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Interactive comment on "Confinement of air in the Asian monsoon anticyclone and pathways of convective air to the stratosphere during summer season" *by* Bernard Legras and Silvia Bucci

Anonymous Referee #2

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This study utilizes a Lagrangian-based approach to quantify the influence of convection on transport to the upper-level monsoon anticyclone. The explicit use of observed clouds, in combination with new proposed measures of "convective influence", is a welcome contribution to the growing number of studies that have been oriented around better understanding the influence of convection on transport into the monsoon UTLS region. While various interesting results are presented I am concerned with the lack of investigation surrounding potential sensitivities of the authors' calculations (e.g. to trajectory length). The overall presentation is also a bit sloppy which renders it difficult to extract the main key messages from the study. Finally, the incorporation of an idealized model at the end is a great addition but better suited for development in a

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separate study that can afford the space. As such I recommend acceptance subject to major revisions that address the following concerns:

Major Comments:

1) There is an overall sloppiness in the layout and presentation. This is especially the case for the writing and overall construction of the various sections and, to a lesser extent, with the figures. In particular, in addition to various typos, the authors repeatedly use single one-sentence paragraphs that break the continuity of the flow and reflect an overall lack of attention to the overall structure of the manuscript. See lines 18-28 on page 6, lines 21-25 on page 7, lines 14-15 on page 9, lines 18-20 on page 10, lines 26-27 on page 20, etc. My concern with this is not just one of aesthetics – rather, I think it reflects a lack of overall coherence so that the paper reads more like a point-by-point description of an exercise that was implemented and less like a coherent story that ties together different results in a clear, consistent manner. The problem this creates is that it makes it difficult for the reader to extract the main messages of the text. Please revisit these sections and try to present more cogently.

2) While I find it admirable that the authors are presenting a range of new diagnostics for quantifying the convective impact on various transport measures I am concerned that little discussion is presented as to how these measures depend on, among others, the time over which the trajectories are evaluated (i.e. the backward trajectories are followed for two months since their initial launch). The transport measures inferred from trajectories are notoriously sensitive to how long they are followed and I am surprised that this is not discussed. Perhaps these tests have been performed in previous studies but no mention is made in the current text. A discussion along these lines (and including other potential sensitivities) should ideally be presented either in the Methods section or in the Conclusions in order to communicate to the reader which measures are more robust, compared to others.

On a related point, I am discouraged by the lack of details in some of the measures presented. In particular, the concept of the "age" is introduced rather haphazardly on line 23 on page 6 and the details at the beginning of Section 3.4 do little to place this definition of an age in the context of previous studies. This would not be an issue so much if the authors did not then proceed on page 9 to claim "Note that the commonly used metric of the mean age might hinder...". In particular, the mean age as defined here is based on a spectrum of ages that are only evaluated for 2 months of the integration. The mean age, therefore, as defined here ignores the influence of longer timescales that may substantially influence the tails of the age spectrum (e.g. Hall and Plumb (1994)) and, thereby, skew the mean to higher ages. Indeed, this is quite evident in the 400K "age spectra" shown in the black lines in Figure 6 where the pdfs of the ages have clearly not converged within the 60 days considered in the study. Numerous previous studies have shown that Eulerian measures of the mean age are the averages of very broad underlying age spectra (e.g. Waugh and Hall (2002)) and that not considering these longer timescales (which physically measure the contribution of older recirculating air parcels) can have a significant impact on the mean age. If the readers do not plan to address this explicitly in their analysis (which would require extending their trajectories) then they should at the very least be very clear that what they mean by the "mean age" is very different than the mean age as defined in previous studies.

3) The use of the cloud information from SAF-NWC is clearly a great contribution afforded by this study. I am concerned, however, about the potential dynamical inconsistencies that are introduced through use of observed cloud fields with large-scale meteorological variables derived from the reanalyses and, moreover, what these inconsistencies imply for transport. Given that the resolved vertical velocities used in the kinematic calculations will be largely dictated by the horizontal resolution of the underlying reanalysis model it is not obvious apriori that the observed clouds will be at all similar to the assumed (parameterized) clouds in the reanalysis models. This potential for inconsistency should be at least discussed in the manuscript.

4) The addition of the simple model in Section 4 is nice. However, given the already

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extensive scope of the study which, upon the improvements suggested in my previous comments would add still more length and detail, I feel that Section 4 is too much. It would be more appropriate to include in another study. This would afford the authors more latitude and space to explore various sensitivities in their age and other transport measures evaluated here. The latter would really help convince the reader about the robustness of their results.

Technical Comments:

General: Please reduce the number of acronyms used throughout as this contributes to the difficulty in following the text. Some acronyms are really not appropriate as they are not standard in the field (e.g. EID and EIZ, FullAMA) whereas others are, clearly, more appropriate (AMA) as they have precedent in the literature.

page 1 line 2: "massive" is a strange word to use here. Is this really necessary? page 1 line 2: reanalysis -> reanalyses. This applies throughout the manuscript. page 2 line 1: leave room "for" not "to" page 2 line 7: "repelling" is a strange word as it does not imply with respect to what (i.e. is a dynamical or transport barrier?) – I suggest removing. page 2 line 19: "a dispersion" -> This term is too physical. Perhaps "lack of consensus"? page 3 line 17: So, which winds are actually used here? Analysis or forecast? page 3 line 18: What is meant by "interspelling". I strongly suggest using a more common term. I also don't understand this sentence. Are the heating rates coincident in time with the winds? Fig. 7: What are blue/red/green lines? Labels are not explained in the caption.

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