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Interactive Comment

Interactive comment on "Seasonal cycles and variability of O_3 and H_2O in the UT/LMS during SPURT" *by* M. Krebsbach et al.

M. Krebsbach et al.

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We thank Referee ♯2 for her/his comments. We are happy to clarify the points mentioned and address her/his specific concerns below (Referee comments are in **bold**):

General comments: The paper summarizes the water and ozone observations during the airborne SPURT campaign. It contains substantial new data, though there seems to be some overlap with other publications by Hegglin and Hoor of SPURT results (and other discussion papers of SPURT participants currently pending).

In other publications from authors of the SPURT community, different concerns are addressed and dissimilar analysis methods are applied. Some results of this paper are *also outcomes from published SPURT studies which is, in particular with respect to the special issue, hardly to avoid. Nevertheless, in these studies different trace gases with distinct characteristics are considered.*

The paper does not present novel concepts, ideas or tools, but lots of new data. (Discussion of robustness of results falls short.)
We fully agree with the Referee that the applied tools are not new. To the authors' knowledge, however, in the literature these tools, in particular the PDFs, are very rarely applied to airborne in situ measurements in the UT/LMS. The outstanding advantages of these tools are indisputable. Both aspects encouraged us to show the extensive and new data that way. For discussion of robustness see below.

Specific comments: The switching back and forth between terms like LMS and physical units (PV $>$ 4PVU) to define different domains is confusing. Maybe a table with definitions would be helpful or to consistently mention the PV definition in brackets.
The definitions are already given in the introduction to Sec. 3 (cf. p 7253, l 11–14) and additionally several times for clarity in the text (e.g. p 7256, l 02-03, p 7257, l 04 and l 13, ...). In our opinion, there is no need for an additional table.

Review references to the Ph.D. thesis and other planned papers. Are the results from these works touched on in this paper really relevant and necessary? I suggest to remove most of them for clarity.
We removed the references given at p 7256, l 09–10, p 7258, l 02–03, p 7259, l 18, and p 7268, l 11–12.

p 7250 "cutting edge for a new concept of aircraft campaigns" I am not sure this is factually true: there have been seasonally resolved campaigns before (e.g. European STREAM, US STRAT, POLARIS missions). And for O_3 and H_2O the CARIBIC and MOZAIC data sets have even better temporal coverage, with, of

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course, reduced max altitude.

We only partly agree with the Referee. The concept of SPURT with its performance of long-term observations in short-term campaigns from a regular basis shows the acquisition of high-qualitative data with low costs and is thus certainly distinct from most of the mentioned campaigns by the Referee. Further, not all of the mentioned campaigns cover each season. The data coverage of SPURT is of course far from that obtained by CARIBIC and MOZAIC, but the latter two do not reach well into the LMS. The successful and effective SPURT concept, whose power has already been demonstrated, has set a good example for following campaigns.

"an extensive and continuous". This is an extensive data set, but does not qualify for continous. There are gaps even in the figures. how many flights were there?

We removed the words *and continuous* on p 7250, l 15. We added the total number of flights to the end of the passage on p 7251, l 21-23 as follows:

Each campaign consisted of a minimum of 2 flight days. On one day southbound and on the other day northbound flights were performed from and back to the Learjet basis Hohn (9.53° E, 54.31° N, northern Germany), resulting a total number of 36 fights.

p 7251 line 6: quantify consistency of instruments.

Though the references are given we included this information the following way:

Intercomparison of both instruments results in high consistency with a 6.4% difference which is in the uncertainty range of both instruments (Hegglin, 2004; Engel et al., 2005).

p 7252 line 3: what is the accuracy of the avionic pressure and temperature?

The uncertainty of the avionic pressure and temperature measurements is $\pm$1 hPa and $\pm$0.5 K. We included this information:

Potential temperature (Θ) is calculated from avionic measurements of pressure (p) and

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temperature (T), the latter two having an uncertainty range of ±1 hPa and ±0.5 K, respectively.

p 7257: Are these water maxima/minima robust? Does removing one flight change the results?
Due to the SPURT concept with two northbound and two southbound flights within each campaign, which were mirrored on the return flight to the base station to sample more or less the same meteorological condition but at different altitudes (cf. p 7252, l 10–18), there is partly an overlapping altitude region, especially during ascents and descents at the base station and the interstations as well as at the second flight level (p 7252, l 12–14). Thus, removing one flight does not change the results conspicuously. Of course, a higher discrepancy is evident when considering only southbound and northbound flights. This is already specified and mentioned in the paper (cf. p 7254, l 25–27 for O_3 and p 7257, l 09–12 for H_2O).

p 7257, l 12. The statement "distinct maximum during summer and a minimum during autumn and winter" is not substantiated in the figure, Why is May as low as November for PV 4–6, but is not discussed? This relates to my general comment above.
cf. reply to Eric Ray (Referee ♯1)

p 7257, l 25: could one problem be the resolution of PV? Is the real PV field in summer much more variable than the assimilation and hence higher H_2O VMR are only due to the effects of assimilation? Also, this result seems substantially different from Hintsa et al. Any reasons for this (e.g. limited sampling of UTLS by ER-2, climate trend, etc). The 2002 and 2003 summers were very active with PyroCB, see e.g. Fromm et al, Jost et al. Could the SPURT data be biased by pyroCB transport into the LMS?
We can not make a comment on the correctness of the ECMWF data. Nevertheless,

