Atmospheric
Measurement
Techniques
Discussions

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

Anonymous Referee #1

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This paper provides the theoretical foundation and first results for merging the four competing MIPAS ozone processors in what the authors call a "mathematically clean" way. The merging methodology is useful, but there are several shortcomings with the text, and in the validation and results. Overall the paper needs more explanation around the mathematical methodology and the meaningfulness of the calculations and results. The grammatical composition is also weak throughout the text, and really needs the engagement of the impressive consortium of co-authors to raise the level of quality of the writing. If this, and the issues raised below, can be satisfactorily addressed, this paper could be suitable for publication in AMT.

-Abstract: - What does it mean that the information content is more important? - The phrase "parent profile" probably needs definition as it's not an overly common term - A statement is made about the change in relative bias with ACE-FTS and about

the absolute bias with respect to MLS. Please make these statements consistent; the change in relative bias seems to be more relevant.

- Why is the study performed with only 2 years of data? It seems that it should be quite easy to apply the methodology to the entire data set and make it publically available. This seems especially important in light of the fact that one of the main motivations for this work, as claimed by the authors, is the confusion in the scientific community about which product is "better" and which should be used.

- The different choice of microwindows for the four processors is a relatively important aspect of this approach, so it would be good to see an indication of just how 'independent' these choices are. What is the actual overlap in the source measurements? Surely it is not zero. It is noted later in the paper (line 122) that at least 2 of the processors use identical microwindows. How can this be taken into account? In this case it's not clear that this merging is a useful exercise, unless the errors from the retrieval algorithms are random somehow?

- The important conclusions of the comparison of these products in Laeng et al., 2015, should be summarized and potentially referenced in the discussion of the results. - Do the authors really mean that the merging weights depend on the "quality" of the error estimates? Or simply the magnitude of the error? Is a small error a "better error estimate"? Please clarify. - The first paragraph of Section 3 seems mostly like a random (no pun intended) collection of facts and the point is not clear. - Equation 5. Can any statements be made about the impact of this assumption? - Equation 6 should be typeset as an equation with an equality (i.e. set to ϵ_{random}) -Line 140: Why can it be assumed that the systematic error component for each processor is constant?

-Is Figure 1 calculated with the entire 2007-2008 data set? If so, the sharpness of some of the structures is difficult to understand. Comments on this would be insightful.

-Line 193: The term "statistical covariance matrix" is confusing. It seems that it means

the authors calculate it directly from the data set and later in the text (eg. Line 201) they refer to the same as "empirical covariance matrix". Also, are "genuine" and "analytic" covariance matrices the same?

-It is not clear how using an analytic covariance can cause the intercorrelation matrix to be singular. Please explain.

-Figure 3 with the ozonesonde profile adds very little to the discussion and nothing of substance is discussed. To simply show an example of the merging, it would be better to provide a plot showing the error bars on the single profiles and the resulting error bars on the merged profile.

-Line 230 and following: The authors state that the merging is "not supposed to remove the bias"; however, the merging will of course change the bias through the averaging and this could result in an improved situation depending on the sign and magnitude of the existing biases in the four products. This should be carefully explained given what is known from the independent validation of the individual products.

-The discussion around the ACE-FTS and MLS comparisons details several altitude regions where the precision and/or bias is "better" in the merged product. In general however, it is not clear whether it is meaningfully better (in some cases the differences are quite small). The authors should be able to quantitatively state whether or not this is the case. Additionally, it is important to know if these comparisons and associated conclusions hold over all latitudes where the shape and magnitude of the ozone profile varies considerably. At least with the MLS data set, the sampling should be sufficient to test this.

- The conclusions need to address the availability of the data product and give a recommendation to the community as to whether or not this data should be used as a general replacement for any specific processor in scientific studies, or if it is only of use in specific cases.

Minor/editorial points: Undefined acronyms in the abstract and throughout Line 12: comma splice Line 31: comma required after "corresponding period" Is there no publication reference for the Oxford product? Line 38: unspecific subject. Please rephrase to something like "The existence of these four products often leads to confusion..." Line 39: What is a "homogenized description"? Line 45: "but" not grammatically correct Line 63: comma splice Equation 3: It should be noted that the calculation of R is discussed below. Line 109: "A typical example" (note spelling) Line 131: Given values of i, j in brackets are typeset in a confusing way – almost looks like q is a function? Line 151: Do you mean "We note that N is the number of profiles ..."? Equation 11: use an equality Line 217/219: Use conventional degrees symbols