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Interactive comment on “Isentropic advection and convective lifting of water vapor in the UT– LS as observed over Brazil (22° S) in February 2004 by in situ high-resolution measurements of H₂O, CH₄, O₃ and temperature” by G. Durry et al.

Anonymous Referee #1

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In this manuscript, balloon-borne measurements of H₂O, CH₄, O₃ and temperature from two flights during the Hibiscus project in Southern Brazil are presented and discussed. The discussion is focused on the observed layers in the vertical profiles which are attributed to isentropic transport between the stratosphere and the troposphere. The authors present results from the MIMOSA model (PV simulations) to illustrate the different origin of the probed airmasses. There are two companion papers using the same data set and giving complementary interpretation: Marecal et al. (ACPD, under review) verify the BRAMS model by reconstruction of the measured H₂O data. Huret

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et al. (in preparation, not available to me) seem to investigate in more detail the origin and complicated interplay between advective transport and convection for the observed cases, using a mesoscale model. The major problem of the Durry et al. paper is, that the scientific interpretation of the data remains rather hypothetical or speculative, and thus the scientific originality is rather poor. The main results are the high-quality data in a so far not intensively investigated region and the reporting of the layered structure - the latter is, however - not a surprise in this transition region between the tropics and subtropics and has been reported previously, as also stated in the manuscript. I expect that the studies by Marecal et al. and Huret et al. give a much more profound insight into the processes which explain the observations. Durry et al. already mention, that MIMOSA has some difficulties as it does not consider vertical transport. I assume that the mesoscale model in Huret et al. does a much better job here. Therefore, I have some difficulties to recommend the Durry et al. manuscript to become a stand-alone publication as it does not contain sufficient own material, which is (probably) given in the companion papers.

In addition to this general comment on the structure and content, there are other important changes required:

The determination of the TTL shows, that for both cases the tropopause altitude was significantly different. For SF2, it is a typical subtropical profile, thus raising the question, whether the concept of a TTL is still valid.

Supersaturation: I assume that a particle instrument was on board (because of the result in Nielsen et al.). Are the supersaturations below the tropopause observed in cloud-free conditions or accompanied (in part) by clouds? The very high supersaturations sometimes observed are a striking result and are worth to be discussed in more detail. Even if some of the results can be found in Marecal et al.. Please state in the instrumental section that micro-SDLA has an open cell and is sampling gas-phase water only.

Oceanic vs. continental influence on the H₂O profiles: There are several papers dealing with the temperature and H₂O distribution at the tropical tropopause based on ECMWF analysis (Bonnazola and Haynes, 2004; Fueglistaler et al., 2004] which show clear regions of cold/war or dry and humid characteristics. They are not necessarily connected to maritime and continental areas.

The use of 2[CH₄]+[H₂O] as an indicator for intrusions of stratospheric air is very questionable at these altitudes as below this quantity can be regarded constant only in the real overworld, without impact of the tape recorder etc. And a typical overworld value can occur below as well. If the authors want to keep this argument, they should display 2[CH₄]+[H₂O] in one of the figures in order to illustrate the specific deviation from the environment above and below.

As the H₂O-filamentation of SF₄ cannot be explained by MIMOSA, do the Marecal or Huret studies a better job, and if so, what is the clue to explain the observations?

The comparison with midlatitude data of the group is rather artificial and does not help in the discussion. I disagree that one can deduce transport from midlatitude air into the tropics from this comparison for two reasons: First, the variability below 420 K is very high, and second, there is indeed a difference between the NH and SH lowermost stratosphere, including the strength of the jets and their seasonality.

Instead of the comparison with own mid-latitude data, a comparison with other tropical profiles which are referenced in the manuscript could be very illustrative. Konopka et al. submitted a manuscript to ACPD just a few days before Durrý et al. with H₂O data from the TROCCINOX project and a similar scientific subject (probably not known to both author groups when preparing their manuscripts). In a new version, I recommend to make use of this data set for comparison as well.

Ozone profiles in Figure 5: I recommend to include a climatological tropical profile in addition to the single reference profiles. Further, can you learn something from the short-term (i.e. a few hours) variability in the UT of the different ozone sounding

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presented?

In summary, I do not recommend publication of this manuscript in its present form. To improve the scientific content, the authors should carefully consider to combine the paper with the results of Huret et al (though I do not know that manuscript in preparation) or to add some additional material (e.g. discussion of supersaturation and cloud data, a deeper discussion with comparison with other data sets, may be including global ones from satellites; variability of ozone profiles).

Editorial and minor remarks:

Figure 4 needs to be enlarged.

Figures 6 and 7: It is sufficient to show only 3-4 specific altitudes, but not 16 per flight day.

Figure 8 should become 2 panels, one for each flight

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