

The authors thank the reviewers for their thorough, detailed, and insightful reviews. They have helped to improve the paper.

Review #1:

OVERVIEW: The quality of the previous manuscript was hampered by the absence of any significant validation of the method. This has now been at least partially rectified by the validation paper, which has shown that the qualitative features of ANCISTRUS results are at least somewhat reliable. With that in mind, I believe that this paper can be published without additional validation work. Before publication, I recommend that minor revisions be undertaken for clarification of the paper. In two places (“major” comments L027-029 and L201), some changes in content are warranted.

Reply: We thank the reviewer for this encouraging evaluation.

Action: See in the detailed list of comments.

I want to provide one important comment to the author with regards to the validation of their work. The ANCISTRUS recovery tests performed in the validation paper are, in my view, a partial validation of the method. Comparing reference and result effective velocities only shows the effectiveness of the method in reproducing its own results. This does give credibility to the claim that the ANCISTRUS tool does not produce spurious structures, which is the most critical requirement of an inverse method. However, the quantitative claims in the paper are difficult to verify because no differences between the reference and result fields were shown. See my comment under “Non-Revision Comments” for L133-134 for more thoughts on that. I am inclined to believe the text in your validation paper on those quantitative claims, but as I cannot verify these results myself, I will not be able to remove my doubt.

Reply: Figs 8-10 in the validation paper ARE difference plots. Be-

yond this, for reasons of traceability, all quantitative results of the tests presented in the validation paper have been published in the KITopen repository.

Action: We have now inserted a reference to the data file.

Furthermore, the absence of a comparison between effective velocities and “standard” quantities remains as a source of doubt on the interpretation of your results. By standard quantities, I mean the various residual circulation quantities. A comparison of effective velocities derived from model tracer fields with the model residual circulation velocities which produced those tracer fields is, in my view, a necessary task. Without this, the relationship between effective velocities and residual velocities will remain unclear, and any comparison of the two from different sources will be clouded by the absence of this validation. For example, your results show a mesospheric overturning circulation and BDC deep branch that are not clearly separated, but what if mixing is playing a role in the effective velocities at this location? That all being said, I think it would be excessive to request a comparison of effective and residual velocities from model results be performed now, before publication. Nor should a more explicit quantitative investigation be requested. However, I think these absences weaken what would otherwise be an exceptionally strong contribution to middle atmospheric science.

Reply: We have studies to disentangle transport and mixing processes on our agenda, but this is a full project in its own right and exceeds by far what we can be done within the framework of this study.

Action: None to this point.

MAJOR:

L027-L029: “the true picture of middle-atmospheric circulation is more detailed and too complicated to be characterized by a scalar intensity of the circulation.” This state-

ment suggests that scalar measures cannot characterize the stratospheric circulation at all. In my view, scalar measures are the very first or most basic characterization of the circulation. I do, however, agree with the notion that scalar measures cannot capture the details of the circulation (such as the activity of different pathways), and that progress towards understanding the stratospheric circulation will require understanding these details. I suggest changing this statement to focus on the insufficiency of scalar measures to characterize the details (nuances, pathways, etc., I think a lot of terminology would fit here) of the circulation, or on the insufficiency of scalar measures to provide a complete description of the circulation.

Reply: Agreed.

Action: Changed to “the true picture of middle-atmospheric circulation is more detailed and too complicated to be FULLY characterized by a scalar intensity of the circulation.”

L039-L040: A comma or hyphen is necessary between “overturing circulation” and “which brings” and between “ozone chemistry” and “has been”. This is necessary to provide a clear boundary on the aside about NO_x rich air and stratospheric ozone chemistry.

Reply: Agreed.

Action: Corrected as suggested.

