

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-356

Anonymous Referee #1

Referee comment on "Comparison of inorganic chlorine in the Antarctic and Arctic lowermost stratosphere by separate late winter aircraft measurements" by Markus Jesswein et al., Atmos. Chem. Phys. Discuss.,
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The essence of this paper is that it uses aircraft data and trace gas correlations to estimate Cly in the lowermost stratospheric (LMS) vortex and the midlatitudes of each hemisphere, and then determines what fraction the Cly is of the total Cl. Differences indicate the SH LMS vortex has higher Cly than the NH; the meaning of the hemispheric differences is potentially interesting. The method used to estimate Cly from halocarbon observations has been done before (Schauffler et al., JGR 2003) but was not properly credited. That study used northern hemisphere N₂O rather than potential temperature as a vertical coordinate, however, N₂O is effectively a vertical coordinate and their results to include the lowermost stratosphere region shown here. The paper would be strengthened by discussion of why their hemispheric profile differences are of interest and what they tell us about the stratospheric circulation. The observed Cly profiles are worth publishing, but not until quite a few issues with the analysis and coordinates are resolved (described below).

Main topic areas to address in revision

Source of tropopause data, lines 155-165. Each profile is analyzed with respect to its height above the tropopause yet nowhere is it stated where this tropopause information comes from. What is the source of the 'local tropopause'? Is the aircraft doing frequent profiling to identify a tropopause (through a temperature minimum)? This must be explained. Later in the paper (line 322) there is talk of using a thermal tropopause, but this doesn't note the data source. Surely the climatological mean tropopause is not used for the constituent analyses. This issue is very important as the results depend on what information is used to determine this coordinate.

The unknown source of your tropopause data leads right into 3 related issues: using the thermal tropopause for your analysis, the decision not to use potential vorticity from a reanalysis, and the use (and source of) of equivalent latitude.

Use of thermal tropopause. The thermal tropopause is inappropriate in polar winter because the temperature profile is often isothermal – the dynamical (PV) definition is needed. See the analysis of dynamical v. thermal tropopause in Zaengl and Hoinka (J. Climate 2001). This means you need PV from reanalysis data (ERA5, MERRA2...whatever). These fields are available in fairly high resolution (0.5 degree or better) and even with interpolation they may more accurately identify the tropopause than does temperature in an atmosphere with weak vertical temperature gradients. Whatever your final analysis method is, you will need to justify it based on 1) showing that reanalysis PV doesn't give sensible results, or 2) proof that the temperature tropopause actually makes sense at high latitudes in winter.

Source of equivalent latitude. Around line 156 equivalent latitude is said to be used to sort the flight data: where does your equivalent latitude come from? Just 20 lines earlier it is stated why use of reanalysis data and its coarse resolution is a drawback to the analysis, but where do you think the equivalent latitude information comes from? It is calculated based on global PV fields which, by necessity, come from a reanalysis. So although you haven't explained the source of either the tropopause or equivalent latitude data used, it seems clear that you are using reanalysis info. This should be acknowledged. It's fine if you want to use the Greenblatt method for identifying profiles, but I'm not sure it's accurate to say that the reanalysis PV isn't good enough for your analysis. (Have you tested this?)

Antarctic and Arctic vortex size differences. These play a role in whether Figure 10 is meaningful. The Antarctic vortex mean edge is at 60S equivalent latitude – it's a large vortex. (Sep avg ~35 million km²). Even in 2019 the Antarctic vortex at 360K had an average size until the last third of September. I'm not certain what the Arctic vortex mean edge is but it's probably closer to 70N equivalent latitude (avg March vortex <20 million km²). Because of this the hemispheric difference plot using equivalent latitude coordinate doesn't make physical sense in the 60-70 degree range. In Fig. 10 the difference at 65 degrees will be a comparison of the Antarctic vortex with the northern midlatitudes. Since the hemispheric vortex profiles are already compared in Fig. 9, perhaps add panels to that figure showing the NH/SH midlatitude differences on the 2 vertical coordinates. I don't think Fig. 10 is very useful and could be eliminated.

Figure 7 discussion (l. 270). What data are used to calculate the mean age shown in Figure 7? There is discussion just prior to this about mean age and the 'arrival time' – is this what's plotted? Maybe I'm missing something but I cannot see what observations or information are used to produce mean ages. But a bigger problem is that mean age values of 5 years are shown for Cly of 1500 ppt. This can't be right. The best estimate is closer to 3000 ppt at 5 years. See for example Newman et al (ACP 2007) or Strahan et al (2014, JGR) or compare the N₂O values you observed with the ACE N₂O/mean age mapping in Strahan et al (JGR 2011). No data have been presented that demonstrate that SouthTrac data, which are entirely from 390K and below, have such old age. It's more likely the maximum age there is near 3 years.

The vortex Cly profile differences (Fig. 9) imply interhemispheric (IH) differences in mean age (and age spectrum) in the lower branch of the Brewer Dobson Circulation (BDC). These are presumably driven by transport and indicate that the NH lowermost stratosphere is younger than the SH. I believe such differences are expected – see for example Birner and Boenisch, ACP 2011. Simulations driven by reanalyses may reproduce these differences (as well as the midlatitude differences), but what about chemistry climate models (CCMs)? It would strengthen this paper to put your measurements in the context of what they tell us about IH differences in the lower BDC. These measurements help confirm our thinking about the stratospheric circulation. You might comment on whether and why it's important for CCMs to reproduce similar hemispheric differences.

