We thank Referee #1 for her/his comments and careful reading and changed the manuscript according to her/his suggestions. Our response is formatted as follows:

Referee's comments

Author's reply

Changes to the manuscript

The paper uses aircraft tracer measurements from the POLSTRACC campaign combined with simulations with the CLaMS model to derive conclusions on the characteristics of transport in the high-latitude boreal lower stratosphere for the winter of 2015-16. The results show an increase of the mean age in the region from January to March 2016, which seems at odds with the increase in CO concentrations. The authors argue that this is due to a change in the age spectrum, which exhibits an increase in both old and young air by the end of the winter.

The article is well written and presents an interesting analysis motivated by in situ observations and nicely complemented with modeling tools. I recommend publication in ACP after the following few minor comments and technical corrections are addressed.

The main point that should be addressed regards the high values of CO observed during phase 1, seen in Fig. 5a at about 70 degrees and 330-340K, and in Fig. 7 (CO values above 45 ppb_v). Although these values are not the main focus of the paper, they stand out, and there are a few parts in the paper where I miss some explanation of their origin. For instance, in the description on Page 13 Lines 25-29 it is mentioned that the 'direct tropospheric impact was greater in phase 1 than in phase 2' referring to these points. What do you mean by 'direct tropospheric impact'? Is this transport across the ExTL or did the high CO values originate in the TTL?

The expression 'direct tropospheric impact' should indicate, that the decrease of CO relative to N_2O occurs at the highest stratospheric values of N_2O (i.e. at most tropospheric influenced air masses), where air parcels which have been recently transported into the lowermost stratosphere have in general the shortest stratospheric residence time.

It is not possible to derive from Figure 7 the information whether transport out of the TTL region occurred in this specific case or from the ExTL. The values you are referring to were encountered during one specific flight (PGS 09) on 22.01.2016. The flight track crossed a filament of air with relatively high values of tropospheric trace gases and a high tropopause with a sharp PV gradient. However, since small scale processes like gravity waves, occurrence of turbulence in regions of strong wind shear at the jet or diabatic heating violate adiabatic PV conservation this may lead to a mismatch of analyzed PV fields and tracer occurrence, which could also have caused the anomalously high CO values in this case.

Since we analyzed our dataset only for PV > 7 PVU, we expect that the overall impact of the ExTL on our analysis is small.

Also in Section 5.2, you could look separately at the age of air spectrum for those air masses, instead of showing the results for all measurement points in phase 1 together. Does that help in interpreting the origin? Finally, some measurements in phase 1 were taken at lower latitudes (over Italy) compared to the rest of the campaign. Does that latitude difference have an impact on the CO values?

The measurements over Italy do not affect our analysis of the observed CO increase relative to N_2O. Due to technical problems, we were not able to obtain N_2O measurements during this flight, so these data points do not appear in the CO-N_2O correlation and our analysis. Furthermore, these data points would be excluded by applying the 7 PVU criterion to our data as described in the manuscript.

The number of data points with high CO between 330 K and 340 K in this region is 297, compared to 5518 data points for the whole distribution of phase 1, which just makes a fraction of 5.3%. Since the data were observed in a region of strong PV gradients as described above and the main focus of the paper is on the region above Θ = 340 K, we did not analyze these age spectra separately.

We changed Fig. 2 of the manuscript. We now distinguish between parts of the flight track below PV = 7 PVU and above. We further removed the flight over Italy since the N_2O data are missing and do not contribute to our measurements.

P1 L22: 'diabatic descent [. . .] adds to the diabatic downwelling of the Brewer-Dobson circulation'. It seems to me you are referring to the same thing twice?

We wanted to refer to the two main processes which lead to diabatic descent during the polar night over the poles, namely the absence of radiation and associated diabatic cooling and the wave driven descent. We changed the section to:

Diabatic descent in the polar stratosphere, which is strongest inside the polar vortex results as part of the Brewer-Dobson circulation (Brewer, 1949; Dobson, 1956) in mid and high latitudes as response to the breaking of planetary and gravity waves (Haynes, 1991; Plumb, 2002; Butchart, 2014) in the upper stratosphere and mesosphere.

P2 L29: tropical pipe

Sentence changed to:

The region between Θ = 380 K and the bottom of the tropical pipe around Θ = 450 K (Palazzi, 2011) is a key region for the transition between these transport regimes.

