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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.

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This manuscript examines the interhemispheric transport from the surface to upper troposphere / lower stratosphere in the other hemisphere using simulations from the CLaMS model. The manuscript contains material that is of interest to ACP readers, and I think contains some new results that warrant publication. However, major revisions are required to the manuscript before it is suitable for publication. As described below, there needs to be (1) improved referencing and discussion of previous studies, (2) more precise discussion of transport in lower stratosphere versus that in upper troposphere, and (3) clearer statements on what is new (as opposed to confirming previous studies).

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MAJOR COMMENTS

(1) There needs be referencing and discussion of previous studies. This applies to both the description of the method and the results from your analysis.

(a) There are many studies before Ploeger and Birner (2016) that have used boundary impulse or air mass fraction calculations (e.g., Holzer et al. 2003, Haine et al. 2008, Li et al. 2012, Orbe et al 2013, 2016). I think it is OK to refer to Ploeger and Birner (2016) for details of implementation used, but it needs to be acknowledged that others had developed similar methods and even some had used to look at similar transport problems (e.g. Orbe et al 2015, 2016). You might need to also discuss difference in implementation (see point 4 blow).

(b) There are some previous relevant transport studies that are not referenced, e.g. Holzer 2009, Orbe et al 2015. Orbe et al (2015) is particularly relevant as it addresses the same issue, and direct comparisons can be made (eg. fig 3 in Orbe et al (2015) can compared with fig 1). The issue of direct comparisons also applies to some of the papers that are already referenced, e.g. compare fig 5 of Orbe et al. 2016 with fig 7 (more on this below). It is notable that the discussion section compares with previous studies, but only those by the authors of this manuscript.

(2) There needs to be more precise discussion of transport in lower stratosphere versus that in upper troposphere. In many cases I think statements on IHT apply for transport into southern lower stratosphere but it is not clear to me that they apply for IHT into the southern upper troposphere (or more generally southern troposphere). I think you need to separate into LS or UT, or maybe be clearer on the potential temperature surfaces that a certain result applies too. For example, do statement about magnitude of air from NH compared to SH hold for both the LS and UT?

I think this separation is particularly important as the majority of the Introduction (i.e. lines 26 to 75) discusses studies of inter-hemispheric transport within the troposphere (usually NH surface to SH surface), but most of the focus of this study is on trans-

port into lower stratosphere, and it is not clear how relevant the results are for inter hemisphere tropospheric transport. In other words, the Introduction discusses in detail previous studies of troposphere to troposphere IHT but the results from this study are not put in context of these previous studies.

(3) There needs to be clearer statements on what is new and what is confirming previous studies. The abstract contains several statements on the variation in the transport, but are these new results? Given the overlap with previous studies and limited mention or detailed comparison with these studies it is not clear which of these statements are new and which are just confirming previous studies. I think it is a bit of both, and this needs to be made clearer.

(4) One aspect that I think is new is the lack of IHT during northern winter. However, I am not sure if this not an artifact of the experiment design.

The results show virtually no transport to 10S in Dec and May (Fig 7a, h) (and according to text same for Jan-Apr). line 270-). This is very surprising, and not what is seen in other studies. I think there are many studies that show there is some IHT during NH winter. The most direct comparison is probably Orbe et al. (2016). The BIR calculations shown in fig 5 of Orbe et al. (2016) shows transport during NH winter that is similar magnitude to the summer. The summer transport in lower panel fig 5 of Orbe et al. (2016) actually looks very similar to fig 7d-e (and shows transport in monsoon and ducts), but the winter transport is very different in this manuscript. This is a clear example of a case where current results are not compared with previous studies by other authors.

Is this because a differences in the transport within your model that in previous studies or is it the method used? I think it may be the latter, as the setting boundary layer values = 0 outside the source regions means that near-surface transport south from NH source region is removed, i.e. if air is transported south near the surface before being lifted into free troposphere it will not be included in your IHT. Whether this is the

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case or not, there needs to be some more discussion of the lack of winter IHT and the reason for this (and inconsistencies with previous studies).

(5) The potential interplay between the ASM and westerly ducts is I think one of the potentially new results. However, I think some care is needed in discussing this. The upper tropospheric westerly ducts are in NH winter and there are typically UT easterlies throughout the tropics in summer (as fig 9a,c shows), and I think most of previous studies on ducts and transport have focused on the winter. You are not seeing this winter transport so I don't think it is fair to say your results are in agreement in this regard (line 359). Also, the existence of summer-spring westerly ducts appears to be altitude dependent (Fig 9) and so interplay might apply in LS but not UT. Also, your statements regarding interplay between the ASM and westerly ducts could be misread to be saying the summer ASM interplay with winter westerly ducts.

MINOR COMMENTS

Figs 1-4: The changing of the scaling used for NH, SH and tropics between these figures gets confusing. I think a reader could very easily compare between columns without seeing this scaling, and once they see this in one figure they may assume similar in next figures (At least that is what I did). It might be better to have separate color bars for each column. Figures will look the same but will I think be clearer from multiple bars that scale differs.

Fig 6 What are the contours in these plots? They differ between each panel. Shouldn't the climatology be the same in each column?

Line 269 "lager"

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