Response to the Reviewer comments (RC1)

I would like to thank Dr. Phil Jones, the reviewer who provided precise and valuable feedback on the manuscript for this resubmission. I addressed all of the points in the responses, and I will submit a revised manuscript that reflects these changes. These changes will significantly improve the quality of the manuscript.

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

This paper is a re-submission of an earlier paper that describes a potential problem in the SCRIP high-order conservative remapping algorithm that requires a correction. This revision has a slightly better understanding of the underlying algorithm but still has some significant problems and the testing appears to be incorrect.

Thank you very much for pointing out many weak points in the manuscript. I will follow your suggestion for the resubmission.

As in the first submission, the author attempts to derive a flux distribution from a Taylor series in section 2.1. As I wrote in the first review, this derivation is incorrect and the author should not attempt to derive this from a Taylor series. In particular, the constraints in equations 4,5 do not follow uniquely from 3 without additional assumptions. For example, it cannot always be true that a flux evaluated at c_n ***will be the mean. DK87 actually makes this point in the sentence referenced by the author in which DK87 says it is a Taylor series "only if the mean is assumed to be located at the centroid". Instead, both DK87 and J99 use the distribution of the flux as***
[can't seem to upload an equation image but latex form is: $f_n = \overline{f}_n + \nabla_n f \cdot (\vec{r} - \vec{r}_n)$ ***]***
which is a construction that automatically satisfies Eq 1 as long as r_n ***is the centroid so that the second term integrates to zero. By making the leap from his eq 3 to the constraints in 4,5, the author is essentially making the same assumption that the original DK87 and J99 approaches have done by construction, so it's better to start with***

that anyway and state it as a common constructions that satisfies eq 1. The only reason a Taylor-like expansion is used is so that DK87 can claim the distribution is second-order as long as the gradient is first-order. The author should simply present the distribution as is done in DK87 and J99 to make the assumption explicit and avoid the incorrect derivation in 2.1.

Now it is really clear, and I fully agree with your suggestion. As you mentioned, I implicitly assumed the relationship between the mean and the centroid point. This should have been mentioned earlier to avoid the leap in the derivation.

Section 2.2, line 132, the author calls ϕ_c, θ_c a reference point and mentions that J99 calls it a centroid. In fact, this is still required to be the centroid and it is not an arbitrary reference point.

All right. I will rewrite them.

The actual demonstration of the issue with longitudinal weights appears correct and the pivot fix seems reasonable as shown in Fig 1.

Thanks! This is the main point of the manuscript.

An additional proposed fix around line 250 should be removed and the C Scheme later also removed. There is a reason why metric terms are included in coordinate system transformations and relevant operators. Removing the cos(lat) weighting in this fix is likely to introduce significant error in more general meshes, especially near the polar singularities.

All right, I will remove them from the derivation.

The biggest problem with the current paper is in the testing section. A couple things are simply oversights: The RLL mesh description in line 337 is wrong and needs to be corrected. Also, they refer to a Scheme O, which I assume was the original formulation which they have now named Scheme N in the previous section. The author needs to pick a consistent name across the sections.

I was not careful enough to detect the error in the RLL description. I will correct it. Also, I will fixed them to be consistent throughout the paper.

More seriously, most of the tests use a high-res mesh as the source and perform remaps to coarser meshes. The high-order terms only impact a coarse-to-fine remap so these tests need to use the coarse meshes as the source mesh and the finer mesh as a destination. The fine-to-coarse operation in the tests presented are an averaging and remove most of the impact of the high-order terms. Indeed, in many cases where the overlap is complete, the high-order terms should integrate exactly to zero.

I really agree with this. Actually, my first manuscript presents the demonstration you suggested, coarse-to-fine remapping. The reason I changed the demonstration to the opposite direction is that one of the reviewers of the first manuscript required it. I will change the demonstration back to the coarse-to-fine case.

The global offset case does not fix the multi-value longitude issue. It only shifts the problem to a different longitudinal branch cut. So the high errors in these tests are more likely an incorrect correction of the multi-value longitude. If a 2-d map of the solution was shown, I would guess all of the errors would be along a branch cut in the domain and are separate from the error in the formulation that this paper is trying to address. In general, it would be a good idea to show a 2-d greyscale or colormap of the resulting field after a remap in addition to showing the global error norms to demonstrate there are no such artifacts. It's possible they would still not show up in these cases due to the fine-to-coarse averaging, but they would absolutely show up in a coarse-to-fine remap.

I agree that the global offsetting does not fix the issue for general grids with multi-value longitude. The introduction of the global offset in the manuscript is much limited to the simple configuration presented in the paper. I will explicitly mention this limitation and discuss the issue.

In the end, there are many issues with SCRIP, including probably the modification in this paper, but this paper requires significant revision to make that case.

Again, thanks a lot for your precise feedbacks. I will include all your vulnerable suggestions.