Atmos. Chem. Phys. Discuss., 9, C4879–C4885, 2009
www.atmos-chem-phys-discuss.net/9/C4879/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Annual cycle of ozone at and above the tropical tropopause: observations versus simulations with the Chemical Model of the Stratosphere (CLaMS)" *by* P. Konopka et al.

Anonymous Referee #1

Received and published: 16 September 2009

1 Major comments

The paper by Konopka et al. uses chemical model simulations of the stratosphere (CLAMS) to better understand the observed annual cycle of ozone in the tropical tropopause layer. This annual cycle must have contributions from annual cycles in vertical and horizontal advection, from mixing, and from chemical production and loss. In a separate manuscript (Konopka et al., JGR, 2009), the authors seem to have developed a conceptual model, which indicates that horizontal mixing (or advection?) is the major contributor to the ozone annual cycle. In the present manuscript, the authors use the much more detailed CLAMS model to substantiate and confirm this finding. The paper does present significant new information, and is, in principle, quite well suited for publication in ACP.

The main problem I have with the paper is with the clearness and conciseness of the presentation. The paper has failed to convince me that horizontal advection (or mixing?) is really causing the observed ozone annual cycle. Only Figure 6 does give some convincing evidence, but this is hardly exploited by the authors. Instead, I am confused by the use of two "parametrizations" of vertical ascent, which give fairly different results in ozone levels and annual cycle amplitude (Figs. 5 and 7). It seems to me that many things can be fudged by tuning parametrizations like this. Who guarantees, that the different ascents are not compensated by different horizontal motions in the model? In fact the authors state, that the absolute values of "in-mixing" depend on the used vertical winds (e.g. page 17950, lines 9, 10).

I guess the authors are trying to show that even with different vertical ascent, the resulting ozone annual cycles are similar. But by doing this extensively, the paper becomes more confusing, and the main point, i.e. horizontal transport variations responsible for the ozone annual cycle, becomes obscured. The paper would benefit much from de-emphasizing the two vertical transport parametrizations, and from focusing much more on real horizontal transports. These must be available and plottable from the model calculations. I think it would be much more convincing to exploit the good insights provided by Fig. 6. Several approaches seem possible:

- Explicit numbers/ figures for meridional ozone transport from the extratropics to the tropics.
- Use of a latitude tracer which can explicitly record meridional/horizontal transport, e.g. around the Asian summer monsoon.
- Use of longitude-resolved data. Comparisons of observed/ modelled annual cycle for different longitudes near the equator(locations with large/ little influence

from the monsoon anti-cyclones, large/ little advection from the extratropics, different SHADOZ stations, longitude resolved MLS data).

The authors focus on the Asian summer monsoon, but the MLS data in Fig. 6a also show monsoon effects in DJF in the southern hemisphere, north-east of Australia, but also above South Africa and South-America. To some degree this is also indicated in the CLAMS results. This may explain the weaker annual cycle in the SHADOZ data (where are the stations? Show on a map!), and should certainly be exploited/ investigated in more detail.

2 Minor comments

"Horizontal in-mixing" is used throughout the manuscript, but it is a really fuzzy term. I think what is meant is horizontal advection from higher latitudes (in the large scales resolved by the model), which ultimately leaves ozone rich air in the TTL, either because it is not re-advected to higher latitudes again, and/or because it is mixed out irreversibly (in the small scales un-resolved by the model). In the end it is irreversible transport. If the authors would clarify, what they mean by "in-mixing", they might also find a good metric for it in the CLAMS model. Then this metric could be plotted, and would show the "in-mixing" explicitly. No more arm-waving needed that $pO_3/(O_3 + fudge)$ is "in-mixing", because "it can't be anything else".

Page 17939, line 12, Randel et al. (2007): Please give a very short description of the procedure. What is done? Calculate measured O_3 on pressure levels, then average?

Page 17941, line 17, Mahowald et al. (2002): What is hybrid potential temperature. Please give a 1 sentence (or less) explanation.

Page 17941, line 21: "we correct this velocity". Is this always done (also in Konopka 2007), or is this a new thing that is done for this paper? Please state this explicitly, e.g.

ACPD

9, C4879–C4885, 2009

Interactive Comment

by adding "for the present paper", or "in a corrected version used here".

Page 17941, line 23: "constant below and above θ=360 K, respectively." This means constant everywhere. Is there not one theta level missing? Please clarify.

In Fig. 2b: It does not look to me like there is a constant factor between the reference and corrected $\dot{\theta}$. If, then it would be below 310 K and above 400 K. Please clarify!

Fig. 2a: With the given colour scale, all the important variations between 350 and 420 K appear in different shades of blue. To me, it is almost impossible to tell if $\dot{\theta}$ goes up or down with altitude and/ or season. I think a colour scale like the one in Fig. 1, or a gray-scale would be much better!! With the current color scale this important plot / region is not clear.

Page 17941, line 26: "with respect to the vertical velocity described in Konopka et al. (2007)." Is this the clear sky radiation $\dot{\theta}$ above 100 hPa combining with ECWMF $\dot{p}$ below 100 hPa. If so, you might state at the beginning of the paragraph, what the "reference" and "corrected" case are, and just refer to the beginning of the paragraph.

