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Interactive comment on "An observation-based climatology of middle atmospheric meridional circulation" *by* Thomas von Clarmann et al.

Anonymous Referee #1

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- - - - - - - - - General Comments - - - - - - - - -

von Clarmann et al. 2019 present results from an inverse method which uses observed (MIPAS) zonal-mean tracer fields to calculate residual circulation fields which are resolved in altitude, latitude, and time. This work expands on the work of von Clarmann and Grabowski 2016 (hereafter CG16) by providing time-resolved circulation fields, and continues the line of investigation of a number of other studies which have sought to constrain the strength of the residual circulation. However, the present work seeks to provide a substantial expansion in this direction by quantifying the circulation strength in terms of two-dimensional, time-resolved velocities. Only one previous work, to my knowledge, has quantified velocities at all - that being Fu, Hu, and Yang 2007 GRL - but this was only for a single profile of upwelling, while other work has provided some

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sense of two-dimensional motion but without velocities, such as the work of Stiller et al. 2012.

The results show several inconsistencies with current theory. For example: The mesopheric overturning circulation is considerably higher (at least 10 km, which seems very unexpected) when southward-bound as opposed to northward-bound; the tropical pipe shows quite a bit of meridional movement rather than isolated upwelling. If reliable, such results would be of substantial and immediate interest for a large section of the middle atmospheric research community.

However, there are considerable issues with the validity of the results, and I do not think the work should be published until they are addressed. I outline them in the following three points:

1. The inverse model robustness (specifically in terms of sensitivity to input fields) has not been explored.

My impression from reading CG16 is that the inverse method requires multiple tracers but that the limit on the minimum number of tracers needed is rather soft (i.e. it is not strictly necessary to have X or more tracers). I suppose that it is possible to use a subset of the nine tracers applied, and thereby estimate the robustness of the method with respect to input data. In my opinion, having even a simple estimate is necessary. Having read this paper, I am left without an idea of how the results depend on the input tracer fields. Even if the method is mathematically sound, there may be biases in the results which extend from errors in tracer measurement or the calculation of chemical sources and sinks, and until this possibility is addressed the results remain in a somewhat skeptical position.

In particular, I would propose something like a jackknife test, calculating the fields with only eight tracers, excluding one tracer for each calculation, each tracer in turn, and seeing how strongly the velocity fields vary. It may not need to be a recalculation of the entire approx. decade-long climatology, but I think a test like this (or something more advanced) in multiple seasons is necessary to establish the validity of the method.

2. The inverse model accuracy has not been quantified well.

In CG16, an accuracy test was performed where dynamical quantities were prescribed in simple distributions, which showed that the method had some inaccuracy in the center of the domain and much more inaccuracy on the borders. I think a test with dynamical and chemical fields that are more similar to observations (i.e. less homogenous and, simply-said, messy) is necessary to ascertain the uncertainty of the results. I would suggest using CCM model data to produced inversted circulation fields and compare them with the actual, directly-calculated model fields. To be clear, the results of such a test could not be used to pin a quantity directly onto the method's results (since, for example, the inverse method includes some effects of mixing, although knowing how the "effective" velocities could differ from the actual velocities would be valuable). However, this would provide some level of confidence in the results where, at the moment, the only indication extends from an example which simply does not resemble the observed atmosphere. As a final note on this, it would be best if the test could also examine the difference between CCM and inverted velocities, and not the residual between tracer distributions (as was done in CG16). The velocities are what matter, after all.

As an example of possible data, the ESCIMO (Joeckel et al. 2016 GMD) simulations have a variety of chemical species included (all the chemicals used in this study are in the model output) and calculations of the residual circulation strength have also been performed. I am not involved in that work, but I would guess that the members of that project would be interested in sharing the data.

3. The inverse model result uncertainty has not been quantified.

This is a rather small point, and not as important as the previous two. In CG16, quantifications of uncertainties of wind and diffusion coefficient fields were shown (in Figure 5) for fields computed using MIPAS data. Those uncertainties seem rather small, but it seems imprudent to exclude an estimation of uncertainties when a method for estimating them is possible. I suggest including a description or depiction of those uncertainties, if the method still applies for the present work.

In my view, the first two points are necessary before these results should be published. Without a clear indication of the model validity, the novel results shown in this work seem as likely to be artifacts of the inverse method (or the calculation of chemical balances) as they are to correspond to reality.

Furthermore, I find that the figures in the manuscript are difficult to interpret, and should be changed before publication. I have made more specific comments on this topic below. In general, the figures provide a qualitative idea of the circulation, in that I can examine the figures and know where the circulation is the strongest and which way it is headed at any one time and know how that strongest location changes with time. That does provide a general characterization of the circulation, but knowing how the circulation strength changes with time in each location is of equal value to knowing where the criculation is strongest. The changing color scales in the first 12 panels makes that assessment difficult. The figures showing standard devations need to be explained clearly, because they are so unusual, but do not seem to be properly explained anywhere. I think the figures should be remade showing contours or heatmaps, or some combination of both methods, so that readers can assess the strength of the vertical and meridional velocities and the variability of them. Maybe including stream lines would maintain the ease in understanding the flow direction. I understand that this likely means a doubling of the number of figures (assuming there is not a clever way to combine the meridional and vertical information), but I think that it is warranted for the clarity of interpretation.

