This new version of the paper is for me a substantial improvement. I had written in my previous review that I was confused by the way the paper was written and that I had difficulties in understanding what the authors had exactly done. I think I have now understood (expect for Section 5, see comment 2 below, and for possibly minor details). That is due to improvements in the paper (for instance, in agreement with the first comment made by R. Todling in his review, the systematic use of the word *residual* instead of *innovation*). It is also due to the fact that a second reading of a paper, after some time, very often leads to a clearer understanding.

The paper is original and instructive and may be useful for many applications, for instance, as mentioned by the authors in their conclusion, for the combined use of analyses or forecasts produced by different Numerical Weather Prediction centres. On the other hand, I think improvement is still necessary, not in the scientific content of that paper, but in the way it is written and in the conclusions that the authors draw from their results. My main comments and suggestions, in approximate order of decreasing importance, are given below. I could have included some of these in my previous review, but I did not either because I had not understood some aspects I have now understood better, or because I considered these comments and suggestions of secondary importance at that stage. My main two comments (points 1 and 2 respectively) bear on the optimality that the authors claim to have defined and on the presentation of the numerical results (Section 5)

1. The authors write (abstract, ll. 7-8) that their paper provides *a formulation of the minimal and optimal conditions to solve the problem* [*i.e.* the problem of estimating the statistics of the errors affecting collocated data] (see also section 6.2 *Optimal setup*, and statements on *e.g.* ll. 84, 544, 559, 645).

The minimal condition to solve the considered estimation problem is to define hypotheses that make that problem exactly determined. The authors indeed define an approach that satisfies that condition.

But concerning optimality, the above claim is largely exaggerated. I understand the authors mean that the uncertainty in the estimated statistics is in some sense minimum, but give no precise criterion by which criterion the corresponding optimality is defined. The 'optimal setup' is schematically described by Figure 6, and is based on *a priori* (and largely subjective) hypotheses as to the degree of correlation of the errors in the various datasets. That is by no means 'optimal' in any precise sense.

The sentence (ll. 600-601) *Thus, multiple independence trees can be defined* … clearly says that different choices of triangles and reference subsets can be defined, which cannot be all 'optimal'.

In addition, the authors state clearly (l. 627) that positive definiteness of the estimated covariance matrices might not be fulfilled with their approach. Positive definiteness is obviously necessary for 'optimality' of estimates of variances and covariances.

I think the authors should either state precisely by which measure they consider their approach is optimal, or (preferably) remove any claim of optimality.

2. I find Section 5, which presents results of numerical experiments, rather confusing and difficult to understand. The experiments that have been performed are not really

described. What were for each experiment the real error statistics that were used for producing the datasets and then computing the residual statistics ? What where then the error statistics that were *a priori* assumed in order to close the problem of estimation of the global error statistics ? Were those *a priori* assumed statistics consistent with the real ones ? I understand that was the case for the experiment whose results are presented in Fig. 2a, but not for the other ones, although that is not clearly said. If they were consistent, were all the *a posteriori* estimated statistics in agreement with the real ones ? I understand that was the case for Fig. 2a, (*… the two remaining dependencies are estimated accurately*, l. 511), with the consequence that the 'matrices' in the two bottom rows of Fig. 2a must be exactly symmetric, with in particular the matrices in the third row being full of zeroes. That seems visually to be the case, but is not mentioned explicitly, leaving the reader in some doubt.

In the other experiments, the assumed statistics were not consistent with the real ones. That was the case for Fig. 2b, about which the authors write (ll. 525-526) *As shown in Fig. 2b, the error covariance of dataset 4 is underestimated by half the neglected dependency between (2;4)*. How can that (including the quantitative assessment) be seen from Fig. 2b ? And how can the reader check that the errors in the *a posteriori* estimated statistics are themselves consistent with the corresponding estimations presented in Section 4 ? The reference to Eq. (42), (51) and (52) (l. 521) is not of much help.

