We thank the reviewers for their comments and thorough evaluation of our manuscript. The aim of our paper was to investigate different methods for merging radar and CML data.

Our main take away from the two reviewers is that we should

- Perform a sensitivity analysis of variogram parameters and neighborhood size
- Perform log transformation of ratio data
- Elaborate on CML uncertainties and consequences for interpolation method in the discussion

We agree to investigate these issues.

Please find our reply in blue to the issues raised

This paper introduces the Python package mergeplg, and demonstrates its value by applying it to two publicly available commercial microwave link (CML) datasets. Several of the merging algorithms in this package are tested, and some sensitivity analyses are carried out. I think the paper is interesting, well-written, and a valuable contribution to the field. The paper needs some clarifications and I think there are some important things missing in the discussion. However, I don't think any new analyses are needed. I therefore recommend minor revisions. Specific comments are listed below.

1. On lines 56-57, I think it would be good to explicitly name mergeplg. I think it is a very valuable contribution to this field, and deserves a bit more attention in the paper.

Good suggestion.

2. Section 2: are the signals rounded/truncated (e.g. 1 dB r 0.1 dB), or do they have a precision that high enough to not be relevant?

The data is truncated, we suggest to elaborate on this in the discussion.

3. Line 91: do you mean 30 dB? Please specify the units.

4. Line 92: can "excessively noisy" be quantified?

Potentially, but it's typically a tradeoff between capturing extreme rainfall events and removing noise. We suggest to elaborate on this to the discussion

5. On line 100, radar-CML pairs are introduced. How is the radar rainfall estimate R defined? Is it the path-averaged radar rainfall estimate or is it the rainfall at the pixel over the center of the link? And are these definitions different for KED than for other methods? It would be good to also include a discussion about the implication of this choice on the results.

It's the average rainfall rate along the CML, and it's the same for all methods. We suggest to elaborate more on this in the discussion

6. On line 112, the ratio between CML and radar rainfall estimates is introduced. It would seem logical to me to log-transform this variable to make it symmetric (i.e., underestimation by a given factor would deviate as much from 0 as overestimation by the same factor). What is the reason for not using such a transform?

The reason was to keep the comparison as simple as possible. We experimented with log transform on all methods earlier, but found that it produced larger errors. However, we did not try log transform after range checks, which tends to remove outliers that are amplified by the back-transformation. However, we have experimented with log transform after range checks which give better estimates. We therefore suggest to perform log transform on the ratio data.

7. On line 115, please include units (I assume them to be mm/h).

We agree

8. On line 116, why is this condition assymetric? I would expect the lower boundary to be the reciprocal of the upper boundary, which is not the case here (0.1 is not equal to 1/15).

It's just a convention, we could use other boundaries.

9. On lines 110-120, I suggest using different symbols for the differences and ratios, and for the values of these differences and ratios on the one hand and their interpolated fields on the other. Now all four of these are called Z(v), and this can be confusing.

The idea is that the interpolator accepts any Z. However, we agree to have another look at the symbols used.

10. In Eq. (6) the covariance function C is introduced. However, the same symbol is used for CML rainfall estimates. This is confusing. Please select a different symbol for one of these variables.

We agree to have another look at the symbols used.

11. On lines 140-141, it is mentioned that the results do not depend too much on the variogram that is used. I think this would only hold if the distance between the points to be interpolated is (much) smaller than the range of the variogram. This could affect the results of the OpenRainER dataset.

We suggest to expand the study to investigate the effect of neighbourhood size and variograms.

12. On lines 141-143, the variogram parameters are introduced. The nugget and sill are expressed in $(mm/h)^2$, so I'm assuming these are the parameters for the additive method. What are the parameter values for the multiplicative method?

Good point, we suggest to clearly express this.

13. On lines 145-154, KED is explained. It would be good to include here that the CML rainfall estimates are directly interpolated, and not a difference or ratio of CML and radar estimates.

We agree.

14. On lines 156-166, it is unclear what the meaning of a block is in this context. Is it the path of a link? If so, it would be good to state that. And with that, it would be good to more precisely define the “average point covariance between blocks” and the “average point covariance within each block”.

A block is indeed the path of a link, we agree to state this more clearly

15. In Fig. 2, it is unclear to me what the gray-scale colors in the background of the third columns are. Can this be added to the caption?

Yes, we will add this.

16. In Fig. 5, I have trouble telling the different lines apart because of slight color-blindness. Is there a way to make the graph easier to read, for example by using different symbols/line types?