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these analyses (4D-Var) are, in our opinion, presently the best meteorological data set to put the measurements in the meteorological context and in atmospheric regions. Of course, in single cases, a wrong classification of tropospheric or stratospheric air is possible. We compared the avionic temperature and horizontal wind velocity measurements with the corresponding ECMWF data, the latter being interpolated in time and space to the aircraft position, and found no larger differences in summer compared to the other seasons. The mean temperature and horizontal wind speed differences are $<$2 K and $<$5 m/s. The summer measurements partly took place near cumulus clouds. This suggests that the measurement data are influenced by convection but there is no clear indication of a bias due to pyro-convection (see also reply by Hoor et al., Atmos. Chem. Phys. Discuss., 4, S1152–S1154, 2004).

p 7261, l 4: "kink is present" what kink (selection of this word seems more appropriate for oral speech while pointing out the feature)? the lines vary dramatically and there are couple of local minima and maxima (e.g. red dashed line in Fig 4). If the authors insist on this description, it should be clarified with an arrow in the figure.
We agree with that. We did not include an arrow in the right panel of Fig. 4 but rephrased the following way:
The large variation of the tropopause location is transparent in the high amounts of H_2O VMRs below ≈340 K, with several local maxima and minima in the course of means (and partly of the medians) between 300 and 320 K.

p 7261, l 16: what is a "rather steep distribution"? Maybe explain on current Figure 4.
We clarified the description of the PDFs as follows:
The shapes of the trace gas PDFs for the winter and spring measurements are quite different from the distributions obtained for summer and autumn (see Krebsbach, 2005). Whereas the former distributions show a more compact shape with a steady rise in

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O_3 VMRs with increasing Θ, the latter are less compact with a relatively weak and partly even no increase in O_3 VMRs towards higher isentropes, resulting in a prominent wedge structure.

Can I see the "prominent wedge structure" in the current figures? I am not sure what the authors are referring to. If these results are relevant for this paper, I suggest to either show the figures, or to leave this whole discussion out. This part almost feels like a teaser ad for the Ph.D. thesis, which is actually a little tricky to find (e.g. using the given author and title, google does not provide the answer. More familiarity with the German indexing system is required to find it. And then it is 50 MB!).

Indeed, you are right in the first point. As stated in the text the wedge structure can not be seen in the spring PDF for O_3 shown. There is always a fine balance between providing details and maintaining the ratio of text and figures. On this account, we do not show all seasonal PDFs and refer to the Ph.D. thesis when necessary, albeit mentioning the gross features. It is thus not supposed to be a "teaser ad" than a reference. Concerning your second point: The Ph.D. thesis has to be cited the way given. Following your described search procedure for the Ph.D. thesis (using google and the given author and title) gives at least as the first and second result the link to the Ph.D. thesis, even when using different top level domains. The same arises when using google and the resolver given in the reference. Nevertheless, we included the URL in the references.

p 7262, l 5: see above about kink.

We rephrased as follows:

In the PV-PDFs, additionally the trace gas gradients in dependence of the coordinate are calculated within 1 PVU intervals (see top numbers in Fig. 5). The slopes are ≈2–4 times stronger in the 2-4 PVU range than within the interval 0–2 PVU. The means and medians show commonly an increase in this range, indicating 2 PVU as a good proxy *for the dynamically defined extra-tropical tropopause during the SPURT missions.*

p 7263, l 4: be more specific about the intercomparison agreements
We specified as follows:
Nevertheless, intercomparisons for each campaign provide evidence for a clear agreement with correlation coefficients of $>$0.98 and slopes of 0.995 ($\pm$0.018).

p 7264, l 12 natural logarithm doesn't need reference. Or is this a reference to the entropy, then move it up two lines
Obviously it is true, that the natural logarithm needs no reference. We rephrased as follows:
[...] (e.g. Srikanth et al., 2000), with ln as the natural.

l 17: it is obvious that bin width is an important parameter for SE. How robust are the conclusions if the bin width is changed?
The conclusions are as robust to the choice of bin width as long as the bin widths are chosen accurately to consider an appropriate amount of data. We naturally considered that previously. Obviously, too large bin widths will smooth a lot, whereas too small bin widths will result in more noise. In addition, we give a restriction already in the text (cf. p 7266, l 02-05).

p 7265, l 9: "seems a little bit strange" Why is this "strange"? Seems not scientific discussion. Rephrase without "strange". "counterintuitive" might be what the authors are trying to say.
We thank the Referee for her/his linguistic advice. We implemented her/his suggestion.

l 23: "also a strong gradient... is evident" I don't see a very strong gradient at 2 PVU. There are other steeper gradients evident in the figure without discussion. e.g autumns at 8 PVU. Why the strong increase from 0–1 PVU in H_2O?
We refer to the reply concerning the robustness of the normalised mixing entropies.

