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

A. Tuck

adrian.f.tuck@noaa.gov

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The authors are to be congratulated on an important paper. The result that methane, nitrous oxide and carbon monoxide are mainly injected to the stratosphere by tropical land convection is a result that 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. That this is not valid can be seen by comparing the HALOE methane maps at 17 km in Figure 2 of Ricaud et al. with the HALOE water vapour maps at 400 K potential temperature in Plate 7 of Rosenlof et al., J Geophys. Res.,102, 13213-13234 (1997). The water vapour minima are not associated with terrestrial convection. The consequences of this geographical dislocation of the injection of mass with the produc-

tion of dry air are perhaps something the authors could point out with benefit. Also, Richard et al.,J. Geophys. Res,111,D13304,doi:10.1029/2005JD006513(2006) show high resolution vertical profiles of ozone, methane, water vapour and temperature over Central America in January 2004 and use them to argue that dehydration occurs above the thermal tropopause in ozone-rich air, by a mechanism that involves the distillation of small ice particles to larger ones. The decrease of methane from 12 km to 16 km in the TTL was interpreted as indicating the presence of stratospheric air. Can Ricaud et al. cast any light on this conclusion? Could it be simply a fall-off because of distance from the surface source rather than the effect of the stratospheric sink? At first inspection, their observed methane values in the TTL and at the tropopause seem somewhat higher than those observed by Richard et al.

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