P3 L23: The McPhaden reference is not about the 2015 ENSO event. A better option could be perhaps L'Heureux et al. (2017).

References changed to Chen et al. (2016) and L'Heureux et al. (2017)

P3 L24: The impact of the 2015-16 ENSO event on the polar vortex has been analyzed by Palmeiro et al. (2017).

Manuscript changed to:

A direct influence on the polar vortex is still under debate and according to Matthias (2016) this strong El-Niño is suggested to account for a weakening of the polar vortex, while Palmeiro (2017) found a connection of this ENSO event to the strong polar vortex and the early MFW.

Only flights that were used for the analysis are shown.

Caption changed to your suggestion.

P8 L1: take → taken

Changed

P8 L20: remove 'respective'

Changed

P8 L27: remove respectively?

Changed

P9 L7-8: is this a hypothesis or do you have an argument to support this statement?

This is a hypothesis based on the assumption that a change in the lifetime of SF_6 would lead to an equal change in the absolute values of mean age for both phases of the campaign. This assumption is also supported by the fact that we do not see any indication of an influence of mesospheric air on our observations. Since the discussion in our study is based on relative changes of mean age between phase 1 and phase 2 it is unlikely that the mesospheric loss of SF_6 affects the differential analysis of the calculated mean age.

P9 L14: Are the physical altitude ranges the same for both phases?

During both phases of the campaign the flight profiles were very similar and nearly every flight reached FL450 to FL480 (pressure altitude ranges to 45000 ft and 48000 ft, respectively).

P9 L18: The mean increase of 0.29 is just below the precision of the mean age estimate from SF6. Do you still consider it a robust change?

We consider the change as significant. Even if the observed change in the estimated mean age from SF_6 is just below the precision for each individual data point, one obtains a significant change in the mean binned mixing ratios of SF_6. Note that the increase of 0.29 years is only valid for the overlapping distribution of phase 1 and phase 2 (Fig. 3c)) and also the CLaMS simulations indicate an increase of the mean age of the same *magnitude, which is consistent.*

The Fig. R1 below further illustrates the change of the mean age distribution towards higher mean ages based on the SF_6 distributions for the two phases. The increase of the mean age over all observed data of the distribution is 0.79 years.

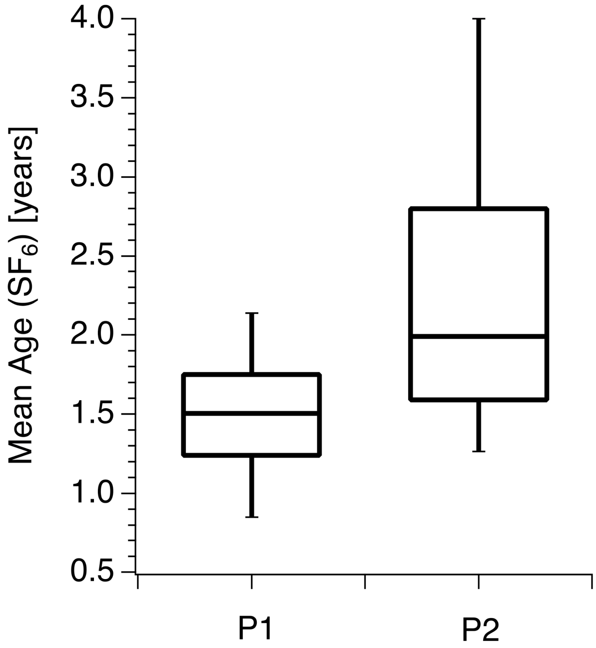

Fig. R1: Box-Whisker plot of Mean Age (SF_6) for phase 1 and phase 2.

P9 L27: "with an variability" → an interannual variability?

Manuscript changed to:

a meridional variability

P11 L2: chapter → section

Phrase „chapter" changed to "section".

These potential influences are discussed in section 6.

P12 L4: [...] general picture of enhanced downwelling of the Brewer-Dobson circulation [...]

Manuscript changed according to the suggestion

[…] which fits well in the general picture of enhanced downwelling of the Brewer-Dobson circulation in late winter/spring.

P12 L9: despite the

Changed

P13 L4-13: Could you refer to the individual panels of Fig. 6 as you describe the figure?

