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Interactive comment on "Observations of water vapor mixing ratio and flux in Tibetan Plateau" *by* S. Wu et al.

Anonymous Referee #3

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Review of "Observations of Water Vapor Mixing Ratio and Flux in Tibetan Plateau" by S. Wu, G. Dai, X. Song, B. Liu, and L. Liu

This paper describes unique observations of water vapor and vertical wind speed using Raman and Doppler lidars in the lower troposphere over the Tibetan Plateau. Radiosondes are used for calibration of the Raman lidar. The paper contains novel and interesting data yet is only publishable after major revision because: 1) The English language is poor, particularly in sections 1 and 2. These two sections must be considerably re-formulated.

2) Throughout the paper I miss an interpretation of the data: we see here interesting observations differing substantially from standard atmospheric conditions that have to be better described and understood. I recommend the use of a simple trajectory model,

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e.g. Hysplit, freely available, to better understand the origin of the air sampled. I am not convinced by the authors' statement that the humidity variations are due to local evaporation variability. Also, additional information such as the weather situation and the lidar backscatter signals are needed to better understand the observed variability, also of the boundary layer height.

3) I am puzzled by the strong discrepancy between lidar and radiosonde humidity in Figure 6: not only are the lidar-derived H2O mixing ratios in the lowest layer, probably the nocturnal boundary layer, persistently larger than the radiosonde values, but also the thickness of this humid layer seems to be considerably smaller in the right half of the lidar plot. This cannot be the effect of noise in the lidar data, which is probably responsible for the high values and scatter at the top of the lidar plot (the authors should discuss this as well, and occasionally reduce their measurement range). It rather looks like a systematic issue that the authors have to find out and to explain.

4) I am also puzzled by the high specific humidities observed. The H2O mixing ratios in Figures 5 and 8b are five times higher than the global average, and still at least two times higher than in a typical tropical atmosphere at corresponding altitudes. Here, trajectory analyses may lead to more understanding. Perhaps the air came from the Southeast Asian warm pool region? Was it associated to the monsoon? The authors should also carefully check their thermodynamic calculations. Namely, I found an error in Figure 8b: the absolute humidity is much too high close to the surface, and probably too low at the top of the plot. Using standard atmosphere air density and multiplying with the H2O mass mixing ratio, which gives the absolute humidity, I estimate the absolute humidity to lie between about 7 g/m3 near surface, 3...4 g/m3 in the middle and 5 g/m3 in the top of Figure 8b.

5) The section on water vapor fluxes is too short and incomplete, and the data also here need to be better interpreted. Since this is a night time scene, there is likely low or no turbulence, and it is justified to use average values in Eq 14 to estimate the mean local water vapor mass flux. It would be very interesting to see a longer time

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series, or another measurement on a different night, for comparison. Where are the mentioned rain and clouds in Figures 8 and 9? Is a positive vertical wind directed up- or downwards?

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