L047-L050: The meaning of this sentence (“While the...”) is not clear to me. Are you suggesting that the comparison of modelled trace gas fields with observed fields is used to estimate the stratospheric circulation? I can’t think of any true, published attempts at that. Is that what you mean? It seems like this sentence needs rewriting. Also, in relation to age of air methods you could cite the work of Fritsch et al. (Fritsch, F., Garny, H., Engel, A., Bnisch, H., and Eichinger, R.: Sensitivity of age of air trends to the deriva-

tion method for non-linear increasing inert SF₆, *Atmos. Chem. Phys.*, 20, 87098725, <https://doi.org/10.5194/acp-20-8709-2020>, 2020), who demonstrate some difficulties of using age of air methods (in particular the AirCore-derived results of Engel et al.).

Reply: The “While the...” sentence does not presume that stratospheric circulation is inferred from such kind of comparison. Instead, this sentence is meant as a justification why some kind of inverse method is needed.

Action: The “While the...” sentence has been rewritten: “The direct comparison of modelled trace gas fields with measured ones is very unspecific with respect to causes of discrepancies, because it reveals only the consequences of any deficiency in the model but provides no direct clue how the discrepancies came about.” The suggested reference has been included. Also, references covering the mesospheric sink issue have been included.

L050-L052: This sentence (“Our results...”) seems to state the same information as the following sentence. Furthermore, the mention of “our results” comes before any mention of the work of this study, which is an a typical form. I do not see any point to this sentence, nor to the previous sentence. I suppose the goal of this paragraph was to clearly establish the need for your work (and there is certainly need) but I think this has not been effectively communicated.

Reply: Agreed. We have reorganized these paragraphs in a way that the former “While the...” paragraph describes only the issues with the existing methods, without mentioning our method and results. These are now mentioned only in the following paragraph. With this, we have removed the redundancy.

Action: We have deleted this sentence and added instead “Beyond this, age- of-air based methods integrate over the time an air parcel spent in the stratosphere and thus provide information on the middle atmospheric circulation only at quite limited temporal and

spatial resolution.”

L201: You write here about air mass transport. I agree with the basic principle (weaker velocities at lower levels may easily transport more air mass than stronger velocities at higher levels). I have some concern, however, that the effective velocities may not correspond directly to air mass transport. In my view, noting the possibility of a discrepancy in this regard is important.

Reply: Agreed.

Action: We have replaced ‘velocities’ with ‘effective velocities’. Further, we have reworded the second part of the statement such that it does no longer claim that transport, driven by real velocities, is actually the driving mechanism. “While its EFFECTIVE velocities and vertical extension are smaller, due to the larger air density at these lower altitudes smaller velocities still can transport considerable airmass to higher latitudes.”

L216-217: You write here about the stratopause location. Would it be possible to include estimates (even approximate) of the stratopause and tropopause locations on your figures? I’m not sure what data source would be most appropriate for that information. Even a long-term monthly-resolved climatology would be very helpful for the reader to interpret the results. In my view, this would be very helpful, but not necessary.

Reply: We are reluctant to do this, because average tropopause (or stratopause) altitudes along with average transport paths can be extremely misleading. Such figures can easily let a tropospheric process look like a stratospheric one or vice versa.

Action: None.

Figure 1: I think you should consider using streamfunctions to visualize the velocity field. The figures as they are do somewhat show the qualitative information, particularly

for stronger velocities, but the quantitative information is somewhat difficult to interpret. In my view, streamfunctions would be a more effective and familiar quantity for the visualization of this qualities of the effective circulation.

Reply: Since, after the long time of the reviewing process, key scientists are no longer available and meanwhile work on other projects, we are not in a position to apply major changes to the visualization software.

Action: We have made the original data available via KITopen to allow the readers to visualize the data in their preferred way. A link to the original data (with doi) is provided.

Figure 1 caption: You mention “non-converged inversions” here, but I do not see that mentioned anywhere else in the text. In my view, that should be addressed in the main text somewhere.

Reply: Agreed.

Action: At the end of the first paragraph of the “Results” section we have added: “The years which went into the mean fields are indicated. Missing years are chiefly attributed to MIPAS data gaps. Only in a few cases (November- December 2003, March-April 2007, December-January 2009, March-April 2009, and June-July 2011) the inversion did not converge or another technical problem was encountered.”

