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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial and Temporal Variability of Interhemispheric Transport Times" *by* Xiaokang Wu et al.

Anonymous Referee #1

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Review of "Spatial and Temporal Variability of Interhemispheric Transport Times"

In this paper, the authors investigate the transport of air from the northern hemisphere (NH) to the southern hemisphere (SH), including the seasonal and interannual variability of this transport, using a specified dynamics model. Three different tracers are utilized in order to highlight different aspects of the distribution of transit times without calculating the transit time distributions explicitly. The results–that the seasonal variability in the tracer transport is driven by the movement of the ITCZ and that the interannual variability is linked with ENSO–are unsurprising and consistent with Orbe et al. 2016 and Waugh et al. 2013. The authors have done several new things in this study, and the paper would be improved by highlighting the novel aspects (interannual variability, robust seasonal analysis, comparison with data) of the work. Generally,

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more discussion of mechanisms, why the results matter, and improved framing would take this paper from being a simple advance in the examination of inter-hemispheric transport to being a more interesting and useful contribution. Overall, this paper currently provides a clear explanation of transport timescales from NH midlatitudes and explains the difference in using tracers with different losses. It would be much stronger if these results were placed into chemical and dynamical context. Because of the extent of these changes, I have recommended the paper be accepted after major revisions, although depending on how much the authors have already thought about these concerns, it's possible the revisions could be very quick.

I have provided specific areas for improvements below, with both specific and general suggestions.

Introduction: The previous literature on interhemispheric transport is treated well. It is not clear from the discussion why neglecting the seasonal and interannual variability in inter-hemispheric transport matters, and why it is important to consider both shorter lived and longer lived trace gases. There are a lot of potential ways to do this–perhaps discuss with respect to the availability of OH and its seasonal distribution (e.g. Lelieveld et al. 2016)? Or look into the work of Prather and Holmes (2013) and see if their work on lifetimes provides a complementary approach or useful motivation. Or what about CO2 uptake in the SH? A stronger connection to the chemistry would go a long way for motivating the rest of the study.

In each section, explain what we expect to see. The ITCZ is known to drive interhemispheric transport and ENSO is known to change tropical variability in DJF, so the results are not surprising. Unfortunately the paper currently reads as a methodical discussion of plots. In my opinion, the story of this paper would be more compelling if it were presented as "this is what we expect to see", "let's check–this is what we see", and "these are interesting details". For the ENSO discussion, an explanation of why the weaker Walker circulation would lead to the pattern of age difference would be nice. The comparison with the wind anomalies in Wang and Fiedler (2006) in Fig. 2c definitely makes

Fig. 9a seem reasonable. The current discussion based on the case study comparison of one El Nino and one La Nina year is less convincing than a more general discussion would be. For example, if the variations in deep convection in the SPCZ region are the norm for La Nina years, a citation here would be helpful. For JJA interannual variability, it is not as obvious what mechanisms are at play. Perhaps the variability is related to changes in the monsoon and therefore the phasing of the MJO (the strong variability in age and the mean age contours are both apparently coincident with the Somali jet, and the authors discuss the ascent in this region). Since ENSO is the primary signal of interannual climate variability, I would also think that checking whether JJA age is correlated with the ONI would be worthwhile, just in case. I do not expect the authors to do any extensive calculations for these dynamical connectionsâĂŤrather, I ask for a discussion of the transport in the context of the tropical dynamics. I think a different, longer model run would be necessary to get at the details here, and that is beyond the scope of this paper.

I find the comparison to observations makes this paper more valuable; this should be emphasized in the introduction, and the discussion should be expanded (p. 7).

In the conclusions, make the point that by verifying these results are consistent with expectations, observations, and previous modeling studies that use TTDs, this study has demonstrated how useful these tracer diagnostics are for understanding transport. The clear explanation of the sensitivity of different tracers to different parts of the TTD and the exploration of the variability of transport from this paper is very valuable, even if much of this has been implied in Orbe et al. 2016, 2017. The point that is made in this conclusion is that by examining a bunch of CCMI models, the robustness of these relationships could be tested. I would contend that we fully expect the gradients of age to be tightly coupled to convection and that Orbe's work has shown as much–these relationships don't really need to be tested. However, differences in these relationships between models might have interesting implications for the impacts of dynamics and convective parameterization on model transport and chemistry and how they differ in

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the CCMI models.

Specific edits (content ones have a *, otherwise these are mostly grammar):

P1L2: “A”–> “an”

P1L7: comma after “loss”

P1L11: no comma after “gases”

P1L14: “aspect for” strange wording–aspect of? or just “is important for”?

P1L19: “intermispheric”–> “interhemispheric”

P2L20-1: Here we examine this issue and examine the seasonality . . .–> Here we examine the seasonality . . .

P3L3: no “mean”

P3L5: no “mean”

P3L9: insert “is” between and and subject

P2L24: comma would be better as semicolon

P2L27: comma after mixing

P2L29: number disagreement between “distribution” and “spectra”–I think you mean spectrum.

P4L1: with ˆa decay time T ˆthat is . . .

P4L6: “the above” is ambiguous–the above what?

*P4L16: Why do you use CAM SD? What advantages (and disadvantages) does using the nudged run have for this study?

P4L27: Comma after variability

P5L7: no comma after troposphere

P5L8: comma after consequence

*P5L35: Can you discuss the cause of the zonal differences? It looks to me like the monsoon pattern of southerly winds. Also, this discussion seems better suited to the next section on the seasonal variability.

P6L22: No comma after tropics

P6L23: "... ITCZ, see surface winds" awkward phrasing, comma splice

P8L6: maxima

*P8L25-...: Why is the analysis of one El Nino and one La Nina event preferable to examining composites of high/low ONI? It seems that with a composite you could be more clear about mechanisms.

P9L10: "above" is not necessary

P9L12: "during El Nino year" is either missing an article or year should be plural.

P10L20: need "of" after "deviation"

*P10L29: Using only one model does not seem to be a concern, since this is an SD run. The use of an SD run for transport is potentially problematic, however, and a brief discussion of how close the model is to conserving mass would help alleviate my concerns.

P11L4: no comma after gases

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