Furthermore, I have the following two points which I think warrant explanation, but are not addressed in the text:

Why do boundaries seem to be included in results, when they were problematic following CG16? Are they somehow excluded?

How were the sources and sinks of each species calculated?

To conclude, I would like to stress that results from this method should be published in the future if the reliability of the method can be addressed. They would be of very substantial interest, and therefore I wholeheartedly recommend resubmission.

- - - - - - - - Specific Comments - - - - - - - -

Line 14: Neither Brewer nor Dobson suggest upper and lower branches to the BDC, nor the mesospheric overtunring circulation. I think it would be best to cite somebody here who talks about that. Maybe Butchart's 2014 review paper would be good, to provide a general reference.

Line 20: The new sentence in this line seems to dismiss the value of having a single estimate of the upwelling mass flux / intensity. Perhaps it's just the use of the words "merely" and "far" (more/too). I'm not sure if anyone has ever suggested that a single quantity could sufficiently characterize the circulation, but certainly the upwelling mass flux has provided a lot of value as a broad estimate of the circulation. I suggest simplifying the sentence - "These studies suggest that the ... is too complicated and detailed to be fully characterized by a scalar intensity." - or something similar.

Line 56: I don't understand what is meant by point b. Stiller et al. also uses MIPAS data. What does the present method do differently? Since the chemistry of SF6 is not considered, any chemistry that is actually happening would create biases in those regions where it occurs (but I am no expert on this, and cannot say if those regions are included here). As a secondary point, there is no over-aging involved in this method because ages are not calculated. What would you expect in the case of your inversion method, if you had this bias? I would assume the result would be a faster lower-strat to upper-middle atmosphere circulation.

Line 78: Where did the data for OH, O1(D), and Cl come from? As I understand it, those JPL publications only contain reaction rate and cross-section information. Did you obtain that data from somewhere else (does MIPAS have those species?) or did you model those in some other way?

Line 86: How necessary is the stabilization of the inversion, that mixing coefficients were assumed to be zero? Can you compute mixing coefficients for even a single pair of months, or perhaps for a pair of boreal summer and a pair of boreal winter months? Otherwise, it’s difficult to say how the effective velocities compare to advective velocities.

Line 88: What does the word "efficient" in "efficient 2D circulation" mean? Do you mean effective? If not, the meaning should be clarified.

Line 89: I have never seen the term "Fickian mixing" before. Having done some searching, I think what you are referring to is more commonly called diffusion. If that’s the case, I would use that term. Otherwise, the meaning should be explained.

Section 2.3: Why do you average every pair of months? I would guess that is due to interannual variability of the phase (and I use that word very loosely here, perhaps it would be better to say timing) of the circulation. Whatever the case is, it should be stated.

Figures 1,2,3, and 4: I find these figures very difficult to interpret. First, the changing color scale does not seem necessary, as the maximum values do not vary strongly between the figures - most of them are around 11/12 - although it would create an issue for the December-January figure. At the moment, however, it is difficult to assess how the magnitude of the circulation changes at each point, month-to-month, except in the most starkly contrasting cases (. Second, it is difficult (if not for most cases pratically impossible) to obtain even a rough magnitude of the vector components because the color scale shows a norm of the vectors. I suggest using contours or heatmaps of the separate vector components instead. Third - and this has nothing to do with interpretation - you show the boundary velocities of these results although it is clear from CG16 that the vectors at the boundaries are difficult for the inverse model.

Figures 5 and 6: These figures are unusual and require some considerable effort to comprehend. I assume the width versus height of the bubbles shows the stardard deviation in the meridional and vertical directions, and that the colors show the standard devation of the norm, but no information is given about that. I would suggest simply relacing these with contours and/or heat maps.

All figures: I suggest using a perceptually uniform colorscale, which makes viewing much easier for those who do not have a standard perception of color.

Line 133: I am not sure that I can agree that the vertical motion over this range creates a transport barrier. I would rather say it suggests one, at the most. But it could also be interpreted as a latitude (or a section of a latitude) where the circulation splits. In that case, there's not really a barrier to horizontal transport, but only relatively little forcing towards horizontal transport / a stronger forcing towards vertical transport. I'm not sure what the case truly is, but I don't think it's certain that this represents a barrier.

Line 138: "would likely look" - I think it's highly likely, even, but no definitive statement can be made until the analysis is done. On that topic, I think that analysis would be very interesting for future work.

Line 142: You might consider showing the tropopause and stratopause level sin your figures. I think that would be very helpful, and could alleviate a lot of confusion.

Line 150: It would be more useful to replace the values shown in Figures 5 and 6 with the values discussed in this sentence. That would be a more direct indication for the reader of where the circulation is consistent. Otherwise, they need to compare these values themselves, which is rather tedious if a thorough comparison is desired.

