Answer to Reviewer # 2

The authors thank the reviewer for his pertinent and helpful comments on the paper. They are grateful for this review work which is always rather time-consuming and cumbersome. The manuscript has been significantly modified according to the suggestions proposed by the reviewer but also from several discussions and iterations between the co-authors. The microphysical and optical properties of the primary and secondary wakes are now presented separately. These modifications have changed the manuscript mainly for clarity objectives. The remainder is devoted to the specific response item-by-item of the reviewer's comments :

1. Introduction. I think that the objectives of the paper could be more clearly outlined: software validation for the reconstruction of optical properties? Providing accurate data for model validation? I suggest adding a paragraph in this section.

The objectives of the paper have been rewritten and refer now to previous published results (particularly in the BAMS special issue) in order to discuss why more measurements are needed (see the revised version of the manuscript).

2. Page 26869, line 20: Since you mention the numerical simulations by Unterstasser et al. 2008, you may also cite other literature of contrail simulations in the vortex phase. I suggest: Lewellen and Lewellen, JAS 2001, Unterstrasser and Gierens, ACP 2010, Paugam et al, ACP 2010.

Done.

3. Page 26876, lines 11-14. This part contains some inaccuracies: the theoretical vortex descent is a robust estimation of the vertical position of the primary wake. What is difficult to predict is its vertical extent which is much more sensitive to the specific aircraft and ambient conditions (vortex instability, turbulence, shear, etc.)

This part has been modified and rephrased, see new paper manuscript.:

4. Page 26875, lines 18-20. The way the sentence is written, you get the impression that NOy and Conc1 are not correlated because of some plume internal heterogeneity whereas in fact it is just that the measurement is taken when the Falcon is crossing the boundary of the plume as reiterated on page 26877.

There are different physical and dynamical processes working on gaseous species and on the particles in the aircraft wake, hence it is not surprising that high NOy concentrations indicative for the primary vortex are not correlated with high particle concentrations or IWC. The NO_y data are highly correlated with trace gas observations of SO_2 and HONO in the A380 contrail (Jurkat et al., 2011). Except for the first encounter at 12:15:37 UT, the primary vortex sequences with high NO_y mixing ratios are not correlated with the particle concentration or the *IWC*. The complex vortex dynamics acting on gaseous species and particles, the inmixing of ambient air as well as particle inertia could explain a separation of particles and trace gas fields in the primary vortices and the secondary wake of the aircraft exhaust. Most contrail ice particles may form at the outer edge of exhaust plumes while emission trace gases such as NO_y are concentrated inside the exhaust plume (Petzold et al., 1997).

5. Page 26877, lines 5-11. Same as above, I don't like the way this part is logically formulated. The cause of data mismatch is that one of the instruments is inside the plume and the other is outside. If both were inside and you could see the same mismatch in Fig 3, then it would be a signature of plume heterogeneity.

The reliability of in situ measurements are hampered by the highly heterogeneous structure the primary vortices. For instance during the first NO_y peak, the extinction measurements by the Polar Nephelometer (which peak up to 7.0 km^{-1}, also seen at the 10450 m level on Fig. 3d) are not correlated with the FSSP-300 data. This feature is now clearly illustrated in Appendix A by much largest PN extinctions compared to the data derived from the FSSP-300 (see surrounded red data on Fig. A1). The FSSP-300 instrument was likely out of the vortex, whereas the PN (mounted on the opposite wing) sampled the plume.

6. Page 26678, line 21 to page 26679, line 8. This part is confusing me, you may consider rephrasing it. What is the message you're trying to convey?

This part has been rephrased (see revised mansucript) and is discussed in the introduction.

7. Page 26679, lines 11 to l4. Since this is quite a strong statement, do you have an explanation why particles are becoming non-spherical (I could not find it in Sec. 4)? Could it be possible that ice crystals sedimented from the above cirrus into the contrail?

As indicated in the manuscript a few large ice crystals embedded in the contrail were detected from both 2D-C and CPI instruments. These particles are likely precipitating out (by sedimentation) of the thin scattered cirrus clouds observed above the A380 contrail (see Fig. 1). These particles are much larger (i.e. > 100 µm) than the contrail particles (3-5 µm) and cannot explain the non-spherical feature of the oldest contrail particles.

The explanation why particles are becoming non spherical has also been rephrased and is discussed now in section 4.2:

8. Page 26882, line 4. Could you think of any particular reason why the retrieved particle distribution in case C does not reproduce the measured distribution?

We recall that a FSSP probe provides the size of a particle considering the energy scattered only in the forward direction or more specifically, between 6 and 15 degrees for the FSSP-300 during CONCERT. Thus, the FSSP-300 data are less sensitive to the particle shape than the PN measurements, which are performed in the large interval of scattering angles. It would appear reasonable that the FSSP-300 and the retrieved size distributions (Fig. 4c, left panel) are interrelated by some nonlinear relationship because the theoretical phase function (blue plus-symbols) and retrieved (solid black circles) scattering phase functions are close at the forward scattering angles (Fig. 4c, right panel).