Thanks for letting us know. We suggest to improve the color scales

17. On line 259, I think “near rain gauges” should be “near CMLs”.

Yes.

18. On lines 261-274, the sensitivity to the range checks is discussed. How many samples were removed (percentage of the total amount of samples) by the different range checks? I think it would be valuable information to include in the paper.

We agree to include this information

19. On lines 261-274, a sensitivity analysis is presented of the range checks. Was it considered to also carry out a sensitivity analysis on the variogram parameters?

We suggest to extend the study to perform a variogram sensitivity analysis

20. On line 264, please include units (mm/h).

We agree.

21. On line 270, the difference between the two datasets in terms of their response to the range checks is mentioned. What could be the cause of this. Are the CML estimates worse for the OpenRainER dataset?

We are not sure. It could be the CMLs that are all on the same frequency, which is not optimal for rainfall estimation. It could also be that OpenRainER just have longer CMLs and fewer CMLs, making it hard to identify the location of the rainfall. WE suggest to elaborate more on this in the discussion.

22. On lines 297-320, the effects of the dataset characteristics on the results are discussed. What I'm missing here is a discussion of the uncertainties or errors in CML rainfall estimates and how they affect the results. I think this should be included in the paper.

We agree to further elaborate on this in the discussion.

23. On lines 340-341 and on line 348, it is mentioned that the path-averaging effect of the CMLs could play a role in dampening rainfall extremes in the interpolated product. If that would be the case I would expect to see a large effect of block Kriging versus point Kriging, but that's not visible in the results.

We agree. It does not seem that block kriging can reconstruct averaged rainfall extremes. We suggest to elaborate on this in the discussion.

24. On line 344 it is mentioned that KED has the tendency to produce smoother rainfall fields. Is this generally the case, or is this specific for these datasets (or only the examples shown in Fig. 2)? I would expect that KED would result in a smoother field only if the radar data are smooth to begin with. Please elaborate on this in the paper. This is important because this apparent smoothing character of KED is repeated throughout the discussion.

We agree to elaborate on this. It could also be due to the neighborhood size being too small.

25. On lines 346-348, the potential effect of non-linearity of the relation between CML rainfall and radar rainfall is discussed. What could cause such non-linearity, and how severe could it be? If there's no reason to expect non-linearity, I suggest removing this statement from the paper because it would then not be relevant.

Good. We suggest to remove this statement, as it is a bit speculative in its current form, raising more questions than answering.

26. On line 349, it is stated that path-averaging of the CMLs combined with possible nonlinearity of the relation between CML and radar rainfall can lead to a smoothed drift surface. Is this really the case? And if so, please explain more clearly in the paper what the mechanism behind this is.

Thanks. Yes, this is a bit speculative. We suggest to remove this, and focus more on the other issues raised in the review.

27. On line 353, I think "from rain gauges" should be "from CMLs".

We agree.

28. On lines 361-366, multiplicative methods are discussed. I think it would be good here to mention that most errors in radar rainfall estimation are multiplicative, and that from that perspective, it would make sense to use a multiplicative method for merging. The fact that the multiplicative methods actually perform worse makes these conclusions even more relevant.

Good point. Let's see how the multiplicative method performs when adding a log transformation.

29. On line 404, I think "performed on pair" should be "performed on par".

We agree.

30. On line 406, what is meant by "rainfall simulators"? Does this refer to the correlation or variogram functions?

Methods that create stochastic reconstructions. We agree to state this more clearly.

31. On lines 410-412, and important characteristic of Kriging is discussed that wasn't discussed before: the interpolation uncertainty estimates. I think it would be good to mention this in the introduction of the paper. And I think that the claim that is made about the improved uncertainty representation of block Kriging cannot be made based on the results presented in this paper (uncertainties are not studied here). So I suggest to reformulate this part.

We agree.

32. It would be interesting to also discuss the effect of the particluar characteristic of Kriging that it will give more weight to isolated observations. For example, if there are 10 links to the south of the point where I would like to have an estimate and there is only one link to the north, the link to the north will have more weight than, e.g., IDW (where all links would get equal weights if they are all at the same distance from the point of interest). This could be an advantage when dealing with a network that has a very heterogeneous distribution in space like the OpenRainER dataset.

We agree to elaborate on this in the discussion

33. I'm missing a discussion about the use of the 12 nearest links. For IDW this makes perfect sense. However, for Kriging, you could miss information that would have received a large weight depending on the link topology. Please include a discusison on this in the paper.

We suggest to extend the study to investigate the size of the neighborhood size