Response to the Reviewer comments (RC2 and RC3)

I thank to the reviewer Dr. Phil Jones who provided precise and valuable feedbacks on the manuscript. In particular, I really appreciate that you provide the detail information about the original formulation that I missed to catch in 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 General comments on RC2 and RC3

At beginning, I want to clarify the situation of the present paper. The referee commented in RC3 as follows:

Ah, yes. Did get a bit sloppy/inconsistent there. I probably should have stuck with the position vector here (phi only in the centroid) and included the cos(theta) metric only when computing distance as in the flux expansion.

Thus it is now agreed that there is an inconsistency in the formulation of the original paper (Jones, 1999, hereafter referred to as J99). Indeed, that is exactly the inconsistency I argued in the present paper.

The essential point of the inconsistency comes from the series of formulation in the original paper J99:

$$f_n = \overline{f}_n + \nabla_n f \cdot (\mathbf{r} - \mathbf{r}_n), \tag{J99.5}$$

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

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

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

where Eq. (J99.5) corresponds to Eq. (5) in J99 and so on. All the above essential equations originate from J99, thus it does not matter to the main point of the present paper, what formulations are inserted before or appended after these combination, even if they do not match the intention in the original paper. The derivation using 2D logical plane in the manuscript is misleading as you get much confused. I am really sorry about that and it is completely my fault in the first manuscript. However, it is only a preparation to obtain Eq. (J99.7).

The final formulation of the centroid longitude that you presented in RC3 is as follows:

$$
\cos\theta\phi_n = \frac{\int_{A_n}\phi\cos\theta\ \mathrm{d}A}{\int_{A_n}\mathrm{d}A}, \tag{RC2.7}
$$

while my proposal in the manuscript is:

$$
\phi_p = \left(\int_{A_n}\phi\cos\theta\ \mathrm{d}A\right) \Big/ \left(\int_{A_n}\cos\theta\ \mathrm{d}A\right). \tag{S24.18}
$$

In the manuscript, in order to avoid the confusion of the formulation, I call the reference coordinate as 'pivot' and use ϕ_p symbol. Whatever I call it, my suggestion is to replace the original formulation of the centroid longitude by Eq. (S24.18).

These two equation differ only in the treatment of $\cos\theta$ term in an integral. If $\cos\theta$ term in the integral of the denominator of Eq. (S24.18) is extracted from the integral as it is, the equation would become identical with Eq. (RC2.7).

You agree with the inconsistency I proposed and suspect it insignificant, as commented in RC3 (to follow the above quotation):

In the end, I suspect it may not make a large difference - basically the difference between average of the product and product of the average. But better to be consistent.

This is true, however, only when the computation of the centroid longitude is done using the longitude coordinate relative to the centroid. Yes, this is a recursion: the centroid longitude is computed with the centroid longitude. Actually, it is much close to what the original algorithm is computing — instead of the centroid, the source cell center is adopted for the reference of relative longitude. The center position of cell is usually not far from the centroid, and thus the effect of inconsistency is kept sufficiently small.

In order to evaluate the influence of the inconsistency, here I show a simple demonstration. Actually, this corresponds to how I found the consistency. For the demonstration, please forget about the formulation of Cartesian coordinate. The following speculation just starts from the latter half of Eq. (J99.10) on the spherical coordinate.

$$w_{3\,nk} = \frac{1}{A_k}\int_{A_{nk}} \phi\cos\theta \ \mathrm{d}A - \frac{w_{1\,nk}}{A_n}\int_{A_n} \phi\cos\theta \ \mathrm{d}A. \tag{1}$$

Introducing $w_{1\,nk}$, it is reformulated as follows:

$$A_k \cdot w_{3\,nk} = \int_{A_{nk}} \phi\cos\theta \ \mathrm{d}A - \frac{A_{nk}}{A_n}\int_{A_n} \phi\cos\theta \ \mathrm{d}A. \tag{2}$$

If the cell is a RLL shape, the integral can be computed as follows:

$$\begin{aligned}
\int_{\mathrm{RLL}} \phi\cos\theta \ \mathrm{d}A &= \int_{\phi_0}^{\phi_1}\int_{\theta_0}^{\theta_1} \phi\cos^2\theta \,\mathrm{d}\phi\,\mathrm{d}\theta && (3)\\
&= \left[\frac{\phi_1^2-\phi_0^2}{2}\right]\left[\frac{(\sin 2\theta_1 + 2\theta_1) - (\sin 2\theta_0 + 2\theta_0)}{4}\right] && (4)\\
&= \left[\frac{\phi_1^2-\phi_0^2}{2}\right]\mathsf{S}(\theta_0,\theta_1), && (5)
\end{aligned}$$

where the coordinates of the corners of the RLL grid are ϕ_0, ϕ_1, θ_0, θ_1, and $\mathsf{S}(\theta_0,\theta_1)$ is introduced to represent the second bracket term.