Figure 5: I like these figures, but I think you only quantitatively reference variability in comparison with local velocities. Due to that, you should consider showing variability relative to the local monthly-mean velocity. In my view, this isn’t necessary, but it has the potential to assist the reader in interpretation greatly.

Reply: We have tried this, but there are many places where the effective velocities are more or less zero. In a relative or percentage representation these areas dominate the picture with close to infinite

relative variabilities, and the variabilities in the physically interesting regions are no longer resolved by the colour scale. Thus we have decided against the relative representation. We see the intention to make small but significant velocities in the lower stratosphere better visible, but actually this type of plot is dominated by areas where nothing is going on.

Action: None.

L496-499: The region you describe here sounds a lot like the startospheric surf zones, but that's not mentioned in the text anywhere. You might consider making a mention of that here, if you also agree that the variability in velocities that you've found in the region could be related to surf zone activity.

Reply: This is a good point.

Action: We have added: "This region coincides with that where planetary-wave breaking is observed, which was first observed by McIntyre and Palmer (1984) and to which they named the stratospheric 'surf zone'."

L525: Mentioning funding is not really appropriate for a peer-reviewed publication. This should be removed and some other introduction to the sentence should take its place.

Reply: Although we have enjoyed writing this, we do agree.

Action: Changed to: "More ambitious researchers may even plan an ANCISTRUS model in other than ..."

MINOR: L055: Probably you should say "effective circulation vectors" right away, as opposed to leaving the information about the "effective" nature of your results for later.

L083: "than with the age-based method" suggests that there is only one age-based method, which I think is not

the case, so this should be “than with age- based methods”.

Reply: Agreed.

Action: Changed as suggested.

L086-087: Here you write the name of the method, but that should probably be written the first time the method is mentioned, which is earlier in this section.

Reply: Agreed.

Action: We now introduce the name of the method in the first sentence of this section.

L089: “future tracer gas” would be more precise. Or “subsequent”.

Reply: agreed.

Action: ‘later’ replaced by ‘subsequent’.

L117: Can you estimate these inaccuracies?

Reply: This is hardly possible because for the underlying model calculations no uncertainty estimates are available either.

Action: None

L118: You should either provide some quantification of this “minor relevance” or at least provide some citation for that information.

Reply: We concede that this statement was too strong; we replace it with a weaker one.

Action: The statement has been replaced by “Inaccuracies in the latter estimates are deemed tolerable since the related loss reaction is only one of three relevant stratospheric loss reactions (Brasseur

and Solomon, 2005).”

L124: Do you mean SF6 sinks? It’s not clear to me. That should be clarified.

Reply: The Stiller et al. reference where this problem is discussed refers to SF₆ but the statement why these sinks are not important in our case is valid for all gases.

Action: Inserted ”Since FOR ALL SPECIES UNDER CONSIDERATION values at the upper boundary are prescribed using...”

L179: About the words “allows to better resolve”, because resolution is something you discuss as an advantage of the method, “resolve” seems to suggest that there is some difference in the method here. I suggest replacing this with the phrase “allow easier interpretation of”.

Reply: We do agree that the term “resolve”, that is used as a technical term elsewhere in the paper should not be used in a common language sense here. However, we doubt that “interpretation” is optimal, because this term goes beyond the mere perception.

Action: We have replaced “resolve” with “discern”

L181: Saying “inter-annual averages” or “climatological averages” might be helpful for the reader.

Reply: Agreed but in this context we prefer “multi-annual” over “inter-annual”.

Action: added “” ... are built from MULTI-ANNUAL averages, ...

L190-191: Was this uncertainty quantified?

Reply: This uncertainty is routinely quantified but not reported in this context, because for the interpretation of multi-annual averages requires the total uncertainty due to measurement errors and natural variability to be considered.

