Response to Report 1 (reviewer 2):

I regret I still have an objection against publication of this new version of the paper. It is relative to the first point I raised in my previous review, concerning the positive definite character of the covariance matrices obtained through the estimation approach described in the paper. The authors have not in my opinion correctly responded to my comment, and I actually suspect there is a misunderstanding between us. As I said in my previous review, this point is not critical for the paper, but it is nevertheless in a sense fundamental, and the paper should not mislead potential readers.

The authors are (rightly) concerned by the possibility that the 'variance' of components of the dataset vectors (or of some linear combinations of those components) might become negative. They write (ll. 404-407) ***However, the generalization to covariances matrices is expected to increase the occurrence of negative values in off-diagonal elements. Because spatial correlations and thus true covariances may become small compared to uncertainties in the assumptions or sampling noise, estimated error covariances at these locations might become negative.***

This seems to imply that it is the occurrence of negative off-diagonal elements in a 'covariance' matrix which might produce negative 'variances'. That is a misconception. Taking again the example of the 2x2 matrix considered in my previous review,

$$A \equiv \begin{array}{|c|c|} \hline 1 & a \\ \hline a & 1 \\ \hline \end{array}$$

If A **is to be the covariance matrix of a 2-vector** $(x_1, x_2)^T$ **with zero expectation, then necessarily**

$$E(x_1^2) = E(x_2^2) = 1, \quad E(x_1 x_2) = a \tag{1}$$

where $E(.)$ **denotes statistical expectation.**

For any β_1 **and** β_2**, the quantity** $q = \beta_1 x_1 + \beta_2 x_2$ **has variance**

$$E[(\beta_1 x_1 + \beta_2 x_2)^2] = \beta_1^2 + \beta_2^2 + 2a\beta_1\beta_2 \tag{2}$$

which must be ≥ 0 **for any** (β_1, β_2)**. This is verified iff** $a^2 \leq 1$ **(with equality if** $a^2 = 1$**), independently of the sign of a.**

It is the signs of the eigenvalues of a symmetric matrix that matters for the matrix to be a covariance matrix or not, not the signs of the entries in the matrix. The matrix A **has eigenvalues** $1 + a$ **and** $1 - a$**, both of which are** ≥ 0 **if** $a_2 \leq 1$**, while one is negative if** $a^2 > 1$**. The matrix is symmetric non-negative for** $a^2 \leq 1$**, and strictly positive definite if** $a^2 < 1$**.**

I have not looked at the connection of the above with the inequality on ll. 403-404 of the paper, but the text that follows is erroneous, at least in part. The correction must be made in the paper.

It is true that the expressions *symmetric definite positive matrices* or *symmetric nonnegative matrices* can be misleading in that they might be understood as referring to the signs of the entries in the matrices. But the established vocabulary of linear algebra is unambiguous, and these expressions refer to the signs of the eigenvalues of the matrices.

I really do not think I am mistaken in the above, but if the authors think I am, and that it is the signs of the entries in the covariance matrices that matter, I am ready to look at their objections.

And it remains of course that nothing in the paper guarantees that the obtained 'covariance matrices' are definite positive (or even non-negative)

Reply: Thank you very much for the clarification. In this paragraph, we intent to only discuss the sign of individual elements in the covariance matrix (only referring to non-negative elements, not positive definiteness of covariance matrix). But we agree that the important aspect for a covariance matrix is the positive definiteness (determined by the sign of the eigenvalues) rather than the sign of individual elements. We therefore added a note on positive definiteness based on the reviewers explanations (ll.407-408):
" However, the occurrence of negative elements does not affect the positive definiteness of a covariance matrix, which is determined by the sign of its eigenvalues. "

Note that the not guaranteed positive definiteness was mentioned shorty before (ll.384-385) so we decided to not repeat it here.

Concerning the other comments and suggestions in my previous review, the authors have responded to them correctly. But I still have a few suggestions for editing corrections.

- **L. 294, text is awkward. From what I understand, I suggest . . . *results from setting to the same value both elements of one pair of datasets in the expression (22) of residual crosscovariances.***

  Reply: Corrected based on the reviewers suggestion (l.294):
  " Each of the four possibilities results from setting both indices of one pair of datasets in definition of residual cross-covariances in Eq. (22) to the same value. "

- **L. 456, . . . *including their asymmetric components.***

  Reply: Corrected as suggested (l.457).

- **L. 527, text is awkward. Change to . . . *has accuracy similar to that of the other two assumptions.***

  Reply: Corrected based on the reviewers suggestion (ll.527-528):
  " . . . if the accuracy of the additional independence assumption is similar to that of the other two assumptions. "

- **L. 545, *The four datasets induces 10 error statistics* . . . → *The statistics of the four datasets depend on 10 error statistical moments***

  Reply: Corrected based on the reviewers suggestion, but we decided to avoid the word "statistical moments" to avoid potential confusion as this term is not used elsewhere in the manuscript (ll.546):
  " The error statistics of the four datasets consist of 10 matrices . . . "

In addition to the reviewers suggestions above, we performed two purely technical corrections in the introduction: comma added before *as well as* in l.7, and plural of *datasets* in l.11 .