Atmos. Chem. Phys. Discuss., 5, S2911–S2914, 2005
www.atmos-chem-phys.org/acpd/5/S2911/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurements of NO, NO_y, N_2O, and O_3 during SPURT: implications for transport and chemistry in the lowermost stratosphere" *by* M. I. Hegglin et al.

Anonymous Referee #2

Received and published: 28 September 2005

The UT/LMS region is clearly important for issues related to ozone and climate change, but one that is characterized by high variability, confounding our efforts to systematically represent it in large-scale models. Furthermore, our climatologies are data poor in this region. This paper presents a very useful and reasonably unprecedented UT/LMS dataset that will augment our climatology, and offers a useful interpretive framework for important features of the dataset. The paper is well-organized, contains good figures, and has clear expression of ideas. As a result, I recommend publication after the following comments are addressed/considered.

Important points: 1) Overall the conclusions are a bit soft in the sense that most could be anticipated and not all are fully convincing. The authors hedge by the use of 'implies;, 'may', 'can be' or simply assert the conclusion without justification. Examples are p8660, ln 5 ' The observed changes . . . . can be explained by . . . the circulation.' And p8861, ln5 'This implies that the extratropical tropopause acts as a barrier . . .' The conclusions are not unreasonable and have not been stated for the first time in this paper. However, since the data represent a snapshot of the region and the annual cycle is assembled from more than one year, the authors, like many authors, are at a disadvantage in claiming certainty for the cause(s) of the features observed in their dataset. A more convincing explanation requires more; for example, the use of a large-scale chemistry/transport model, which in itself will have limitations. My suggestion after all of this is that the authors make a over-arching statement somewhere in the paper that reflects the authors understanding and awareness that, without further investigation, the many of the conclusions presented lack convincing proof and that other factors that cannot be taken into account might also play a role. This also foreshadows the fact that the greatest value of this large dataset might be realized in the future, for example, in comparison to a model or other dataset.

2) In discussing what air moves into and out of the LMS and when, I suggest that the results presented in a paper by Ray et al. be consulted (Journal of Geophysical Research 104, 26565-26580, 1999). This paper makes some very specific conclusions about the role of isentropic transport and stratospheric descent in influencing tracer abundances in the LMS at low and high latitudes that could augment the present conclusions.

3) The NOy/N2O scatter plots would be valuable to show since many have used this correlation in other interpretive studies. On the other hand, showing NOx profiles is not very instructive since NO2 is not conserved. Instead, I suggest the first NOx-related plot shown be that of NOx/NOy.

4) p8667 ln 26. The 'upper tropospheric conditions' in the model should be specified and/or compared for the species measured in the dataset other than NO, i.e., NOy, CO,

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O3, etc. The value of the Grooss et al. model here depends on how well it represents the sampled air masses.

5) p8669, ln6. The disagreement with Grooss et al. seems fundamental to this study, i.e., worthy of resolution, and, hence, worthy of further effort and discussion in this paper.

6) p8669, ln 10, In the discussion of the calculation of NOcrit here, the source of CO values is not acknowledged.

7) p8669 ln 10+ and Figure 10. I am confused about the source of the red line in Figure 12. My best guess is a climatological value of NOcrit. The value of drawing conclusions from measured NO vs this quantity is not clear. If CO and O3 values are available along each flight track, then NOcrit can be calculated point-by-point and compared to measured NO in a scatter plot.

Less important points: 8) p8665, ln 12+. I suggest citing the NOy/N2O correlation slopes for the NH reported in the Keim et al. reference and perhaps including them in Fig. 10.

9) Minor point: p8660 ln 11: Few would agree with the statement that ozone is distributed uniformly in the troposphere; more, but not all, would agree concerning N2O. Be more specific.

10) p8668, ln16. Many of the recommended rate constants in Brasseur and Solomon have likely changed since 1986. Please reassure the reader by stating adequate agreement with more recent evaluations, e.g., that from the Jet Propulsion Laboratory in the US.

11) In Figure 9: Suggest adding the April fit line to the other panels as an aid to the eye in evaluating differences. Also explain the two colors of datapoints in the caption and the nature of the fit line.

12) There are a few instances of ‘allow to determine’ which is not generally accepted

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usage, e.g., p8667 ln 20.

13) Define the seasonal time windows used, i.e. winter is DJF, etc., since definitions vary.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 8649, 2005.