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Interactive comment on "Merged ozone profiles from four MIPAS Processors" *by* Alexandra Laeng et al.

Anonymous Referee #2

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The manuscript 'Merged ozone profiles from four MIPAS processors' by A. Laeng et al. presents the method of merging MIPAS ozone retrievals derived from four independent retrieval algorithms. The manuscript requires a major revision. I found it very difficult to read and follow the manuscript. I think it is partially due to the way the manuscript is organized. I would suggest authors to rearrange the order in the paper (see my comments below). Specifically, I feel that authors should start Section 2 by describing the way they defined the error covariance matrices (currently Sections 4 and 3). I also feel that authors use too many equations in the manuscript, some of them are not really needed. Instead, I would suggest authors to describe some of the results in words. In some places, it is difficult to understand statements; some statements are vague and require more explanation and references, while other statements are irrelevant to the topic of the study and can be skipped. Finally, I feel that authors need to put more

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efforts to justify their approach of merging data and provide a stronger validation.

Specific Comments:

Page 2, lines 50-54: It says that only KIT processor accounts for horizontal temperature gradients, however, it was noted above that the Bologna processor uses a “full 2D-approach”. I thought that the 2D approach should take care about horizontal inhomogeneity (gradients) in T, O3 and other parameters. Please, comment on this.

Page 3, line 61: Why did you merge data only for two years 2007 and 2008? Is there is a specific reason for that? Do you plan to merge data for entire time period of MIPAS operation? Please, clarify that in the text.

Page 3, line 63: It is not clear from the context what are the “corresponding” covariance matrices. I would suggest to call them “statistical covariance matrices” (as you do in Section 4) and add in parenthesis “(see Section X below for the details)”.

Page 3, line 66: I don’t quite understand the statement: “. . .the source measurements can be considered as nearly independent with respect to the primary measurement error.” Do you mean that the errors can be considered as independent because processors use different spectral micro-windows? Please, edit this sentence.

Page 3, line 67:I think that the statement “to be better” requires further explanation: do you mean better precision, accuracy or information content?.

Page 3, lines 70-72: Which dataset do you refer as ’climatological datasets’? I think that this discussion is irrelevant to the topic of the study and can be removed.

Page 3, lines 73-74: Usually, term “biases” is used when one compares two datasets. In the context here do you mean biases relative to the ensemble mean or biases relative to the specific reference?

Page 3, lines 75-78: I don’t quite agree with the statement that ’different degradation of instruments’ is completely irrelevant. For instance, for the same instrument degradation could be different in different spectral ranges. Thus, if two algorithms use measurements from different spectral ranges, one could be more sensitive to instrumental degradation than another. If you make this statement specifically about MIPAS instrument, then please make it clear. I would strongly recommend adding a reference to the study that demonstrates that MIPAS degradation is similar in all spectral windows used by different groups for the O3 retrievals.

Section 2, page 3: The section starts by stating that the value of the merged profile at any given layer is a weighed sum of O3 values from all four parent processors and all levels. However, it remains unclear (until Sections 4 and 3) what is actually used as weights for the merged profile, which makes it very difficult to follow the paper. I think it would be better to provide a definition of errors first, and then move to the merging technique (put sections 4 and 3 first, and then move on with Section 2). Also, the motivation for considering contributions from all the layers is not clearly explained in the text. The standard approach would be to simply average O3 values from 4 processors at the given layer. You propose to weight contributions from each processor based on uncertainties, and additionally account for out-of layer contributions. You should clearly explain motivation for doing this. Why the contributions from other levels are so important for the merged product?

Page 4, equation 1: I don't quite understand this equation. The x_merged is a merged profile, and thus should be a vector with n elements (where I assume n is a number of altitude levels (n should be introduced in the text)). If matrix e has dimensions [n x n], then the matrix (e e e e) will have dimensions [n x 4n], and the final product of eq. 1 will be [n x n] matrix, and not a vector [n]. Please, correct this equation.

Pages 4-5, lines 104-120: I feel that this discussion is irrelevant to the main topic of the paper. Essentially what was calculated and evaluated in this study is variability relative to ensemble mean. It is not a random or systematic error. As authors noted in Sect.1, there are many common features in considered MIPAS retrievals, thus analysis of noise relative to ensemble mean will tell you very little about the instrument systematic errors.

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So I am not sure how you can isolate a random component here, and specifically it's not clear to me how you define a systematic component in eq. 6 (I guess it is assumed as zero). On page 6, line 140, authors state that "at fixed height systematic error component is constant". I don't quite agree with this statement, there are many examples of systematic errors that vary with season or over the lifetime of the sensor. I think that the statistical error considered in this study is a composite of random and systematic components. I would strongly advice to not use term "random".

Page 5, line 116-117: Equation 5 assumes that the errors are additive. I am not quite sure what is the meaning of the following statement: "Our choice is to neglect both these facts ..."