Page 17942, line 14: "decreases the upwelling above 360 K". In my plot 2b, the decrease is above 380 K.

Page 17942, line 20: "(annual cycle of $\dot{\theta}$)". Why does the radiation dominated equatorial stratosphere have an annual cycle? The sun crosses the Equator twice per year, so one might expect a semi-annual cycle. Why is there an annual cycle? Because of ozone/ the Brewer Dobson circulation? Please explain.

Page 17943, line 5: "all APs above θ=500 K are prescribed." What are APs?

Page 17943, line 29: "permanent upwelling ... excludes downward transport in the tropics". This is only true for the model. How can you exclude in the real world, that there are downward transports in some regions, maybe compensating very strong upwards transports in other regions? Remember, model vertical speeds have been fudged to give mass-conservation. So model transports have been modified already to match

ACPD
9, C4879–C4885, 2009

real world constraints.

Page 17944: "SHADOZ stations" At some point a map showing the locations of the considered SHADOZ stations (or a table) would be helpful.

Fig. 4: pO_3 and age-of-air are really the same thing, since both use a passive tracer. One of the two panels of the figure is probably redundant. Good to have the horizontal wind isolines, though.

Fig. 5: In the discussion, the authors should give some explanation why the HALOE values are lower than the SHADOZ values, and which values are considered better.

Page 17945, lines 3 to 9: "As we discuss ..." Somewhere, you should also say that CLAMS ozone is likely to be off in Southern hemisphere spring to summer, because there is no ozone hole in CLAMS

Page 17946, line 1, 3: "STJ" is that the sub-tropical jet? Maybe better to spell it out?

Page 17946, line 27: "MLS are slightly higher". 120 ppb instead of 50 ppb, seen for much of the 0° N to 20° N band, is not slightly higher, it is substantially higher!!

Page 17947, lines 14 to 22: Suggest to omit this entire paragraph.

also page 17947: The DJF MLS data show clear "monsoon" signatures over Indonesia, Southern Africa, and South America. Why are these not discussed? Just because CLAMS does not resolve them as clearly? Why does CLAMS not resolve them as clearly? Is this a question of vertical averaging (kernels)? I think this is a key-issue, directly related to horizontal transport/ "in-mixing" and should be discussed in detail!!

Section 6 Discussion, 1st/2nd paragraph: As mentioned, I don't think the authors have clearly separated vertical and horizontal transport. So pO_3 just shows the effects of all transport. The authors should explicitly show and discuss the horizontal transport.

page 17948, line 9: "undesireable vertical transport". Why would this not change the seasonality? The "undesireable vertical transport" could have its own seasonality?

ACPD

9, C4879–C4885, 2009

same paragraph, last sentence: I doubt that pO_3 would be zero in the TTL without horizontal in-mixing. Prove it, e.g. by setting pO_3 to zero when air crosses 15° or 20° latitude equatorwards.

page 17948, lines 16 to 25: Why show $pO_3/(O_3 + 40)$ and not the simpler pO_3/O_3?

page 17948, line 27: $\theta \approx 360$ K: In Fig. 2 the cross-over is nearer to 380 K.

page 17949, lines 1, 2: "remove the the gap" → "removes the gap", "reproduce" → "reproduces"

same paragraph: drop "results of our investigation"

page 17949, line 7: "2-3 months later" The strongest ECWMF upwelling is in April and in December, the weakest in August. $\Delta O_3/\langle O_3 \rangle$ and pO_3/O_3 peak in July/August, and are lowest in March/April ($\Delta O_3/\langle O_3 \rangle$), or April/Max ($pO_3/O_3$). For me, April to July/August are 3 to 4 months (not 2 to 3), and August to April are 8 months (certainly not 2 to 3). Pleae correct text (or my misunderstanding).

Same paragraph: Again lots of arm-waving, but no hard evidence, that would show what is going on. Quantify, and show the horizontal and vertical transports!!

Page 17950, 1st paragraph: Show those meridional transports!!

same paragraph: To paraphrase: "In-mixing" is not driven by mixing, but is driven by advection. So, "in-mixing" is not mixing, but advection. Why call it "in-mixing" then?

Page 17950, line 7: "be" → "by"

3 Summary

The paper by Konopka et al. uses CLAMS model calculations to investigate the causes of the annual cycle of ozone in the tropical transition layer. Generally the paper does

ACPD

9, C4879–C4885, 2009

a good job. However, I feel that the presentation puts too much emphasis on vertical transport parametrizations. It fails to convincingly demonstrate that the claimed horizontal transport / advection are indeed causing much of the observed annual cycle. I strongly suggest that the authors try to show this more convincingly, e.g. by

- explicitly diagnosing horizontal transport/ advection in the model.
- giving a better idea about the role of monsoon related anticyclonic circulations.
- looking at the related longitudinal variation in observed data and simulations.

If the authors can demonstrate horizontal transport effects in a better way, this will be a very convincing paper, well suited for publication in ACP.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 17937, 2009.