Action: None

L208: About “signal of subsidence”: do you mean in Figure 6? If so, that should be specified (“signal of subsidence in the multi-annual variability”). If the velocity figures are indicated instead, I think this should be expanded upon, as there are certainly some cases where subsidence seems to occur, in particular november-december of Figure 4.

Reply: Here we refer again to Figure 1. We do not mean that these subsidence effects always average out perfectly but that they can cancel out.

Action: We have inserted another reference to Fig. 1 after “...no clear signal of subsidence.” Further, we have modified the next sentence: “which OFTEN causes subsidence effects to be averaged out when latitudinal averages are considered”

L217: New paragraph at “Most parts”. **Figure 1 caption:** You mention missing species are indicated in the headers, but I don’t see any species indicated.

Reply: Yes, indeed. This text seems to refer to older versions of the plots.

Action: The obsolete sentence has been deleted.

L298: “there” is not very precise. I think “present” would be more precise.

Reply: Agreed.

Action: Changed as suggested.

L343: The word “it” could refer to multiple entities. It would be better to state this explicitly.

Reply: Agreed.

Action: “it” replace by “this branch”.

L416-417: “**Figures 7-8 (middle right panels)**” or “**the right hand middle**”

Reply: Agreed

Action: “middle right panels” now in parentheses.

L444: “**large inter-annual variability is expected based on current theory**” would be more precise

Reply: Agreed.

Action: Changed as suggested.

L444-L446: About “The stability...”, I don’t see how this sentence helps. As far as I can tell, the flow of the paragraph would be better without this sentence and the meaning of the paragraph would not change. The next sentence is much more to-the-point anyway.

Reply: Agreed.

Action: Sentence deleted.

L461: “From our results” is better, as you just mentioned MIPAS “data”.

Reply: Agreed.

Action: Changed as suggested.

L494: I don’t understand what “this” is referring to, or what this part of the sentence (everything after “km,” means).

Reply: The respective subclose is indeed confusing rather than

clarifying.

Action: The subclause “as this is also ... can reach” has been deleted.

L510: “common” is confusing here because it seems to leave the possibility that you use an uncommon a priori distribution to nudge your method.

Reply: Agreed. The prior we use is zero throughout. Thus, all signal we see is from the data. We have reworded this to remove the detected ambiguity.

Action: The sentence has been rewritten: “This behaviour cannot be attributed to the use of any a priori velocities that would push the results towards the expected circulation patterns. On the contrary, our a priori effective velocities are zero throughout, which guarantees that all structures seen in the results are produced by the measured trace gas contributions.”

L511: After “patterns.” might be a good place to add a sentence briefly describing the iterative nature of the method. “An initial velocity distribution was used to begin the iterative inversion calculation, but the choice of this initial field does not have significant effects on the resulting fields” or something like that.

Reply: Agreed.

Action: Added: “The zero a priori field is also used as initial guess of the iterative inversion, but its only effect on the results is a certain smoothing of the retrieved structures (von Clarmann and Grabowski, 2021). Another sign of the stability of our method is that the transitions between the circulation... patterns of subsequent months”

L515: I think what you mean by “features” is “novel results”. Of course there are very many more features, but these are certainly the most interesting ones of your re-

sults. They are very interesting, by the way.

Reply: Agreed. We are happy about the appreciation of these results.

Action: Changed as suggested.

L520: “The particular figure quoted” is somewhat strange. I recommend removing the citation in the previous sentence and replacing this text with “For example, the schematic of Bnisch et al 2011 (their Figure 1)” or something similar.

Reply: Agreed.

Action: Changed as suggested.

Typos etc. L006: “THE stratospheric circulation is found to be”

L018: “and is called THE ’Brewer-Dobson circulation’ ”

L027: “the true picture of THE middle-atmospheric”

L090: Comma after “coefficients”

L098: ’field’ not ’fields’

L102: ’is started’, not ’ist’

L102: ’final’ not ’finally resulting’

L104: “Since inferred velocities, due to the correlation of velocities and atmospheric composition, are not the zonally-averaged velocities but include eddy transport effects, we call the inferred velocities’effective velocities’.”