Lines 220-240. Isn't the semi-direct Cly calculation nearly identical to Schauffler's method (JGR 2003)? While that was referenced much earlier it seems far more relevant here. If this is true then you can reference Schauffler here and shorten the description, only describing any way your method differs.

This paper uses measurements to calculate Cly from only the long-lived Cl-containing species, but there are contributions from short lived (VSL) Cl species too (e.g., CH₂Cl₂, C₂Cl₄). It should be explicitly stated that such species are excluded from this study. The estimated size of this neglected contribution could be noted. See Hossaini et al., JGR 2019 for an observational and modeling study that estimates the VSL Cly impacts.

Minor Comments

Title: It doesn't make sense to say 'comparison' without saying what you're comparing with. The abstract reveals it is Cly in the Arctic LMS a few years earlier. Perhaps 'Comparison of Cly in the Arctic and Antarctic lowermost stratospheric vortices'?

First sentence of the abstract. You're really talking about stratospheric inorganic chlorine so please say so. And the strat inorganic chlorine comes from all chlorine containing source gases with a lifetime of more than 5 months (see Hossaini et al JGR 2019), so that's the long-lived and many of the VSL species.

line 16. "Based on the results of these two campaigns, the difference of Cly inside the respective vortex is significant and larger than reported inter annual variations." Each campaign was a single winter – there is no information on interannual variability. I realize you are citing another paper on Cly variability in Antarctic lower stratosphere, but what about Arctic variability? Unknown? As written this statement is misleading and not supported.

line 20: '1980-ies' is 1980s

line 23 OCIO isn't involved in depletion. Null cycle.

line 45. In both hemispheres, polar winter temperatures are above radiative equilibrium because of dynamical (wave-driven) heating. It's not just the absence of insolation.

Since the paper is comparing Cly in the 2 vortices, do you have any comments/conclusions about differences in maximum potential O3 depletion in each LMS vortex?

line 85. You don't need to define payload

line142. I would emphasize that you mean mixing within the vortex. E.g., "...benefits mixing on isentropic surfaces inside the vortex..."

line 143. 'descent' not 'descend'

line 146. What you're describing is that as you approach the tropopause the vortex ceases to exist so there is no longer a barrier to mixing. There is nothing to distinguish.

line 152. By "Stratospheric transport and mixing is related to the isentropic surfaces" do you mean that transport and mixing occur on isentropic surfaces? This is unclear, please rephrase.

line 155 'had contact to the vortex core'? This is awkward and unclear. Is the intended meaning that the reference profile was entirely inside the vortex, away from the edge and mixing at the edge?

line 189, 'extensively'

lines 194-7. "The metric describing the combined effect of all ozone depleting substances (ODS) as an equivalent amount of inorganic chlorine in the stratosphere, related to tropospheric source gases in a simple, is the equivalent effective stratospheric chlorine

(EESC)". Awkward sentence. I suggest: "Equivalent effective stratospheric chlorine (EESC) is a simple metric that sums the effect of all ozone depleting substances (ODS) as an equivalent amount of inorganic chlorine in the stratosphere..."

General comment on 'pre-filtered', 'pre-required'. Drop the 'pre', it's not needed.

To clarify the meaning, I'd suggest a slight rewording (line 220): "Cly can be calculated as the difference between total chlorine entering the stratosphere and the organic chlorine that remains bound in chlorinated halocarbons"

line 256: 'rations'...you meant 'ratios'

line 239: "in the following..." Move this statement to the beginning of the next paragraph where you actually describe the semi-direct method and then reword. For example you can begin the next paragraph with: "The semi-direct Cly calculation is used in the case where no measurements of chlorine containing substances are available. This method is based on [trace gas?] correlations found in previous measurement campaigns."

Figure 9. It would be more useful to give titles to each panel other than 'a' and 'b'. Those labels normally go inside the panel.

Fig. 9 shows that the NH data reaches 405K while in the SH 385K is the maximum. Do these represent the same maximum altitude for flights, and does this difference indicate that the SH LMS vortex is much colder than the NH vortex?

Fig. 10 is saying that the NH midlatitudes are older than the SH. Anything to say about that?

Lines 298-302: Since you are identifying profiles as vortex, midlatitude, or edge already, I imagine the effect of the SSW is that you measured more edge and midlatitude profiles in November than you might have in another year. But you've pointed out that vortex descent has essentially ceased by September, so as long as you are sampling vortex air that hasn't mixed with midlatitudes, I would expect that the Cly profiles you measure aren't affected by the SSW. In other words, the mean age profile for air masses that are truly vortex air masses might well look similar to other years. Thus, the statement implying that the fraction of CCly found as Cly being affected by the SSW might not be right. On the other hand, there aren't data from other years and maybe this point should be made. There is no information on interannual variability.

In general, 'data' is a plural noun, thus, 'data are...' not 'data is'.