Line 189: Leaving aside the term "latitudinal barrier" (again, I am not sure how to distinguish between a barrier to latitudinal transport or a region of weak latitudinal transport), I do not see that agree that with the term "contribute to the formation".

Line 234: I've mentioned this already, but I think this case shows clearly why the variability should be depicted in another way. It's too difficult to compare the stanard deviations here to the circulation strength, for the most part. But your argument does seem plausible.

Line 237: It seems like you wanted to specify a figure in Ploeger et al. 2017. I suppose you mean Figure 5? You should specify the figure.

Line 323: What is independent about the ANCISTRUS results?

Line 323: "resulting fields are stable" – The statement regarding field stability should be more nuanced. Some parts of the fields do seem to be stable, sure, but this statement suggests that the fields are generally/always consistent.

Line 324: "increases confidence in the robustness of the analysis method" - I do not agree. If the method is robust, then a rather stable circulation field over the approx. decade of measurements in one region would suggest that the circulation field is a typical phenomenon for that region, at least for that decade. However, the robustness of the inverese method cannot be assessed by seeing consistency in its results without a second point of reference (preferably, other observation-based circulation estimates, which - to my knowledge - do not exist).

Line 330: It's not clear to me if a clear separation between these two pathways would be expected or not. Could you provide any context on that, in terms of earlier literature?

Line 333: "consistent with the assumption" - Has anyone previously suggested this idea, or are you saying that your results would only be consistent with a northward pole-to-pole circulation if it was above the domain of MIPAS? If nobody has suggested this, then this statement should be written differently to clarify the novelty of the result.

Line 337: To my understanding, this intrepretation of the tropical pipe would be novel. I only wish to note that here.

Line 341: This could be consistent with some earlier results. See Butchart 2014 (The

Brewer-Dobson Circulation, Rev. Geophys.) Figure 6 and discussion of that figure. If mean downwelling during winters where the polar vortex is not disturbed is the same between both hemispheres, then one would expect stronger climatological descent in the southern hemisphere because the vortex is disturbed less often there.

Line 343: To my knowledge, this result is not expected.

Line 347: I've mentioned it already, but I do not think the term "barriers" is justified in this context.

Line 348: Same to point 2.

Line 364: In the broad stroke, I agree with this statement. However, the absence of a southward mesospheric overturning circulation means that this statement cannot be written in the absolute. Furthermore, the results do not characterize these patterns in an expected fashion (tropical pipe, for example). This statement should be rewritten to reflect those points.

- - - - - - - - Technical Comments - - - - - - - -

Abstract, line 1: HCFC-22.

Line 14: The citation of Brewer and Dobson 1949 is incorrect. That's a single-author publication, just from Brewer.

Line 14: I think it's better to write abbreviations in a separate set of parentheses, just so it's clear that the abbreviation isn't some part of a citation.

Line 16: The last part of this sentence seems to suggest that only aerosols affect major chemistry-climate processes. It would be more clear with "as aerosols, all of which affect major chemistry".

Line 19: This sentence is a bit of a run-on. It would be better to write one sentence for Engel's balloon studies and another for the satellite studies.

Line 22: "Offline model simulations ... analysis data have also confirmed ...", or add a comma after "Also".

Line 28: It looks like you meant to write "has been investigated" or something similar. At the moment, the sentence doesn't make sense.

Line 30: If Funke et al. (from all those years) showed this, then I would write "has been" not "could be".

Line 35: It would be more precise to simply say that the picture of the middle atmospheric circulation is better resolved in space and time, cutting out the "more detailed" part.

Line 38: The ending, "over the years", isn't necessary here.

Line 46: I would say not just monthly but "monthly-mean". Furthermore, the sentence suggests that this is the only way to infer the circulation, so you should specify "is inferred in this work".

Line 49: "The resulting circulation fields..."

Line 62: I don't understand what "related software" refers to. That's the inversion method, right?

Line 79: "source reactions were also considered"

Line 83: "the neglect of sinks above that altitude"

Line 91: See comment on line 14 about abbrevations.

Line 103: "From MIPAS, measurements"

Line 104: You explained the data gaps in the last section.

Line 116: "up to 30 km"

Line 123: move "also" form "also the standard..." to "are also shown". Furthermore, it

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should be clear to all readers that standard deviations are a measure of variability, so the "which are a measure of their variability" is not necessary.

Line 127: You can just say northern hemisphere or winter hemisphere.

Line 146: "has its maximum ... and at 30*S"

Line 155: You are clearly comparing this month-pair with the previous, but this sentence should make that explicit.

Line 167: "will give rise"

Line 170: The abbreviation SH was already used before this point. The notification of the abbrevation should be shifted to the first usage of "southern hemisphere". Ditto for NH.

Line 379: "their figure" - This part of the sentence addresses a particular figure, but the earlier part speaks generally of schematics. I suggest sticking to one approach or the other.

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