

Interactive comment on “Interpreting SBUV smoothing errors: an example using the Quasi-Biennial Oscillation” by N. A. Kramarova et al.

Anonymous Referee #1

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The authors investigate the smoothing errors of SBUV. The main problem in estimating smoothing errors typically is that no reliable a priori covariance matrix is available. The authors solve this problem by constructing a a priori covariance matrix from MLS measurements. This paper fits in the scope of AMT. However, there remain a number of issues which need clarification (see specific comments).

Specific comments: p 2723 1st par: Is there any paper available where the most recent retrieval algorithm is described in more detail? If so, a reference to this would be appropriate. If not, it might be helpful to add some more details on the retrieval, e.g. with respect to clouds, surface albedo, further fit variables except ozone partial columns,

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constraints and a priori used, etc. (c.f. comment on p2734 I22)

p 2724 I6: The smoothing error represents differences not only due to vertical smoothing but also due to biasing by the a priori (particularly if the sums over the averaging kernels are not unity). I suggest to write "...due to vertical smoothing OR ANY OTHER EFFECT OF THE CONSTRAINT TERM by the retrieval algorithm."

p 2725 I19: "an ensemble of true states" sound a bit vague to me. Care must be taken that C is representative for the sampling of the instrument under investigation.

p2726 I3: "on the magnitudes AND INTER-ALTITUDE CORRELATIONS of the..."

p2726 I17: Why are ideal AKs Gaussian? Why not rectangular or triangular?

p2727 I6: Some more motivation why the normalization is done would be helpful. Is the Rodgers formalism still applicable to normalized smoothing errors? This issue deserves a more thorough discussion. Does this imply that there is somewhere a hidden transition between two representation systems (e.g. concentrations vs. partial columns)? I.e. is the retrieval performed in concentrations but the data are finally represented in partial columns? I am a bit confused about this issue. I trust that it is correct what the authors are doing but I am afraid that I miss an important piece of information to understand the rationale behind this. And please make clear at any point in the manuscript where you use the original averaging kernels and where the normalized ones are used.

p2728 I9: "... is the A PRIORI covatiance matrix." (because in the retrieval there is also the measurement error covariance matrix).

p2728 I12: Why only "year-to-year" variability? Don't you lose variability if you restrict yourself to year-to-year variability? (c.f. comment on p2734 I22)

p2729-2730: You have made tests that the use of only the off-diagonal elements of C is a justified simplification in your case. This is a consequence of the limited number of altitude gridpoints of the retrieval. For other applications the off-diagonal elements

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might be essential. I suggest to add "in our case" here and there in order to avoid that an unexperienced reader understands this as a universal statement.

Sect 3.1. It is not clear to me if full C or diagonal C is used here. Further, it remains unclear to me why the investigation is not made on the basis of the full C.

p2730 l15 "caused" seems more appropriate to me than "defined".

p2731 l8-11: With diagonal A with diagonal elements lower than one, A does not smooth the profile but just tells how large the weight of the measurement is with respect to the weight of the a priori. This leads to another question: Are the mean values of SBUV and MLS the same? The MLS covariance matrix tells you how the MLS values vary around the MLS mean value. What you need, however, is a covariance matrix which tells you how the true values vary around the a priori actually used. Is there a "mean smoothing error" to be considered, which would result in a bias? I do not want to urge you to do the study again, but if these considerations are not included, this should be clearly stated. This issue may deserve some discussion. Another issue: Have you subtracted the MLS measurement error covariance matrix to get the covariance matrix representing only the natural variability?

p2731 l15/16: The recommendation "to convolve ... with the SBUV AK (or integrated kernels)" is a bit vague. Are indeed both these approaches correct? What would be the difference?

p2731: Not sure if the Rodgers book is the appropriate reference. I think C. D. Rodgers and B. J. Connor, Intercomparison of remote sounding instruments, J. Geophys. Res., 108, D3, doi10.1029/2002JD002299, 2003 is the appropriate reference. To my knowledge, this particular concept of intercomparison is not yet covered by the book.

Eq. 5: It would be helpful to derive Eq 5 from generalized Gaussian error estimation: The partial column of a merged layer is the sum of the partial columns of the individual layers. The 2-layer merging operator thus is (1,1). The resulting error is thus (1,1)

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$S_{\text{serr}} \sim (1,1)^T$ where \hat{T} denotes the transposed and where $S_{\text{serr}} \sim$ is the submatrix of S_{err} with elements related to the layers to be merged. This results in Eq 5. The reader might appreciate some guidance how Eq 5 is obtained.

p2732 I21: It is not at all clear to me how the DFS can increase by merging.

p2733 I8/9: This is certainly true for concentrations but I doubt that it is true also for partial columns. A more precise wording is needed here. From Eq. 5 at least it is not obvious how the error becomes less, unless there are large negative correlations.

p2733 I23: what do these standard deviations refer to? Standard deviations of the differences between SBUV and MLS? Please specify.

p2734 I7: "2-sigma range" is a bit ambiguous. Do you mean plusminus 1 sigma (which gives a span of 2 sigma) or do you mean plusminus 2 sigma?

p2734 I19: Is this really a limitation of the SBUV algorithm, or a limitation of the measurement system. The wording "limitation of the algorithm" suggests that with another algorithm better information could be retrieved from the same measurement data. I doubt that this is what you intend to say.

p2734 I22: Here it is stated that seasonal prior is used. Without this information some of the earlier contents of the paper cannot be understood, e.g. why this study focuses only on year-to-year variability (c.f. comment on p2728 I12)

p2735 I20: Is the DGF of the thick layer really larger than the sum of the DFS of its "parent layers"? How can this be? Merging into one layer is a kind of "hard constraint" which should reduce the total DGF of the profile. Or do you mean larger than the DFS of each of the parent layers?

p2735 I22: I do not understand how it is possible to get more information by merging layers. I agree that the DFS of the thick layer can be larger than any of its parent layers but this does not maximise information. If it is argued with the term "information", the Shannon information content of the entire profile needs to be formally evaluated.

Fig 3: It is misleading to represent the DFS by a continuous line because these are discrete numbers referring to the layers. The total DFS is not the integral over the line but the sum of the discrete DFS. Thus, symbols should be plotted. If need be, these can be connected by a faint line to guide the eye but the information is contained in the symbols, not the line.

Fig 4 vs. Fig 5: According to the plots, the smoothing error can even be larger than $\sqrt{\text{diag}(C)}$ (particularly near 100 hPa) i.e. after the retrieval there seems to be less information than before. How can this be? Does the retrieval destroy information?

Technical comments:

p 2723 l23: The abbreviation mzm is defined only in the abstract but not in the body of text. However, since both the abstract and the body of the text must stand alone, a definition in the body of the text is necessary, and the definition in the abstract might be obsolete.

p2734 l1: Shouldn't the caption read "QBO"?

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