

Interactive comment on “Measurements of NO, NO_y, N₂O, and O₃ during SPURT: implications for transport and chemistry in the lowermost stratosphere” by M. I. Hegglin et al.

T. Birner (Referee)

thomas@atmosp.physics.utoronto.ca

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Review of "Measurements of NO, NO_y, N₂O, and O₃ during SPURT: implications for transport and chemistry in the lowermost stratosphere" by Hegglin et al.

This paper analyzes a very interesting and valuable dataset of atmospheric tracers in the extratropical UTLS. Vertical cross-sections in equivalent latitude–potential tem-

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perature coordinates are presented, as well as vertical tracer distributions relative to the tropopause, tracer–tracer correlations, and considerations on the prevailing chemical regime of the LMS. The paper extends previous results from the same campaign (SPURT) to other tracers. The findings add to the general understanding of the processes that shape the UTLS and are doubtless worth publishing and fit well into the scopes of ACP. However, I do have some major comments (see below) that should be addressed in a revised manuscript.

Major comments

1. The number of individual plots (more than 70) doesn't fit the number of outcomes in my view – I think they can be reduced without qualitative loss, which would increase clarity. In Figs. 2 through 5, and Fig. 9, I recommend showing 'seasonal' means, i.e. Jan+Feb, Apr+May, etc – those plots seem (a) very noisy (not many data points per bin?) and (b) don't show strong differences within a given season. Further, the differences between inner–season months aren't discussed that much in the paper, anyway.
2. The mapping on ECMWF fields seems somewhat problematic given the coarse vertical resolution of the model in the UTLS that certainly misses a great deal of the measured structure. E.g., ECMWF (potential) temperatures should have a positive bias just above the tropopause (see e.g. Birner et al., GRL 2002).
3. There seems to be a misconception of the relevance of the vertical PV structure (Fig. 7b, end of p8662, end of p8670). Note that only quasi–horizontal gradients of PV along isentropes maybe interpreted as mixing barrier. The vertical PV structure depends on the choice for 'Theta' and is, therefore, somewhat arbitrary (see the original paper by Ertel, 1942, or e.g. Lait, JAS 1994, Müller Günther, JAS 2003). A mixing barrier at the tropopause in the vertical exists because of

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the jump in static stability (N^2) which shows up (somewhat accidentally) in PV as well ($PV \propto N^2$). The general PV structure in Fig. 7b can be explained by this N^2 jump taking into account that this jump is reflected in $d(\Theta)$ as well (-5 K approx. correspond to +20 K, using a ratio of N^2 values of 1 to 4 between the troposphere and stratosphere). I suggest removing Fig. 7b and reformulating the respective discussion solely in terms of the (well known) jump in N^2 . Note that the vertical N^2 structure in the ECMWF model cannot be considered as being very realistic (e.g. Birner et al., GRL 2002).

4. Although I think the paper is generally well written, clarity could be improved in my view by pointing out more strongly the salient features and by avoiding loose statements such as 'good agreement' without further quantification (see examples below). Further, some of the structure of the paper is not fully consistent (some of the method description is presented in the results section and the conclusion section is mostly a summary).
5. Although the paper includes and discusses a rather comprehensive reference list, the recent paper by Pan et al., JGR 2004 should certainly be included and its results should be discussed in relation to the present paper.

Minor comments

General

1. The results are based on a limited number of flights over a limited geographical region. This could lead to sampling problems and should be discussed somewhere.
2. It should be more emphasized that equivalent latitude (EL) is, essentially, a PV coordinate. Therefore, conclusions regarding geographical dependencies drawn

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from the EL framework strongly rely on the background PV distribution. This is not straightforward (e.g. Birner 2005 finds, that meridional PV gradients are very small in the UTLS away from the tropopause). Nevertheless, I agree that PV coordinates are superior to conventional geographical coordinates for the reasons described in the present paper.

3. The integral statistical measure used in the paper is median values instead of mean values. A comparison to mean values should at least be mentioned in the text (or included in Figs. 6 etc, e.g. as thin lines).

Specific

(P – page, L – line)

P8650

- second sent.: suggest divide into two, e.g.: The measurements cover all seasons between November 2001 and July 2003. They probe ...; provide geographical latitude range instead of EL for clarity

- L15: see major comment, remove "horizontally" and "transport and" (next L)

p8651

- L7: either move exact NO_y definition from section 2 here or remove what's in parenthesis

- L13: remove "with respect ... composition"

p8652

- L5: global mean -> residual; or -> the

- L6: suggest remove "processes through the extratropical tropopause"

- L7: "... studies of STE, e.g. by using ..."
- L13: define what you mean by "mixing layer"
- L14: suggest remove "showing a strong tropospheric influence"
- L24: "filling partly the gap" -> too loose, please be more specific
p8653
- shorten outline, details of subsections don't need to be here
p8654
- L15: to what extent does this NO_y artifact influence your results?
p8655
- L4-6: "good agreement": please be more specific; are the ascent/descent measurements included anyway?
- L10: lacking -> missing
- L11: "excellent agreement": please be more specific
- suggest remove first part of first sent. of 3.1, start with "Rossby and smaller scale waves ..."; the rest of the EL motivation should be written more concise, see also general comment above
- L22: suggest: traditional -> geometric
p 8656
- L8: your $A(\text{PV})$ must have been normalized by earth's surface area ($4\pi a^2$), please correct
- L12: remove "the" and "pairs"; ECMWF model values: forecast or analyses?, next line remove "coordinates"

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- L16: "good approximation": see above

p8657:

- L1: is PVU defined somewhere?

- L15: "the two tracers ...": spell it out

p8658

- L20: "Mean distributions ...": you show median distributions

p8659

- remove first sent., has been said before, also L7

- how many data points go into each bin on average? 5 is not a large number to produce robust statistics.

- a lot on this page is method description, so should be moved into section 3

- L15: "geographical coverage" see general comment 2.

p8660

- L5: "can be explained": too strong -> "might be explained"

p8661

- state somewhere that discussion of pronounced PV gradients refers to the tropopause

- L15: little or no sunlight and NO_x : you said earlier that you cannot calculate nighttime NO_x - please comment

p8664

- L12/13: Fig. 7b shows vertical PV gradient, maybe ref. to Fig. 2?

p8665

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- L1: "well mixed": the concept should be explained shortly somewhere, say in section 3

- discussion of Fig. 10: I cannot say that I find the seasonality of the $\text{NO}_y\text{-N}_2\text{O}$ correlation from Fig. 10b convincing – variations in this correlation don't seem to be strong enough to show that there is a seasonal cycle from the number of data points. Also, the Figure caption doesn't describe everything that's in the Figure, e.g. what are the error bars and why are some point encircled and others aren't? Finally, why did you only choose half the SPURT points to plot in Fig. 10d?

p8667

- L18: remove "good"

- L22: insert "on" before "NO"

- L24: "It reproduces" -> "It is based on ..."

p8668

- L23/24: suggest remove "followed by ... Eq. (9)"; "the expression" -> "an expression"

p8670

- "Conclusions" would be better termed "Summary" given its content, although I wished the authors could actually present some conclusions

- L17/18: state that this refers to the tropopause

p8671

- first para: please elaborate on these points

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

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