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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Probability density functions of long-lived tracer observations from satellite in the subtropical barrier region: data intercomparison" *by* E. Palazzi et al.

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Response to reviewers' comments by Palazzi E. and co-authors

Authors kept in regard the reviewers' comments and suggestions, and thank them for their useful remarks. All points raised by the reviewers have been carefully discussed and addressed. The answers to each specific comment are detailed below (reviewers' comments are shown in bold), while a summary of the most significant changes to the manuscript is shown here:

1) In the Introduction, two more references describing the Lyapunov diffusivity, another diagnostic of atmospheric mixing and transport, have been added (d'Ovidio et

al., 2009; Shuckburgh et al., 2009), to complete the state-of-the-art description. The list of references is changed accordingly.

2) A complete list of the chemical species measured by all satellite instruments considered in this work is supplied; in the previous version, the list of measured species for MLS and MIPAS was incomplete.

3) From Figure 6, figures have been renumbered, so that: old Fig. 6b is now Fig. 7; old Fig. 7 is now Fig. 8; old Fig. 8 is now Fig. 9; and old Fig. 9 is now Fig. 10. Figure 6b does not exist anymore.

4) The following figures have been modified, according to the reviewers' comments: Fig. 1 (zonal mean zonal winds overplotted in blue), Fig. 2 (panels in the last row have been arranged differently so that the same seasons in the two hemispheres are lined up); Fig. 4 (the edges of the plot are now clearly visible); Fig. 5 (the bottom right panel shows now the difference SMR-HAL rather than HAL-SMR);

5) In table 2, the following changes have been made: 1) the last column shows now SMR-HAL values rather than HAL-SMR values. Therefore, the mean values have opposite sign with respect to the previous table version. 2) Three rounding errors in the mean value MLS-MIP (SH), mean value SMR-HAL (NH), standard deviation value MIP-HAL (NH), and one typing error in the MLS-HAL mean value for the SH have been fixed.

6) Substantial changes have been made in section 4.3 to better discuss the way in which the different sensors capture the effects of the QBO on the subtropical barrier position; Figure 8 (Fig. 7 in the previous paper version) has also been discussed more deeply.

Authors do think that the manuscript has improved and do hope that it is now more fluent and ready for publication.

Answers to the specific reviewers' comments (please, note that figures are uploaded separately)

Referee 1

The only minor comment is that in Fig. 2 I think the authors mean seasonally averaged, not annual mean

The caption of figure 2 has been changed ("annual mean" → "multiannual mean seasonal mean"), also following the referee 2 comment (see substantive point 3 below).

Referee 2

Response to substantive points

1) p18388, L10-20: A recent paper by Shuckburgh et al. [JAS 2009] that used Lyapunov diffusivity to investigate seasonal and interannual variability in mixing should also be cited in this discussion.

In the introduction, before the reference Sparling (2000) is cited in the manuscript, a short discussion of the papers by d'Ovidio et al. (JAS, 2009) in which the Lyapunov diffusivity diagnostic is introduced and by Shuckburgh et al. (JAS, 2009) in which that diagnostic is applied to investigate the variability of mixing in the Upper Troposphere Lower Stratosphere (UTLS) region, has been added.

2) p18395–18397: The various species measured by HALOE and SMR are listed, whereas for MLS and MIPAS only N2O is mentioned. This could give the misleading impression that the latter two instruments measure only N2O.

The list of the various species measured by MLS and MIPAS has been added in section 2.2 when the aforementioned instruments are presented. In this way, all four sensors are consistently described.

3) Figure 2: Rather than starting in DJF for both hemispheres, it might facilitate comparisons to arrange the panels such that comparable seasons are lined up (i.e., start the bottom row with JJA). Also, "annual mean" –> "multiannual mean". It should be

made more clear in the figure caption (not just in the main text) that these are averages over all of the years of measurements available from each sensor.

Figure 2 has been modified following the referee suggestion, comparable seasons in the two hemispheres are lined up. A short sentence in both the main text and the caption has been added to explain the new panels arrangement. In the figure caption, "Annual mean" has been replaced with "multiannual mean" (please, note also the referee 1 comment at this regard) and precise information on how the averages have been calculated is now given here, as well as in the main text.

4) p18405-18406: I have a number of comments on these pages.

(4.1) "The first impression is that there is a quite good agreement among the four sensors, especially in the Southern Hemisphere, since the satellite-to-satellite comparisons generally have many matches." I do not think that "have many matches" is quite the right wording; I believe what the authors mean to say is "agree well" or "show little scatter" or some similar phrase.

This sentence has been re-phrased.

(4.2) The authors go on to list some specific instances of disagreement, but I think that a more general statement could also be made that their analysis does not indicate consistent offsets between the various instruments in different seasons, years, hemispheres, or altitudes.

