Referee report to "Tomographic retrievals of ozone with the OMPS Limb Profiler: algorithm description and preliminary results" by D. Zawada et al.

I appreciate the extended validation and some text adjustments made by authors in accordance with my comments. Although the answers to the remaining comments are still not fully convincing I agree to close the discussion with respect to the majority of the issues as they are rather minor and do not justify the decision to do not accept the paper. However, two of the remaining issues (as listed below) I still consider major and recommend to sort them out before the manuscript can be accepted for publication.

Major issues:

Author's reply: *... We choose to show anomalies for two reasons. The first is that the primary use of the dataset so far is in trend analyses, where the anomalies are used. Second, tropical anomalies are shown to demonstrate that features such as the anomalous QBO disruption are present in the dataset. Anomalies for other latitude bands and the seasonal cycle of the dataset have already been studied in detail by Sofieva et. al. (2017), and we do not see the value in repeating similar analyses here....*

May be the authors misunderstood my comment, I do not require to replace the anomaly plot by the plot with absolute values. I just would like you to add another plot showing the differences in absolute values. As you already have all the data it requires, to my opinion, just a minor effort. I agree that the anomalies are very interesting to see, but the agreement in the absolute values is also of a great interest and I cannot understand the reasons why you refuse to show it. The reference to Sofieva et. al. (2017) is irrelevant in this respect, as this paper does not consider MLS data, which your comparisons are focused at.

Author's reply: *We agree that the atmospheric inverse problem is typically ill-conditioned, but this is only without regularization present. The purpose of including regularization is to improve the conditioning of the problem so the first statement does not apply to our retrieval. It is true that including the LM term changes the path to the solution, but we do not agree that this changes the solution. In a comment on the discussion version of the Ceccherini et al. paper (https://www.atmos-chem-phys-discuss.net/9/C9660/2010/acpd-9-C9660-2010.pdf) Dr. von Clarmann argues that "converged Levenberg-Marquardt retrievals are characterized by the same covariance matrices and averaging kernels as Gauss-Newton or optimal estimation retrievals." In the reply to this comment by Ceccherini et al. (https://www.atmos-chem- phys-discuss.net/9/C10551/2010/acpd-9-C10551-2010.pdf) they state "The differences that exist between our results and the expectations of Dr. von Clarmann can be explained by the fact that the test retrievals presented in the discussion paper do not use external constraints (R=0)" and furthermore that "In the case of well conditioned problems the formulas of the discussion paper produce the same results as those of Dr. von Clarmann." Therefore it is not correct to include the Levenberg-Marquardt term* *in the error analysis and characterization.*

My original comment did not refer to the solution. The issue was that the method to calculate the averaging kernels and solution variances is to my opinion not quite correct. In this respect, I see no contradiction between the findings published in the final paper by Ceccherini and Ridolfi (2010) and comments by Dr. von Clarmann to the discussion paper. Considering the issues listed below, both documents also perfectly conform to my comment.

- Dr. von Clarmann in his comments starts from Eq.(2) to arrive to the conclusion that "converged Levenberg-Marquardt retrievals are characterized by the same covariance matrices and averaging kernels as Gauss-Newton or optimal estimation retrievals". However, the discussion flow and the conclusion are only valid if the matrix $\vec{R}$ ($R^T R$ in the notations of the manuscript) in Eq.(2) in invertible. This is however not the case for the regularization matrix given by Eq. (7) of the manuscript.
- As pointed out by Dr. von Clarmann in his comments "With a large Levenberg-Marquardt term it is easy to obtain a small relative variation of the chi-square although the retrieval is still far from convergence, and retrievals where the iteration has been interrupted without making sure that $\vec{x}_{i+1} - \vec{x}_i \rightarrow 0$ even without a damping term should be discarded and by no means be accepted as a solution". In Sect. 2.6 of the manuscript the authors state that they do a fixed number of iterations and analyze only the chi-square. Thus, it is highly probable that the retrieval is non-converged and the situation highlighted by Ceccherini and Ridolfi (2010) occurs: "... the LM method acts as an external constraint and the solution depends on the path followed by the minimization procedure in the parameter space. This latter conclusion applies also to retrievals in which the iterations are stopped when a physically meaningful convergence criterion is fulfilled, i.e. before achievement of the numerical convergence at machine precision"

Thus, to my opinion, the study by Ceccherini and Ridolfi (2010) is highly relevant for the retrieval used in the manuscript and the method to calculate the averaging kernels and variances described by Ceccherini and Ridolfi (2010) needs to be applied. Furthermore, authors should reconsider their iterative approach to ensure that the retrieval converges.

Ceccherini, S. and Ridolfi, M.: Technical Note: Variance-covariance matrix and averaging kernels for the Levenberg-Marquardt solution of the retrieval of atmospheric vertical profiles, Atmos. Chem. Phys., 10, 3131-3139, https://doi.org/10.5194/acp-10-3131-2010, 2010.

Minor comments:

Page 19, line 33: “upwelling” $\rightarrow$ “upwelling radiation”

Page 21, line 7: Please make a statement whether the selected 251 orbits are evenly distributed over the seasons or not.

Page 21, lines 13 - 14: “The generally good agreement ...” $\rightarrow$ it should be noted here that the observed standard deviation includes the natural variability while the predicted one does not. So, the good agreement between these two values is not necessary a good sign.