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Interactive Comment

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

Anonymous Referee #1

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The paper "Impact of land convection on troposphere-stratosphere exchange in the tropics" by P. Ricaud et al. addresses the relative importance of different mechanisms for troposphere - stratosphere exchange. To this end, satellite measurements of different species at 17 km altitude as well as measurements of OLR are evaluated for their longitudinal variation. The paper also compares the measurements to simulations with the MOCAGE model. From their results, the authors draw the conclusion that convective uplift is the dominating factor in STE, that the main areas of STE are over Africa, South-America and to a lesser extent Indonesia and that the model has the right pattern of convective lifting but underestimates the amount of STE.

The paper is well written and combines satellite measurements of different quantities in a novel way to highlight where convective uplifting is relevant for STE. The results,

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although qualitative and only for one season, are interesting and I therefore recommend publication in ACP. There is however in my opinion a number of problems in the analysis (see below) that makes the case less clear than the authors claim, and this needs to be discussed in more detail in the final version of the paper.

1) Vertical resolution of the measurements. As the authors explain, the vertical resolution of the measurements is between 2 and 4 km (and even poorer for MOPITT CO), and therefore it is not clear to which extent the enhancements shown in the measurements are stratospheric, within the TTL or even in the troposphere. Thus the signals while indicating convective uplifting do not necessarily indicate STE. And even if it is STE, it still could be convective uplifting to the upper troposphere followed by slow adiabatic uplifting.

2) Horizontal and vertical inhomogeneity of trace gas distributions. The amount of a species brought into the TTL depends on the strength of convection but also on the concentration distribution at the surface. The authors discuss this in the case of CO but similar arguments can be made for the other tracers. How large the anomaly is at 17 km then depends on the vertical profile of the species, and one could argue that part of the observed signal (and lack of it) is related to the distribution pattern of the species in the troposphere.

3) **Model comparison.** It is not clear to me what we learn from the comparison with the model simulation. The differences between measurements and model results are large in many aspects, and this could have a number of reasons. Although some of them are discussed in the text, it is difficult to draw any clear conclusions from the comparison. If the problem in MOCAGE is the underestimation of the maximum altitude of convective uplifting, what is the reason - a parameterisation used, the ECMWF data used as input, the spatial resolution of the model, ...?

Minor comments

- In the description of the HALOE measurements, should it really read "profiles are

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continuously retrieved"?

- I do not see any link of the paper to the HIBISCUS project apart from the reference in the introduction but I may have missed it

- To me, the fire count map looks unusual - there is a surprisingly large number of fire counts in the south of North America and also in Australia. Is this an accumulation of all fire counts over March, April, and May of the three years?

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