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Interactive comment on “Methane as a diagnostic tracer of changes in the net circulation of the middle atmosphere” by E. E. Remsberg

Anonymous Referee #2

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General

The paper by Ellis Remsberg analyzes the HALOE methane measurements, in combination with the HALOE H₂O and HCl measurements, to infer changes in the stratospheric residual circulation over the period of the HALOE measurements (1991-2005). Methane is a useful tracer to address this question since its decrease in the stratosphere by oxidation depends on the time spent in the stratosphere. The connection between the fraction of methane oxidised to the residual circulation, however, is more complex than portrayed in this paper. This concern has been addressed by reviewer 1; I believe that a careful re-wording of the manuscript (and title) about what exactly one can expect to learn from methane trends would suffice to address this point. Below, I will provide some specific comments, but my overall concern with the paper is that

C9398

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[Discussion Paper](#)



it is not obvious why we are not shown the full latitude/pressure structure of trends. I understand that there are binning issues with the low sampling rate of HALOE, but the full latitude/pressure structure would be helpful for the reader to judge for him/herself whether there are robust patterns or not (which then implies that just presenting a few selected latitudes/pressure bins has a high arbitrariness). In addition to the binning issue (further discussed below), I think the paper would benefit if the discussion of the results in the context of previously published papers (for example the Nedoluha et al. (1998) study) were in a separate (sub-) section. In this context, I wonder whether the present study should not also refer to the Rosenlof (2002) paper that had a very similar objective as this study.

Specific comments:

(P=page, L=line)

P24187/L10: "tracer-like molecule" - should it not be more like "molecule that contains information about the circulation"?

P24188/L28ff: Formulation unclear, what does "net vertical transport of CH₄ was suppressed" mean?

P24189/L11ff: Width of bins. With the relatively sparse sampling by HALOE, wide bins are required, but gradients within these bins are not small, and uneven latitudinal sampling that varies from month to month might be a problem. Has the author looked into this? A simple analysis would be to calculate for each month the average latitude of all profiles that fall in that month in this bin (with the assumption is that longitude gradients are much smaller - but is this true?). This information could be integrated into the MLR.

P24189/L14ff: It would be nice if it were stated in the text which terms are included, and how the regressors are defined.

The subsequent text gives some information, but I must say I did not quite understand

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it. Do you say you fit the data against harmonic regressors with constant periods of 6, 12, and 28 months? Plus a 21-month period for the subtropics? (I don't understand the latter, please explain better.)

P24190/L17: "net seasonal heating" - I interpret this to mean the radiative effect of changes in solar radiation and upwelling longwave, correct? Perhaps state somewhere (before this section) your conventions for referring to the "radiative forcing" and the "dynamical forcing" (since the dynamical forcing results also in a seasonally evolving variation in net radiative heating, just saying "net seasonal heating" is ambiguous).

P24190/L21: This is exactly the problem I mention above ("width of bins"). What do you do with this? If these situations are not statistically evenly distributed over time, they will induce erroneous trends.

P24191/L10ff: I don't understand your argument - mixing per-se does not have to be non-periodic, does it? In other words, even if mixing plays a role, the variations could be still periodic if the mixing is periodic (which we would assume to be the case).

The question then is whether this indicates months with "anomalous" mixing, or simply months where the latitudinal distribution of the HALOE sampling is biased?

P24191/L23: Linear trends of tropospheric methane (assuming this is equal to CH₄entry): The trend is flattening in the late 1990's and early 2000's. You look at data with an age of up to a few years, so the tropospheric methane should be taken from an earlier period ... I assume that this is why you take the 1990-2003 period when your stratospheric timeseries is for 1991-2005? If so, please state this explicitly. How large is the uncertainty from not knowing the exact period for the troposphere that corresponds to the period in the stratosphere at different heights and latitudes?

P24192/L26: "The conceptual idea of ... for an acceleration of the hemispheric BDC from a species like ..." seems to miss "to detect"?

P24193/L1: My concern here is that the 3%/decade is not accurate enough due to

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[Interactive
Comment](#)

uncertainty in the age spectrum ... in the figure, the 3% line is shown without any error bars. Could you please add error bars that arise from the statistical uncertainty for a specific period, and the uncertainty that arises because we do not exactly know which tropospheric period to consider. (I am also happy if you can show that these uncertainties are small - but then please state this.)

P24193/L18: "Checking for structure ... important ..." True, but also subjective. Would be good if more about the origin of these structures could be said (i.e. again testing whether there are sampling issues - which then could be taken into account in the MLR analysis).

P24193/L1: I don't like the use of the word "correct" here - it suggests that the BDC has accelerated (as a fact); but we don't really know, and the whole point of the paper is to see whether methane can tell us something about this, no? So, instead the formulation may be "and the sign of the trends is consistent with expectation for an acceleration of the BDC".

P24195/L10: Yes, that's the problem ... and why I think that more should be done trying to eliminate all "outliers" that can be tracked to sampling issues.

P24195/L21ff: Here, and also earlier, you refer to pictures on the HALOE website. I've tried to look at these pictures, but am not sure whether I generate the same pictures as you do, and whether I see what you see ... is there any possibility to include this information in this paper (rather than referring to a website which may be obsolete in a few years, and where the reader is not sure he/she looks at the same plot as you did), without having an excessive number of figures?

P24196/L6ff: I am not sure I understand your reasoning - wouldn't the changes in "stratospheric wintertime warming activity" also be part of the trend? (i.e. the BDC is forced by these events.)

P24196/L25: The 2:1 ratio of trends is only true if H₂O entry is constant. Whether

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H2Oentry shows trends is a matter of debate. Reference to the Randel et al. (2006) study is ok, but I don't see why you point out seasonal variations in that context - should seasonal variations not be picked up by the 6 and 12 month harmonics of the MLR? It is interesting that you don't see more signal from the large drop in H2Oentry in 2000; for example, based on Fueglistaler (2012), Figure 3, one would expect that the trend ratio over the period 1992-2005 should not be 2:1; it may be worth to look again into this in more detail (i.e. the departure from this 2:1 ratio may not be just noise, but highly significant).

P24197/L29ff: It is interesting that there is no trend in the SH: with the ozone hole one might have expected largest trends on the 15-year timescale of HALOE in the SH; while for the NH one might have expected more noise due to larger dynamical variability.

P24198/L8: I am not convinced that H2O trends are seen in the NH but not in the SH because of more vigorous meridional mixing. All locations in the stratosphere have the H2Oentry drop, the mixing only affects the age of air, and as such the timing when the H2Oentry signal is observed. Hence, the argument would have to be that due to the mixing the age of air is much younger in the NH, and that the trend would be seen also in the SH if the HALOE data were extended beyond 2005. However, I am not convinced that the age of air is that different, and could imagine that other problems (sampling?) may play a role.

P24198/L19ff: I'm not sure I understand the message here - surely there is interannual variability, it's not expected that the harmonics capture everything, is it?

Figure 12: Please state the units of the residual.

Figure 11 and Figure 12: The two figures contain information that should be (anti)correlated. It would be great if this information could be shown in a figure that combines the information from the 2 tracers.

References:

Fueglistaler, S., Stepwise changes in stratospheric water vapor?, J. Geophys. Res., 117, D13302, doi:10.1029/2012JD017582, 2012.

Rosenlof, K.H., Transport Changes Inferred from HALOE Water and Methane Measurements, J. Met. Soc. Jpn., 80, 4B, 831-848, 2002.

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