L109: Comma after H₂O.

L110: Comma after “band”. These commas, and the ones I mentioned earlier, are called “Oxford commas” if you want to look that up. It’s a practice used to avoid confusion in lists.

L112: ‘photolysis’

L112: “and” instead of “, as well as”

L115: Probably you want to say something like “equilibrium assumption”,but I know this as the “steady-state assumption”.

L119: Comma after CO.

Figure 1 caption: “, the months” should be “, and the months”

L459: “pole-to-pole”

L467: “THE NH atmospheric circulation”

L490: “transport pattern”

L501: “broadly reproduces well” doesn’t make sense, just say “broadly reproduces”

L502: “but” instead of “however”

Reply: We thank the reviewer for their careful reading of our manuscript.

Action: All corrections suggested so far have been implemented.

L506: remove “of air sampling instruments” as it’s not necessary

Reply: Due to a typo (“of” instead of “or”) the meaning of our statement was distorted. With this typo-correction in place, we think it does make sense to mention the air sampling instruments.

Action: Typo corrected.

L507: “the sense” not “a sense”

Reply: Agreed.

Action: Corrected.

L518: Comma after e.g.

Reply: This correction has become obsolete after rewriting as described above.

Action: See above.

L522: “future steps” should read “future steps for this work”

L523: “analysis”

L523: remove “the” in front of “interannual”

Reply: Agreed.

Action: These three corrections have been implemented as suggested.

NON-REVISION COMMENTS (I.E. NO CHANGE SUGGESTED):

L133-134: I found it difficult to verify this claim in the validation paper. The validation paper does show the two (reference and result, if you will) velocity fields, but does not display the differences between them as far as I can tell. Because the claims of the present paper do not depend on quantitative information, there is no need to establish this point further. However, I think you should be aware of this in future work. What would help is a simple depiction of the reference velocities, the result velocities, and the differences between them, all next to each other. That would make interpretation rather easy for readers. Again, I do not think this is necessary for this paper, but please consider this for future work.

Reply: The differences are shown in the lower panels of the relevant figures of the validation paper.

Action: None

L525: In my view, the distinction of transport and mixing is absolutely the most important future step for this work. It is still not clear to me what aspects of your results are due to the inclusion of mixing, and this brings me to view the results with some uncertainty. It would also be very, very cool to have estimates of mixing in the resolution that your results have.

Reply: We agree that this is interesting, and we have it on our agenda. However, our primary intention has been a refinement of the age-of-air approach. Indeed our results can be interpreted as

inverse incremental age differences. And the age-of-air concept does not disentangle transport and mixing either.

Action: None for this paper.

Review #2:

This paper uses MIPAS satellite observations of 9 long-lived trace gases, with various lifetimes, in an inversion model to calculate the effective mean vertical and meridional transport velocities over the 2002-2012 period. The inversion model was validated in a separate publication. This method provides interesting results that show two dimensional mapping (latitude-height) of the seasonality of the middle atmospheric circulation and mixing. The standard deviations of vertical and meridional transport velocities, also mapped, identify where transport is most variable. These results are important because they are observationally-based and they provide a unique spatially and temporally resolved quantitative analysis of transport in the stratosphere and mesosphere the entire middle atmosphere. As important and interesting as these results are, the paper needs a number of revisions before it should be published. Below you will find comments organized by the topic areas in need of revision. Kudos to the authors for the Supplemental figures with an alternative color scheme! That's a very considerate touch and please do not hide this information in a footnote; state this in the main text.

Reply: We thank the reviewer for the encouraging evaluation of the paper.

Action: The hint at the alternative versions of the figures has been moved to the main text as suggested.

