Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1153-RC2, 2020


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Asymmetry and pathways of inter-hemispheric transport in the upper troposphere and lower stratosphere" *by* Xiaolu Yan et al.

**Anonymous Referee #2**

Received and published: 30 December 2020

**Summary**: The authors present a model study of zonally resolved pathways of interhemispheric transport (IHT) for air originating at the surface and arriving in the NH/SH upper troposphere and lower stratosphere. The contribution of different source regions (NH, SH, tropics) is quantified using air mass fractions for inert trace gas pulses in either region. It is concluded that the Asian summer monsoon affects IHT from the NH to the SH by interacting with westerly ducts, driving an interhemispheric asymmetry in IHT.

**Major comments**:



1. **The motivations/justifications for the study remain somewhat elusive**, making the novelty of the study and of the results unclear at times. The introduction lists mechanisms that have been proposed to explain IHT; what is new in the current approach? Does the current approach confirm existing literature, or does it expand on it? The introduction does not make a particularly good case for the need to answer the questions listed at L79-80. In order to make the study accessible to more readers, it may be useful to answer these basic/naive questions:

   – Are processes linked to IHT expected to be different from processes linked to transport within a given hemisphere?

   – What is the magnitude of the bias introduced to the composition of e.g. the stratosphere by the assumption that NH and SH have the same boundary conditions?

   After the comments on non zonally averaged pathways in the introduction, it was unexpected to be presented with zonally averaged pictures in the first figure (figure which is associated with commentary about non zonally averaged pathways nonetheless). Some reorganization would be helpful. Side note: I would stay away from listing the Hadley circulation itself as a mechanism for IHT (L49)– rather, I would mention the migration of the ITCZ across the equator.

2. **Some aspects of the methods and how they are applied lack discussion**. Much needed details in the methods are glossed over. Setting tracer concentrations to zero in the boundary layer outside the region of origin (L103) necessarily means that IHT pathways that go through the boundary layer will not be visible in the AMFs. This is an issue for direct comparison with previous literature, and it must be discussed. Is it possible to leave the boundary layer unperturbed, or perhaps to use parcel trajectories to address this caveat?
   What are the decay rates used for the 40 inert tracers (L100)? Do the different transit times mentioned at L104 refer to different transit time *distributions* implied

by different decay rates?
The tropics and extratropics are often separated using 30$^o$N/S, but some discussion of the reasons why this threshold is used in this study is still needed. 30.0$^o$ is an arbitrary number after all. Some discussion about regions where 30$^o$N/S is more or less suitable to separate tropics from extratropics would also be welcome. How much would your results vary if using e.g. 25$^o$N/S? Would there be substantial benefits/caveats to defining the tropics using a dynamical, zonally resolved boundary?

3. **The interpretation of some results needs more discussion**: in the present model setup, there is virtually no NH to SH transport during Dec-May (figure 7 and associated text). Why is this? This result differs from existing literature (see e.g. Orbe et al. 2016) and needs to be further discussed, especially in light of the model setup having zero tracer concentration in the boundary layer outside the NH source region.
Given the maps in figure 9, the coupling between the ASM and westerly ducts must be altitude dependent with transport occurring either in the upper troposphere or in the lower stratosphere. This distinction is generally absent in the study and should be included.

4. In general, I would raise the question whether the proposed approach with AMFs allows to state mechanisms the way they are, e.g. L362-363 "[. . .] coupling with the ASM and causing strongest cross-equatorial transport". **Can we be sure that it is the coupling between the westerly ducts and the ASM that causes IHT, only using maps of AMFs and meteorological composites?** Perhaps more discussion would help clear this up.

**Minor edits/suggestions** (At random and non exhaustive for the time being. More comments can be provided on a revised manuscript):

– L17: "the ozone" → "ozone".



– L21-22: "Although most [. . .] by the BD circulation" is redundant with the previous sentence.

– L22-25: "significant contributions [. . .] Wu et al., 2018)" this sentence needs a verb.

– L32-33: "the anthropogenic" → "anthropogenic", "the natural" → "natural".

– L68-69: "showed that the mean AoA [. . .] is around 1.4 years." Do you mean that the mean difference in AoA between NH and SH near-surface is 1.4 years?

– L79: "preferential/favored" pick one.

– L83: "tropics" → "the tropics". "NH extratropics" → "the NH extratropics".

– L100: "120 inert pulse trace gas species" → "120 pulses of inert trace gas species".

– L123: "total sum" → "sum".

– L132: Figure 1 shows zonally averaged results, yet the Asian summer monsoon is discussed here. This is in line with my comment about the motivations for the study: if mechanisms have been proposed to explain this zonally average picture, what is new here?

– L171: same comment as L132.

– L173: the striking feature in Figure 3 did not strike me until L173!

– L344: "patches" → "latitude bands".

– L351: "rendering" → "making" or rephrase using "granting".

**Comments on figures**:

- Generally: all captions could be shortened significantly and made clearer (see examples below). Scaling and offset factors might advantageously be avoided or made much more obvious. Many axis labels are actually repeated in white font, which takes up space that could rather be used to make the figures more legible. See for instance Figure 1 and 3 where the "Latitude [deg]" label is repeated in panels (a) through (i).

- Figure 1: please use a color scheme that does not saturate as much. To save space and make the figure clearer: put letter labels inside the panels and do not repeat the y-axis labels on each panel. Use larger coordinate ticks on all axes. I initially did not realize that the color scheme was scaled/offset for each region, and I think this may be a source of confusion for the reader. Using separate color scheme may be necessary. Otherwise, show the scaling factor ($\times$0.2) and the offset factor (+94) in bold, much larger font. I suggest showing "$\times$1" for the NH. The caption could be shortened by removing information made available in the text or in the figure itself: "Average zonal mean AMF (1999-2017) originated from the NH, SH, and tropics (columns) for each season (rows). The white line is the WMO tropopause. The color scheme is for the NH; scaling and offset factors are provided for the SH and tropics. AMFs for each region add up to $\sim$1."

- Figure 2: add a colorbar and units for the streamfunction. Line up the black contours with the color shading for clarity. The title can be shortened to something like "Average residual mean mass streamfunction (1999-2017, color shading with a subset of values highlighted in black contours). The grey line is the WMO tropopause. White contours show potential temperature levels in kelvins. The black arrows illustrate the upwelling in the Hadley circulation and the shallow branch of the BDC."

- Figure 3: same comments as for Figure 1. For the caption I would suggest "Average (1999-2017) zonal mean AMF departure from the annual average. Note



the scaling factor for the SH (0.2). The absolute AMF contributions from Figure 1 are shown as black contours. Grey contours show the mean zonal winds. The black line is the WMO tropopause."

- Figure 8: highlighting the equator with a colored line would be useful.

- Figure 9: using less color shades would help read the wind speed map.

**Recommendation**
The manuscript presents an interesting approach to IHT and the mechanisms/couplings driving it. In light of my comments I suggest accepting the manuscript for publication after major revisions focused on clarifying and further discussing the methods and on improving the figures. Further improvements to the text clarity and concision can be included once major revisions are submitted.

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