Reply to referee #2

We are grateful to the thorough reading and constructive comments on our manuscript. We believe we have incorporated all aspects pointed out. The detailed description on the revision follows:

Overall comments:

This paper draws upon the SOWER program's unique set of water vapor sounding data from the equatorial Pacific to address the question of TTL dehydration in a straightforward and direct manner, and the authors do an extremely careful job of preparing the sounding data to remove instrumental noise and to characterize the error structure of the measurements. For this they should be applauded, and we can place a high degree of confidence on the mixing ratio values they use in their analysis. They also provide clear, albeit anecdotal, evidence of high supersaturation within layers of ice particles in the TTL.

Reply: We are glad that our careful analysis has been fully rewarded.

The main thrust of the paper however is to establish via statistical analysis the relationship between observed mixing ratios and air parcel temperatures upstream, but the results are not clear cut. While, the scatter diagrams in Fig. 14 certainly suggest a relationship involving homogeneous nucleation at 370 and 380 K in particular, the authors appear to shy away from making a clear statement of how the results taken as a whole are consistent with the cold-trap hypothesis per se. While I think there are questions about the reliability of the trajectory calculations that should have been addressed in the paper, the lack of a clear set of conclusions I think can be attributed to the absence of a statement of a set of testable hypotheses at the beginning of the paper. Instead, the concept of the 'efficiency' of dehydration is introduced early but never defined, and it is unclear how the evidence will be used to assess this efficiency, much less discriminate between cold-trapping via homogeneous nucleation and other processes affecting the water vapor mixing ratios. Instead, other processes such as sedimentation of ice particles and radiative heating and cooling are introduced in the breech. For example sedimentation of ice particles from higher levels is invoked as one possible explanation for the presence of OMR values exceeding the homogeneous threshold at the 360 and 365 K levels. Another explanation offered for the same observation is that dehydration was ongoing at those levels.

Reply: The paragraph in p. 25837, l. 12-26 has been rewritten in line with the comment:

In the present analysis, TTL water mixing ratio is examined on isentropes using the water vapour sonde data taken at Tarawa (1.4 N, 172.9 E), Biak (1.2 S, 136.1 E), Watukosek (7.6 S, 112.7 E), Bandung (6.9 S, 107.6 E), Kototabang (0.2 S, 100.3 E), Hanoi (21.0 N, 105.8 E), and San Cristobal (0.9 S, 89.6 W) since 1998 to shed light on the climatological differences along the equator. The longitudinal structure thus obtained must have been brought about by meteorological conditions such as local convective activity, isentropic level changes, season and El Nino/Southern Oscillation (ENSO) phase. Our primary focus is to examine whether the difference could be interpreted by the "cold-trap" hypothesis. We will thus pay special attention to the origin and

Lagrangian temperature history of the observed air parcels, and derive mutual relationships between OMR and SMRmin. The difference between the statistical features on each isentrope will be interpreted as an indication of the progress of dehydration (or hydration) as the air gradually ascends diabatically in the TTL. Before starting such analyses, efforts are made to create the most suitable dataset for this purpose by carefully estimating the observational errors and paying attention to the possible phase delay in the instrumental output. The data obtained by SW are not used, while those using the FPH and CFH are analyzed to achieve better homogeneity in the analyzed data. Homogeneous nucleation and supersaturation are discussed by combining the information on water concentration from sondes and ice particles from lidar and backscatter sondes when available. The estimation of the typical time scale for dehydration and the degree of supersaturation critical for initiating ice nucleation, both important measures of the efficiency of cold trap dehydration, will be done in a separate paper (Inai et al., 2013) that deals with the water vapour "match" as some detailed examinations establishing the "match criteria" need to be made. The role of large scale atmospheric waves on the TTL dehydration will be discussed in a separate paper under preparation.

