Response to the Reviewer comments (RC1)

I thank to the reviewer Dr. Moritz Hanke 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.

Fuyuki Saito found an error in the formulation of the weights for second order conservative remapping paper by P. W. Jones from 1999 (referred to as J99). This error also made it into the SCRIP library, which is based on J99. This software library has been the basis for conservative interpolation in many climate models for many years, which makes the finding presented here a substantive contribution to the community. Unfortunately my limited understanding of the underlying math prevents me from commenting on the correctness of the presented formulas.
The author reproduces the results from J99 and describes in detail the error made therein. This is followed by an analysis of the SCRIP source code and the description of preprocessing step in SCRIP. This step avoids the error having an actual impact on the interpolation results for many common use-cases. Multiple solutions to problem are derived and explained in-depth. Afterwards the impact of error is analyzed in great detail and compared to presented solutions.
Overall the paper is very well written and after a minor revisit, I would recommend it for publishing.

Thank you very much for your summary and the positive evaluation.

General remarks:
Even through J99 and the SCRIP library have been widely used in the past, the author seems to wrongfully assume that this is still the case for current software (e.g. ESMF). This is apparent in the additional remarks and the summary of the paper. However, ESMF and other software (e.g. YAC or XIOS) use implementations for first and second order conservative remapping, which significantly differ from the SCRIP library and are therefore not prone to the presented error. Second order conservative remapping in these implementations is based on Kritsikis et al., 2017 and not J99.

I really appreciate that you have kindly instructed me the recent situation. I fully agree that J99 and the SCRIP library were used in the past, and the communities have been switching to the other software as you tell. However, as far as I surveyed, some (although not many) recent studies still use the J99 scheme for the second-order conservative method. For example, Ding et al. (2024), Chtirkova et al. (2024) and Ren and Zhou (2024) explicitly mention that they used the second-order conservative remapping scheme of J99.

In the summary of the paper the author encourages the further use of the SCRIP library. However, he fails to mention the various other drawbacks of it, which would lead me to a different conclusion. These drawbacks include inaccuracy for cells close to the poles (also mention by the author) due to how trigonometric functions are used for intersection computation and the misrepresentation of the true cell shapes for everything but RLL rectangular grids (Taylor, 2024). This limits the use of the SCRIP library in my opinion to remapping between two RLL rectangular grids, which could be implemented much simpler and more accurate.

Yes, I agree that I may have encouraged too much in the manuscript. I do not suggest to replace the other remapping softwares by the SCRIP, but just want to support the past and current application of J99 and SCRIP.

Therefore, in order to respond the two comments above, in the revised manuscript I will summarize the current situation around J99 and SCRIP: First, there are other softwares with different implementation

from J99 or SCRIP and their application is recently increased significantly. Except for the RLL rectangular grids, the J99 algorithm has drawbacks which may reduce the remapping accuracy. The other softwares are not based on J99 algorithm, and the argument of the present paper do not affect their application. Second: there a few recent studies still utilize the J99 or SCRIP to compute the second-order conservative remapping. Even the argument of the present paper (i.e., the derivation of J99 is invalid) is correct, it may not mean that the result and interpretation is completely wrong.

Specific comments:
Line 3: missing reference for "1987" and "1999"

The abstract will be much rewritten after restructuring of the manuscript, and this part will be no more exist.

Line 3-4: duplicated "has"

I will removed the first one.

Line 5: missing reference for "pioneer study"

Also this part will be fully rewritten.

Line 18: Conservative interpolation is just one of multiple methods being used in ESM's. However, this sentence implies that it is the most commonly used method.

I agree. Conservation properties is merely one choice for the users. Some spatial interpolation schemes without conservation are also used in common. I will change this sentence to mention about this aspect.

Line 34-37: The CDO's use code extracted from YAC for the first order conservative interpolation. (see Taylor, 2024)

Right. I will clarify this point, where only the second-order scheme in CDO is based on SCRIP.

