Atmos. Chem. Phys. Discuss., 15, C10565–C10581, 2015
www.atmos-chem-phys-discuss.net/15/C10565/2015/

Atmospheric Chemistry and Physics Discussions
Open Access

ACPD
15, C10565–C10581, 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

F. Hasebe and T. Noguchi

f-hasebe@ees.hokudai.ac.jp

Received and published: 18 December 2015

article amsmath

Reply to Dr. S. Fueglistaler (referee)

We are grateful to the thorough reading and constructive comments on our manuscript. We have made substantial changes to the manuscript in response to the review as described below. Each panel of figures are labeled "a", "b" and so on as suggested, and

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

the following reply refers to these labels. These revisions have significantly improved the manuscript, and we hope we have answered all of the concerns. We think all these improvements will satisfy the reviewer and hopefully make the manuscript suitable for publication in ACP.

Major issues

1. **Comment:** 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 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.

   (a) **Comment:** Should be edited by a native English speaker.

   Reply: We are sorry to have caused problem in reading the manuscript. As

ACPD
15, C10565–C10581, 2015

is mentioned, perhaps ACP provides the service. We will follow the instruction by the editor and editorial office on English editing.

(b) **Comment:** The attribution of processes leading to the decrease in water vapor is not clearly explained.

Reply: Revisions are made as can be seen from the reply to **Comment 2** below.

(c) **Comment:** The discussion of the SST changes is very qualitative and could be considerably shortened.

Reply: The paragraph discussing the SST changes has been slightly shortened. However, some more descriptions have been made to follow the comments from another reviewer. Figure 10 has been rewritten to clarify the features we see from the SST variations.

2. **Comment:** 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.

   (a) **Comment:** The method to decompose the average entry mixing ratio into the sum of contribution from different location is difficult to understand.

   Reply: The following description has been added in Section 3 of the revised manuscript.

ACPD
15, C10565–C10581, 2015

Interactive Comment

- **Page 28043, line 13, top**: The calculations are made on a monthly basis using the three initialization days (5th, 15th and 25th of each month) at a time. The following description refers to a specific month omitting the suffix for time. Let start by assuming that the minimum saturation mixing ratio along i-th TST trajectory ($i = 1, \cdots, N_{\mathsf{TST}}$) is denoted by $\mathsf{SMR}_{\mathsf{min}i}$. The entry value of water to the stratosphere $[\mathsf{H_2O}]_\mathsf{e}$ is defined as the ensemble mean value of $\mathsf{SMR}_\mathsf{min}$ as in Fueglistaler et al. (2005):

$$[\mathsf{H_2O}]_\mathsf{e} = \frac{1}{N_{\mathsf{TST}}} \sum_{i}^{N_{\mathsf{TST}}} \mathsf{SMR}_{\mathsf{min}i}. \tag{1}$$

- **Page 28045, line 7**, after “TST trajectories.”: Let assume that i-th TST trajectory ($i = 1, \cdots, N_{\mathsf{TST}}$) takes minimum saturation mixing ratio ($\mathsf{SMR}_{\mathsf{min}i}$) at bin j ($j = 1, \cdots, M$), that is, the Lagrangian cold point (LCP) for i-th TST trajectory is found at bin j. If we denote the number of LCP events at bin j as $N(\mathsf{LCP} \in j)$,

$$N_{\mathsf{TST}} = \sum_{j}^{M} N(\mathsf{LCP} \in j). \tag{2}$$

Because some trajectories do not satisfy the TST condition in general, $N_{\mathsf{TST}} \leq N$, where N is the total number of initialization points used for the calculation. The probability of LCP events at bin j, $P(\mathsf{LCP} \in j)$, is defined by

$$P(\mathsf{LCP} \in j) = \frac{N(\mathsf{LCP} \in j)}{N_{\mathsf{TST}}}, \tag{3}$$

so that the normalization condition $\sum_{j}^{M} P(\mathsf{LCP} \in j) = 1$ holds.