A general sentence that highlights the overall consistency among the different sensors has been added at the end of that paragraph.

(4.3) In Table 2, does it make sense that the differences MLS-MIP and MLS-SMR are both 0.9 (-1.1) in the NH (SH), but that the absolute values of the MIP-SMR differences are larger [-1.2 (0.9) in the NH (SH)]?

First of all, the authors point out that the last column of the table now shows the SMR-HAL values rather than HAL-SMR values (see the reviewer comment at point 4.5 below). By consequence, the mean values in both hemispheres have reversed sigh with respect to the previous paper version. The values reported in Table 2 have been carefully checked to verify their correctness or the presence of possible typing errors before addressing the reviewer remark. Actually, the authors noticed three rounding errors in the table: the first one in the MLS-MIP mean value for the SH (value changed from -1.1 to -1.2), the second one in the SMR-HAL mean value in the NH (value changed form -3.8 to -3.7), and the third one in the MIP-HAL standard deviation value for the NH (value changed from 5.4 to 5.3). But, most importantly, they noticed a typing error in the MLS-HAL mean value for the SH: the correct value is 1.1, not -0.3 (the authors should have noticed this typo simply looking at the right panel in the upper row of Fig. 5). As regard the reviewer comment, authors do think that the reason for that discrepancy is that the timing of the overlap period for MLS-MIP and MLS-SMR (similar to each other) and for MLS-SMR is not identical (the latter is longer than that of MLS-MIP and MLS-SMR). A remark on that has been added in the Figure caption.

(4.4) Since Figure 4 showed that the behavior was quite different at different potential temperatures and in different seasons, is it really meaningful to quantify the offsets in the subtropical barrier latitude between sensor pairs by grouping together data from all altitudes during each pair's entire overlap period?

The authors faced out the problem raised by the reviewer when preparing the manuscript, and they decided to use such a representation for two main reasons. The first reason is that, with few exceptions, grouping the data for each sensor pair level by level would have caused a too low number of data point for every level and sensor pair to make any statistically significant analysis. Grouping all potential temperature levels together, on the contrary, has allowed a measure of possible biases and consistency among sensors to be supplied and has allowed to present all results in a much compact and quick way. The second reason is that, on the other hand, the analysis made at the single potential temperatures for each pair of sensors is consistent with the analysis made grouping all levels together, though with the caveat that the single

level analysis is not always meaningful due to the low number of data points. For the sake of completeness, Figs. 1-8 below show the same analysis presented in Figure 5 of the manuscript but for each level separately, from 520 K to 1100 K. Authors would prefer not to show the same figures in the manuscript for the reasons outlined above.

In the manuscript, after the sentence “Figure 5 shows the histograms of the sensor-to-sensor barrier latitude differences grouping all altitude levels together.”, the sentence “It is worth pointing out that the same analysis has been performed also at each level separately (not shown here), giving overall consistent results, though, in some cases, the low number of data points at one single level and for one pair of sensors did not allow performing any statistically significant analysis.” has been added.

(4.5) Since the text discusses MLS-HAL, MIP-HAL, and SMR-HAL and notes that "the subtropical barrier latitude calculated from HALOE is always greater than that calculated from MLS, MIPAS and SMR", it might be better to show SMR-HAL in Table 2, rather than HAL-SMR.

The authors do agree with the reviewer; last column of Table 2 shows now the difference SMR-HAL and the bottom right panel of figure 5 has been changed accordingly. Moreover, this statement does not appear to be true for MLS-HAL in the SH (-0.3), assuming that "greater" means higher in latitude, i.e., more poleward, and not larger in absolute value.

Authors have corrected the typing error in Table 2 (see point 4.3 above); now that statement is true also for the pair MLS-HAL (the difference is in fact 1.1). The sentence in the main text has been changed in that “greater” has been replaced with “more poleward”

(4.6) MLS-HAL is characterized as "good agreement", yet the difference in the NH is nearly 3 deg (with stddev of 4.7 deg), which is only slightly better than the values for MIP-HAL or HAL-SMR

The sentence has been re-written to highlight that the agreement between MLS and HALOE can be considered good only in the Southern Hemisphere.

5) Figure 4: It might be better not to clip the edges of these plots, so that the results for SON 2005 can be more easily seen. The vertical dashed line should also be defined in the caption, not only in the main text. What are the horizontal grey lines – they are not explained in either the text or the caption?

The figure and its caption have been modified, following all referee comments. Horizontal lines have been explained in both the caption and main text.

