Response to the Reviewer comments (RC4)

I thank to the reviewer Dr. Vijay Mahadevan who provided precise and valuable feedbacks on the manuscript. I addressed all the points in the responses as follows, and I will submit the revised manuscript that reflects these changes, which significantly improves the quality of the manuscript.

The reviewer comments are quoted in italic with some minor editorial adjustments, followed by responses by the author.

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.

Thanks a lot for, in particular, positive evaluation on the derivation in the present paper. Although my derivation in the manuscript is not yet accepted by everybody, I am really encouraged by your review. I suppose I can manage to respond all the criticism on the derivation, with including your suggestion to be added into the revised manuscript.

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 main point of the present paper is about following three equations, (6) (7) (10) in the J99 paper:

$$\mathbf{r}_n = \frac{1}{A_n}\int_{A_n} \mathbf{r}\ \mathrm{d}A, \tag{J99.6}$$

$$\overline{F}_k = \sum_{n=1}^{N}\left[\overline{f}_n w_{1\,nk} + \left(\frac{\partial f}{\partial \theta}\right)_n w_{2\,nk} + \left(\frac{1}{\cos\theta}\frac{\partial f}{\partial \phi}\right)_n w_{3\,nk}\right], \tag{J99.7}$$

$$
\begin{aligned}
w_{3\,nk} &= \frac{1}{A_k}\int_{A_{nk}} \cos\theta(\phi-\phi_n)\,\mathrm{d}A \\
&= \frac{1}{A_k}\int_{A_{nk}} \phi\cos\theta\ \mathrm{d}A - \frac{w_{1\,nk}}{A_n}\int_{A_n} \phi\cos\theta\ \mathrm{d}A.
\end{aligned}
\tag{J99.10}
$$

What I proposed is that those three equations are inconsistent. The derivation using 2D logical plane in the manuscript is somewhat misleading, but, it is only a preparation to obtain Eq. (J99.7), and has little influence on the conclusion. Thanks to RC2, RC3 (referee comments by reviewer 2, Jones), I can formulate Eq. (J99.7) starting from the spherical coordinate. In the revised manuscript, I will rewrite the preparation block with the 2D logical plane into the one with the spherical coordinate. It will be much clearer to satisfy your concern. Thanks a lot.

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.

First of all, the primary issue of the present paper is (was) not to propose a new scheme, but to demonstrate the possible impact of the error (still just a proposal, not accepted by everyone) in the past application.

I wrote in the manuscript that the choice of representative coordinate is in principal not under control of the algorithm, but as pointed in RC2 (Jones), it is recommended already in J99 to choose the source cell center in order to simply deal with multiple-valued longitudes. It means that for most practical cases, the representative coordinates are always close to the centroid coordinates, which will result in insignificant influence on the remapped field. Thus the impact on accuracy measures should be almost identical with a fully compatible implementation with the original. The extreme representative coordinate experiment will be reduced or even removed in the revised paper, because it may be far from practical application.

In this perspective, the suggestion above may be too much for the revised manuscript, because it is beyond the scope of the main topic, to show that the past application using J99 algorithm is little damaged by the original formulation.

However, the present manuscript may be regard as one to propose some variants of the second-order scheme, as commented. They are all minor variations, not so different from the original algorithm, but still it may be useful to present a convergence order study.

Therefore I am now much postive to include your suggestion. Thanks a lot for this comment. It will improve the quality of the revised manuscript.

Also, as I wrote in the manuscript, computation of the remapping weights using the relative coordinates still works as a side effect to reduce the error of invalid derivation. The error is canceled only in the case when the representative (source cell center) coincides with the centroid of the source cell. Although cell center usage is a simple way, it is possible to overcome the multiple-valued longitude in different ways. The three variations of second-order scheme are insensitive to the choice of the representative coordinate while the original is much sensitive. In this sense, a convergence order study for cases of extreme representative coordinate might be still useful. I will consider whether or not include after restructuring the main part of the present paper.

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.

Great. I agree to introduce MIRA for the metrics.

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.

Right. I will delete the equation (8) to be simplified, such as "Under Eq. (7) constraints with Eq. (4), the flux approximation (Eq. 2) automatically satisfies the conservation characteristics of Eq. (1)."

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?

What I wanted to say is that it is sufficient if this transformation is valid for a linear basis at least for the topic of the present paper. However, I agree it is really confusing. I will delete this block to simplify.