For example, we can compute the weight in a very simple case (see Fig. 1): a RLL source cell is divided into two equal-area RLL along the latitude. The source cell is assumed to be ϕ_0, ϕ_1, θ_0, θ_1 RLL shape. Then the cell A_{nk} is ϕ_0, ϕ_1, θ_0, θ_c RLL cell, where the corner latitude θ_c is defined as:

$$\sin\theta_c = \frac{\sin\theta_0 + \sin\theta_1}{2}. \tag{6}$$

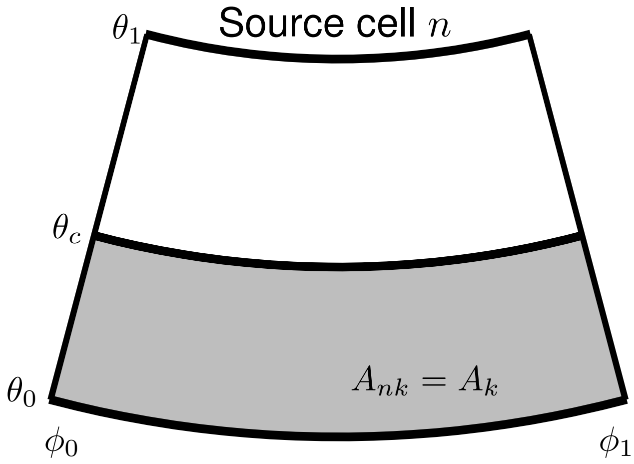

Figure 1: Example configuration

Actually, since the longitude span of cell A_{nk} is the same as the source cell, the weight in the longitudinal direction becomes zero (for the J99 algorithm). Then Eq. (2) is computed as follows:

$$A_k \cdot w_{3\,nk} = \left[\frac{\phi_1^2 - \phi_0^2}{2}\right] \left[\mathsf{S}(\theta_0, \theta_c) - \frac{A_{nk}}{A_n}\mathsf{S}(\theta_0, \theta_1)\right] \tag{7}$$

$$= \left[\frac{\phi_1^2 - \phi_0^2}{2}\right] \left[\mathsf{S}(\theta_0, \theta_c) - \frac{1}{2}\mathsf{S}(\theta_0, \theta_1)\right], \tag{8}$$

where $A_{nk}/A_n = 1/2$ by definition, and it is expected to be 0. The second bracket term is not zero even with substituting θ_c term as Eq. (6). Therefore, it means that Eq. (7) is satisfied only when $\phi_1 = \phi_0$ (trivial, zero width), and when $\phi_0 = -\phi_1$. The latter relation means that the origin is the center point of the source cell. Thus, if we compute each weight with rotating the longitude such that the origin correspond to each cell center, then the weight is correctly computed as zero. If the longitudes are not specified relative to the cell center, then it suffers from the absolute value of the longitude, which can be significantly large at the worst case. This example is only a simple RLL case, but it is sufficient to show that the original formulation of the weight breaks the expected behavior.

Please remember again that the above speculation does not depend on the incorrect assumption of plane coordinate in my manuscript.

This inconsistency originates from the treatment of $\cos\theta$ term in Eq. (RC2.7). If you keep the $\cos\theta$ term inside the integral as Eq. (S24.18), then the complex S representation in Eq. (8) is simplified and, actually, it is equivalent to zero for any absolute longitude values. Thus the weights can be computed as expected wherever the longitude origin is located.

Also, the speculation above is on a simple RLL case as a demonstration. Generally, the treatment of $\cos\theta$ term in the original algorithm does not satisfy the expected characteristics coming from the flux assumption of Eqs. (J99.5) and (J99.7) in J99 paper, which again are independent on the incorrect assumption of my manuscript.

So, I will reconstruct the discussion using the spherical coordinate as a starting point throughout the revised manuscript. I suppose then I can explain my derviation much clearer than the first one. I will withdraw the story of Taylor series expansion from the revised manuscript. I hope you would be satisfied with my proposal, after restructuring of my derivation from the beginning.

2 Point-to-point comments on RC2

I will first note in this review that my original publication (J99) and subsequent implementation is far from perfect and has some serious issues. I've always wanted to go back and correct those but unfortunately never got the time to do so. I say this to emphasize that the critical review below is not meant as a reactive defense of J99 or the SCRIP implementation. However, in reviewing the paper, I found the author has made some significant errors and incorrect assumptions that negate the conclusion. I do not believe this paper can be published in its present form since it is incorrect.

