the choice of representative coordinates and its impact on accuracy measures should be accompanied by a convergence order study. It is important to understand and verify the rate of convergence, and the constant involved to see if the new schemes offer a significantly better advantage in terms of stability and accuracy for remapping fields conservatively. This is mandatory for a comparative study presented here.

I also recommend using the MIRA package (referenced in Mahadevan et al (2022)) to generate the metrics data for remapping a given analytical field (both spherical low/high order harmonics functionals and a double vortex field) to understand stability, conservation, and accuracy degradations if any in L_2, L_∞, H_1 norms. Such a study can provide better intuition on the numerical performance and asymptotic behavior of the remapping method.

2. NOTABLE COMMENTS

Other major comments are listed below.

1. What is the relevance of Eq (8)? This is the same as Eq (2) except that Eq (4) has been substituted in. This discussion can be simplified.
2. L96: "The author speculates that it is non-trivial to satisfy transformation from Eq. (5) to Eq. (6) for general coordinates.". If you use a consistent linear basis for the reconstruction with a constant gradient across a cell, then this should be true. What do you mean by "general coordinates" here?
3. Eq (9) is true for a rectangular projection of a spherical coordinate system defined on the surface of a unit sphere. Please be explicit about this if you claim it "is just the analogue to the (x,y) Cartesian representation".
4. Is it correct that σ density term in Eq (12) refers to the physical coordinate transformation on the unit spherical surface to the logical lat-lon 2D plane coordinate system? There is no further discussion related to this term, which I think is necessary to set up the derivations that follow.
5. In Eq (13), the second term in the integral equals zero according to the assumption in Eq (14). However, even with the assumption that the flux derivatives are constant across a cell, I fail to see how the individual terms are equated to zero in Eq (15) and Eq (16). Is this imposed specifically to derive what the optimal pivot coordinates need to be? This is only a sufficient condition and not a necessary condition.
6. I do not see a clear reason why $cos(\theta_p)$ was substituted with $cos(\theta)$ in Eq (11). You replaced a point

value with a spatially varying term, which leads to differences in Eq (18) and Eq (20). This seems to be a key argument stating that the centroid and the pivot on the logical plane are different in longitudinal direction. However, if you had retained $cos(\theta_p)$, the formulations will be identical. This is also mentioned in L151.

- Edit: After reading Phil's review comments, the reasoning is clearer.

7. Eq (31) implies that J99 is using $A_i = \int_i dA_{s,i} = \int_i cos(\theta)dA$, where A_i is the area of the logical element i, and $A_{s,i}$ is the area of a spherical element i. With the definition of $dA = cos(\theta)d\theta d\phi$, the derivation of w_{3nk} looks consistent. This negates the conclusion that J99 derivation yields a wrong remapping weight term. Please clarify as this is one of the primary conclusions that drives the motivation for the manuscript.

8. Scheme C_g seems like an approximation of Scheme P, where $cos(\theta)$ is replaced by $cos(theta_c)$ everywhere and simplified. In that respect, it is closer to Scheme P than Scheme C_d in contrary to what the author has suggested in L315.

9. Is the ϕ_{rep} defined in Eq (49) used to replace the center latitude-longitude values in the input grid file so that the reference J99 implementation uses it as is without modifications? It is unclear in the text and I see `src_grid_center_lat` and `src_grid_centroid_lat` in the testO/rmp map files distributed in the artifact at DOI:10.5281/zenodo.10892795. Please clarify.

10. In Fig (3), can you explain the smaller differences in l_2 metric between Y_2^2 and $Y^1 6_3 2$ as compared to l_∞, which indicates a contrasting behavior? Can you also comment on whether the larger errors near the poles are dominating in these metrics? This may be important since it is my understanding that there is a separate treatment for elements at the poles compared to everywhere else.

11. In Fig (4), why are figures 4(d) and 4(f) compared against 4(b), instead of 4(a). You have established in Table (1) that Scheme O (J99) is sensitive to α. So error differences against the exact solution will provide a better way to compare profiles in Fig 4(c) against 4(e) and 4(g).

The same comment applies to Fig (5) as well.

12. I recommend replacing Fig (5) with a similar experiment as Fig (4) using Scheme P instead of Scheme O.

13. L435: "Which is better for the general problem is difficult to conclude."

Certainly. But since the manuscript is focused on the consistency of second-order schemes, you should use the analytical closed for functionals to compute the order of convergence going from say a refined RLL grid (1024,2048) to (90,180), (180,360), (360, 720), (720, 1440). The source and destination grids mustn't be embedded to avoid any aliasing errors to creep in. Such a convergence study can also provide insight into the constant in the second-order scheme that will determine overall accuracy measures.

14. Another suggestion here is to use the dual-stationary vortex (Nair and Machenhauer, 2002) as another test case to verify the performance of the schemes.

15. Fig (6) and Fig (7): It is unclear which scheme is better or what the real conclusions are from these results. What do the changes in Schemes C_g and C_d relative to scheme P tell you? There is no clear value in this particular experiment and the text does not explain the significance of this result either. Please clarify, and improve the text/figures appropriately.

3. MINOR COMMENTS

1. L49: Add "grid": regular latitude-longitude (RLL) rectangle grid.

2. L64: Add comma, after "in a conservative manner"
3. Eq (27) and Eq (28): please stay consistent with notation; use J99 instead of ORG
4. Eq (29): Do not change bracket notation unless you intend to specify something different. For example, $(\theta - \theta_p)$ in Eq (25) is replaced by $[\theta - \theta_p]$ in Eq (29).
5. L218: please specify that ϕ_{rep} is the representative coordinate, eventhough this is mentioned again later
6. L359: "using the official SCRIP implementation."
7. L426: Rephrase: "This was similarly confirmed for the other two schemes, Schemes Cg and Cd (not shown)."
8. L430: "in the results of Scheme Cg/Cd and Scheme Cd" - remove the first **/Cd** mention?

REFERENCES

Nair, R. D., and B. Machenhauer, 2002: The mass-conservative cell-integrated semi-Lagrangian advection scheme on the sphere. Mon. Wea. Rev., 130, 649–667, doi:10.1175/1520-0493(2002)