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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mixing and ageing in the polar lower stratosphere in winter 2015/2016" *by* Jens Krause et al.

Anonymous Referee #3

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This paper analyzes aircraft observations of N2O, SF6, and CO from a recent polar aircraft campaign to show that the Arctic lowermost stratosphere has diabatic descent between January and March (this is well known, not new), and that while the mean age increases, CO measurements indicate that younger air mixes into the vortex during winter. A trajectory model simulation of the same period is used to support their interpretation of the measurements. The main result presented is that when a region of the atmosphere gets older, it doesn't mean that younger air didn't mix in! That is, there can be simultaneous changes in the age spectrum where both ends of the distribution change. The results show how 2 tracers with different lifetimes can be used to identify simultaneous contributions from young tropospheric and older stratospheric air. This isn't exactly how the message is stated but this is the interesting result.

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Discussion paper

The analysis of the aircraft data set is fine, the results are not surprising; they are consistent with what we know about the BDC and horizontal mixing in the LMS. Because the results are consistent with expectations, please present the data and analysis more succinctly.

The paper is far too long for what it has to say and it is not clear what the motivation is for this study. There is too much rudimentary discussion that is inappropriate for a journal article; some of the CO production and loss processes discussed are not actually significant or relevant. It feels like I am reading a thesis chapter not a journal article.

There are minor scientific issues regarding assumed CO values from the TTL and the assumption that CO in the lowermost stratospheric vortex is coming from the TTL. The assumed CO values are out of date (too low) and ignore copious recent data from MLS. There is large seasonal variability in tropical CO due to biomass burning (i.e., the CO tape recorder). Revisions should be made with up to date CO and this will change the results. I am not your thesis advisor but I would like to help you produce an article that others will be interested to read. There are many suggestions below intended to improve the readability of this article. This study is worth publishing after considerable shortening. This paper has 14 figures and well over 7000 words, but there is one main result.

General science and writing suggestions

The BDC is driven by waves. Wave energy deposited in the polar region warms the stratosphere which then radiatively relaxes, resulting in diabatic descent of polar air. So please do not say (p. 1, line 22) that diabatic descent in the vortex ADDS to the BDC. It is PART of the BDC. This occurs several times in the paper.

It is very rare that it makes sense for a paragraph to have only 1 or 2 sentences. Please review your choices of paragraph breaks. The small paragraphs here generally belong in a nearby paragraph.

The words ‘respective’ and ‘respectively’ are misused in nearly every occurrence. Even when used correctly here they are not necessary. Please delete all uses of these words.

Regarding the use of passive voice, many science papers unfortunately use expressions like ‘In Figure 3, it is evident that. . ..’. A paper will be more concise and interesting to read with ‘Figure 3 shows. . ..’. I suggest eliminating passive voice wherever possible. In general, replace ‘it is evident that’ with ‘shows’. And ‘it is possible to analyse’ with ‘we analyzed’.

Line by line

Introduction. What’s missing here is any motivation for this study. Why are you investigating these data sets? Why would a reader want to continue reading?

p. 1, line 7: “We find an increasing influence. . .” Where?

p. 1, lines 22: diabatic descent does not add to the diabatic downwelling of the BDC. It is a part of the BDC. Same issue on p.2, line 7 and line 32. ‘strong diabatic descent. . .AND the wave-driven BDC’. Again, these are one and the same thing. And later, mixing caused by breaking planetary waves is not ‘in addition to’ the BDC. Breaking planetary waves are what drives the BDC.

p. 2, line 3, 4: ‘deep stratospheric air masses’ Unclear. Can you be more specific than ‘deep’? Define the LMS. Yes it’s in the figure, but say here the latitudes, pressures, thetas, etc.

p. 2, line 6: The first sentence of the paragraph refers to ‘these’ air masses. Which ones?

p. 2, line 29: there is only a tropical pipe, no subtropical one.

p. 2, line 34. The reason H2O and O3 are modified by this transport is because they have large gradients. This is the underlying idea you might want to mention.

p. 3, lines 1-4. These 2 sentences about radiative effects come out of nowhere. What do they have to do with the rest of the introduction? Do you ever discuss radiative effects again in the paper? (I don't think so.) Either integrate these sentences into the context of this introduction or delete them.

p. 3, line 5. Define what you mean by subvortex. The vortex extends below 410K, so this is unclear.

p. 3, lines 8-13. The coldest early Arctic winter since 1948? What records do you use that go back to 1948 (no reference given). In looking at the met statistics since 1979, I see there is the occasional DAY in December 2015 that breaks a record, but that's all. This statement needs to be backed up with a reference and needs to be more specific in what way it was the coldest. Since the vortex extends below 400K, it doesn't make sense to say that the chemical influence of the vortex is seen below 400K.