Considering figures, they must help then reader, who must be able to distinguish, among the statements made by the authors, what can be seen in the figures, and what cannot (in the latter case, the simple mention *not shown* is necessary).

These are only examples. I do not suspect that anything is scientifically wrong or disputable in Section 5 (nor anywhere else in the paper for that matter). But I think the Section must rewritten, with a precise description of each of the experiments that have been performed, with explicit statement of the associated hypotheses, and with a precise description of the obtained results, as well as of the conclusions that must be drawn from these results.

The only difficulty left to the readers must be with a proper understanding of the approach taken by the authors and of what the latter have done. Anything that has to do with deciphering the figures and drawing the conclusions must require no additional effort from the reader.

3. As already mentioned in my previous review, Eq. (23) can be obtained directly without going through the error statistics **C**, **X** or **D**. The authors write in their response that they have rearranged the equation to show the equivalence between innovation and error statistics in a clearer way. My point is that there is simply no need to refer to a 'truth' or to 'errors' in order to obtain Eq. (23). The equivalence between innovation and error statistics has been clearly shown by Eqs (19-22). Introducing quantities in places where there are useless can only confuse the reader (the fact there is a link between innovation and error statistics is irrelevant at this precise point).

4. Ll. 303-304, *The equivalence demonstrates that the exact formulations of error statistics from residual covariances and cross-covariances are consistent to each other* (incidentally, the correct wording would be *consistent* ***with*** *each other*). I understand the authors stress the 'equivalence' of those formulations because they will show later that, under the assumption of 'independence', they may lead to different results. But the reader cannot

understand at this stage why it is useful to stress the equivalence. I suggest the authors write rather *The equivalence demonstrates that,* ***as they must be****, the exact formulations* …. And the equivalence does not ultimately result from the fact that Eq. (20) is a special case of Eq. (22) (ll. 305-306), but from the basic definitions (4) and (5).

A similar argument holds before that for Eq. (32) and the comment that follows. I suggest to write (l. 289) … *the formulation of error covariances based on residual cross-covariances in Eq. (31) is,* ***as it must be****, symmetric and equivalent* …

5. Ll. 553-554, … *there are two requirements for the setup of datasets* It does not seem to me that the need for either one of two stated requirements has been shown. It has only been shown that those two requirements are sufficient for solving the underlying estimation problem (actually, I understand the sentence ll. 569-570 as meaning that other possibilities exist).

6. L. 552, … *without introducing additional degrees of freedom*. That formulation is confusing. From what I understand, the purpose is fundamentally to *eliminate* degrees of freedom by introducing hypotheses that render exactly determined the problem of estimating the error statistics. It seems that you implicitly anticipate on the text (ll. 602-608) where it is suggested to combine different estimates.

7. Ll. 551-552, *The only restriction is that all assumed error statistics must be fully determined* … From what I understand, the wording *all* ***additional*** *assumed error statistics* would be more appropriate.

8. Concerning positive definiteness of the estimated error statistics, the authors write (ll. 393-394) *However, the generalization to covariances matrices is expected to increase the occurrence of negative values were* (incidentally, the proper spelling is *where*) *correlations between two entries of the state are low, thus relative differences and sampling errors become large*. I am not sure I understand what that means. Is it that the occurrence of non definite positiveness is likely to increase with the number I datasets, or what ?

9. L. 370, *All three estimates become equivalent if the residual cross-covariances are symmetric* Yes, but it might be useful to mention here that these equivalent estimates may not be positive definite.

And l. 388, *Estimated error covariances might even contain negative values* … You must mean *might even not be positive definite* … .

10. Ll. 626-627, *While the presented method ensures symmetry of error covariances,* Not necessarily (Eqs 37 or 38 do not ensure symmetry). Say that symmetry can be enforced if necessary.

11. L. 2 (abstract), … *an ill-posed problem* … Not *ill-posed*, but *underdetermined*

12. Ll. 325-326, "*assumption of independence*". I mention that the word *independence* is not used here in its accepted standard meaning in probability theory. The hypothesis made here is actually an hypothesis of no correlation, which is a weaker property than independence. I suggest the authors briefly mention that fact.