ACPD
15, C10565–C10581, 2015

- **Page 28045, lines 7 to 10**: The top two panels of Fig. 5 show ... posterior to the drop).

has been changed to

The top two panels of Fig. 5 show the horizontal distributions of $P(\text{LCP} \in j)$ thus defined for those trajectories initialized in September 1998 and 1999 (a; prior to the drop) and September 2000, 2001 and 2002 (b; posterior to the drop). Because N_{TST} is different among individual September, N_{TST} for each month has been used as a weight in taking the averages. In other words, the calculations are made by combining the trajectories of two or three prior- or posterior-months together for the illustrations. To be more specific, the TST trajectories of September 1998 and 1999, selected from $N = 2952 \times 3 \times 2$ trajectories, are combined together for the illustration of Fig. 5(a), while those of September 2000, 2001, and 2002, selected from $N = 2952 \times 3 \times 3$ trajectories, are used for Fig. 5(b).

- **Page 28046, lines 1 to 3**: Figure 6 is the same as Fig. 5 ... rather than the probability of LCP events.

has been changed to

The ensemble mean value of SMR_{min} at bin j, SMR(LCP$\in j$), is defined by

$$\text{SMR}(\text{LCP} \in j) = \frac{1}{N(\text{LCP} \in j)} \sum_{i}^{\text{LCP}\in j} \text{SMR}_{\text{min}i}, \qquad (4)$$

ACPD
15, C10565–C10581, 2015

where $\sum_{i}^{\mathrm{LCP}\in j}$ indicates the sum with respect to the subset of TST trajectories that take LCP at bin j.
Figure 6 is the same as Fig. 5 except that SMR(LCP$\in j$) is illustrated rather than $P(\mathrm{LCP} \in j)$.

- **Page 28046, lines 16 to 20**: the comparisons based only on the changes ... $\mathrm{SMR_{min}}$ (Fig. 6) together for each bin.

has been changed to

the comparisons based only on the changes in SMR(LCP$\in j$) could be misleading, because the values of $P(\mathrm{LCP} \in j)$ are much higher in the former than in the latter (Fig. 5). The expectation value for bin j, $E(\mathrm{LCP} \in j)$, is defined by the multiple of $P(\mathrm{LCP} \in j)$ and SMR(LCP$\in j$) to quantify the contribution of each bin to $[\mathrm{H_2O}]_\mathrm{e}$. The sum of $E(\mathrm{LCP} \in j)$ with respect to all bins reduces to

$$\sum_{j}^{M} E(\mathrm{LCP} \in j) = \sum_{j}^{M} P(\mathrm{LCP} \in j) \times \mathrm{SMR}(\mathrm{LCP} \in j) \quad (5)$$

$$= \sum_{j}^{M} \frac{N(\mathrm{LCP} \in j)}{N_\mathrm{TST}} \times \frac{1}{N(\mathrm{LCP} \in j)} \sum_{i}^{\mathrm{LCP}\in j} \mathrm{SMR}_{\mathrm{min}i} \quad (6)$$

$$= \frac{1}{N_\mathrm{TST}} \sum_{j}^{M} \sum_{i}^{\mathrm{LCP}\in j} \mathrm{SMR}_{\mathrm{min}i} \quad (7)$$

$$= \frac{1}{N_\mathrm{TST}} \sum_{i}^{N_\mathrm{TST}} \mathrm{SMR}_{\mathrm{min}i}. \quad (8)$$

ACPD
15, C10565–C10581, 2015

This is the entry value of water to the stratosphere $[H_2O]_e$ (Eq. (1)) shown as a time series in Fig. 3. $[H_2O]_e$ is thus decomposed of the sum of $E(\text{LCP} \in j)$, which is interpreted as the contribution of bin j to $[H_2O]_e$. What is important here is that it is neither $P(\text{LCP} \in j)$ nor SMR(LCP$\in j$) but the product between the two, $E(\text{LCP} \in j)$, that is directly responsible for composing the value $[H_2O]_e$. By comparing the distribution of $E(\text{LCP} \in j)$ between the two periods, prior and posterior to the drop, we can see how the drop in $[H_2O]_e$ is brought about by the change of water transport from individual region.

Figure 7 shows the horizontal distribution of $E(\text{LCP} \in j)$.