p. 3, line 14 and beyond. I thought the commonly used expression was 'stratospheric final warming' (SFW), not MFW. Is there a reference for the statement about MFWs being rare earlier than mid-March?

p. 3, line 22. The QBO phase impact on vortex strength is only true for the northern hemisphere (Baldwin et al., 2001).

p. 3, lines 23-24. Suggested rewrite: "Matthias et al. [2016] argue that the strong El Nino weakened the 2016 Arctic vortex, but this is still under debate." (Same meaning using half the words. When sentences are short and to the point, the paper becomes more interesting to read.)

p. 4, line 14. Spell out what PVU stands for. And what are your PV units? Yours seem to be a factor of 10 smaller than in other papers. That's not a problem as long as you define your units.

p. 4, lines 24-29: 'Resolution of 10 seconds' and 0.1 Hz are the same thing. Infrared is one word.

p. 4, line 30 and beyond. Reword. "...operating between wavenumbers X and Y that measures CO, N2O, and CH4. The instrument uses a multi-pass White cell with a constant pressure of 30 hPa to minimize pressure broadening of the absorption lines. All measurements were integrated for 1.5 seconds." And later "In-flight calibration is performed against compressed ambient air standards that were calibrated against primary standards before and after the campaign. The primary standards are connected to..."

p. 7, lines 17-20. A better way to describe this would be in terms of the spatial resolution you have after the averaging. So if you are smoothing over a 20 minute period, then based on the aircraft speed, what spatial resolution do you end up with?

p. 8, lines 6-7. Suggested "A Clams simulation with full stratospheric chemistry was integrated as described by Grooss et al (2016)." Why does it matter that it is typically run for 6 months? Delete this unless you explain why this matters. p. 8, line 9. 'formulation' not formation.

p. 8, line 17. Why are the gravity wave flights excluded?

p. 8, lines 22-23. Equivalent latitude is not just 'linked' to PV, it is derived from PV. This paper concerns itself with diabatic descent in the vortex, but you note that PV is conserved under adiabatic conditions (true), and clearly the polar vortex is not adiabatic. How to you justify the use of PV? (The answer lies in the time scales for radiative relaxation.)

p. 8, line 27. Suggest "The transport pathways create an age spectrum or transit time distribution."

p. 9, lines 3-8. Which lifetime did you use for SF6 in your model? Another rewrite: "Models and observations both show a high bias of up to 1 year in the polar vortex." At what altitude in the vortex does this apply to?

p. 9, line 9-10. This is the only correct usage of 'respectively' I saw. Still, it's not

necessary because the 'respectively' is implied.

p. 9, lines 21-31. Way too much detail on N2O. Instead of 'distinct background value' it's really that it has a near constant value throughout the troposphere that makes stratospheric influence identifiable. I would delete everything between "The tropospheric background value of N2O. . ." and ". . .Ko et al (2013)." You've already said the sources are at the surface, so delete the last 2 sentences of this paragraph and add: "N2O has a weak negative gradient above the tropopause that strengthens in winter due to increased diabatic descent."

Paragraph beginning p. 9, line 32. A clearer way to say this: "Figures 4a and 4b show N2O values between 276-325 ppbv were measured during Phase 1 and values below 200 ppbv were measured during Phase 2 above 400 K. Figure 4c shows an overall decrease in N2O in the polar LS due to diabatic descent during winter, consistent with mean age changes (Fig. 3c)."

Section 4.1.3 CO, first paragraph. This has way too much detail. Delete the 2nd sentence. 3rd sentence: "Due to the high variability of anthropogenic surface emissions, CO mixing ratios in the northern hemisphere vary from 70-200 ppbs [prinn] and the CO lifetime is on the order of weeks." 4th sentence ok. 5th & 6th sentences: "The main sink is oxidation by OH. The CO lifetime during polar night is a few months." Don't need reaction (1).