Topic areas requiring Revision

Climatology. The authors note in a footnote that 10 years is not a climatologically relevant period. I strongly agree,

so please do not call it one; there is an alternative. MIPAS has measured the atmosphere for 1 decade, so I recommend framing this paper as a BDC analysis of the period 2002-2012. That's factually what this is, but by calling it that you can set up the idea that your analysis provides a basis for comparison in future studies. (Such a future study with Aura MLS data is even called out in the conclusions.) This will be helpful for examining the question of whether the middle atmospheric transport is changing. You are probably aware that this is of enormous interest to many, including chemistry climate modelers who are predicting an acceleration of the stratospheric circulation this century (e.g., Polvani et al. 2019, JGR), while observations show a different and asymmetric response (Strahan et al., 2020 GRL). This paper can be an important part of the answers we need. My recommendation is that you remove climatology everywhere it occurs and rewrite as a 10-yr mean or analysis. Because climatology appears in the title, this too needs change, perhaps: **The Middle Atmospheric Meridional Circulation for 2002-2012 derived from MIPAS observations**

Reply: Agreed.

Action:

- Title changed as suggested, and short title changed in the same spirit.
- “Monthly climatologies of” changed to “Multi-annual monthly mean”
- “From these we calculate a climatology of the circulation in terms of multi-annual monthly means” changed to “From these we calculate multi-annual monthly mean circulations ...”. The now obsolete footnote has been deleted.
- “and our scheme to calculate climatologies from the monthly circulation patterns” replaced by “our scheme to calculate multi-

annual monthly mean circulation fields from the individual monthly circulation fields”

- “Our derived climatologies of middle atmospheric circulation” changed to “Our derived multi-annual monthly mean circulation fields are discussed”.
- “The resulting circulation fields are analyzed in terms of first and second moment statistics to provide a climatology of the middle atmospheric circulation.” replaced by “The resulting multi-annual monthly mean circulation fields are analyzed in terms of first and second moment statistics.”.
- “The Climatology of middle atmospheric meridional circulation” replaced with “The multi-annual monthly mean middle atmospheric meridional circulation”.
- “to form the 12-monthly climatology” changed to “to form the 12-monthly data set”
- “appears weaker in these climatologies” changed to “appears weaker in these multi-annual monthly means”
- “new climatology of middle atmospheric circulation fields” changed to “data-set of multi-annual monthly mean middle atmospheric circulation fields”
- “The climatologies...” changed to “These circulation fields...”
- “...in the climatology.” changed to “in the multi-annual monthly mean.”
- “from this new climatology” changed to “from this new data set”
- “seen in these climatology fields” changed to “seen in these multi-annual monthly averages”

Attributing all variability to SSWs - wheres the discussion of the QBO? There is no mention of the Quasi Biennial Oscillation (QBO) anywhere in this manuscript, yet the QBO is the largest driver of stratospheric variability after the annual cycle. For example, around line 205, NH winter polar variability is attributed (without proof) to SSWs. Sure,

SSWs certainly cause NH high latitude variability in winter, but any discussion of the cause also requires a discussion of the QBO. The QBO modulates SSW occurrences. The QBO exists in the stratosphere and mesosphere, and affects both tropical and extratropical latitudes. Baldwin et al. (2001, Rev. Geophys.) provides a great overview, with discussions of the QBOs impact on the meridional circulation and chemical constituents. The solution is either to talk about the QBOs influence on variability, or to keep Section 3.1 completely descriptive no attribution of features without an analysis or suitable citation. This applies to all of Section 3.

Reply: We partly agree. We agree that the QBO should be mentioned as a driver of variability. However, the analysis of multi-annual monthly means, where the annual cycle is resolved, while periodic or quasi-periodic variations of longer time-scales are not resolved, is a less than optimal framework for the analysis of QBO effects. We thus defer this to a study which will be based in the full time-series rather than multi-annual monthly means. Beyond this, many of our statements related to the large variability of effective velocities in polar regions refer to altitudes above 50 km, and the knowledge of the causal chain between stratospheric tropical winds and mesospheric dynamics still seems to have some gaps. We are not even sure if the direction of the causal arrow is known with certainty. While in the revised version of the manuscript we consider the QBO as a driver of variability, we take care not to over-exaggerate this issue. Relevant parts of the manuscript are:

- Abstract: We agree that both SSWs and QBO should be mentioned.
- Intro, par. 4: We think in this context it is correct to limit the discussion to SSWs.
- Section 3.1.1 (January-February): The old text referred to the maximum of variability above 50 km. We do agree that the secondary maximum at about 30 km deserves to be discussed, too.
- Section 3.1.12 (December-January): Agreed to mention the QBO here.

