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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A Match-based approach to the estimation of polar stratospheric ozone loss using Aura Microwave Limb Sounder observations" *by* N. J. Livesey et al.

Anonymous Referee #1

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General

This paper extends the Match technique from sonde observations to global MLS satellite data. In the Match technique, pairs of ozone observations that are connected by an air mass trajectory are combined to a global ozone loss estimate.

The paper is well written and is a valuable addition to the scientific literature. With the use of the MLS data set, the method largely benefits from the huge amount of data, although the single data profile is less accurate than an ozone sonde. With this method, the authors are able to provide much more detailed statements about chemical ozone

loss than the "classical" ozone sonde Match.

Especially, the additional use of N_2O for separately deriving the transport error is significantly strengthening the method. Also the sensitivity with respect to various assumptions (Fig. 2) is valuable. I would recommend it for publication in ACP, although I have a few suggestions summarized below.

Major Issues

p. 10050 / fig 1. Figure 1 shows an example of the data that contribute to one Match result. With MLS data, there are orders of magnitude more matching pairs per data point than in the sonde Match analysis. This gives much better statistics. However, the evaluation is chosen such, that the fitted line includes the origin (0,0). In turn this means, that the weight of individual match is larger, if the sunlight hours are larger. In the shown example, the data with more than 100 hours likely have larger error in trajectory location. By eye, it looks like if the data were restricted e.g. to 60 sunlight hours, the slope would be steeper. A sensitivity with respect to a "cutoff maximum sunlight time" or "minimum matches per hour/20ppb bin" would be interesting. This is similar, but not identical to the shown sensitivity with respect to the match trajectory length.

p. 10056f, section 3.5. constraints on nighttime ozone loss. Nighttime ozone loss can be shown to be negligible in certain regions/periods and is not to be discriminated in other regions. Although it is currently not believed that there is nighttime ozone loss and it had been shown by the bi-variate analysis by Rex et al., that nighttime ozone loss is not likely, I have the feeling, that more could be concluded from this analysis. Do I understand this correctly, that one would get no nighttime ozone loss in some cases and not enough accuracy to discriminate in other circumstances? I suppose that there is no period/region with a statistically significant derived nighttime ozone loss. If this is the case, I think that it would be valuable to show these regions in which there is no significant nighttime ozone loss, as a “partial” proof.

Variability of the results. As said in the paper, the variability of ozone loss in the different studies is indicative of the individual methods (data locations and time, vortex edge definitions). It results from the wish to deriving one single average number for a quantity that is variable, especially as a function of equivalent latitude. The high statistics of the matches shown, could also be used to highlight this point, i.e. to show the ozone loss rate and/or the accumulated ozone loss sub-sampled as a function of equivalent latitude or sPV bins and time.

Dependence on the vortex edge criterion. (p. 10051/6ff, 10052/4ff, 10057/16ff, figure 5, tables 1+2) The point that the number “vortex average ozone loss” does depend on the time period and the vortex edge criterion was made earlier (e.g. Grooß et al., 2008). Most methods for deriving chemical ozone loss are limited due to data availability. However with a model or a data set with the coverage in space and time as MLS, such comparison is possible, i.e. a comparison with each publication of the value derived for the corresponding time range and vortex edge definition (e.g. as in Grooß et al., 2008). This may require some diligent work for Table 1, and as it stands now the paper is already very good, and this is not a necessary addition. But this comparison may potentially contribute to the understanding of parts of the differences. However, it would be at least interesting to see the comparison with Kuttippurath in table 2 using equivalent latitude 65°N instead of sPV=$1.4 \cdot 10^{-4} s^{-1}$.

Minor Issues

p. 10043/13. PSCs do not form from gaseous sulfate species but from sulfate aerosol. Also heterogeneous reactions not only take place on the surfaces of the particles but also in the bulk of the liquid particles.

p. 10044/19. Here the paper by Müller et al. (2005) should also be mentioned that discusses the ideas of Michelsen et al. and Plumb et al.

p. 10045/6ff. You could mention the advantage of using limb sounding as MLS over solar occultation data (POAM, ILAS) due to much better statistics.

p. 10045/9. As an example for determining chemical kinetic constants from atmospheric observations, I would rather propose to cite von Hobe et al. (2007) and/or Suminska-Ebersoldt et al. (2012) as the Schofield study has a rather large uncertainty (see e.g. fig 2-11 of the 2010 WMO ozone assessment)

p. 10048/section 2.3. The matches with a strong divergence in sPV between the central and flanking trajectories are discarded similar to the ozone sonde Match studies by Rex et al. The idea behind this is of course to discard matches with a possibly large trajectory error. However, this implicitly means that also areas of large wind divergence are avoided systematically. Is it possible to add one point in the panels figure 2 that represents an evaluation without the Flank divergence criterion? Likely it should be in the vicinity of the 500 km point, but it would be better to show it in this figure.

p. 10049 /8ff. Good. It is important to check whether the destination observations is also within the polar vortex, which has not been done always in the case of the sonde Match (see e.g. Fig. 7 of Grooß et al., 2008, ACP)

p. 10053 / 14f. How exactly are the loss rates integrated over the winter? Is it done on descending surfaces following the average vortex descent rate? If so, how is the descent rate determined? By the use of MLS N_2O?

p. 10054 / 3f. Potentially other possible error sources could be mentioned as errors in the wind fields taken from the meteorological analyses or interpolation errors.

p. 10058 /4. Mention also the chosen vortex edge criterion.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 10041, 2015.