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Interactive comment on "Direct inversion of circulation from tracer measurements – Part 2: Sensitivity studies and model recovery tests" *by* Thomas von Clarmann and Udo Grabowski

Anonymous Referee #2

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Review of "Direct inversion of circulation from tracer measurements – Part 2: Sensitivity studies and model recovery tests" by T. von Clarmann and U. Grabowski

This manuscript is meant to demonstrate the robustness of the "Analysis of the Circulation of the Stratosphere Using Spectroscopic Measurements" (ANCISTRUS) algorithm described in Part 1 several years ago (von Clarmann and Grabowski, ACP, 2016; vCB16). ANCISTRUS is a continuity equation inversion methodology that relies on monthly differences in trace gas distributions to derive "effective velocities" that describe trace gas transport. I very much appreciate the concept and it would be a great boon to the community if it were demonstrated to be successful in providing information

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Discussion paper

about the stratospheric transport circulation. The paper is mostly well-written and easy to follow and the model recovery tests and sensitivity tests do indeed demonstrate that the model is relatively robust in terms of being able to reproduce its own results. However, the lead author has referred to this manuscript as a "validation" of the method in the interactive discussion of a second paper under review at ACP (von Clarmann et al., ACP, 2019; vC19), and I find that it falls far short of that description. The model recovery tests, in particular, demonstrate only that the model will retrieve more or less the same effective velocities from more or less the same tracer distribution but do not provide any assessment of whether those effective velocities have any physical meaning or are a unique solution to the continuity equation (these comments are explained in more detail below). If the ANCISTRUS method is to be used to study stratospheric transport in a meaningful way (and the authors indeed attempt use the method to provide a climatology of the meridional circulation in vC19), then those properties must be demonstrated. I therefore cannot recommend publication of this manuscript in ACP without major revisions that address these concerns.

Major technical comments:

Lines 32-33: It has been demonstrated several times (Neu and Plumb, 1999; Linz et al., 2016; Linz et al., 2017) that the age of air is not a good measure of the meridional circulation, but that the age difference between upwelling and downwelling regions is, in fact, equivalent to the diabatic circulation. This methodology does not require assumptions about the age spetra. Certainly if ANCISTRUS were able to successfully retrieve the BDC then it would have some advantages over the age difference, but to compare it to the use of age itself as a circulation diagnostic is somewhat disingenuous.

Lines 39-41: The fact that the interannual variability in the ANCISTRUS-derived circulation is small, particularly in the tropics (from having looked at the figure in vC19), is a red flag for me. We know that the QBO's secondary meridional circulation has a large influence on trace gases in the tropics and subtropics, and any tracer-derived circulation should pick up this variability. It is a very clear signal in trace gas anomalies.

Lines 55-59: I feel that the entire concept of the meridional circulation in this manuscript is highly over-simplified, and this is one example of such over-simplification. The stationarity condition can, in fact, define any number of circulation fields with different mixes of horizontal and vertical advection. In principal, these components might be separable with the right set of trace gases, but there is no evidence presented here that the suite of trace gases used is sufficiently orthogonal to separate horizontal and vertical advection unequivocally.

Lines 69-72: This is another example of over-simplification. The change in amplitude of the structures is also affected by mixing in the real atmosphere. More importantly, while the *simplest* (not necessarily best) explanation might be a southward velocity, another explanation would be a shift in the upwelling region (which brings high mixing ratios upward from the tropopause) by 5 degrees south. This would indeed appear as a change in effective southward velocity based on the tracer inversion, but that southward velocity is not a meaningful description of the meridional circulation. In fact, if anything, the effective velocities seem to represent *anomalies* in the meridional circulation rather than the circulation itself. The effective velocities are derived from the change in trace gas distributions from one month to the next, but that distribution for any given month already reflects the mean meridional circulation when using real stratospheric trace gases. The familiar shape of tracer isopleths, with an upward bulge in the tropics, strong gradients in the subtropics, relatively flat isopleths in midlatitudes, strong gradients at the vortex edge, and a downward bulge in the vortex are all a reflection of the balance between sinks and the mean meridional circulation and effects of mixing. When you look at the change in this trace gas distribution from one month to the next, it reflects at best the month-to-month change in the circulation, but not the overall circulation itself. All of this highlights the absolute need to understand how well the ANCISTRUS method retrieves an actual circulation field rather than an idealized one (as in Part 1 of the manuscript) or one that it has already generated itself (as in the model recovery tests in this manuscript).

Lines 76-77: I am not sure I agree with the statement that the circulation fields roughly match our expectations of the meridional circulation. For one thing, it is extremely difficult to tell whether this is the case or not from the vector plots. The streamfunction should be plotted instead, with the vectors superimposed over the streamfunction contours if desired. From the plots in the manuscript, the only thing that fairly clearly matches expectations is the circulation in the mesosphere, though the seasonal differences in the height of the circulation are odd (but might more accurately be called interannual differences since two different years are used). I certainly do not clearly see the "branches of the BDC" (line 80, and this phrase should be referenced and defined) – in fact it is hard to see any coherent tropical upwelling region at all. Again, plotting the streamfunction would make the circulation characteristics much clearer.