Page 5, line 125-130: Authors discuss possible reasons for error correlations between a pair of processors. I would assume that a priori assumptions could play a role here. A priori assumptions together with the assumed instrumental noise define the amplitude of the retrieved noise. Additionally, a priori covariance matrix defines a range of layers where O3 variability should be correlated. I would think that if two processors use the same a priori information, you might expect stronger correlations. I am wondering if authors considered a contribution of a priori information in this study.

Page 6: Authors provide numerous lengthy equations, but it would be nice to summarize some results in words. Essentially you analyze the noise relative to ensemble mean by considering retrievals for all geolocations and entire time period from 2007-2008. I think you don't need to reproduce well-known equations for the expectations and correlations. You need to clearly define the way you compute anomalies. And I think it is not clearly explained or defined here. Do you derive correlation coefficients by considering all data over all locations or do you sort data by season and latitude bands? It is interesting how robust these covariance matrices when you rearrange data. Did you check if these covariance matrices representative for all seasons and latitude bands?

Page 7, lines 171-173: The high inter-level correlations of retrieval noise between a pair of instruments do not necessarily suggest that the retrieved ozone from one processor at altitude z1 is connected to retrieved ozone from another processor at altitude z10, especially if these two layers separated in altitude. You should have some physical reasons to believe that errors at 20 km are connected with errors at 50 km.

Section 4, page 8, lines 180-200: I think that these two paragraph are completely irrelevant. Here, authors discuss the error covariance matrices reported by different MIPAS algorithms and provide equations for these error covariance matrices, but these error covariance matrices have not been used in this study. Then what is the purpose of describing these covariance matrices? I think that this information is irrelevant and distractive for readers. I think you should limit this discussion by saying that due to a lack of reported retrieval metrics you decided to move forward with the statistical covariance matrices.

Section 4, page 8, equation 17: if I understood this equation correctly, the empirical matrices are constructed by calculating covariance between anomalies at different layers over entire range of geolocations. Here, I have two questions. How sensitive these covariance matrices to the order of data (by time, geolocations etc.)? It's well known fact that the morphology of vertical ozone distribution is quite different in the tropics from that in mid-latitudes. In mid-latitudes the ozone peak is broader and is located lower compared to the tropics. I would expect the inter-level correlation to be different if you sort data by latitudes. I am wondering how changes in constructing these empirical covariance matrices affect your final product. For instance, if you assume no inter-level correlation of errors, how will your merge product change? How large is the contribution at any specific layer (in %) from other layers? This is something that I would encourage authors to examine. This would demonstrate a benefit of this approach compare to the standard approach when merged value at the specific altitude is a simple weighted sum of parent values from the same layer. Section 5. I feel that validation section can be greatly improved in the revised manuscript. The main difference of this proposed

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technique of merging several datasets is in accounting for out-of-layer contributions (by considering full covariance matrices including off-diagonal elements). I would explore and validate this deeper in Section 5. Does your merged product perform better than the simple average? It would be nice if you can provide some stats on the contributions from other layers? Do you see a specific altitude range or latitude band or time period where the out-of-layer contribution is the largest? Do you see improvement in the merged product compare to the simple ensemble mean in the same places and time periods?

Figure 3, page 9, lines 216-221: It is not clear to me what is the purpose of Figure 3 and the corresponding discussion. Why did you show a sonde profile if you believe it is biased? Could you find another sonde station that you trust?

Page 10, lines 233-235: I do not see that the merged product have smaller biases that any of four parent profiles. The orange line, which represents the merged profile, always goes close to the ensemble mean. Specifically, at altitudes above the ozone peak (≈42 km) all four processors are biased high against ACE, and merged values agrees with KIT; around the ozone peak (39 km) the merged value is right in the middle; and below the peak (36 km) the merged value is close to zero, which is expected because ESA and KIT have negative biases and Oxford and Bologna positive. Please, explain what did you mean here?

Technical comments:

Page 1, line 2: should be 'infrared';

Page 1, line 4: I suggest to replace colon (:) with 'developed by';

Page 1, lines 9-10: Last two sentences of the abstract require some revision. I would suggest to replace them with 'Hence, information content of the merged product is greater and the precision is better than those of any parent dataset.'

Page 4, lines 95-96: I would recommend adding here "(see section 3)".

Page 4, lines 91-92: Replace Sx1 etc. with S11, because they are calculated in the same manner as any Sij, but for the case i=j.

Page 5, line 109: Should be "example".

Page 5, line 110: Please add "a" to "a strong random component";

Page 5, line 123-124: Please, re-phrase this sentence. Do you mean "algorithmic differences" when saying "or if the differences in the retrieval algorithm dominate";

Page 5, line 126: Add "Bologna's correlation coefficients".

Page 7, line 171: Replace "errors are non-negligibly correlated" with "errors are correlated".

Page 6, equation 12, the matrix "r" is in lowercase, while on page 4 it defined as "R" (uppercase). Please, use consistent terms throughout the paper.

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