

Comment #1: How did you determine/choose the spatial Match condition of 150 km? You mention in the paper that this value strictly depends on the horizontal extension and variability of the cirrus. If you didn't have available any space-borne instruments information, on what basis you have chosen this value?

-In absence of any satellite information, the $R_{\max}$ was chosen on the basis of the cirrus type (tropopause thin cirrus) whose characteristics have been observed and studied over OHP by Keckhut et al. (2005) and Montoux et al. (2010) and described in the paper at the end of section 4.3. To clarify this issue, a part has been added in the paper in the new section 4.4 that include also a replay to Comment#3: 'As stated in Section 3, one of the main requirements to provide meaningful Match observation is a correct value of $R_{\max}$. In absence of any space-borne instrument information, $R_{\max} = 150$ km has been fixed on the basis of the structure characteristics of the observed cirrus (see Section 4.3) and $R = 80$ km, resulting from Hysplit model, fulfills this condition. However, at RTV site the studied "quasi-stationary" time period is approximately 1 hour long (from 23:37 to 00: 50 UT, see Figure 2b), which, with a 100 km/hr horizontal wind speed, could mean a "quasi-stationary" spatial extension of the cloud of 100 km. Thus the fixed value of $R_{\max}$ appears to be too large and, for this case, a probably reliable value could be 50 km (centering the cloud on the air mass).'

Comment #2: This also triggers the question how representative it is to use temperatures and pressures measured 80 km away from one of the lidar stations.

- We agree with the reviewer: co-located temperature measurements are important also in the case of evaluating temperature fluctuations, however at his stage (feasibility study) temperature values have been used only as reference value. New text added in section 4.4: '..... For this purpose collocated temperature measurements are also recommended in substitution of operational radiosoundings that, in case of OHP, are 80 km away from the lidar station.'

Comment #3: From the contour plots of the backscattering ratio one can see that the cirrus cloud at the end of the lidar session at OHP actually vanishes. So the question is whether at all you capture the same cloud and the same air mass. I guess not. So, here I agree with the first referee comment that I'm not convinced that the technique actually works. The results from Hysplit model show about 80 km distance of the air masses to the RTV site. Conversely, the lidar image at RTV shows that the "quasi-stationary" time period is at best 1 hour long, which with ca. 100 km/hr horizontal wind speed means that the "quasi-stationary" length of the cloud is possibly 100 km. Therefore, I think the match distance of 80 km is too large to provide a meaningful match.

- We agree with the reviewer, see reply to comment #1.

Comment #4: I have also to agree with the first referee comment concerning the importance of mesoscale fluctuations. They are not mentioned at all in the paper and for sure they would have a large effect on the cirrus proper ties, therefore on sedimentation velocity. Some comments about this topic need to be added.

-An extensive part on this topic has been added in the new section 4.4: see reply to comment #3 of Referee 1.

Comment #5: Even when assuming that there is no problem with the match, how do you exclude that new ice nucleation could have happened below the cloud, moving the lower cloud edge to lower altitudes? You determine a model isentropic shift of 100 m upwards, and could the corresponding cooling of 1 K have led to new ice formation? If that were the case you would misinterpret a shift in the lower cloud edge as sedimentation, while in reality it was nucleation. Arguments need to be provided that exclude this possibility.

- We don't exclude this possibility. At this stage we wanted to illustrate a possible way to evaluate the mean changes of backscattering profile by assuming isentropic transport and frozen conditions inside the cirrus during the advection between the two sites. This is explained in section 4.3 where some new parts have been added after equation (1): 'The equation (1) is valid only with the hypotheses of adiabatic transport and of 'frozen' conditions inside the cirrus (i.e. cloud microphysical evolution and mesoscale fluctuations are not taken into account at this stage) between the two sites. We will assume these hypotheses in the remaining part of this section, while their validity will be discussed in the next section. For this 'frozen' conditions, the only mechanism of ice crystals destruction is by sedimentation while any generating mechanisms of new ice crystals are not foreseen.'

- The possibility of a new nucleation caused by the isentropic shift has been added in the new section 4.4, see reply to comment #6.

Comment #6: Finally, I do not understand the behavior of the trajectories shown in Figure 4. I look at the narrow time range 20-21 UT on 13 March. In this period the black air parcel rises by 200 m, and if the motion was adiabatic, I would expect the temperature to drop by 2 K. Instead, temperature increases by 1 K. Why? The only reason might be release of latent heat by condensing ice, but this can hardly overcompensate the adiabatic temperature decrease. Possibly this behavior is due to significant mixing processes of the air parcels happening exactly when the air crosses the Sea Alps and is exposed to orographic perturbations? This needs to be discussed in the manuscript.

- This behavior has been discussed in the new section 4.4: 'The analysis of backward trajectories is useful to discuss the validity of the 'frozen' approach used in Section 4.3 to estimate Δz through eq. (1). In particular, the Figure 4 reveals that between 20-21 UT on 13 March, in correspondence of the crossing of Maritime Alps, air masses seem to rise by 200 m, with an increase in temperature by 1 K. This behavior could be related to the effect of significant mixing processes of the air parcels exposed to orographic perturbations. In the case considered, the cirrus maintenance could have also been affected by the $\Delta z_{\text{theta}} = 100$ m upward (and a corresponding cooling of 1 K) that could have caused to new ice nucleation. This could lead to a misinterpretation of cirrus shift that could be due to nucleation instead of sedimentation.'

Smaller issues:

There is a typo in one of the author's name (Kekchut instead Keckhut). This should be corrected.

-Done

Abstract: It is not clear what the "r" in v_r stands for. Later this symbol changes to v_s.

-Done

Abstract: There is a confusing, long sentence, "These systems have similar. . .". Probably separating the clauses would help, e.g. "Two lidar systems with similar instrumental characteristics are located at the Observatory of Haute Provence (OHP, 43.9 deg N, 5.7 deg E) in France and at Rome Tor Vergata (RTV, 41.8 deg N, 12.6 deg E) in Italy. At a distance of approximately 600 km they provide systematic measurements for many years. . ."

-Done.

Abstract: What is "cloud shape"? (And how is it used?)

- The new sentence states: 'The analysis through lidar principal parameters (vertical location, geometrical thickness and optical depth)....'

Abstract: What is a "balloon size particle"?

- Corrected. New sentence: '....particle distribution measurements utilizing multi-wavelength lidar or balloon-borne size particle sampling.'

Intro: What is "ice crystal fall"?

-Corrected. New sentence: 'Sedimentation of ice crystals....'

The quality of the plots is generally bad (fuzzy).

- New plots have been inserted with higher quality.

To have a better overview of the major revisions performed, please see the revised discussion paper, added as a supplement file, in the general reply to the referees.