Changed

P13L21: anCO→aCO

Changed

P13 L25-29: This description is unclear. What you mean by ‘shows higher CO relative to N2O?’. Perhaps it would make it easier to follow if you referred to the isentropic levels approximately corresponding to the N2O values when you describe Fig. 7 (it is hard for the reader to combine mentally Figs. 4, 5 and 7).

This sentence shall highlight the main result of this figure and refers to Fig.7. Higher CO values relative to N_2O are evident between N_2O = 275 ppb_V to 320 ppb_V. Compared to Fig. 6c), this is an indication that this change of the correlation can only be due to a change of the effectiveness of mixing. At this point we leave the geometric (or isentropic) coordinates since the tracer coordinate N_2O in Figure 7 serves as natural tropopause following coordinate.

If one would try to deduce the results from isentropic coordinates one could not differentiate between mixing and transport processes. Since both tracers will undergo the same transport and mixing processes these processes are accounted for in tracer tracer correlations. Relative changes of two tracers of very different lifetime like CO and N_2O therefore indicate changes of either sources or sinks or the transport efficiency.

P17 L4-5: would it be more accurate to refer to these figures as ‘scatter plots’ rather than ‘correlations’? Also on Fig. 9 caption.

Changed to the suggestion

P17 L18: remove ‘which is’

Changed

P18 L8: Green’s function

Changed

P21 L1: what do you mean by ‘mass balance systems of transport pathways’?

This refers to equation (4) and is changed to:

[…] the mass balance equation

The analysis of the CO-N_2O correlation and the mass balance equation as well as the model simulations consistently point towards …

P22 L27: Eventhough → Although

Changed

P24 L8-10: This sentence is unclear. Do you mean that the high fraction of young air reaches higher latitudes in 2015/16 as compared to the climatology? If so, what is the variability (e.g. standard deviation) around the climatology? Is this winter statistically different from the climatology?

The below graph R2 shows line plots of the relative difference of air masses with transit times smaller than six months (MF06) from March 2016 to March of the climatology (thick line) at 350 K and 400 K.
At latitudes northwards 60° there are up to 10% more MF06 air masses as compared to the climatology, which also supports our hypothesis.

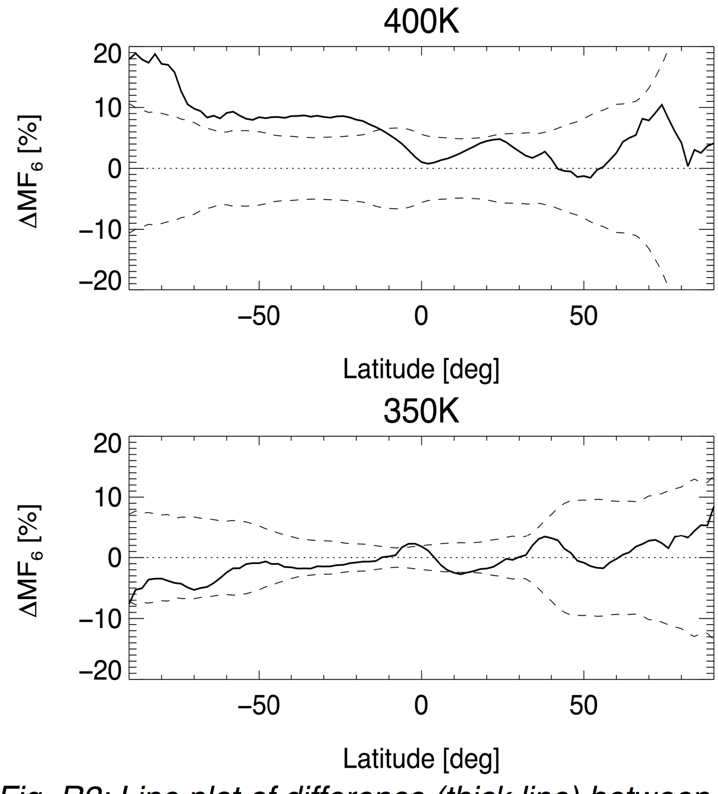

Fig. R2: Line plot of difference (thick line) between March 2016 and March of the climatology. Dashed lines denote the standard deviation.