

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

Anonymous Referee #3

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Review of “Impact of land convection on troposphere-stratosphere exchange in the tropics” by P. Ricaud et al.

The main conclusion of the paper is that “rapid uplift over land convective regions is the dominating process of troposphere-stratosphere exchange.” This is an interesting conclusion, but I have some concerns that this is not adequately supported by the analysis.

Major problems

1. I find the method of this paper to be flawed: “The method proposed here for studying the relative importance of slow radiative heating compared to fast convective troposphere-stratosphere exchange at global scale is to look at the horizontal distribu-

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tions of trace gases of tropospheric origin, in the TTL and the lower stratosphere.”

To see why it’s flawed, consider a constituent that has a much, much higher abundance in the boundary layer than it does in the TTL. In that case, even a small amount of convection could perturb the TTL abundance of this gas. Conversely, if the abundance of the gas in the boundary layer and TTL is the same, then even massive convective transport would not perturb the TTL abundance.

By looking at constituent abundances, this analysis only tells us about transport of that constituent — not about transport of mass. Although it’s not entirely clear, I assume that their main focus of this paper is mass transport, so again this analysis does not provide the insight they claim.

Ultimately, I do not believe that this type of qualitative by-eye analysis is capable of proving the point they want to make. What they’ve actually shown here is that convection is vigorous enough to perturb the background constituent field. This tells us little or nothing about the global balance between convective transport and slow ascent.

Rather, they need to make the paper more quantitative, with more rigorous evaluations of the various fluxes and show that the magnitude of the convective flux is greater than the slow ascent flux.

2. (a bit of a repeat from above, but worth repeating) The authors need to consider the role of surface variations in the source gases. For example, CO is quite high in the African boundary layer because of biomass burning. Given such high abundances, even a small amount of convection could perturb the TTL abundance of CO. I suspect that this effect is contributing to the dominance of Africa in the CO plots. Similarly, since TTL CO in the Western Pacific is comparatively low, then massive convective transport occurring there might still not perturb the TTL CO abundances.

3. A major problem with this conclusion is the vertical resolution of the satellite data. The vertical resolution of the satellite instruments is 2-4 km, so while the analysis is

done at 17 km or 100 hPa, the longitudinal variations seen in the “17 km” data might actually be coming from 1-2 km lower down, which would be below the tropopause. Because of this, I don’t think the authors have proven that convection is reaching the tropopause.

4. What is the role of lateral transport of stratospheric air into the tropics? The CH₄ over the Western Pacific is (by my eye) less than 1.6 ppmv. Since the lifetime of CH₄ here is 100 years, the reduction from boundary layer values to 1.6 ppmv must be due to stratospheric influence (most likely) via lateral transport. If such lateral transport occurs more frequently into the Western Pacific, then it would appear the same as less convective transport.

Another possible explanation: Sherwood [2000] argued there was descent over convection in the Western Pacific. This might also explain lower values of CO, CH₄, and N₂O by brining stratospheric air down into the TTL. Does their mechanism contradict this one? Or can they coexist?

Less serious problems

5. The best coordinate system for this type of analysis is potential temperature. Would these plots show the same story if plotted at, say, 375 K instead of 17 km?

6. They are drawing general conclusions from a single time of year (late winter/early spring). What reason do we have that other times of year also show this pattern? The authors should pick another time of year when there is little biomass burning in Africa and see what the CO fields look like.

7. There are many places where the writing is ambiguous. For example, the statement in the abstract “The Maritime continent in the Western Pacific never appears as a source region for the stratosphere” is unclear. Do they mean a source of trace gases, like CO, or a source of mass? For gases that have extremely high values in the boundary layer, like CO in Africa, it’s possible for a region to be a major source of mass but

not a constituent.

And do they mean a source of mass that's directly transported through the tropopause or as a source of mass to the bottom of the TTL. Air need only be transported above the bottom of the TTL for it to eventually make it into the stratosphere (by slow ascent). Isn't there a lot of air being transported into the bottom of the TTL over the maritime continent?

9. In the abstract, it is written: "At the top of the Tropical Tropopause Layer (TTL), all gases show significant longitudinal gradients \ddot{E} " The authors place the top of the TTL at 19 km, but all of the analysis in the paper is at 19 km. Thus, there is no analysis in the paper about the top of the TTL.

10. More discussion of the convective scheme. I realize the references to the model certainly contain info on the convective scheme, but since this is so central to the paper it would be useful for the following info to be provided: 1) which scheme is being used?, 2) does it include overshooting?, 3) if not, how high is convection going? Is it getting into the TTL?

11. Missing references: There are a bunch of papers that I think should be referenced in order to better place the work in context. First, both Dessler [2002] and Gettelman et al. [2002] have estimated the influence of convection on the TTL. Neither concludes there is a significant influence above the tropopause. For the impact of convection on CO in the TTL, the authors should cite Schoeberl et al. [2006], who also showed the importance of convective transport of CO.

Finally, I cannot resist commenting Adrian Tuck's comment. He writes that this analysis "invalidates the assumption that has been made both explicitly and implicitly since Brewer's 1949 paper. This assumption is that air entered the stratosphere where and when the air was dried (dehydrated) to its stratospheric values." I would argue that this assumption is already completely discarded by the community. In the abstract of Sherwood and Dessler [2000], for example, they write "Dehydration and genuine entry into

the stratosphere are separate processes that happen on much different time scales.” And there are probably at least a dozen other papers that make the point. I do NOT recommend the authors follow Tuck’s advice and try to take credit for this idea.

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