First of all, I really appreciate you kindly to become a referee of this paper. Your comment will really fill the gap of my understanding the J99 and SCRIP. I may agree that this paper cannot be published in its present form, not because it is incorrect but it starts from different assumption to the J99.

Even I starts from those you provided below, I still suppose it is transformed into the invalid formulation as speculated above.

The first error is the derivation in section 2.1. The author attempts to derive the flux distribution from a Taylor series expansion. However the constraints in equations (4),(5) do not necessarily follow from (3) or at least not uniquely so. They are merely a reasonable and obvious choice among a number of possible solutions. For this reason, neither Dukowicz and Kodis (DK87) nor J99 derive this form from a Taylor series. The two previous papers (JK87, J99) simply show that the flux form:

$$f_n = \overline{f}_n + \nabla_n f \cdot (\mathbf{r} - \mathbf{r}_n), \tag{RC2.1}$$

meets the conservation condition as long as the reference point ($\mathbf{r}_n$ ***) used in the flux approximation is the centroid. It is an assumed distribution that meets the conservation condition. This might seem a minor quibble since the author arrives in a similar place as the two prior papers, but it is important because the author takes the Taylor series approach later as well and this is incorrect.***

Response to the series of paragraphs from here will be inserted at the end of the series.

The author correctly notes that equation (J99.6) ***only holds for Cartesian coordinates. In spherical coordinates, the dot product and the unit vectors are spatially dependent and cannot be formally pulled out of the integral and are more complex in form. The original paper J99 arrives at this form in a different manner. There is a similar issue with the centroid definition, but I will address both of these choices below.***

The real problems with the current paper come in section 2.2 where the author incorrectly represents the J99 derivation by stating we use a Cartesian space in lat/lon. This is not the case. All of our derivation occurs in spherical coordinates or a local spherical surface

approximation with the appropriate metric factors included. The author can be excused in misunderstanding the derivation since much of the derivation is left to the reader in the original J99 paper. But this mistaken assumption leads to the incorrect form that is the core of the paper.

To elucidate the error, I have to explain how we actually derive the weights in J99 and show some additional steps. We start with the form of the flux approximation shown above (noting again, that this is not a Taylor series expansion):

$$f_n = \overline{f}_n + \nabla_n f \cdot (\mathbf{r} - \mathbf{r}_n). \tag{RC2.1 revisited}$$

We also assume the gradient is fixed with the form:

$$\nabla f_n = \left(\frac{\partial f}{\partial \theta}\right)_n \hat{\theta} + \left(\frac{1}{\cos\theta}\frac{\partial f}{\partial \phi}\right)_n \hat{\phi}. \tag{RC2.2}$$

As noted previously the dot product in spherical coordinates is in general not simply a component-wise product as in Cartesian coordinates since the unit vectors can change direction based on position on the sphere. Here we can take two approaches which lead to the same approximation. One is to say that the unit vectors are nearly aligned so that local orthogonality is almost true (it is true for r ***in this case, but not quite*** r_n***). We use the fact that the local displacement on the unit sphere is:***

$$\mathrm{d}\mathbf{r} = \mathrm{d}\theta\hat{\theta} + r\cos\theta\,\mathrm{d}\phi\hat{\phi}. \tag{RC2.3}$$

Then we can approximate the flux as:

$$f_n = \overline{f} + \left(\frac{\partial f}{\partial \theta}\right)_n (\theta - \theta_n) + \left(\frac{1}{\cos\theta}\frac{\partial f}{\partial \phi}\right)_n \cos\theta(\phi - \phi_n). \tag{RC2.4}$$

The same result can be obtained by using a local quasi-Cartesian approach but including the spherical metric factors. This form is very close to the author's equation (11) except that the first term is the mean flux and the author's pivot point cannot be an arbitrary pivot point. It is required to be the centroid r_n***. These differences again***

arise from the mistaken use of a Taylor expansion rather than the flux form.

At this point, the author makes the mistake at the core of the paper. The author assumed we were working in some sort of Cartesian space in (theta,phi) with none of the metric factors and then assumes that a density factor is required in equation (12) to correct the integrals and must occur in all integrals. In fact, we are working in spherical coordinates where the area element dA is defined as

$$\mathrm{d}A = \cos\theta\,\mathrm{d}\theta\,\mathrm{d}\phi. \tag{RC2.5}$$

So we do not need this imagined sigma density in equation (12) and equation (18) is incorrect in the denominator.