(b) **Comment:** Figures 5, 6, 7 and 12 are difficult to understand.

Reply: The top two panels of Figs. 5 and 6 show the spatial distributions of $P(\text{LCP} \in j)$ and SMR(LCP$\in j$), respectively, for September, and those of Figs. 7 and 12 are the spatial distributions of $E(\text{LCP} \in j)$ for September and January, respectively. The September (January in Fig. 12) values prior and posterior to the drop are estimated by averaging two or three years of the corresponding month. Because N_{TST} is different among individual September, N_{TST} for each month has been used as a weight in taking the averages. In other words, the calculations written above are made by combining two or three prior- or posterior-months together for the illustrations of these figures. To be more specific, the TST trajectories of September 1998 and 1999, selected from $N = 2952 \times 3 \times 2$ trajectories, are combined together for the illustration of the panel (a) of Figs. 5, 6, and 7, while those of September 2000, 2001, and 2002, selected from $N = 2952 \times 3 \times 3$ trajectories, are used for the illustration of panel (b) of these figures. These explanations are supplemented in Section 3.1 as described above.

ACPD
15, C10565–C10581, 2015

3. **Comment:** 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).

   Reply: The LCP distribution may appear "highly correlated with the distribution of the difference relative to the tropical mean." However, closer look will find that high values of $P(\text{LCP} \in j)$ are concentrated in the region over the Bay of Bengal and Malay Peninsula in September (Fig. 5), while low values of SMR(LCP$\in j$) are found widely distributed in the western tropical Pacific (Fig. 6). The novelty of our analysis is to have distinguished the distribution of $P(\text{LCP} \in j)$ against SMR(LCP$\in j$) and introduced $E(\text{LCP} \in j)$. Please see the Reply to **Comment 4** below.

4. **Comment:** The problem then is the interpretation.

   (a) **Comment:** The high correlation of the perturbations in the spatial distribution of the LCP density, and temperature (i.e. the cross term f-prime $\times$ smr-prime) prevents an interpretation in terms of "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 H2O entry - if only says that fewer air parcels are last dehydrated there (i.e. the term "f-prime $\times$ smr-mean" can only be interpreted for the total domain sum, not for individual regions!) But perhaps I simply don't quite understand

ACPD
15, C10565–C10581, 2015

Interactive Comment

what exactly you show in Figure 7 - you need to provide an equation to explain properly what exactly you calculate.

Reply: We hope the equations (1) through (8) above, being added in the revised manuscript, have made the quantity shown in Fig. 7 clear. We can interpret the values of $E(\mathrm{LCP} \in j)$ as the contribution of bin j to $[H_2O]_e$. As the quantity that directly drives $[H_2O]_e$ is $E(\mathrm{LCP} \in j)$, any statement that attributes solely $P(\mathrm{LCP} \in j)$ or SMR(LCP$\in j$) to the cause of changes in $[H_2O]_e$ is mathematically wrong. However, we believe it is quite interesting to see individual changes in $P(\mathrm{LCP} \in j)$ and SMR(LCP$\in j$) to interpret the variations in $E(\mathrm{LCP} \in j)$. The paragraph that includes the "very problematic" sentence is a part of our effort along this line, trying to interpret the noticeable features in $E(\mathrm{LCP} \in j)$ as meteorological words with the hope to help understand the changes in terms of modulated trajectories and perturbed temperature. The cited sentence and the one that follows are replaced by the following in the revised manuscript.

- **Page 28046, line 23 to page 28047, line 1**: The contribution from this core area ... decrease in $\mathrm{SMR_{min}}$.

has been changed to

The contribution from this core area remains dominant during the posterior period (Fig. 7(b)). While the reduction of $[H_2O]_e$ cannot be free from the general cooling (lowering of SMR(LCP$\in j$)) in posterior years over most of the tropics (Fig. 6), it is interesting to note the increase of $E(\mathrm{LCP} \in j)$ despite the decrease in SMR(LCP$\in j$) over the central Pacific. This is because the increase of $P(\mathrm{LCP} \in j)$ more than compensate for the decrease of SMR(LCP$\in j$) over there. In this sense, it is not appropriate

ACPD
15, C10565–C10581, 2015

to attribute the cooling over the western and the central Pacific to the drop in $[H_2O]_e$. The similarity in the spatial distributions of $P(\text{LCP} \in j)$ and $E(\text{LCP} \in j)$, especially that of the location of maxima over the Bay of Bengal and Malay Peninsula together with the post 2000 decrease over there and the western tropical Pacific, suggests that the relocation of LCPs (change in $P(\text{LCP} \in j)$) is a leading factor that has caused the drop in $[H_2O]_e$ in September 2000.