The phrase 'equilibrium value' is not right – this is not about chemical equilibria. I believe you are trying to describe a minimum stratosphere CO that comes from being far above tropospheric CO sources but well below where high mesospheric CO can influence the lower stratosphere. Please delete usage of equilibrium CO (happens a lot on p. 13). Also, 'stratospheric background value' used on the next page and elsewhere is a vague expression. Can you say what you mean by this?

p. 10, line 15. All you need to say is "The reaction of CH4 with Cl is an insignificant source of CO in the lower stratosphere [Flocke]."

p. 11, line 6. Figure 7 is mentioned before Figure 6. Reorder.

p. 11, line 10. The CO change during winter at 340 K is not so consistent with N2O. The CO changes small and are both positive and negative in this region. The results are mixed, not clear.

p. 11, line 15. Change 'rise the hypothesis' to 'suggest'

p. 11, line 17. Since there are no CO surface observations used, you can't really say anything about the strength of tropospheric source emissions.

p. 12, lines 2-5. I don't agree with the statement that the 'lowest values' are found in the 'furthest regions from the troposphere'. The difference plots (3c and 4c) show a range of differences (even a few increases), so if you were to quantify this (rather than showing color blocks) it's not obvious that you statement would be true. Can you be more quantitative, or at least modify the words here?

p. 12, line 4. 'contradicting' should be replaced, perhaps with 'unexpected'.

p. 12, lines 8-16. Here – and in general – there are too many sentences telling the reader what you are going to write about (or discuss later) rather than just writing about the subject. CO's use as a tracer has already been mentioned (Fig. 5) and the insignificant source form CH4 + CO has already been noted. Delete.

p. 13. Line 1-2. Use active voice as much as possible, e.g., 'tropospheric data have high N2O [how high?] and are accompanied by high CO while stratospheric data have N2O < 328 ppb'. p. 13, line 7. N2O is 'longer-lived' not 'chemical inert'.

p. 13, line 13. Regarding the line showing meso CO on Fig. 6d, it should point to a value of 0 N2O because there is essentially no N2O in the mesosphere.

p. 13, lines 23-28. This result is not so mysterious and it would not seem so if this were discussed in terms of the species' gradients. The changes observed during winter come from 2 processes: descent and mixing. The change in each tracer depends

on the tracer's vertical and horizontal gradients as well as the balance between vertical and horizontal motions. This is a more physically meaningful way to explain the observations.

p. 13, line 31. Try instead 'The region measured during both phases is 340-380K.'

p. 13 line 34-35. Why do you assume that the increased tropospheric fraction comes from the TTL? The POLSTRACC data didn't sample air south of 40N, so how can you be so certain of its origin? Isentropic mixing across the LMS tropopause (320-360K range) between the midlatitude UT and the polar LS would have the same effect, right? Without observations or a trajectory analysis, there isn't convincing evidence presented that the TTL is the source.

p. 16. Equations 2, 3, and 4 use mixing rations and fractions. I don't see any use of mass here. Why then is this described as mass balance?

p. 16, line 15. Why are you citing 10-20 year old studies for the value of CO at the tropical tropopause? You could be using something far more precise and current by looking at MLS CO measurements (2004-present, as low as ~300 hPa). CO at the tropical tropopause has considerable seasonal variation due to seasonal biomass burning influence. MLS shows CO above 100 ppb in the TTL... much higher than what is used in your study – see Huang et al. [ACP, 2016]. This assumed TTL CO value impacts your results. This section needs to be revised based on recent measurements of TTL CO.

p. 16, lines 28-29. Should be 'longer transit times' and 'mixed with young tropospheric air with shorter transit times...'

p. 17, line 17. Try 'Figure 10b shows there is...' 0.3% is a very small change. What is the uncertainty on this number? Is 0.3% statistically different from zero?

p. 18, line 2. Try '...the observed CO increase, indicating increased mixing with air...'

p. 18, line 7. This is really the lowermost not the lower stratosphere you are talking

about.

p. 18, lines 11-12. Try ‘To further investigate the relationship of young versus aged air, we calculate the accumulated fraction. . .’

p. 18. The paragraph beginning on line 15 should be part of the previous one.

p. 21, line 1-3. You could make a good point here that mean age is an incomplete or oversimplified way to characterize air. The age spectrum shows there changes in the contributions from older and younger age masses that determine the change in mean age.

p. 21, lines 5-10. You can cite Rinsland 1999 and Rosenfield 1994 that mesospheric CO just doesn’t go this low into the stratosphere, and then you are done with this issue. The CH4+CL sources of CO has already been labeled insignificant. There is no need to talk about these insignificant sources again. This means that all of Section 6 (Discussion) before about p. 23 can be deleted.

p. 23, 10. Here’s a suggestion for Fig. 14. Since you say that the SH results should not be compared to the NH, then don’t show them. Figure 14 would be more readable if you just showed the regions of interest. Try making a 4-panel figure (Jan and March), 0-90N or 30-90N only.

p. 25, line 5. What are the significant figures here? Is 0.29 really different from 0.27, or are they both 0.3?

p. 25, line 18. The conclusions should be about the lowermost, not lower stratosphere as all the results are below 380K.

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