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Interactive comment

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

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In this paper, the authors use measurements of a variety of trace gases from MIPAS to infer the stratospheric and mesospheric circulation. They calculate a climatology and determine that the deep branch of the Brewer Dobson circulation is connected to the mesospheric pole-to-pole circulation. They verify a number of known characteristics of the circulation, such as sudden stratospheric warmings increasing variability.

Using chemical tracers to infer the circulation is an excellent idea. Tracers are what we can measure from space, so to validate any model, we need to quantitatively relate the tracers to the dynamical output from climate models. The inverse methods used here are promising. Unfortunately, the approach from the authors is lacking in a number of

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Discussion paper

ways. 1) The validity of the method has not been established. 2) Uncertainties are not calculated and, perhaps most importantly, 3) The utility of the resulting product from the method is unclear.

The authors have not demonstrated that this inversion recovers velocity fields in a model. The closest is an idealized case in their 2016 paper and even in that case, idealized tracers are used instead of real tracers. In order to use the method on data, essentially an entire separate modeling study needs to be performed: a) using a CCM, with full knowledge of the tracer fields used here, the inversion needs to be performed and compared to the model velocity and stream function. If this is successful, then the next step is: b) with the same CCM, the sampling characteristics of MIPAS (coverage and averaging kernels) need to be applied to the tracer fields so that now the limitations due to sampling and retrieval characteristics are applied. This seems especially important for vertical and horizontal resolution. Then the inversion needs to be done and compared once again to the model velocity and stream function output. This test will illuminate what the method actually means.

The errors caused by the method with full tracer fields and then with the limited sampling can then be characterized for the model as well, hopefully beginning to address point 2) above. This would also work towards addressing point 3) above. This “transport circulation” that the authors obtain is not obviously relevant. Without being able to meaningfully compare values to model output or reanalysis products, this quantity does not seem to be of interest, and so the authors claims of being able to assess quantitatively the variability of the circulation fall flat. In fact, this is the reason that age of air is such a useful tracerâĂŤit has been quantitatively related to the circulation of the stratosphere in a way that allows direct comparison of data (including the MIPAS data) to models (Linz et al. 2017).

I would strongly encourage the authors to perform such a study and then to rethink their results for this work in the context of the information provided by their validation study. That would be an excellent paper that I would be truly excited to see.

Beyond this overall assessment, I have included a few additional comments below: 24: What about the lifting of the circulation? (e.g. Oberlander-Hayn 2016)

Introduction: What is the gap that this research is filling? The introduction reviews the literature but does not identify any motivation for the present study.

2.1 This discussion of age of air is surprising. What is the so-called “traditional observation-based characterization of the circulation”? The authors do not provide a citation. Some recent work that uses age of air observations to characterize the circulation is Linz et al. 2017. Recent work by Ray et al 2016 combines the TLP with chemistry to examine transport, and the improvement this offers over that method should be addressed. Ray et al. 2010 is also a relevant comparison here. Furthermore, it is not clear how the method can reveal causes of “discrepancies” between these age and chemical tracer based methods since there are no error estimates.

2.1.2 There is no discussion of degrees of freedom. How much independent information is gained by including additional tracers? Specifically, how are sinks included?

3.1: Plots are very hard to understand. Streamfunctions would be much better.

3.1.1 How are horizontal transport barriers identified? Why, physically, are they associated with this vertical motion? Is this purely a result of continuity and the fact that this is a 2-D calculation? If so, this should not be referred to as a barrier.

3.1.6 How precisely do you identify that this circulation is associated with the monsoon? Are there particular tracers (e.g. water vapor) that mark this as a monsoon signal? Or is it just about the timing and the fact that it’s in the Northern Hemisphere? “Our results show overall agreement with the one shown by Ploeger et al. (2017).” What “one”? What is meant by “overall agreement”?

3.2.1 337: What is meant by this? How does this reconcile with the well-established water vapor tape recorder results?

4 368-374: This seems to be saying that this study is a good validation of the method.

That may be, but it’s not the stated goal, and more stringent validation is needed especially so as to be able to actually interpret the resulting “effective velocities”. 384: What applications would use these effective velocities?

Citations: Linz et al. 2017 10.1038/NGEO3013 Oberlander-Hayn et al. 2016 10.1002/2015GL067545 Linz et al. 2016 10.1175/JAS-D-16-0125.1 Ray et al. 2016 10.1002/2015JD024447 Ray et al. 2010 10.1029/2010JD014206.

Final note: I apologize to the authors for the tardiness of this review. I really wanted to like this paper, and I hope that they follow up with the modeling study to put these results in context.

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