Line 42: Give Equation number from J99 ("(10)"?).

I will insert the equation number.

Section "Introduction": This sections could mention other issues of SCRIP and solutions to this implemented in other software (see General remarks).

Right. It is good place to include the other SCRIP issues. Other referees have told the issues in their review, which will be included here in the revised manuscript.

Line 73 Eq. (2): "r" is not described

I will define r as the point vector.

Figure 2: Instead of showing the actual source code, a mathematical description of what the code does might be easier to understand.

It sounds better. I will introduce the mathematical description corresponding to the actual implementation, possibly with the source code moved to Appendix.

Line 245: This may not only happen at the poles. A cell with the longitude bounds of [179;181] may be represented by [179;-179] independent of that latitude.

Right. I will revise the original sentence to describe this is an example.a

Line 205: The title could be more concise. In general this paragraph contains in my view too much speculations and opinions of the author. It could probably be shortened without loosing significant information.

How about 'A relative longitude formulation in the native package'? Although it is not shorter, I suppose it be more concise to describe the subsection.

Line 256: The implementations of trigonometric function can be more accurate for small absolute values. This may be another reason for this code in SCRIP.

I agree it is another merit of relative coordinate. Actually other referee (Jones) has pointed out that the intention of this adjustment is to simplify multiple-valued coordinate in the longitude, with citing the original J99 paper. I will correct the sentence according to the original intention.

Line 366, 375, 381: repeated use of "naturally...within the cell"

I agree, they are verbose. I will clean these parts.

Section "Additional remarks": In this section the discussion of the results for ESMF and other software tested by Valcke, 2022 assumes that it is based on J99, which is not the case.

I suppose the above point is not by Valcke(2022) but Mahadevan (2022). Thanks a lot for this information. I am not aware that the ESMF in Mahadevan (2022) is not based on J99. I will rewrite or remove this part.

Line 380: "less impact than a change in magnitude" this is not further explained and quantified. Is this due to numerical inaccuracy of SCRIP for cells close to the poles?

This part is really confusing. What I am describing here is the difference between the results of O and P (solid and dashed line in Fig 3) is very small compare to the magnitude of scores. I will rewrite this part to be clearer.

Line 399: "which has the maximum deviation from the pivot longitude within a source cell" this has already been explained above

I will delete the repetition.

Line 403: Maybe you could explicitly mention that Figure 4(c) shows deviations from the exact fields, which are independent from the issue discussed in this paper. Additionally, you could state that in order to be able to visualize this issue, you have to compute the difference (d-b/f-b) instead of (d-a/f-a).

Yes. I will introduce a description to mention the Fig 4(c) does not directly related to the topic of the paper but just a demonstration. The difference plotted in the figures are already d-b (Fig. 4e), f-b (Fig. 4g) as you suggested. I will rewrite the captions to be clearer.

Line 522: duplicated "this"

I will delete it.

Line 578,585,594: duplication of "https://doi.org"

Thanks a lot. I will check all the references in the revised manuscript.

References

Chtirkova, B., Folini, D., Ferreira Correa, L., and Wild, M.: Shortwave Radiative Flux Variability Through the Lens of the Pacific Decadal Oscillation, Journal of Geophysical Research: Atmospheres, 129, e2023JD040 520, https://doi.org/10.1029/2023JD040520, e2023JD040520 2023JD040520, 2024.

Ding, S., Zhi, X., Lyu, Y., Ji, Y., and Guo, W.: Deep Learning for Daily 2-m Temperature Downscaling, Earth and Space Science, 11, e2023EA003 227, https://doi.org/10.1029/2023EA003227, e2023EA003227 2023EA003227, 2024.

Ren, Z. and Zhou, T.: Understanding the alleviation of "Double-ITCZ" bias in CMIP6 models from the perspective of atmospheric energy balance, Climate Dynamics, https://doi.org/10.1007/s00382-024-07238-7, 2024.