And l. 325, *equals* is to be changed to *implies*

13. Ll. 440-441, … *absolute uncertainties of estimations from residual covariances and cross-covariances differ only in the uncertainties w.r.t. the basic triangle* … Well, these uncertainties depend also in the uncertainty in the hypothesis $\mathbf{D}_{i;\mathrm{ref}(i)} = 0$.

14. L. 315, *the determination of uncertainties* ***resulting from possible errors in the*** ~~*caused by*~~ *assumed error statistics*

15. L. 201, what are *unbiased error statistics* ? Error statistics are here the unknowns, not the data, and whether they are biased or not makes *a priori* no sense.

16. L. 323, $\mathbf{X}_{i;j} = 0 \Leftrightarrow \mathbf{D}_{i;j} = 0$ to be changed to $\mathbf{X}_{i;j} = 0 \Rightarrow \mathbf{D}_{i;j} = 0$

L. 414, similarly, the condition $\mathbf{X}_{i;\mathrm{ref}(j)} = 0$ is actually stronger than the assumption $\mathbf{D}_{i;\mathrm{ref}(i)} = 0$ made on l. 405. Be more consistent as to which hypotheses you make.

17. L. 558, $I \leq 3$, you must mean $I > 3$?

18. L. 318, $U_I \geq 0 \rightarrow D_I \geq 0$

19. L. 352, … *four equivalent formulations for each pair of other datasets* … . You have assumed here $I = 3$, so that there is only one pair of 'other datasets' (see also l. 358).

20. L. 34, what are exactly the *corners* ?

21. Ll. 40-41, sentence starting *Up to now,* … awkward. From what I understand, I suggest *Up to now, only scalar error variance estimation has been implemented in data assimilation with the TC method (e.g.* …

22. The word *exemplary* is used mistakenly in several places (the word designates something that is meant to be imitated, while the authors obviously think of illustrative examples)

L. 543, … *including an exemplary visualisation,* → … *including an illustrative visualisation,*

L. 566, *An exemplary setup* … → *An illustrative example of setup* …

Caption of Figure 5, *An illustrative example of visualization* …

See also l. 7 (abstract)

23. Ll. 35-36, … *this particular error estimation problem can only be closed under the assumption of optimally*. I do not understand what this means (and the proper wording should in any case be *assumption of optimality*)

24. Ll. 54-55 (and later) *error cross-variances* … You must mean *error covariances* ?

25. Ll. 65-66, … *affect different formulations of the estimated error statistics*? → … *affect different estimations of the error statistics*?

26. L. 613, I understand the *N-CN method* is what was called previously the *N-cornered hat method* (l. 603), or what ?

27. L. 24, *arises* → *raises*

28. L. 588, *effect* → *affect* (or *impact*)

29. L. 589, *triple* → *triple**t*** (the same correction is to be made elsewhere, *e.g*. l. 407 ; please check)

30. Ll. 574-575, … *can be interpreted* ***as being*** *similar to* …

31. L. 583, … *is comparably well known,* … word *comparably* inappropriate here. I suggest … *is known to some degree of accuracy,* ……

32. L. 544, … *algorithmic summary* ***for*** *the calculation* …

Although the authors have obviously been very careful with their notations, in particular as concerns indices, I have noticed a few typos

33. Ll. 177 and 184, $\mathbf{D}_{i;j}$ should be replaced with $\mathbf{D}_{i\text{-}j;k\text{-}l}$ and $\mathbf{Y}_{i\text{-}j;k\text{-}l}$ respectively

34. Eq. (32), (22) above first = sign should rather be (30)

35. L. 442, $m_{f\text{-}1} \rightarrow m_{F\text{-}1}$

And finally, it is the first time I have seen superscripts above equality signs to give reference to previous equations. I think that can be useful, and it is undoubtedly in the present case.