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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: Evaluation of simultaneous measurements of mesospheric OH, HO$_2$, and O$_3$ under photochemical equilibrium assumption: Statistical approach" *by* Mikhail Yu. Kulikov et al.

Anonymous Referee #1

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This technical note uses a statistical approach to the analysis of simultaneous measurements of OH, HO2, and O3 in the mesosphere from the Microwave Limb Sounder (MLS). A simplified algebraic equation is presented that relates these species under photochemical equilibrium, and this analytic formulation is compared to simulations from a chemical transport model. A Bayesian statistical approach is applied to the analytic formulation and constrained by the MLS measurements and their errors, in order to produce "retrieved" sets of trace gas profiles which are then compared with the original measurement profiles.

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Discussion paper

Overall, this methodology is interesting and appears to have some merit. However, there are a number of major issues that should be addressed in order to make this paper acceptable for publication.

Major Points:

1. There are a number of problems in the way the MLS data are presented and used. First, it is not clear that the day-night differences are being used for the "standard" HO2 product, as recommended in validation papers and in MLS data quality documents. This is critical since HO2 is a major focus for the paper. Second, there is no mention of the version of the data used (v3.3?) and whether all of the appropriate data screens were used. Third, all of the figures show data and profiles to 0.01 hPa, when in fact the useful range for HO2 is clearly stated at 0.046 hPa, and for O3 at 0.02 hPa. Data above these levels are not recommended for science purposes and should not be part of the analysis. This paper would clearly benefit from some interaction and discussion with members of the MLS team to ensure that the data are being applied correctly.

2. The connection between the CTM results and the analytic formulation is not sufficiently explored. Figure 1 is used to show that the difference between the CTM and equilibrium equation is a few percent or less, and that this justifies the use of the analytic formulation in the subsequent statistical evaluation. However, there is not enough information given about the CTM to judge whether or not this is really an independent validation. Presumably, the CTM uses the same set of reactions and the same equilibrium assumptions along with the "family" chemical species approach, so it is not surprising that they agree. It is also not clear whether the CTM also includes NOx chemistry, which might have an impact in the upper stratosphere and lower mesosphere. As a further example, there are known effects on HOx and NOx in the mesosphere from energetic particle precipitation events [e.g. many papers by Jackman et al and Verronen et al], and it would be important for the CTM to include these if it is intended to be used as a validation of the equilibrium expression. In fact, Figures 2 and 3 display data from January 2005, during a period with a documented SPE event [Jackman et

al., ACP, 2011]. The possible influence of SPE perturbations needs to be discussed and addressed.

If the CTM includes a comprehensive simulation of all known reactions and processes impacting HOx, and there is still agreement with the analytic approach, then the results are more credible. The paper should demonstrate that in fact the CTM provides a complete description of HOx photochemistry. Finally, there is a problem with converting pressure altitudes in the model to pressures by assuming a constant scale height of 7 km. This assumption can lead to offsets of 1 km or larger and is particularly critical when comparing vertical profiles with large gradients. For example, at 62 km the US std atmosphere has a pressure of 0.1671 mb. If a constant 7-km scale height is used in the expression in the paper, one would calculate p=0.1442 at 62 km. The analysis in this paper should use the temperatures in the model to compute convert altitude to pressure using hydrostatic equilibrium on a layer-by-layer basis.

3. There is not enough detail on the conclusion that the offline HO2 product better agrees with the statistical retrieval. First, there is no direct comparison between the offline product and the statistical retrieval, as there is for the standard product and the statistical retrieval (figs 4-6). There is only a presentation of the offline HO2 product, and the reader has to refer back to figs 4-6 and then estimate just how much better the agreement really is. The improvement needs to be quantified more directly. Also, there is no discussion as to why the agreement is better and this really gets down to the science. Does this paper intend to suggest that the offline HO2 product is superior to the standard product? If so, then the discussion/conclusion needs to be expanded to clearly justify why this is so. There also needs to be some explanation for why the offline product is different in the first place, without forcing to reader to go back and review the Milan et al [2015] paper.

Minor Points:

The grammar and wording is awkward or incorrect in a number of places, for example Abstract: “statistically correct approach” is not illuminating. One would not expect ACP to publish a “statistically incorrect approach”. Suggest a ”Bayesian statistical approach”. “air concentration” is more generally “neutral density”, “air temperature” is generally just “temperature”. “We have performed *a* one-year simulation”. Last sentence is unclear. Also “MLS primary data”, what is primary data? Perhaps this should be MLS radiances.

In terms of the reaction sets, there appears to be a gaping omission of the HOx production reactions, H2O+hv and H2O+O(1D). These should be included for completeness; however, I suspect their terms may drop out when forming the ratio OH/HO2.

When deriving equation 14, which forms a major basis for the paper, I was not able to reproduce the authors’ results and cannot comment on the validity of this result. It seems much more complicated than the text would suggest. I strongly recommend an appendix which details the step-by-step process at arriving at equation 14.

Figure 3 is unclear what ranges of latitudes are being shown.

Figs 4-6 contain horizontal dotted lines but these are not explained in the text or caption.

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