In J99, we compute the centroid, using the standard definition

$$\mathbf{r}_n = \frac{\int_{A_n} \mathbf{r}\ \mathrm{d}A}{\int_{A_n} \mathrm{d}A}. \tag{RC2.6}$$

This leads to the correct latitude centroid in equation (17). The position vector r must be dimensionally (and metrically) consistent with the displacement equation above, so the centroid term in longitude includes the cos(lat) metric scale factor and

$$\cos\theta\phi_n = \frac{\int_{A_n} \phi\cos\theta\ \mathrm{d}A}{\int_{A_n} \mathrm{d}A}. \tag{RC2.7}$$

Substituting this form of the centroid, we obtain equation (28) (equation 10 in J99). The author's equation 30,31 are incorrect since they depend on the incorrect equation (12). As another aside, we note that a more careful computation of a real centroid should follow, for example, the approach Du, Gunburger and Ju (2003, SIAM J. Sci Computing) in which the centroid is the full 3d centroid constrained to the spherical surface. The centroid here is a very close approximation to that form and is consistent with the dot product assumptions made in the flux approximation so that actual conservation is ensured in practice.

Thanks a lot for the detail explanation. Now it is really clear to me how to formulate the original equation. I am really happy to learn the background of J99, which definitely must replace my wrong assumption in the manuscript.

I fully agree that to start from a Taylor series expansion is one of possible choices, and does not match to the derivation of the original paper. However, as you may agree, even if I start from your derivation, the final formulation for the weight in the longitudinal direction, with regard to the treatment of the cosine latitude term.

The author's error in equation 12 renders the remaining discussion essentially moot. However, I will note that in the later discussion the author misunderstands the longitudinal correction shown in Figure 2. This correction is necessary to avoid computing a line integral across the branch cut in the multiple-valued longitude as explained in section 3(d)(1) of J99.

Again, I am really sorry to confuse you by the wrong assumption of the starting point of the original formulation. I will reconstruct the discussion and reformulate everything using the spherical coordinate as a starting point throughout the paper, and I hope then you proceed to the core formulation of my derivation in the revised manuscript.

Also, thanks a lot for this point, about the longitudinal correction. I should have noticed this point when I read J99 many times repeatedly. So now I understand what is the actual intention to introduce relative longitude to compute the remapping weights.

Even if the objective of the longitudinal correction is for simply treatment of multiple-valued longitude, it still works as a side-effect to reduce the inconsistency in the original formulation. There is no strong constrain to adopt the source cell 'center' for the reference point of longitude correction. For this particular objective, any longitude within the source cell (for example, one of the source cell boundary) should work as the same. However, as I demonstrated in the manuscript, remapping is a little but certainly influenced by the choice of the reference point.

What I examined in the manuscript is to shift `grid_center_lon` variable within source cells in the SCRIP input file and input to the original SCRIP. The remapped fields are affected slightly but significantly.

I admit that the demonstration of this impact is not clear enough (similar concerns are raised in RC4, by Mahadevan). I will reconstruct the discussion and make it clearer in the revised manuscript.

I hope this review is clear and helps the author understand what was done for J99 and the SCRIP implementation. As noted at the beginning, there are other problems with SCRIP, especially the parametric form for cell sides used in equations 16, 17,18 of J99 and in the SCRIP implementation. Both the choice of a linear form and the failure to include the cos(lat) metric factors in the longitude during intersection computations (even though I did so everywhere else in the paper) are the source of most of the problems in SCRIP and are especially amplified in the 2nd-order terms, making SCRIP difficult to us for higher-order cases.

Yes, it is really clear, helps enough to improve the manuscript. Also, thanks a lot for the comment on the original implementation. This should be also mentioned in the revised manuscript.

3 Point-to-point comments on RC3

Ah, yes. Did get a bit sloppy/inconsistent there. I probably should have stuck with the position vector here (phi only in the centroid) and included the cos(theta) metric only when computing distance as in the flux expansion. Guess I got used to tossing in the metric factor everywhere and it does make for a cleaner weight calculation and is probably better behaved. In the end, I suspect it may not make a large difference - basically the difference between average of the product and product of the average. But better to be consistent.

Actually, that inconsistency is the topic of my manuscript. It has been insignificant for the past application of the original algorithm, not because the difference between average of the product and product of the average is small, but because the relative longitude formulation works to cancel the inconsistency as a side effect, which is demonstrated in the general comment. I will reformulate the discussion to be clarified my point.

References

Jones, P. W.: First- and Second-Order Conservative Remapping Schemes for Grids in Spherical Coordinates, Monthly Weather Review, 127, 2204 – 2210, 1999.