6) p18406 and Figures 6 and 6b: "The subtropical barrier position on average ranges from about 10N to about 40N (30N) in the NH (SH)". From what I can tell looking at these plots, values reaching as low as 10N are rare, and those reaching as high as 40N are nonexistent. What do the error bars in Fig. 6 illustrate – standard deviations, or min/max range, or ?? The grey lines, which do not appear to be the same as those in Fig. 4, should be described in the caption. Finally, it seems to me that Fig. 6b (another multi-panel figure, as is Fig. 6) would be more appropriately labelled Fig. 7, and all subsequent figures renumbered accordingly.

The right range of the subtropical barrier latitude position in both hemispheres has been indicated in the text. The authors have specified in the text that the error bars indicate the standard deviations of the subtropical barrier average position. The caption contains now the description of what the grey lines represent. As suggested by the referee, the figures have been re-numbered (Fig 6b → Fig. 7 and the subsequent figures have been renumbered accordingly).

7) p18408, L15-17: "Results shown in Fig. 7 (600 K) are also consistent with tracer fields shown in Fig. 1." It's difficult for the reader to quantitatively judge this statement, because of the lack of minor tick marks on the y-axis in Fig. 1, but to me it appears that the equatorward shift is bigger in Fig. 1 than it is in Fig. 7. In addition, it might make it easier to interpret the changes evident in Fig. 1 if the phase of the QBO could

somehow be indicated on this figure.

First of all, the authors have provided a new version of Fig. 1, with the zonal mean zonal wind at 30 hPa overplotted to the contour plots (the y-axis is the same for the zonal winds, expressed in m/s and the subtropical barrier latitude, expressed in degree). In the text, the few lines describing the QBO data set used in this work, which appeared after in the previous paper version, have been moved here. Authors do hope that the figure is now clearer, and the interpretation of results and comparison with Fig. 8 (Fig. 7 in the previous paper version) easier than before. Concerning the comparison between Fig. 1 and Fig. 7 (now Fig. 8), it is important to stress that the latter shows the multiannual average of the wintertime subtropical barrier latitude as a function of the QBO (with the caveat that each winter months tern - DJF NH, JJA SH - experiencing a change of phase between easterly and westerly or vice-versa does not contribute to the averages), while Fig. 1 simply shows the time series of the subtropical barrier latitude, without any averaging. This could explain the impression that the equatorward shift of the subtropical barrier during winter is bigger in Fig. 1 than in Fig. 8.

8) p18409, L5-12: First, the notion that different results are obtained for HALOE than for the other datasets because of its longer measurement period could be tested by repeating the HALOE analyses over a comparable (i.e., shorter) interval and seeing if the ensuing differences from the original HALOE results are in line with the interinstrument differences. Second, is there any reason to expect inherent differences in the subtropical barrier behavior in the two time periods sampled by HALOE and the other instruments (1990s vs 2000s), as suggested in these lines? That is, have any previous studies proposed any mechanisms to cause such variations or trends (e.g., impact of climate change on QBO behavior, etc)? Perhaps small differences in the magnitude of the changes diagnosed from N2O and CH4 might be expected because of their slightly different vertical and horizontal gradients? In any case, I think that more discussion of these points is warranted.

The reviewer makes constructive and useful observations at this regard. The authors have added some more discussions on that in the manuscript, leading to substantial changes in section 4.3 before describing Fig. 9 (Fig. 8 in the previous paper version). The authors made some tests and repeat the HALOE analysis with a shorter database (that is, comparable to that of the other sensors considered in this paper). Figure 9 (10) below (not added to the paper) shows the same analysis reported in the paper but with the HALOE dataset restricted to the time period 1992-1999 (2000-2005). In both cases, there are no substantial differences in the position of the HALOE-derived subtropical barrier position as a function of the QBO with respect to what shown in Figure 8 (previous Fig. 7) of the paper. Therefore, the notion that different results between HALOE and the other data sets arise only from the longer measurement period of the former with respect to that of the other instruments is objectionable. Some other possible hypotheses have been added in the paper: as the referee suggests, small differences in the magnitude of the changes diagnosed from N2O and CH4 might be expected because of their slightly different vertical and horizontal gradients. When interpreting the differences among sensors, moreover, it should be taken into account that the temporal sampling in terms of barrier position is not the same for all sensors and levels. The analysis of possible trend features in the subtropical barrier position deserves further investigation, which will be addressed in a future work, making also use of CCM (CCMVal 2) data.

9) Figure 7: Since these are multiannual means (not "interannual means"), why are points for some instruments missing (e.g., only SMR data in the SH at 900 K, no HALOE data during QBO-E in SH at 830 K; many MIPAS points also missing)?

Points for some instruments are missing in Fig. 8 (old Fig. 7) because the multiannual mean winter position of the subtropical barrier during the westerly and easterly QBO phase has been calculated only for the three-month periods (DJF NH, JJA SH) entirely characterized by the same QBO phase, westerly or easterly, excluding the periods experiencing a transition of phase. Moreover, we put a threshold in the number of winter data allowed to calculate (and plot) the average (N greater or equal to 2). A

sentence has been added in the Figure caption.

