Atmos. Chem. Phys. Discuss., 7, S1425–S1427, 2007
www.atmos-chem-phys-discuss.net/7/S1425/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of land convection on troposphere-stratosphere exchange in the tropics" *by* P. Ricaud et al.

T. Peter (Referee)

thomas.peter@env.ethz.ch

Received and published: 26 April 2007

I think the paper by Ricaud et al. and the subsequent interactive comments concern an important issue that the scientific community has to deal with. The other reviewers have made a number of serious critical comments, many of which I share and to which I have little to add (besides one more point that is possibly important and that I outline below).

While I agree with the other reviewers that

- a more quantitative approach would be desirable,

- the other seasons also need to be looked at,

- the vertical resolution of the satellite instruments is poor and jeopardizing any sort of conclusion,

- and that therefore the conclusions of the manuscript are overstated in some respects,

I do remain sufficiently puzzled by Figures 1-3 to think that this cannot be just discussed away.

I am puzzled to see that 4 different satellite instruments - despite massive discrepancies amongst each other - show unequivocally highest concentrations of N2O, CH4 and CO in the TTL over Africa instead of over the Maritime Continent / Western Pacific (MC/WP). Most of us think that the MC/WP region is where (1) lowest tropopause temperatures and strong vertical transport into the tropopause region, apparently associated with regions of frequent deep convection, and (2) the effect of upper level monsoon circulations which advect air near the tropopause through regions of lowest temperatures coincide. In the "ECMWF world", i.e. the picture derived from ECMWF data which might differ from the real world, most air parcels at 380 K in the tropics cross 340 K irreversibly in the MC/WP and the frequency of location of lowest saturation mixing ratio is also in the MC/WP (e.g., Fig.5 in Fueglistaler et al., JGR, 110, D08107, doi:10.1029/2004JD005516, 2005). Therefore, I would expect to find also the highest long-lived trace gas concentrations exactly there. How can it be that this is clearly not the case?

As the other reviewers remark it could be that tracers with predominantly continental sources and strong land-ocean contrasts lead to a bias in favor continental convection. However, this is not the case for N2O with its very long lifetime, and is also unlikely for CH4 (for sure not of the order of 0.2-0.3 ppm as would be required to explain the difference in HALOE-CH4 over Africa and MC/WP). So I remain being puzzled.

The story takes another confusing turn when looking at AVHRR-OLR (Fig.4). This shows lowest OLR over MC/WP and South America, but not over Africa. I find this hard to reconcile with the author's hypothesis and think they need to address this issue.

ACPD

7, S1425–S1427, 2007

Finally, the other reviewers have pointed to many weakness of the comparison with the MOCAGE calculations that need to be addressed by the authors. However, Figs. 6a,b on N2O, Figs. 7a,b on CH4 and Figs, 7c,d on CO do indeed look very much like the observations, stressing the apparent role of Africa. I am confused, though, by the N2O vertical net fluxes across the 150- and 100-hPa surfaces in Figs. 7c,d. The authors show this picture but leave it hanging. Are Figs. 7c,d not in direct contradiction with Figs. 7a,b? If the upward fluxes are highest over the MC/WP, suggesting that this is the TTL source region of N2O, should the concentrations not also be highest? Are Figs. 7c,d no also in direct contradiction with Fig. 9d which seems to suggest that highest vertical transport in MOCAGE takes place over Africa? I think this needs to be explained and I admit that I am confused because the authors seem to assume that there is no problem and also the other reviewers did not pick up this point.

In summary, I think that Ricaud et al. have an important point. As with all suggestions questioning current scientific wisdom also this paper has received / will receive a lot of criticism. If I were the editor of this paper I would have a hard time on where to go from here. The current manuscript has a number of severe weaknesses that need to be addressed before going forward to ACP. At least a rigorous check on avoiding any overstatements will be required. I am aware that this might end in an undesirable "may-be" and "it-is-conceivable-that" paper, but the main message of Figs. 1-3 would then at least be properly published. It will be beneficial to review the paper again before it goes into print.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3269, 2007.

ACPD

7, S1425–S1427, 2007