One sentence in Concluding remarks (p. 25855, l.19) is modified:

The statistical features of cold trap dehydration are investigated using scatter diagrams between the observed water mixing ratio and the minimum saturation mixing ratio along the back trajectories associated with the observed air mass. The distribution of the points display the manner in which water vapour is removed from air parcels during the course of quasi-horizontal advection accompanied by cross-isentropic slow ascent in the TTL.

Another question that is not adequately addressed is how adequate are the data and the analysis approach for making such discriminations? I think this question is most important for the trajectory-based upstream histories of air parcel saturation mixing ratio. As the authors show in Fig. 4, both the ERA40 and ECMWF operational analyses are cold-biased relative to the SOWER soundings by up to 2 K in the middle of the TTL (350-360K). The impact of these biases on the SMRmin estimates is not discussed in the text, and if the SMRmin estimates are in fact too dry below 365 K, then the agreement with the observations might well have been better. The other important aspect is confidence in the wind fields underpinning the trajectories. Fig. 4 shows substantial biases in the zonal wind especially. The bottom line is that a more careful accounting of the errors in SMRmin needs to be provided before the strong conclusions can be drawn from Fig. 14. While the caption for Fig. 14 does includes a statement about the SMR error bars, this is no substitute for a careful consideration of all the sources of error in tropical trajectory calculations.

Reply: Although thorough investigation of the possible bias in the ECMWF dataset is beyond the scope of the present study, we agree that more considerations need to be made. The wind biases in the TTL we see in Fig. 4 are mostly confined in zonal wind between 375 and 390 K. The spatial variability of wind field and thus the impact of wind bias in this altitude range will be smaller than those in the lower TTL. Fortunately the wind bias is not significant in the region 360 to 365 K. On the other hand, as is suggested by the reviewer, a mean bias of 2 K in the potential temperature surfaces 355 and 360 K (but not on 365 K and above), is worth being considered in the estimation of

SMRmin. One of the possible ways to take it into account is to assume 2-K constant bias in the ECMWF temperature data and add 2 K to the temperature along the trajectories without considering any change in the trajectories themselves for the sake of simplicity. This is over simplification of the problem, but provides a better estimate of SMRmin making the judgement of the encounter with deep convection more realistic. Fig. 14 has been rewritten following this idea in which the temperature along the 360 K trajectories is uniformly increased by 2 K. As is expected, the points move to the righthand side and come closer to the dashed line indicating that the supersaturation along the trajectories does not greatly exceed the homogeneous nucleation limit.

Sentences from p.25844, top to l. 6 are modified:

There are some noticeable biases, one in zonal wind at around 380 K and 440 K in the former period (center; red) and the other in temperature at around 430 K and 360 K (left; black) in the former and the latter period, respectively. It is interesting to note that the biases are reduced in the statistics after December 2006 (bottom) not only in temperature but also in wind components. The improvement in the quality of meteorological fields in general may indicate the model's response to the high resolution COSMIC temperature data adjusting the dynamical fields to be consistent with observed temperature. However, there is one exception; the cold bias in ECMWF temperature at around 360 K, common in both periods, becomes statistically significant in the latter. The impact of this bias will be taken into account in the estimation of saturation mixing ratio along the trajectories.

One sentence has been inserted in p. 25849, l. 12:

For the calculations on the 360 K surface, the ECMWF temperature has been increased by 2 K based on the cold bias in the analysis field against the SOWER sonde data we find in Fig. 4.

Some discussion is made in the second paragraph in p. 25851:

Although growing numbers of global analyses have emerged with notable differences (e.g. Liu et al., 2010; Schoeberl and Dessler, 2011), our use of the high-resolution model-level ECMWF analysis is based on our recognition that fine vertical resolution in the TTL is important for our study as the temperature perturbations and wind shear associated with large scale atmospheric waves could play a key role in the TTL dehydration. On the other hand, there is a clear limitation as can be seen from the comparison between the analysis field and sonde data as seen in Fig. 4. Fortunately the wind biases in the TTL are mostly confined to zonal wind between 375 K and 390 K, while the spatial scale of variabilities becomes larger in this altitude range than in the lower TTL thus the impact of wind bias on the estimates of SMRmin is more limited. A mean temperature bias of 2 K on the potential temperature surfaces at 355 K and 360 K (but not at 365 K and above), on the other hand, needs to be considered in the estimation of SMRmin. Our present approach is to assume a 2 K constant bias in the ECMWF temperature field and add 2 K to the temperature along the trajectories without considering any change in the trajectories themselves. This is over simplification of the problem, but provides a better estimate of SMRmin making the judgement of the encounter with deep convection more realistic. As is expected, the points have moved to the righthand side and come closer to the dashed line in Fig. 14 as compared to those without taking this bias into account (not shown). It is interesting to see the uneven shift at potential temperatures 360 K and 365 K does not go far beyond the dashed line suggesting that the supersaturation along the trajectories does not greatly exceed the homogeneous nucleation limit.

Another potential source of error in SMR values was the choice to calculate trajectories adiabatically. For example, while the magnitude of the clear-sky radiative heating in the TTL is less than 1 K/day, air parcels in the western Pacific TTL are likely to spend a considerable fraction of time over extensive cloud decks. This and other potential sources of error in the trajectory calculations should have been addressed.

Reply: The validity of adiabatic trajectory calculation has been investigated empirically by Inai et al. (2013), formerly referred to as Inai et al. (2012), during the screening procedure of match analysis. We now adopt their results suggesting 5 days as the acceptable range of adiabatic assumption and have rewritten Fig. 14 changing the search period of SMRmin from seven days to five days. Related changes in the text are:

p. 25849, l.13-14:
The search for the values of SMRmin along the backward trajectories has been limited to five days based on the investigation on the validity of the adiabatic assumption by Inai et al. (2013).

p. 25863, l. 13 (moved to Sec. 4.3):
"seven days" is changed to "five days"

p. 25884, l. 2 in the caption:
"seven days" is changed to "five days"

More specific comments:

1. *I don't understand why a long passage on the statistical nature of dehydration is relegated to an appendix. The points raised here are very germane and deserve to be in the main body of the text. Appendices should be used in my opinion to discuss technical issues that may be critical to the methodology, but not to the scientific argument itself. The authors should consider incorporating the material in Appendix B into Section 4, although this may require some trimming.*

Reply: Following the suggestion, we first incorporated Appendix B into Section 4.2. Some trimming is surely possible, but we are afraid Section 4.2 in this form might appear too lengthy. Then we decided to move Appendix B to create Section 4.3. We think that the current version will be better for general readers who are not willing to pay attention to the detailed isentrope by isentrope description but want to get broad scope on the progress of dehydration quickly. The titles of Sections 4.2 and 4.3 (former Appendix B) have been changed together with some modifications in the text.

2. *Many of the figures are too small and difficult to read. This is particularly true of Figs. 5 and 14 as a whole and for the legends in Figs. 7, 10 and 11. Fig. 14 is the most important figure in the paper, and it is particularly hard to distinguish the symbols of the individual stations amidst the forest of error bars. If nothing else, the panels in this figure should be doubled in size.*

Reply: It is true that Fig. 14 looks too small in the download file. However, it appears to be due to technical issue necessary to fit in a single-page format with a rather lengthy caption. We think it will appear in a proper size by type-setting as two-column figure in ACP. Fig. 5 has been prepared in the form of two separate panels, and could be placed side by side if necessary. Figs. 7, 10 and 11 are rewritten by magnifying inset legends.

3. *The text will require copy-editing to correct grammatical errors and various malapropisms. A common example of the former is the insertion of the definite article where none is required (e.g., page 25837, line 1: '. . .efficiency of the "cold-trap" dehydration. . .'). An example of the latter is the word 'conveniently' in line 12 of page 25836.*

Reply: Whole manuscript has been read through by one of the coauthors (native speaker of English) and all errors and malapropisms have been corrected.

Other revision:

Fig. 2 has been rewritten by plotting the descending profiles first and ascending data later so that masking due to overlapping error bars be minimal.