(b) **Comment:** 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.

Reply: The statistical significance is always an issue to be payed attention to. We cannot conclude anything from the example indicated above. By employing large number of trajectories, however, we believe we have attained the statistical significance high enough to derive meaningful results. The statistical significance of the estimated differences between

ACPD
15, C10565–C10581, 2015

the two periods in $P(\mathrm{LCP} \in j)$ and SMR(LCP$\in j$) are shown at the bottom panels (d) of Figs. 5 and 6. Our argument in the manuscript is concerned only with those regions we confirmed the statistical significance is high. The following revision has been made.

- **Page 28041, lines 5 to 13**: The trajectory calculations are started from uniformly distributed gridpoints ... horizontal resolution of 0.75° by 0.75° longitude–latitude gridpoints prior to calculations.

has been changed to

The backward trajectory calculations are started from uniformly distributed gridpoints (every 5.0° longitude by 1.5° latitude) within 30° N and S from the equator on 400 K potential temperature surface. The initializations are made on the 5th, 15th, and 25th of every month during the period since January 1997 till December 2002 relying on the European Centre For Medium-Range Weather Forecasts ERA Interim dataset (Dee et al., 2011). The number of initialization points is 2952 for a single calculation resulting in 8856 for the estimation of monthly values. This number compares well with that of the reduced set of trajectories in the study on the sensitivity of number of trajectories by Bonazzola and Haynes (2004) and turned out to be enough to derive statistically significant results as can be seen later in Section 3. All meteorological variables given on 60 model levels have been interpolated to those on 91 pressure levels keeping the horizontal resolution of 0.75° by 0.75° longitude-latitude gridpoints prior to calculations. The time step has been set to 30 minutes, similar to 36 minutes taken by Bonazzola and

ACPD

15, C10565–C10581, 2015

Interactive Comment

Haynes (2004), by applying spatiotemporal interpolations to the 6-hour interval ERA Interim dataset. As for the limitation and caution of this method, see, for example, the pioneering studies by Fueglistaler et al. (2004) and Bonazzola and Haynes (2004).

Some further comments

1. **Comment:** P28038/L3: "... after a prolonged increase through the 1980's and 1990's." I'd formulate this a bit more careful.

   Reply: The sentence has been revised as follows:

   - **Page 28038, lines 2 to 3**: Stratospheric water vapor is known to have decreased suddenly at around the year 2000 to 2001 after a prolonged increase through the 1980s and 1990s.

     has been changed to

     The sudden decrease of stratospheric water vapor at around the year 2000 to 2001 is relatively well accepted in spite of the difficulty to quantify the long-term variations.

2. **Comment:** 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.

   Reply: The following sentence has been inserted after "in these months."

   - **Page 28047, line 16**: Actually Fueglistaler and Haynes (2005) demonstrated in their Fig. 2 that the trajectory model shows large increase of lower-stratospheric water ($[H_2O]_{T400}$ which takes non-TST trajectories into account in addition to $[H_2O]_e$) associated with

El Niño and that the increase is accompanied by the eastward shift of the high density region of LCP.

3. **Comment:** 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.

   Reply: It is true that Figure 4 cannot show any quantitative change in the LCP distribution; such a role is assigned to Figure 5. The purpose of Figure 4 is to get a clear view on how the TTL trajectories in August are distributed. This is a basic information to proceed to the interpretation of the results shown later, and thus Figure 4 is retained.

4. **Comment:** 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.

