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Interactive comment on "Where do the air masses between double tropopauses come from?" *by* A. C. Parracho et al.

Anonymous Referee #2

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This study presents results of a trajectory analysis designed to identify the source of air in the lower stratosphere and between tropopause altitudes identified during double tropopause events. The authors present results as a function of particle history for trajectories initialized in these layers during environments of both single and double tropopauses. In particular, particle latitude and potential vorticity are used to illustrate the transport history of lower stratosphere air in these cases. Although I believe there is ample scientific merit in an analysis that aims to identify sources of air between these double tropopause events, the analysis presented in this paper is incomplete and I believe the manuscript needs a major revision before it is acceptable for publication in ACP. In addition, I found the description of the methods used to be especially confusing and contradictory at times. I have listed detailed comments below that outline important

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issues that the authors need to address before I can consider the paper for publication.

General Comments:

1. The title of the paper claims to address where the air between double tropopauses comes from, but the analysis presented in this study is focused on the likelihood of one possibility: tropospheric intrusions. In addition, only DT events in the month of January are considered. There are two problems with this approach: 1) restricting the analysis of trajectories to the centroid position of all particles does not allow for quantification of all potential source mechanisms, which you outline in great detail in Section 1. 2) Tropospheric intrusions are most prevalent during the spring and fall of each hemisphere, where the structure of the subtropical jet is conducive to poleward breaking Rossby waves (see Homeyer & Bowman, JAS, 2013: http://dx.doi.org/10.1175/JAS-D-12-0198.1). Restricting the analysis of DT and ST to January alone neglects the seasonality in UTLS transport and potentially varying sources of DT events. In order to appropriately address this problem, this study should be updated to include the entire annual cycle. I understand that computation expense would be larger if the entire study period in this paper was analyzed, but 30 yr of analysis is not necessary to identify the mechanisms contributing to DT formation.

2. Analysis of the history of PV in the identified layers does not provide sufficient information for classifying the source as tropospheric. The occurrence of a DT regardless of transport introduces a significant reduction in the magnitude of PV in the lower stratosphere compared to that during a ST event. For example, suppose a region of DT remained a region of DT throughout the history of a particles path. In that case, the transported air would remain in the lower stratosphere throughout its history despite having lower PV than that in the environment of a ST. In addition, the instantaneous magnitude of PV provides limited information in this case due to the mixing of tropospheric air into the lower stratosphere over time in a tropospheric intrusion, which is evident in the PV distributions in Figure 5. A more appropriate analysis in this case would be to quantify the frequency at which these DT structures are found in the trop-

ical upper troposphere (i.e., south of the tropopause break or south of the subtropical jet) 10 days prior and what the reduction in PV associated with that transport is.

3. An important result missing from this study is the geographical variation in the source of air between DTs. Such an analysis would be very beneficial for identifying contributions from differing mechanisms and providing confidence in the analysis techniques. For example, tropospheric intrusions should contribute more to DT events immediately downstream of the eastern Pacific and Atlantic basins in the northern hemisphere.

4. I found Section 2.3 to be very confusing. The complicated subscripts in the equations (e.g., what is the '5' for in h5ST?), contradicting descriptions of bounds, and discussion of how these conditions were met were all important issues that led to my confusion. I think that a majority of this confusion can be eliminated by simpler descriptions of the restraints on a measurement. Further detail on my confusion is given in the specific comments below.

Specific Comments:

Page 1352, line 20: What is the vertical resolution of the profiles used to identify the tropopause? Both model level and pressure level fields from ERA-Interim have sufficient vertical resolution, so why not apply the WMO definition directly rather than the modified version in Birner 2010 and Reichler et al 2003? Those modified versions of the WMO definition can introduce unwanted error in the identified tropopause for higher vertical resolution analyses. Regardless of model resolution, interpolating the profiles to an even height grid linearly or using cubic splines will allow routine satisfaction of the WMO criteria in all cases.

Page 1353, Section 2.2: The motivation for selecting arbitrary domains for trajectory initialization is not at all clear to me. What was the motivation for choosing the points in Figure 1, and how sensitive are the results to changes in these locations? Why not initialize trajectories everywhere at a set resolution?

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Page 1353, equations 1 & 2: Are these boundaries identified for each domain? If not, wouldn't that introduce an unnecessary amount of additional variability? e.g., some domains may have a mean separation of 3 km between tropopauses and others may have 5 km. Lumping everything together in this way may remove a substantial amount of additional information in these cases.

Page 1354, lines 20-23: This description is very confusing. Were particles initialized twice a day, or every 6 hours?

Page 1355, lines 11-12: Below which tropopause? The primary tropopause? Because tropospheric intrusions are the result of tropical upper troposphere air being transported above the extratropical tropopause, wouldn't it be more illustrative to diagnose how many particles are found in the troposphere and below the tropical tropopause? Figure 3 illustrates that a 14 km threshold on tropopause height will separate tropical from extratropical air masses.

Page 1355, line 26: Shouldn't h1 represent the upper boundary here? Also, there should be a detailed description somewhere of how that boundary is defined.

Page 1356, lines 1-4: This description is confusing. What is the lower boundary of Di? (see previous comment) Also, in what way would applying the WMO definition to the ERA-Interim fields result in an erroneous identification of the tropopause? Do you have evidence that the tropopause altitude is incorrect from comparisons with coincident radiosonde observations? These conditions seem overly restrictive in this case.

Page 1356, line 16: Again, a description on how these upper boundaries are defined is missing here.

Page 1356, lines 17-18: This is confusing. How is the domain between the two tropopauses? Isn't the approach here meant to identify only instances where double tropopauses exist?

Page 1357, lines 1-3: I disagree that the vertical resolution of the ERA-Interim would

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allow for false identification of a tropopause. If anything, the number of DTs identified would be biased low relative to observations because the model resolution is not sufficient enough to resolve all temperature structures contributing to observed DTs, as defined using the WMO definition. In some cases, the primary tropopause would be missed and in most the secondary tropopause would be missed.

Page 1357, lines 7-13: What do you mean by air found below the domains? And what is the bottom boundary in this case? In reference to the use of condition 5, I thought this was accomplished via equations 1 & 3?

Page 1362, Section 3.3: Is the TPf in Fig. 7 an accumulated fraction of tropospheric particles during the entire trajectory history? If so, doesn't this limit your understanding of transport? In order to accurately identify the fraction of tropospheric air in a DT layer, only the final trajectory time (i.e., 10 days prior) should be analyzed here, since undoubtedly some varying fraction of the history will take place in the stratosphere, even for a tropospheric intrusion event.

Figures 2 & 6: The centroid and mean trajectory paths are hard to distinguish from the color-filled backgrounds in these plots. I would suggest using different colors such as gray and black to help distinguish these paths.

There are some important references missing in this paper:

Homeyer et al, 2011, "Dynamical and chemical...", JGR, http://dx.doi.org/10.1029/2010JD015098

Sofieva et al, 2014, "A novel tropopause...", ACP, http://www.atmos-chem-phys.net/14/283/2014/acp-14-283-2014.html

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