Lines 77-79: This is another example of an important difference between the effective circulation based on tracers and the BDC. This upward velocity is not meaningful as part of the meridional circulation, which is still downward but weaker than prior to the vortex displacement. Again, it may be more appropriate to view the effective velocity not as a proxy for the BDC, but as anomalies on the background BDC circulation. But this must be demonstrated using an actual circulation field.

Lines 87-94: The plots using annual mean tracer values are, in fact, the only ones that look like the prototypical middle atmospheric circulation to me. The authors seem to indicate that the lack of a pole-to-pole circulation is a deficiency, when, in fact there is no coherent pole-to-pole circulation in the annual mean (nor is there one during the equinoxes, from which the sink terms were used). I also see evidence of the "tropical pipe" ending at $\sim$25 km, where there is strong poleward advection, rather than "reaching up to the mesosphere". The "pipe" is not defined by upwelling, but rather by a lack of communication with the midlatitudes.

Lines 105-112: The authors assert that many tests of this nature were performed for vCG16, but the only ones described or shown used very simplified velocity and tracer fields. What is required is a model recovery test using a realistic meridional circulation

(with vertical and horizontal components and satisfying the continuity equation) and realistic trace gas distributions with both vertical and horizontal gradients. I am not convinced that ANCISTRUS can successfully retrieve a unique solution to the continuity equation that does not alias horizontal and vertical components of the circulation into one another.

Lines 120-130: I am unable to understand why one cannot take the 2-D Transformed Eulerian Mean circulation from a CCM and use it to advect an initial MIPAS trace gas distribution and then retrieve the circulation using ANCISTRUS to see if you recover anything like the model circulation. This would be similar to the tests in vCG16, but using realistic velocity and tracer fields. Some sort of test of this type must be performed before ANCISTRUS can be found to inform our knowledge of the middle atmosphere meridional circulation.

Lines 131-136: As far as I can see, all this demonstrates is that ANCISTRUS is capable of retrieving the same answer when you invert the same field. The effective velocity fields were generated based on the change in trace gases between two months. There is no reason that applying those velocity fields to the initial trace gas distribution should result in a different change in the trace gases than what was used in the initial retrieval, and so for the same distribution, ANCISTRUS essentially gets the same answer twice. This test does not in any way validate that the effective velocities derived are in any way related to actual transport velocities, nor does it demonstrate that the retrieved circulation is a unique solution to the continuity equation that properly resolves both the vertical and horizontal components of the circulation.

Lines 137-139: Even with the reduced vertical scale plots, it is again very difficult to see and interpret these results from vectors. The streamfunction should be plotted, as well as difference plots between the initial and final streamfunction.

Lines 142-143: Again, I do not easily see the “stratospheric branches of the BDC”. Please plot streamfunctions and define what you mean be “branches” (I do understand

what is meant, but many readers may not).

Lines 147-148: I'm not sure I agree that the "the slow circulation patterns in the tropopause region and the lower stratosphere are well recovered". If plotted as percent differences, I think some very large discrepancies would emerge.

Lines 153-154: The right panels of Figure 5 are the only figures in the manuscript that seem to resemble the canonical stratospheric meridional circulation. They show rapid poleward transport by the shallow branch (below 15 km here) and strong tropical upwelling, poleward transport, and high latitude downwelling. No other plot shows a coherent upwelling region like this one does. Of course differences are to be expected given the seasonality of the circulation, but the upwelling branch moves back and forth across the year rather than disappearing.

Lines 157-158: While it is true that the second retrieval did not create significantly different patterns than the first, it has not been established that the patterns retrieved in the first place are not artificial given that the algorithm does not appear to have ever been tested with a realistic circulation pattern and realistic tracer distribution.

Figure 3: There are obviously large differences in the velocities at 60S, 60 km for Feb-Mar. Why don't these show up in the difference plots? There are also other examples where the difference plots do not seem to reflect the visual differences between the top and middle plots.

Lines 201-204: Why does withholding CFC11 give the opposite signal to CFC12 in the Arctic? If the sinks are properly accounted for, the effective transport for these two species should be similar.

Line 208-211: I do not understand what is meant by "compressed colour scale". Again, the streamfunction and percent differences might be more useful for seeing the stratospheric changes.

Lines 211-212: The water vapor "tape recorder" has been extensively used for deriving

vertical transport in the tropics, yet water seems to do nothing to inform the tropical upwelling. Can this be explained?

Lines 232-233: If this is meant to refer to circulation patterns and structures, then I have to say I strongly disagree that there is evidence that ANCISTRUS is fit for purpose. It does indeed generate a consistent set of patterns and structures from a given set of trace gas fields, but there is no evidence that these patterns and structures are physically meaningful in any way. Until this is demonstrated using a known, realistic circulation field with the MIPAS tracer measurements, I cannot recommend publication of this manuscript.

Minor comments:

Line 1: The wording “allows to infer” is not grammatically correct (it needs a subject). I suggest “provides an inference of”.

Line 2: The phrase “both given by” should be “given by both”

Line 4: Using “have shown” in the past tense makes it sound as if these tests were performed in another paper rather than here.

Abstract in general: The abstract does not provide sufficient context for this work or provide any indication of the meaningfulness of the results.

Lines 66-67: The phrase “does effectively not work” should be “effectively does not work”

Line 84: Should “or equatorward transport” be “of equatorward transport”?

Line 88: The reference should be made to “bottom panels” rather than “right panels”.

Line 140: I believe “September – October 2005” should be “March – April 2005”

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