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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The latitudinal structure of recent changes in the boreal Brewer–Dobson circulation" *by* C. Shi et al.

Anonymous Referee #1

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General comment:

In this paper the authors challenge the analysis by Mahieu et al. (2014) of increased HCl column abundances since 2007 observed in Northern midlatitudes. Mahieu et al. conclude that a deceleration of the Brewer-Dobson Circulation in Northern midlatitudes is the cause of increased HCl amounts, since longer transport times allow for more photolysis of CFCs, while the authors of this paper claim that the same phenomenon can be explained by a speed-up of the Northern mid-latitudinal branch of the BDC. This speed-up is, according to the authors, the result of a "narrowing" of the mid-latitudinal downward branch of the BDC; at the same time the upward branch near the tropical tropopause experiences a slow-down.

At first: How the speed-up of the midlatitudinal downward branch results in an increase of HCl is not further explained. The authors write with respect to this: "Under the approximate conditions of no divergence and no horizontal HCl gradient, the strength of the local downwelling dominates the extratropical lower stratospheric HCl concentration by downward transport. Therefore, the increase in the mid-latitudes and decrease in the Arctic of HCl after 2006/07 can support the local speedup in mid-latitude and slowdown in the Arctic of the downwelling resulting from the narrowing equatorward of the downwelling branch of BDC." It is not clear to me what the authors really want to say with this statement. I might speculate that they wish to say that high HCl amounts from the higher atmosphere are transported downwards, but at the end I am left alone without any explanation.

At second: The paper fully ignores all observational evidence that a positive trend of age of air has been found for the Northern mid-latitudes during the last decade (roughly 2002 - 2012), and, from single balloon observations, even for the last 30 years. This observational evidence comes not only from direct observations of age tracers, but also models driven by re-analyses like ERA-I produce increasing age of air over these years. It would be necessary to demonstrate how increasing age of air comes along with decreasing transport times within an accelerated mid-latitudinal BDC branch - one possibility would be significantly enhanced aging by mixing. The authors do not comment at all on these aspects and make no attempt to discuss their findings in the context of these earlier observations.

At third: I am not able to follow the authors (and this might be my fault) with their numerical analysis that finally allows them to arrive at their conclusions, and at statements like the following: "The local acceleration of the mid-latitude downwelling results from the branch narrowing equatorward which is related to weak planetary wave activity and cold polar vortex enhancement." I am not aware of a mid-latitudinal downward branch of the BDC that is separated from the downward branch in the polar regions, so that I do not know how this narrowing can be caused, nor have I found any demonstration

in the paper that the planetary wave activity was weak(er) or the cold polar vortex was enhanced (what exactly, was enhanced?). This part of the paper would need severe improvement. For more details see my specific comments below.

Besides these three main points I found that the paper is full of inaccurateness with respect to references, and full of unclear statements (like the example just given). In summary, I cannot recommend publication of this paper.

Specific comments:

Abstract, p24404, l6/7: "... decreasing extratropical middle-lower stratospheric HCl." I do not understand this statement. Weakening of the BDC would result in longer transport times and, thus, an increase of HCl due to longer exposure of CFCs to photolysis.

Abstract, p24404, l7-10: "However, the global ozone chemistry and related trace gas data records for the stratosphere data (GOZCARDS) show that the tropical lowermost stratospheric WV increased by 18%/decade during 2001–2011 and the boreal mid-latitude lower stratospheric HCl rose 25%/decade after 2006." Is this your own analysis, or do you refer to previous published material? In the latter case, a reference is required.

Abstract, p24404, l11/12:"... a speedup of the mid-latitude downwelling." As above, I do not understand this conclusion, since, to my understanding, a slow-down of the BDC would indeed result in an increase of HCl in the extratropics.

p24405, l5-8:"In addition, in December, January, February and March (DJFM), a large proportion of WV transport into stratosphere over the Tropical Western Pacific (TWP) is known as “stratospheric fountain” (Geller et al., 2002; Bannister et al., 2004; Bonazzola et al., 2004). Thus, the variability of the BDC in the boreal winter affects the annual tropical lower stratospheric WV (Dessler et al., 2014)." The BDC is understood as a zonally averaged phenomenon. In this sense it is not correct to assign longitudinally restricted processes like the transport over the Western Pacific to the BDC.

p24405, l19/20: "However, the contrary HCl trends after 2006 in the mid-latitudes and the Arctic in boreal middle stratosphere (Fig. 2d and f) cannot be explained simply by the slowdown of the downwelling argued by Mahieu et al. (2014)." This is a bad organisation of the paper. Figs. 2d and f are referred to in the introduction that already show result of the analysis here - the introduction is not the correct place for this. Beyond this, Figures need to be referred to in the order of their appearance.