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".

The derivation from here will be completely rewritten according to RC2, RC3 (by reviewer 2 Jones). The core derivation is formulated using the basis of spherical coordinate.

This part will be introduced in later part, and I will be explicit about the rectangular projection at a new place.

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.

The density comes from the conservation of the flux, and simply computed from $\mathrm{d}A = \cos\theta \,\mathrm{d}\theta \,\mathrm{d}\phi = \sigma \,\mathrm{d}\theta \,\mathrm{d}\phi$. But, again, this part will be reformulated with the basis of spherical coordinate.

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.

Exactly, they are imposed to derive the pivot coordinates for a source cell A_n, in one way among possible solution.

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.***

Yes I definitely agree. It is a weak point of my original derivation, and honestly I do not have confidence to justify the replacement. In the revised manuscript, this part will be reformulated from the spherical basis, and it will be much clearer. Thanks a lot.

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.***

The definition of A_i you proposed above implies that the remapping coefficient w_{1nk} (the first-order coefficient) should be computed also with it. The original formulation of the integral term (J99 Eq.12) is:

$$\int_{A_{nk}} \mathrm{d}A = \oint_{C_{nk}} -\sin\theta \, \mathrm{d}\phi, \tag{J99.12}$$

is rather consistent with the other (i.e., the present paper's) definition of the area element of no additional cosine term insertion. Thanks a lot for this point. I will describe the possibility of the different formulation of area term in the revised 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.

Right. I will change the explanation following the comment above.

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.1089 Please clarify.***

You are right. I only modified the value of `src_grid_center_lat` and `src_grid_centroid_lat` in the SCRIP input files which is input to the original SCRIP test program. I will clarify this.

10. In Fig (3), can you explain the smaller differences in l_2 metric between Y_2^2 and Y_{16}^{32} 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.

First point. I suppose that it reflects the wide insensitive area (Fig 5e) in the result of Y_{16}^{32} experiment to reduce the l_2 metric as compared to l_∞. I will discuss further in the revised manuscript to explain these difference in detail. Second point. I should have mentioned in the text that the separate treatment for elements around the poles is switched off in this demonstration for simplicity.

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.

Yes, partially. Fig 4(d) is much close to 4(b), such that difference between 4(d) and 4(a) may look similar to 4(c). On the other hand, difference between 4(f) and 4(a) will be better as you comment, I agree. I will revise this part.

Actually, as the referee 1 (Hanke) pointed out, the primary issue of the present paper is not to propose a new scheme but to present the possible impact of the error in the past application (which is expected to be reasonably small). Therefore the comments on this figure is inconsistent between referee 1 and 3. I will mix the revision according to the both two suggestion, to keep 4(e) but to replace 4(g), which will be also consistent of the primary issue of the paper.

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

It is possible, but Scheme P is really insensitive to the choice of α, so the figures b,d,f will be equivalent except for the minor precision. In order to confirm this, I will insert these figures in the Supplement.

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.

Honestly I felt it too much for the issue of the present paper. However, in a sense the paper may be one to propose new schemes, I agree to your suggestion, at least as additional issue. Good idea. I will revise the demonstration to overcome this issue.

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.

Agree. Same as above.

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.

As I described repeatedly, the primary issue of the present paper is not to propose a new scheme but to present the possible impact of the error. I will clarify the main topic through the paper. Also, when I obtained the additional experiment you have suggested, maybe I can tell which is better or not, as side information.

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

Clearly the word 'grid' is missing. I will correct.

2. L64: Add comma, after "in a conservative manner"

Comma will be inserted.

3. Eq (27) and Eq (28): please stay consistent with notation; use J99 instead of ORG

Good idea. ORG will be replaced by J99.

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).***

Thanks a lot for pointing it out. I will unify the notation through the paper.

5. L218: please specify that ϕ_{rep} ***is the representative coordinate, even though this is mentioned again later***

All right. I will insert the definition again around here.

6. L359: "using the official SCRIP implementation."

I will append the word 'implementation' accordingly.

7. L426: Rephrase: "This was similarly confirmed for the other two schemes, Schemes Cg and Cd (not shown)."

Will be rephrase as: "Similarly, Schemes Cg and Cd are not affected (not shown)."

8. L430: "in the results of Scheme Cg/Cd and Scheme Cd" - remove the first /Cd mention?

Right. I will delete '/Cd' in the first place.