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To clarify the points addressed by the Referee we rephrased the following way with additional discussion:

In all used different coordinates, normalised mixing entropy values show a reversed course for O_3 and H_2O (for mixing entropies related to Θ, Θ_2 and $\triangle\Theta_4$ see Krebsbach, 2005). Total water VMRs are highly variable in the troposphere, whereas ozone is comparably homogeneously distributed with respect to the bin size of the considered coordinate. Due to the strong gradient of both trace gases at or in the vicinity of the tropopause, normalised mixing entropy values (SE/SE_{max}) increase for O_3 and decrease for H_2O with further penetration into the LMS. Therefore, at a PV value of ≈2 PVU also stronger gradients in the course of normalised mixing entropies are evident for almost all seasons. The spreading of trace gas VMRs in different bins of the reference coordinate is reflected in the value for SE/SE_{max}. Owing to the tropopause crossings of the aircraft (at least 16 times each season) and the first flight leg near the tropopause region, there is a conspicuous amount of data in the vicinity of the tropopause, reducing data gaps with respect to the chosen bin widths. This holds at least for O_3, and, due to the strong gradient of H_2O in that region with VMR variations covering 2–3 orders of magnitude, also for H_2O above ≈0–1 PVU.

The strong increase in SE/SE_{max} from 0–1 PVU for H_2O is due to some data gaps in the selected bin widths and the result of the strong variation in H_2O in the tropopause region with different p_is.

There are also strong gradients in SE/SE_{max} for H_2O, e.g. near 8 PVU. These result (i) from the compact relationship between the trace gas and the reference coordinate in that region (cf. PDFs) and/or (ii) from a bimodal distribution above ≈8 PVU, both decreasing SE/SE_{max}. During spring the O_3 variability is highest in the LMS, probably due to the enhanced downward motion. The O_3 entropy values are hence maximal during this season. The same arises for the enhanced H_2O content and its variability in the LMS during the summer months. Due to the compactest H_2O-PDF for the observations in autumn, the normalised mixing entropy values result in the lowest values in the range 6–8 PVU. The seasonal trace gas cycles are therefore reflected by the *seasonal course of the mixing entropies.*

p 7266, l 1: H_2O SE/SEmax is only marginally higher in summer (see summary traces in lowermost, right panel of Fig 5). But autumn is significantly lower between 7-8 PVU without discussion. See general comment above.
The SE/SE_{max} in summer is about 0.1 larger, which is not marginally, but we agree to the absence of discussion and included it in the passage above (reply to p 7265, l 23).

p 7268: see also p 7257, l 25
see reply to p 7257, l 25

p 7269: see p 7266 above.
We added the following sentence to this paragraph:
Despite the extensive SPURT data set, conspicuously more data are necessary to derive more robust statements from the mixing entropy.

Fig. 2 and 3: it may be helpful to connect the median points to guide the eye for the trends. The boxes could also be slightly offset horizontally to prevent hiding of lines.
We agree in principal to these points and have realised the suggestion for a slight horizontal offset of the box plots. We do not connect the medians with lines for the reason that the measurements are not consecutively in month. In addition, for clarity we added the years in which the SPURT campaigns were performed below the corresponding months.

Figure 4. Caption: Text seems to say figure shows only one season, but not caption.
We do not agree with this. It is definitely stated in the caption that PDFs for the spring season are shown, as in the text. Only the means and medians for the other seasons are additionally shown to compare to each other, as likewise mentioned in the caption.

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The trace gas gradient numbers are not very obvious. What are the units? Why are the bin sizes doubled. These numbers seem completely arbitrary and poorly motivated. Not really discussed in the text.

The gradients are calculated for a doubled bin size for the reason of a weak smoothing and revealing the general feature. The computed trace gas gradients have the unit [trace gas]/[reference coordinate], as they reflect the slope of the linear fit of data points in each doubled reference bin width. The numbers are not arbitrary. We only partly agree with a poor motivation of these numbers. We show these numbers to give an additional information as a numerical value. In our opinion, these numbers need no thorough separate discussion, since they are already discussed, albeit indirectly, in the passages concerning the means and medians. We correct the 2 PVU to 1 PVU on p 7262, l 03 and rephrased the last sentence in the caption of Fig. 4.

Moreover, to give a numeric value for the trace gas gradients with respect to the reference coordinate, the top numbers (unit [trace gas]/[reference coordinate]) reflect the linear fit of data points, obtained during the spring measurements, in each doubled bin width of the reference coordinate.

In addition, we included this information in the caption of Fig. 5:

[...] The top numbers give a numeric value for the trace gas gradient with respect to PV in a doubled bin width of 1 PVU. [...]

We changed the display direction and colour of the gradient numbers in Fig. 4–6 and computed them consistently for a doubled bin width of the reference coordinate.

General: We thank the referee for her/his linguistic advice.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 7247, 2005.