Atmos. Chem. Phys. Discuss., 15, C9140–C9144, 2015
www.atmos-chem-phys-discuss.net/15/C9140/2015/

Atmospheric Chemistry and Physics Discussions
Open Access

ACPD
15, C9140–C9144, 2015

Interactive Comment

Interactive comment on "A Lagrangian description on the troposphere-to-stratosphere transport changes associated with the stratospheric water drop around the year 2000" *by* F. Hasebe and T. Noguchi

S. Fueglistaler (Referee)

stf@princeton.edu

Received and published: 10 November 2015

Hasebe and Noguchi present an analysis of the evolution of stratospheric water from the late 1990's to the early 2000's, where water entering the stratosphere experienced a remarkable, sudden drop in the year 2000. They use kinematic trajectory calculations based on ECMWF ERA-Interim reanalysis data, where the dehydration along the trajectories is estimated based on the temperature evolution (i.e. one assumes complete dehydration down to the minimum saturation mixing ratio encounted during ascent from

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

the troposphere to the stratosphere). The method is similar to that in previous studies that have shown that this model calculation provides a reasonable reproduction of observed variations in water entering the stratosphere - with some caveats concerning the drop in the year 2000 (see discussion in Fueglistaler et al. 2013). Before I go into the details, I would suggest that for a revised version, the paper should be edited by a native English speaker (or perhaps ACP provides this service) - I ignore these problems here in my review. My main difficulty with the paper is that one of the key steps in the paper - the attribution of processes leading to the decrease in water vapor - is not clearly explained. If my understanding of the procedure (outlined below) is correct, I would have some serious concerns. Also, the discussion of the Sea Surface Temperature (SST) changes is very qualitative, and could be considerably shortened.

My difficulties in understanding the method refer to Sections 3.4/3.5, and Figures 5-7, and 12. It is tempting to decompose the average entry mixing ratio into the sum of contributions from different locations, with sum[f(lon,lat,time) * smr(lon,lat,time)] = H2Oentry(time), with the normalization of frequency sum[f(..)] = 1. By comparison of the map of "f" and "smr" between 2 times (say, before and after the drop), one hopes to decompose the change in H2Oentry(time1) - H2Oentry(time2) as a result of a change in the spatial distribution ("f"), and temperature (equivalent to "smr"; ignoring pressure variations). Although not formulated in this way, this is my understanding of what the authors do in Sections 3.4 and 3.5; and accompanying figures 5,6,7, and later 12.

Figure 5 then shows a shift in the locations where the last dehydration occurs, and Figure 6 then shows a change in temperatures. What is then observed is that some regions cool more than others, and that in these regions the fraction of "Lagrangian cold points" (LCP) increases. In other words, the LCP distribution is highly correlated with the distribution of the difference in temperature relative to the tropical mean. (As demonstrated in Fueglistaler and Haynes, 2005; their figure 2c,d).

The problem then is the interpretation. The high correlation of the perturbations in the spatial distribution of the LCP density, and temperature (i.e. the cross term f-prime x smr-prime) prevents an interpretation in terms of "contribution from temperature" versus "contribution from spatial change in LCP density". Very problematic are statements like (Page 28046/Line 24): "The reductions (bottom panel) are mainly due to the decreases of the LCP-event probability ..." One cannot say that because some region now has a lower frequency, that this contributes to a lower average H2Oentry - it only says that fewer air parcels are last dehydrated there (i.e. the term "f-prime x smr-mean" can only be interpreted for the total domain sum, not for individual regions!) But perhaps I simply don't quite understand what exactly you show in Figure 7 - you need to provide an equation to explain properly what exactly you calculate. To make my point clearer, consider the following case: The temperature field is homogenous (i.e. constant) at the tropical tropopause with a just a little bit of noise. The resulting LCP distribution would be pretty much random, but because of finite sampling, there would be some regions where there would be a bit higher densities, and some regions with lower densities. Now we do the experiment a second time, and look at the differences in the LCP distribution. We would see some regions with a decrease, and some regions with an increase in density. Now, the regions where the density decreases (i.e. "f-prime" would be negative) now seem to contribute to a "drying" if we quantify the contributions to the average H2Oentry as being the product of smr and density. However, since the locations simply have shifted in space and no real temperature change has taken place, we would observe similarly regions that seem to have contributed to a "moistening" simply because "f-prime" in these regions is positive. Of course, the "moistening" would simply balance the "drying" elsewhere, and the net change in H2Oentry is zero. Hence, this method produces spurious results.

Some further comments:

P28037/L3: "... after a prolonged increase through the 1980's and 1990's." I'd formulate this a bit more careful.

P28047/L16: I would think that Figure 2B of Fueglistaler and Haynes (2005) pretty convincingly shows that indeed the high values in the first half of 1998 are due to

ENSO.

P28048/L16ff: Figure 4 and the discussion here is not convincing; surely panels (a) and (b) look somewhat different but it's impossible to say anything quantitative. I suggest to eliminate this figure.

P28048/L1ff: Figure 9 is very nice! I wonder whether this figure should not be presented before the "Lagrangian Figures 6,7,8", since this Eulerian perspective really helps to understand what happens in the Lagrangian perspective.

P28048/L8ff: You state that post-2000 there was a "loosened grip" of the Tibetan high on air parcels - are you sure that this is the main reason for the shift in the spatial distribution of the LCPs? Alternative explanations: (i) Even with identical path, the post-2000 temperature pattern would induce a shift in the LCP distribution simply because the probability to encounter minimum temperatures has increased over the tropical Western Pacific region; and (ii) the temperature pattern change came about by a shift in deep convection, with more convection over the tropical Western Pacific, and the air masses reaching the TTL in that convection may never be part of the Tibetan anticyclone.

P28049/L18ff/Figure 10: I am not convinced by what I see in this Figure, nor by your description. What is visible are variations due to ENSO - I would argue no neutral person not knowing about the drop in the year 2000 would see anything special around the year 2000 in this figure.

P28051/L4ff: This is an interesting hypothesis! My only concern is that in our studies we operated with monthly means, and I would be cautious about the significance of a 1-month difference.

P28051/L22/Figure 12: As said for Figure 7, I need to see an equation to fully understand what this figure shows.

P28052/L3ff: "These evidences ..." I could not follow your arguments here. The east-

ACPD

15, C9140–C9144, 2015

ward expansion of warm water should lead to a cooling over these regions, but in Figure 12, the "difference" shown in the bottom panel is "red" over the central Pacific, while it is blue over the Maritime continent - supposedly to the *WEST* of the convection anomaly? Please clarify.

Figures: Please add labels ("a", "b" etc) to all sub-plots.

Figure 3: I understand that you are concerned that a 6-year period is too short to define a reliable climatology, but I would still consider a decomposition into mean annual cycle, and anomalies thereof, to be the better solution. Since you use the same method and data as Fueglistaler et al. (2013), you could check whether your anomalies look similar to those that they published (e.g. their Figure 8b) to make sure that the comparatively short timescale does not distort the anomalies too much.

Figure 6: Please change the color scale of panel "c" to the same as in Figure 5 (i.e. going from blue to red with white at 0).

Figre 7: As mentioned before, please provide an equation for what is shown in this figure, and improve the figure caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 28037, 2015.