- Section 3.2.3 (Variable Phenomena): We agree to mention the QBO as another important driver of variability.
- Section 4. Discussion: Agreed to mention the QBO here.

Action:

- Abstract: “Sudden stratospheric warmings cause increased year-to-year variability of the vertical component of the circulation.” replaced by “Sudden stratospheric warmings and the quasi-biennial oscillation cause a pronounced year-to-year variability of the meridional circulation.”
- Intro, par. 4: No action.
- Section 3.1.1 (January-February): The related part has been rewritten: “Above 50 km at Northern polar latitudes there is some subsidence. Associated year-to-year variability in vertical effective velocities is large, reflecting the irregular appearance of sudden stratospheric warmings (Fig. 6, upper left panel). Their irregular occurrence and the related impact on subsidence is discussed, e.g., in Funke et al. (2014). Large variability over the North pole at stratospheric altitudes does not come unexpected, since Haenel et al. (2015, see their Fig. 9) found in their age-of-air time series analysis largest amplitudes of the signal of the quasi-biennial oscillation (QBO) at polar latitudes. Strahan 2020 et al. (and references therein) highlight the importance of the QBO for stratospheric circulation. Baldwin et al. (2001, 2021) also discuss the possible interaction between the QBO and sudden stratospheric warmings and mention mesospheric QBO effects.”
- Section 3.1.12 (December-January): Inserted: “As discussed in Section 3.1.1, the QBO is another important driver of the interannual variability of circulation.
- Section 3.2.3 (Variable Phenomena): Inserted: “[between 25 and 30 km,] which is further enhanced by an interaction between the QBO and vortex dynamics (e.g., Strahan et al., 2015).”
- Section 4. Discussion: Inserted at the end of the 1st paragraph: “The QBO is another driver of stratospheric variability, and

Haenel et al. (2015) found that the the contribution of the QBO to the explanation of age-of-air time series is largest in the polar stratosphere.”

Similarly, at lines 357 and 381 a feature is attributed to the Asian monsoon circulation. Please give proof or cite an appropriate paper.

Reply: We have seen the Monsoon effect in time-resolved longitudinally resolved MIPAS data. We now make reference to the relevant work.

Action: Inserted: “A clear monsoon signal is visible in MIPAS data resolved in time and longitude (See, e.g., Vogel et al., 2019), and is obviously strong enough to survive zonal averaging.”

At line 384 (and somewhere earlier in the paper), the equinoctial mesospheric pole to pole transport toward the winter pole is mentioned. This is a is well known feature and should be referenced somewhere.

Reply: We do not understand this. Which pole is, at equinox, the winter pole? This statement looks somewhat like an oxymoron. We take the reviewer to be speaking about the solstice pole-to-pole circulation, that is indeed well known, but this is not what we are discussing here.

Action: None.

In the Conclusions, lines 512-514 incorrectly attribute variability to SSWs. They are not the only phenomenon driving large interannual variability (IAV) in the winter hemisphere - the QBO does too. In fact, the SH shows large IAV yet has no SSWs. See Strahan et al (2015, GRL) for an example of observed large IAV in the SH in the 25-30 km range and an explanation for the QBOs effect on trace gases in Ploeger & Birner (2016, GRL). Recently large amplitude extratropical variability with a 5-7 yr period was identified in observations that is likely driven by the QBO (Strahan et al 2020, GRL).

Reply: We find larger variability in the northern polar winter atmosphere, and we explicitly attribute this NH variability to the SSWs. In addition we now mention the QBO as a driver of variability. Besides the Strahan et al (2015) reference we add also the reference to Haenel (2015) et al.

