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Interactive comment on "Analysis of water vapor LIDAR measurements during the MAP campaign: evidence of sub-structures of stratospheric intrusions" *by* P. D'Aulerio et al.

Anonymous Referee #2

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In this paper low- to mid-tropospoheric LIDAR measurements taken above a fixed site in northern Italy during MAP are analysed. Two cases are discussed when troughs (PV streamers) crossed the Alps. The origine of air mass above the LIDAR site is determined using backward trajectory technique. The article is interesting because it demonstrates that associated with streamer, low-tropospheric layers of dry air occur which presumably originates from the stratosphere. However, the article suffers from various deficiencies, given in the list of major comments. Therefore, I recommend acceptance of the paper subject to the authors modifying their manuscript in accord with the following major points.

Major comments:

(1) Sect. 3 on the Lagragian simulations is very wordy. It should be shortened and reduced to some key references. The corresponding text in Lininger and Davies (QJRMS) and Stohl et al. (BAMS 2003) could be used as a guideline.

(2) The authors discuss two events, however, it becomes not clear why to present both because they seem to be similar. Additionally it is not discussed if there are differences which give some reason for showing both events. The authors should concentrate on one case providing a more sound discussion for it.

(3) The comparison of LIDAR data with sounding data is confusing and needs a more thouroughful discussion. For instance, the temporally shift between LIDAR site and Milano sounding is not clear. It is assumed that the layered structures are tranported easterly with the trough. On the other hand the flow at the trailing edge of the streamer comes from the north. It is contradictory that in Fig. 1 at the earlier time two and at the later time one dry layer in the LIDAR picture whereas in the soundings there are just one at 18 and two at 00:00 UTC.

(4) It is mentioned that there are further LIDAR data on the 8 and 9 November. Would it be possible to use these data to demonstrate that the application of Lagrangian trajectories give reasonable results also for the vanishing of laminar structures.

(5) The results of the CET technique, indicating the origine of the LIDAR site air, should be mentioned, but the extended text as well as the table are not necessary. This is, because most of the tables' content is not discussed (e.g. theta, alt, lat). Therefore, the corresponding text must be strongly shortened and the tables should be removed.

(6) It becomes not clear why showing Fig. 2. It shows that the structure appering in the LIDAR data (Fig. 1) occurs about 10 hours later than determined by the LTR data. This needs an explanation. In the present context one would expect that the authors investigate a similar figure but for a later time e.g. around midnight for comparing it with

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Fig. 1. A further cross-section, determined for 6 Nov. 15:00–16:00 UT, could be used for comparison with measured LIDAR data (Hoinka et al.) and with trajectory derived data (Lininger and Davies; LFP technique) in order to proof if the applied LTR technique provides similar results.

(7) Additionally the authors should make Figs. 1, 3 compatible with Fig. 2. This figure shows a zonal cross-section from W (left) to E (right), which represents a series of longitudinal depending profiles at the same time. If a feature, measured at a fixed location (e.g. cold fronts, streamers etc.), moves eastward then with increasing time at first the easternmost profile is taken and at the end of the measurement period the westernmost one. Therefore in order to compare both data series the temporal evolution must start on the left side with the westernmost profile. Thus in the corresponding figure the time runs from right to left.

Minor comments:

(1) All times given in the text (in p.m. and a.m.) should be given in UT similar to the figures.

(2) p. 8328, l. 8: The sentence, “These events...phase”, should be reworded by explaining what has happened. What means the ridge’s “break-out phase”?

(3) p. 8328, l. 21: The definition of the abbreviation PV should appear at first place where the term ‘potential vorticity’ appears.

(4) p. 8328, l. 20: The sentence, “Moreover...”, is very vague. How can PV-rich and dry air influence the dynamics?

(5) p. 8329, l. 9; and in the text and references: Langford or Langfort?

(6) p. 8330, l. 20: There are more tasks for PV-streamer within MAP. Therefore “...requiring also...” should be added.

(7) p. 8331, first para.: One should add some comments on the temporal and spatial

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resolution of the data, finally used in the figures, and by which techniques these data are achieved. Move text from first paragraph on p. 8335, which is misplaced there, to section 2.

(8) p. 8334, l. 13: It should be make more clear what means “breaking phase”.

(9) p. 8335, l. 7: The term “average fall speed” suggests that there is a flow in the cross-section plane. But this is not obvious because it is a three-dimensional flow problem.

(10) p. 8335, l. 10: For what reason is there a “dehydration” process. The LTR results indicates a three-dimensional transport.

(11) p. 8336, l. 10: Is there a reason for choosing 8.5 E and 45–47 N. Why not a similar range in longitude?

(12) p. 8336, l. 16: The definition of pvu is missing.

(13) Caption Fig. 3: At what time is the ECMWF profile? Why are the shown profiles different in time: LIDAR 1:00 UT and simulation 24:00 UT?

(14) p. 8337, l. 13: The term “vertical speed” suggests that there was a lifting. The discussion provides no reason nor explanation for this.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 8327, 2004.