p24405, l26 and throughout the paper: "air age" - the term commonly used is "age of air" (in full: stratospheric mean age of air, abbreviated: AoA)

p24406, l1/2: "Additionally, the HCl concentrations can be different in the air parcels with the same age but different transport pathways (Waugh et al., 2007)." This fact is accounted for in the concept of mean age of air by the age spectrum - indeed depends the mean age of air on the shape of the age spectrum since it is the first moment of the spectrum. Two air parcels with the same mean age but representing different transport pathways would have different age spectra that - only by chance - could have the same first momentum. Not impossible, but not very realistic. My impression is that the authors are not familiar with the concept of the age spectrum?

p24408, l14-16: "During 2001–2011, the temperature near the tropical tropopause (10S–10N) (Fig. 1b) rises with a trend of 2.21 K/decade based on the GOZCARDS-MERRA data." Is this consistent with other data sets, eg. SPARC temperature trend analysis? It seems to be a tremendous and unrealistically high amount!

p24408, l19-22: "In addition, the decline of BDC during 2001–2011 was confirmed by air age increases in the boreal main stratosphere from Lagrangian transport models and observations (Ploeger et al.,2015; Mahieu et al., 2014; Stiller et al., 2012)." The increase and decrease of AoA shows a rather complicated pattern in these publications; I do not agree with this general statement made here; this argument mixes observations of AoA trends in the Northern mid-stratosphere with trends of uplift velocities in the tropical lower stratosphere.

p24408, l24: "There is not only a shift of the BDC trend ..." What kind of shift? In what domain? There was no mentioning of any kind of shift before in the paper.

p24409, l2: "... the flow would be accelerated if the flow pipe narrows" Where has it been shown that the "flow pipe" narrows? What is meant with the "flow pipe"? The tropical pipe?

p22409, l6-27: This entire paragraph is a mystery to me. I cannot follow where these structural changes in the BDC are deduced from.

p22409, l13-15: "In other words, the downwelling branch narrowed equatorward and sank faster in the mid-latitudes (downward black thin hollow arrow in Fig. 1d)." I cannot follow at all. My understanding of the BDC is that BOTH in midlatitudes and polar latitudes downwelling is the predominant direction of the BDC; what do the authors want to say with their statement? That downwelling occurs only in the polar vortex regions?

p22409, l15/16: "The regression of EPFp from Fig. 1a with temperature of DJFM in 2001–2011 (Fig. 1e) can ..." What has been plotted over what, and which kind of regression analysis has been done? I cannot follow what has been done here.

p22410, l1-4: "The local acceleration of the mid-latitude downwelling results from the branch narrowing equator ward which is related to weak planetary wave activity and cold polar vortex enhancement." Again, I do not understand what is meant with "branch narrowing equatorward". Where has it been demonstrated that the planetary wave activity is weak, and where has the cold polar vortex enhancement been shown?

p22410, l15-16: "Fueglistaler (2012) analyzed tropical stratospheric WV from HALOE and also found the trend after 2000." Since HALOE terminated its operation in 2005, the trend derived by Fueglistaler is certainly for another time period than the trend in this paper?

p22410/11, section 3.4: I understand this section as follows: since HCl vmr increases

with altitude, a faster downward transport in mid-latitudes brings down higher HCl amounts from above, while in the Arctic, where the downward transport is not accelerated according to the authors, the decrease of HCl continues in line with the decrease of CFCs. This interpretation is purely speculative. The authors fully ignore in their argumentation that there is independent observational evidence of increasing age of air in the Northern mid-latitudes. This is in clear contradiction to their result of accelerated downward transport in Northern midlatitudes. Even without this contradictory observational evidence, the claims of the authors would need to be manifested by some thorough assessments on the amount of HCl increase due to pure transport versus HCl decrease due to air becoming younger and less photolyzed. Besides this, faster downward transport does not necessarily mean that air from higher up in the atmosphere (where HCl vmr is larger) is transported down.

p24411, l18/19: "The trends of boreal BDC tended to decrease and create latitudinal structural changes." Where has this been shown in this paper? Isn't this merely a summary of previous publications? By the way: do the authors really mean that the trend of the boreal BDC has changed, or the BDC itself?

p24411, l23: "...weaker planetary wave activity and the stronger polar vortex after 2000." Where exactly has this been shown in the paper?

p24411, l26/27: "The increasing HCl in the midlatitudes is caused by the local speedup of downwelling after 2006/07." This interpretation is in clear contradiction to the Mahieu et al. paper. The exact mechanism for this increase is not explained at any place in this paper. The reader might assume that the increased downward motion might bring higher HCl vmr from higher altitudes down to the middle stratosphere. However, the competing process of reduced photolysis during shorter transport times due to accelerated BDC has not been assessed. Without this assessment of competing processes the claim of the authors remain purely hypothetical.

p24412, last para of the paper: The last para consists of a few random citations about

possible future evolution of the BDC that does not help at all for the argumentation in this paper here.

Technical comments:

Fig. 1: Is there any reason to squeeze all these panels within one single figure?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 24403, 2015.