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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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The strong points of this paper are that it looks at a variety of species using a variety of instruments, and has some model comparisons. The weak points of the paper are that it is not sufficiently quantitative enough to demonstrate its conclusions. It tries to use chemical tracers to distinguish between the "slow ascent" versus "convective overshoot" theories but I think the field has moved on from from thinking of STE as one extreme or the other, and in not being quantitative, the paper is not as useful as it might be. More comments on these issues below.

I would recommend publication only if there were a serious attempt to address the issues listed below

To summarize:

(1) To demonstrate that "rapid uplift over land convective regions is the dominating process of troposphere-stratosphere exchange" (abstract) one would have had to use the observations to constrain the magnitude of the convective mass flux into the TTL over land regions, and demonstrate that it was larger than over ocean regions, or that it was almost equal to the upward mass flux from the TTL into the stratosphere. This has not been done, so I think the conclusions are overstated. Or else there should be an attempt to be much more quantitative.

(2) Slow ascent versus convective overshooting. The references on page 3271 are incorrect. Slow ascent through the TTL is associated with authors such as Holton, Gettelman, and Fueglistaler in the context of horizontal advection through cold pools playing an important role in dehydrating TTL water vapor to a vapor pressure smaller than would be expected from the zonal mean temperature. The Sherwood and Dessler paper is associated, not with slow ascent, but convective overshooting + irreversible mixing above the cold point tropopause, followed by detrainment into the TTL and subsequent ascent. The earlier Danielsen paper is associated with overshooting irreversible convection followed by detrainment directly into the stratosphere. I would disagree that this mechanism is generally accepted; it is no longer believed that STE in the tropics requires detrainment above the tropical tropopause, although it may exist. It is now believed that the adiabatic layers near the tropopause seen in this paper are probably associated with turbulent wave breaking rather than mixing due to radiative destabilization of cirrus anvils. It is also a bit simplistic to categorize the STE problem as one of slow ascent versus convective overshooting. In order for STE to occur, everyone would accept that there must be some convective detrainment above the Level of Zero Radiative Heating. The outstanding issues are (1) How does the convective detrainment profile profile vary with altitude? (2) Does it exhibit regional variations, and (3) whether irreversible mixing with higher PT air in the TTL/lower stratosphere is required for convective detrainment in the TTL to occur. I.e. what is the magnitude of the cooling term associated with irreversible convective overshooting? In order to move the field forward, there must be some attempt at this stage I believe to give quantitative

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answers to (1), (2), and (3), not posit extreme positions (caricatures or straw men) that no serious scientist should accept. I believe that chemical tracers do have a role to play in giving quantitative answers to these questions. The current paper suffers a bit in posing the issues in a bit of a simplistic way, and in over-interpreting, at times, the data.

(3) For example, it is argued in the abstract that because CO/CH4/N2O exhibit stronger perturbations over the continents, convection over the continents is more important for tropical STE than convection over land. One problem is that all three tracers have stronger sources over land. Suppose one could measure a marine tracer such as methyl iodine from space. One would presumably see strong enhancements of CH3I at 17 km over the western tropical Pacific and Indian Oceans. Could one then reach the opposite conclusion that marine convection is much more important for STE than continental convection? The authors don't seem to have made an attempt to correct for the biases associated with land sources of CO/CH4/N2O in reaching their conclusions. Second, the magnitude of a chemical anomaly at 17 km does not necessarily have a direct relationship with the strength of STE. An air parcel that detrains at 15 km above the LZH may have a nearly equal probability of reaching the stratosphere as an air parcel that detrains at 17 km. Yet detrainment at 17 km will typically give rise to a much larger chemical anomaly at 17 km than detrainment at 15 km. The authors are reaching conclusions that, strictly speaking, could only be reached if (somehow) the chemical tracers were used to put quantitative bounds on convective outflow as a function of altitude in various regions. Since this hasn't been done, I think the authors should modify their conclusions with appropriate qualifiers.

(4) There are other conceptual difficulties at times. It is stated on page 3272 that slow ascent would not be expected to be associated with chemical anomalies in convective regions. If by slow ascent one means that most convective outflow into the TTL occurs just above the LZH, it is true that chemical anomalies would tend to be more strongly damped with altitude, but presumably would still non-zero at 17 km, since horizontal

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mixing is not infinitely fast. This is also partly due to the undulation of PT surfaces. This would give rise to maxima of tropospheric tracers over convective regions when plotted on a height surface, even in the case where the distribution of the tracer on a PT surface is homogeneous.

Other comments:

(5) page 3279: the authors should say which month the average temperature of 191 K corresponds to. There is a large seasonal cycle.

(6) page 3279: "indicating that fast overshooting over land tropical convective systems is the predominant mechanism of troposphere-stratosphere exchange at global scale". As discussed above, I don't think this statement is supported simply by the figures. Second, I think the authors should define what they mean by STE: direct convective injection above the cold point tropopause? Convective outflow above the LZH? Something else? Also, values of equivalent potential temperature as high as 365 K are not uncommon in the marine boundary layer, and perhaps over continents also. This would give rise to a level of neutral buoyancy (LNB) for these air parcels near 17 km. There is no thermodynamic requirement that convective outflow near 17 km be overshooting that I am aware of. By overshooting, do the authors mean reversible overshooting followed by subsequent detrainment at the LNB, or overshooting + irreversible mixing with higher PT air?

(7) page 3280: It mentions that the MOCAGE model uses "parameterizations for physical processes such as convection". The ECMWF model has a convective scheme that gives rise to some implicit convective transport (presumably) in the large scale ECMWF winds that are used to run the MOCAGE model. Does the MOCAGE model then introduce additional convective transport beyond that present in the ECMWF winds? Some additional explanation is needed, I think, due to the importance of convection to the arguments of the paper.

(8) page 3282: fluxes. I think it would be of interest to show convective + grid scale

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fluxes of the various species as a function of altitude, perhaps averaged in some tropical band. How high does convection inject these species in the MOCAGE model? Are there any regional variations? It might point the way to a more quantitative approach in the future, and enable comparisons of the results with other models. It would be really nice to see some attempt at a quantitative characterization of the vertical profile of convective outflow in the MOCAGE model.

(9) page 3282: It mentions that the land/ocean contrast for N2O is about 15 ppbv. What instrument does this refer to? Obviously the huge differences between the measurements complicates interpretation.

(10) page 3272: if the lifetime of CH4 is 10 years in the troposphere but one year in the stratosphere, then the tropospheric and stratospheric sinks of CH4 are roughly equal, since there is about 10 times as much mass in the troposphere than the stratosphere. I don't think it is true to say of methane that its sink is "essentially in the stratosphere". This statement seems to be contradicted by the paper itself.

(11) The titles to the legends were written in a small gray type which was very nearly illegible on my laptop screen.

(12) Many of the figures use different colour schemes for the same species on the same height, if they are showing measurements from a different instrument. This complicates enormously the interpretation of the figures, and at times obscures important differences between instruments. For example, in Figure 1, there is a huge bias between N2O as measured by ODIN/SMR and as measured by AURA/MLS, with the AURA/MLS values being clearly erroneous - the reader deserves some mention of these biases; why are they never discussed? A mention would be appropriate in Section 3.1. Similarly Figure 3 should use the same color scheme for all three CO plots, and similarly for figures 6 and 7. Similarly, comparisons between the model and the measurements would be greatly facilitated if the same color schemes were used.

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

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