Action: We have added: “The QBO is another driver of stratospheric variability (see, e.g., Strahan et al., 2015,2020), and Haenel et al. (2015) found that the contribution of the QBO to the explanation of age-of-air time series is largest in the polar stratosphere.”

Improvements to Figures. Consider that the data for each month for altitudes below 30 km are actually included in 4 different figure panels! Currently each figure type requires 2 pages of 6-panel figures. Reduce redundancy by using 6 bi-monthly averages (e.g., Jan-Feb, Mar-Apr, etc.) and then the annual cycle for a given variable, such as effective meridional velocity, can easily be viewed and understood from a single figure. This cuts the number of figures by 2.

Reply: The increase of the number of figures was in reply to the first review of reviewer #1. We do not see a lot of redundancy. Bi-monthly averaging would remove important information. Similarly, a lot of information would be lost if only annual cycles of certain selected variables were presented. We would no longer see the interesting processes like the subsidence of the deep branch of the BDC over the winter, or the connection between the deep branch and the overturning circulation. It is the spatial and temporal resolution which makes our results special, and we are reluctant to hide this information by averaging or picking out single locations for time series. If we pick out one latitude/altitude bin and show the seasonal variation, we totally miss the relation to neighbouring bins, i.e. the phase shift that occurs by the subsidence of the deep BDC branch.

Action: None.

For figures 5 and 6, the standard deviations would provide more information if each were divided by its mean value.

Currently, because means and std devs are so much smaller at lower altitudes, these figures tell us very little about the lower and middle stratosphere. Normalizing the std devs by the mean (i.e., to show fractional or percentage deviations) fixes this and allows the reader to easily identify enhancements in variability at all altitudes.

Reply: We have tried this, but these figures were not useful. Huge percentage errors where velocities are almost zero masked all useful information.

Action: None.

I'd like to suggest a new figure that sums up how the transport processes vary spatially and seasonally. Identify the month where each point of the velocity field (or its std dev) maximizes. Then make a contour plot that shows how the timing of these maxima varies pole to pole, 6-68 km. This might make a really interesting 4-panel figure (vel meridional, vel vertical, and each of their std devs). At a glance one could grasp the timing and location of where and when transports processes are the greatest. (Conversely you could try this for the minimum of the variable.) In addition, this would also show the relationship between vertical and horizontal transport processes (easier to see because all the panels would be in the same figure).

Reply: Maybe this would work as an additional figure but cannot replace the existing figures. The velocities are so different over altitude that the lower part of the stratosphere would be totally ignored (the largest velocities are always in the upper stratosphere and mesosphere). And they are not necessarily from the same transport branch. Look, for example, at the two very first panels: Fig. 1, J/F and F/M: Here the maximum meridional velocity would jump from 20N/45km to 50S/60km. All the other structures in the circulation would be lost. Such a presentation might be appropriate for a dedicated study on a specific process, but not for this paper where we present an overview over the multi-annual monthly mean results.

Action: None.

The atmosphere has 4 seasons but Section 3.1 has 12 sections. This section feels very long. Conceptually this discussion ought to be about seasonal behavior, i.e., what happens near solstice or equinox, and how the transitions between them occur. Currently this section contains a blow-by-blow description of every figure panel. Please organize these descriptions around seasons and transitions, as this is how the conclusions are described.

Reply: The first draft of this paper was indeed organized as suggested by the reviewer but we have given up this way to present the data. The processes we see cannot unambiguously be assigned to seasons. Each altitude regime seems to have its own seasons, and it is the smooth transition of the global picture from one month to the next that makes our results interesting. Also the processes seen cannot be categorized. Sometimes there is a smooth transition between the symmetric stratospheric circulation cells and the pole-to-pole circulation. That is to say, many of the processes seen cannot be unambiguously categorized. This had led to excessive cross-references in the first draft. Thus we have decided against reorganizing the current manuscript.

Action: None.