Comment #1: From looking at the backscatter profiles you determine that the "BSR mass centre" has shifted down by 200 meters. From this shift, the time taken for a parcel of air to advect from one lidar site to the other, and the model isentropic shift (100m upwards) you infer the ice particles have fallen for effectively 300 meters relative to the air mass in which they are embedded, and hence their terminal velocity in still air would be approximately 1.5 cm/s. The error due to the range resolution of the lidars is useful - however I think this error actually applies *twice* - once at each lidar site, so potentially the height error is 150m which is three-quarters of the observed change in height, and an error of 50% in the derived fall speed. The other source of error, which is not considered in the paper at present, is any uncertainty in the model-derived vertical air motion. Can you put a figure on this? This could be a significant source of error - if the model is out by 1cm/s then the derived fall speeds would be essentially meaningless. So this really needs to be addressed in the paper.

- We agree to the fact the height error applies twice, however the error considered is not the range resolution of the lidars but the error associated to the estimation of mid-cirrus height (z_{cir}) through equation 2. As written in the text, this error is equal to 50 m for both the systems. Therefore an error of 30%, that is 0.5 cm/sec., is associated in the derived fall speed. This value has been corrected in the text.

- We agree with the reviewer: the uncertainty in the model derived vertical air motion is important. At the stage of feasibility study, this error has not been calculated and we don't have significant figure on this.
However we include some considerations, as suggested by the reviewer, in the new section 4.4: 'Another source of error, which has not been considered, is $d\Delta z_{theta}$, the uncertainty associated to air masses isentropic shift between the two sites. The representativeness of thermodynamic variables is a critical point in meteorological models and depends upon local effects as well as the larger scale gradients of the variable and how well a gridded field can represent a continuous function. The accurate estimation of this term is difficult and, for the aim of the paper, not necessary. However if we consider that an error of 100 m would lead to $d\Delta z_{theta} \approx 0.6$ cm/sec, the vertical resolution of the Hyplit version used (50 hPa in upper troposphere) is not adequate for the required precision.'

Comment #2: The other question that was in my mind as I was reading this paper was "why use this approach?". Doppler radars and lidars do exist, and can measure the vertical velocity of ice particles in clouds. So what advantage (or potential advantage) does this match technique have? This should be spelled out at the start if possible.

- We agree. We include an extensive part on this topic in the middle and last part of the introduction: 'Vertical velocity in cirrus can be measured through Doppler radar by averaging Doppler velocity over long period (Orr and Kropfli, 1999) and by solving explicitly ice crystal velocity within the sample volume using 3 Doppler moments (Deng and Mace, 2006). The main limitation of the technique is that it is mainly sensitive to large particles (i.e. effective ice crystal radius > 50μm). On the contrary the application of coherent Doppler lidar, which provides very accurate measurements of vertical velocity also in cirrus nucleating zones (Grund et al., 2001), is limited to optically thin clouds. A methodology which couples radar and lidar measurements has been developed (Tinel et al., 2005), but, as shown by Donovan et al. (2001), this cloud radar–lidar synergy cannot retrieve ice crystals with an effective radius lower than 10 μm………………In comparison to a lidar-radar synergy, the employment of this observing strategy could provide unique measurements about the mean changes of the microphysical cloud properties during a cirrus advection over hundreds of km. This will bring helpful information for the parameterization of cirrus formation and evolution in GCM. Furthermore the investigation of both optically thick and subvisible cirrus (SVC, with ice crystals < 10 μm) is possible. Finally, in the future, this approach could be also used with a lidar-radar synergy.'

Comment #3: Backscatter plots. The time-height cross sections don't look particularly similar, and clearly have quite a bit of variability in their structure. You do mention this, and speculate sedimentation has led to an evolution of the cloud microphysical profile. The other option I suppose is mesoscale fluctuations in the dynamics - some people think gravity waves are ver y impor tant for these kinds of clouds. Either way, this variability and/or microphysical evolution is going to affect your sedimentation rate estimates which assume the cloud structure is frozen for 6 hours. Can you quantify or in any way try and estimate what effect this is going to have? What happens if you perform the match technique over slightly different sections of the time series - do you get the same results?

- We agree to the reviewer and an extensive part on this topic has been added in the new section 4.4: 'Furthermore the advection over the Alps could trigger the formation of gravity waves. Several works have shown that mesoscale fluctuations induced by gravity waves in the upper troposphere and lower stratosphere influence the formation and the evolution of high cirrus clouds considerably (Haag and Kärcher, 2004; Jensen et al., 2005, Kärcher, 2012). The analysis of the observed lidar quasi stationary periods cannot provide information about these effects, excepting the consideration that the maximum mid-cirrus height displacement of the quasi stationary cirrus over the two sites is 100 m (from 11.4 to 11.3 km for OHP and from 11.1 to 11.2 km for three last quasi-stationary cirrus single layer periods for RTV, see Table 3a and 3b). This vertical displacement could be connected to the fact that temperature fluctuations on scales of hundred kilometres in the upper troposphere are of the order of 1 K (Gierens et al., 2007).'

Other comments:

Abstract line 1 - notation seems inconsistent here, using vr rather than vs

-Corrected

Line 14 - "the analysis through lidar primarily parameters" - can you rephrase this?

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

Section 1, page 5789, line 26 - say satellite instruments are "nearly blind" for a "large part" of cirrus cloud population. Can you be more specific here.

- Ok. New sentence: ' Space-borne passive remote sensing instruments in the infrared, due to their low sensitivity, are not able to vertically characterize a part of these clouds as in case of semitransparent cirrus (high thin cirrus) or multi-layer clouds (thin cirrus overlying low clouds) (Stubenrauch et al., 1999).'

Section 1, page 5791, line 9 - say range of cirrus crystal velocities is 0.1-10cm/s. Can you provide a reference for this? I think it rather depends on what you define as "cirrus". Note this range is also inconsistent hereas in the first paragraph of section 1 you mention Jakob who uses a range of 10-200cm/s.

- There was an error, we intended a range of 0.1 – 200 cm/s. New sentence: '(between 0.1-200 cm per sec for ice crystals with effective radius from 5 to 500 μm, Schmitt and Heymsfield, 2009)'.

Section 4, page 5797, lines 15-18 - maybe delete this paragraph, repetitive of previous material

- Done.

Page 5799, line 8 - I wasn't clear here if top height was determined from lidar or sonde?

-The top height was determined by lidar while local tropopause by the nearest operational radiosonde. New sentence to make it clear: '….and a top height that is around the local tropopause (11.24 and 11.10 km for OHP and RTV), which was calculated using radiosonde temperature data and the definition of thermal tropopause.'

Section 4.3 paragraph 1 - I think it would be useful here to clearly note in the text that the points of interest are the par ts marked at the very right hand side of the graph - this will help the reader get their bearings (otherwise it seems like there are much more dramatic changes in the back histor y than are actually relevant to the match analysis)

-Ok. New sentence: 'Focusing to the to the trajectory portions highlighted by the horizontal black lines at the very right hand side of the graph….'

page 5804, line 3 "(radius < 5 microns)" - but you estimated radius of 10 microns (stated in previous paragraph)?

-Corrected.

Other questions:
Are there any geostationary images which might be used to show the Ci layer propagating between the sites.
- No. A phrase has been added in the last part of new section 4.4: 'In our case, the small optical thickness of the cirrus limited the use of passive sensors (we do not have any geostationary images).'

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.