   Reply: Our main purpose is to examine the change of $[H_2O]_e$ from a Lagrangian perspective and try to interpret it from a meteorological point of view. Figure 9, together with Figure 4, is quite impressive and suggests the direction of further research. However, it does not necessarily mean Figure 9 is better presented earlier than Figures 6, 7, and 8. The sequence of figures is kept intact.

5. **Comment:** P28049/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

ACPD
15, C10565–C10581, 2015

Interactive Comment

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.

Reply: The description "loosened grip" comes from our speculation based on the changes in the TTL trajectories shown in Figure 4 combined with the weakened Tibetan high in the posterior years seen from Figure 9. The alternative explanation (i) will not apply because we can see from Figs. 4 and 5 that the density of trajectories circulating Tibetan high is substantially lower while more trajectories are found in the southern hemisphere in the posterior years. The alternative explanation (ii) is interesting in that the trajectories do change even if there is no modulation in Tibetan high. However, we do not adopt this interpretation since we do see weakening of Tibetan high in posterior years. In our opinion, the speculative expression "loosened grip" is acceptable in discussion section.

6. **Comment:** 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.

Reply: Figure 10 has been rewritten to shed light on the eastward migration of 28°C SST contour. The reason why we pay attention to this contour is based on the paper by Gadgil et al. (1984), who pointed out that 28°C is a threshold of active convection (page 28049, line 22).

7. **Comment:** 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.

Reply: The September values of $[H_2O]_e$ are calculated using the trajectories ini-

ACPD

15, C10565–C10581, 2015

Interactive Comment

tialized on 5th, 15th and 25th of September. The backward 90-day trajectory calculations rely on the meteorological fields in August, July and June; there is no reference to October values. In this sense, the one month difference is significant.

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

   Reply: Done.

9. **Comment:** P28052/L3ff: "These evidences ..." I could not follow your arguments here. The eastward 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.

   Reply: Your expectation concerns the change in SMR(LCP$\in j$) while the above sentence deals with E(LCP $\in j$). The decrease in SMR(LCP$\in j$) is more than compensated by the increase in P(LCP $\in j$), resulting in the increase in E(LCP $\in j$) in the central Pacific (red). Related sentences are revised as follows:

   - **Page 28051, line 27 to page 28052, line 5**: The difference between the two periods ... the central Pacific (Fig. 10).

   has been changed to

   The difference between the two periods (Fig. 12(c)) shows decrease over Indonesia and increase over the central Pacific during the period posterior to the drop. The former is due to the combination of the decreases in both P(LCP $\in j$) and SMR(LCP$\in j$), while the latter is brought about by the interplay between the increase in

ACPD
15, C10565–C10581, 2015

$P(\mathrm{LCP} \in j)$ and some decrease of SMR(LCP$\in j$) (not shown). This situation is the same as what we see in September (Section 3.5). The similarity of this pattern, that is, the decrease in the equatorial western Pacific (over Indonesia) and the increase over the central Pacific, to that of the second component of September response suggests the existence of a common driver of the drop in $[H_2O]_e$ irrespective of the season. These evidences suggest the idea that the drop of $[H_2O]_e$ in northern winter has resulted from the response of the TTL circulation to the eastward expansion of the warm water to the central Pacific (Fig. 10) in such a way that the decrease of $E(\mathrm{LCP} \in j)$ in the western Pacific exceeds the increase of that in the central Pacific.

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

    Reply: Done.

11. **Comment:** 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.

    Reply: I understand the time series labeled by EI in Figure 8(b) of Fueglistaler et al. (2013) is the anomalies from the mean of more than 20 years from 1989 to 2011. The anomalies of our 6-year time series is not suitable for comparison because of the large influence of 1997-1998 El Niño. This is what we found at the earliest stage of our analysis.

ACPD
15, C10565–C10581, 2015

12. **Comment:** 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).

Reply: Done.

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

Reply: The equations are provided in response to **Comment 2** of Major issues. The figure caption has been changed to the following.

- **Figure 7.** The same as the top three panels of Fig. 6 except that $E(\text{LCP} \in j)$, the contribution of bin j to $[H_2O]_e$, (ppmv) is illustrated. See text for the definition of $E(\text{LCP} \in j)$.

ACPD

15, C10565–C10581, 2015

Interactive Comment