

## ***Interactive comment on “Seasonal cycles and variability of O<sub>3</sub> and H<sub>2</sub>O in the UT/LMS during SPURT” by M. Krebsbach et al.***

### **Anonymous Referee #2**

Received and published: 11 October 2005

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) . This work seems to be an extract of a Ph.D. thesis, which is cited a couple of times. Many of the conclusions rely on that Ph.D. thesis and/or other planned papers out of it. Generally, the authors put their work in context of prior work. The authors apply PDFs and other statistical methods to study their extensive data set. Unfortunately, the authors frequently stop short of investigating features in their data, but seem content if a conclusion is in agreement with what has been said before. Below, some specific examples are

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

given. The paper does not present novel concepts, ideas or tools, but lots of new data. Discussion of robustness of results falls short.

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.

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.

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 O3 and H2O the CARIBIC and MOZAIC data sets have even better temporal coverage, with, of course, reduced max altitude.

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

p.7251 line 6: quantify consistency of instruments.

p7252 line 3: what is the accuracy of the avionic pressure and temperature?

p 7257: Are these water maxima/minima robust? Does removing one flight change the results?

p 7257 l12. 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.

p 7257 L25: could one problem be the resolution of PV? Is the real PV field in summer much more variable than the assimilation and hence higher H2O VMR are only due to the effects of assimilation? Also, this result seems substantially different from Hintsa

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

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?

p 7261: l4: "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.

l16: what is a "rather steep distribution"? Maybe explain on current Figure 4. 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!).

p. 7262: L 5: see above about kink.

p 7263 l 4: be more specific about the intercomparison agreements

p7264 l 12 natural logarithm doesn't need reference. Or is this a reference to the entropy, then move it up two lines

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?

p7265 l9: "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.

l23: "also a strong gradient... is evident" I don't see a very strong gradient at 2PVU. 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 H2O?

p 7266 l1: H2O 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.

p. 7268: see also 7257, L25

p. 7269: see p7266 above.

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.

Figure 4. Caption: Text seems to say figure shows only one season, but not caption. 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.

technical corrections The text sounds a lot like oral speech. I hope the authors will try to improve the text. Here some observations: use of word "rather": I don't like this word in scientific papers. It is very unspecific. Most of the times the content would not change if simply removed. Other times, perhaps use "relatively". "anyhow": this word has not been used properly many times, could most of the time just be deleted.

---

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

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)