Minor wording issues

ALL minor wording issues (listed below) raised by referee 2 have been addressed.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C9957/2011/acpd-11-C9957-2011-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18385, 2011.

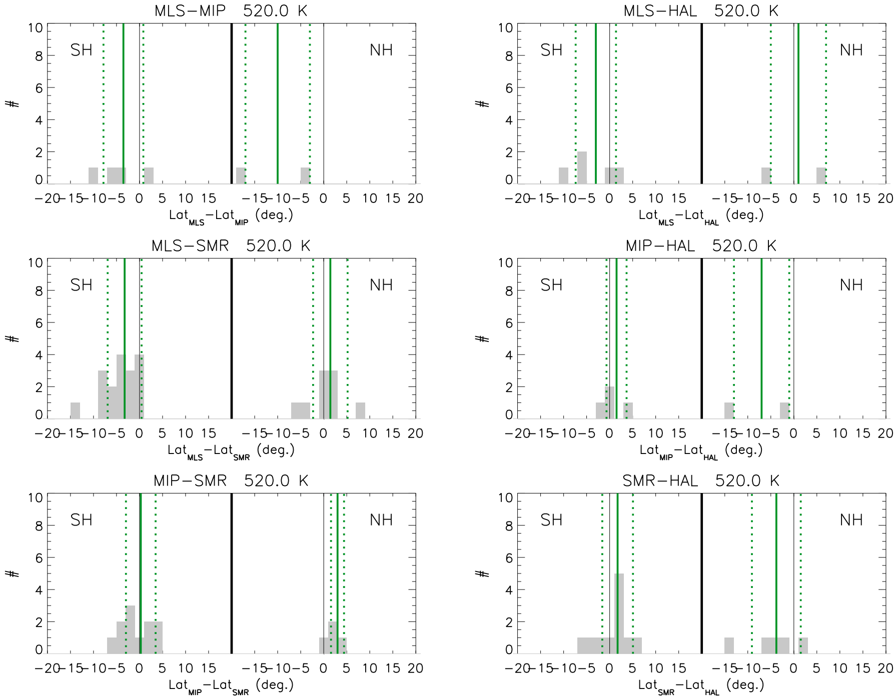

Fig. 1. Histograms of the sensor-to-sensor differences (MLS-MIPAS, MLS-SMR, MIPAS-SMR in the left column and MLS-HALOE, MIPAS-HALOE, HALOE-SMR in the right column) in the subtropical edge position at 520 K.

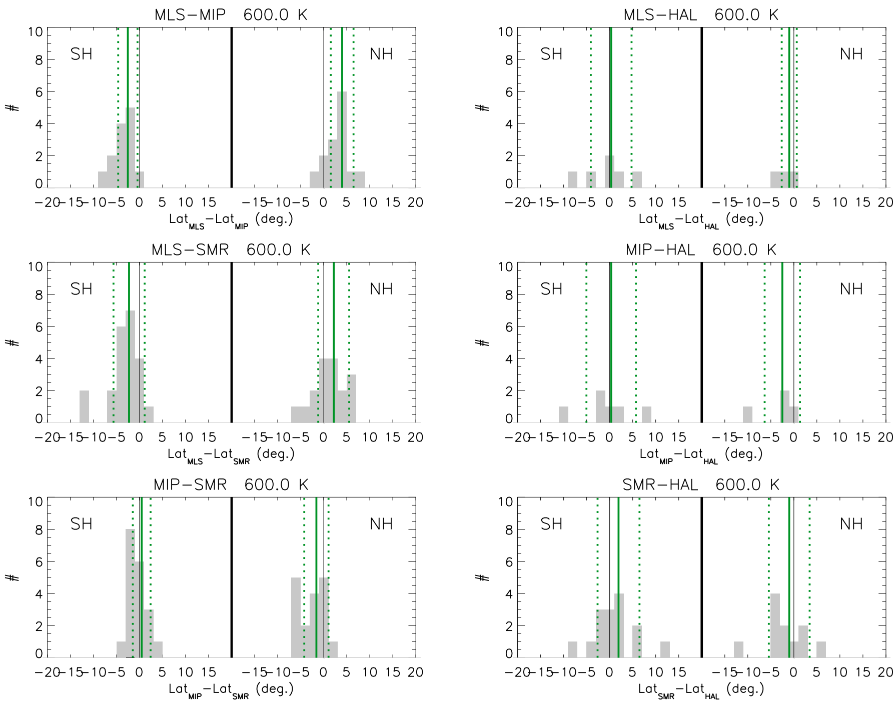

Fig. 2. the same as Fig. 1 at 600 K.

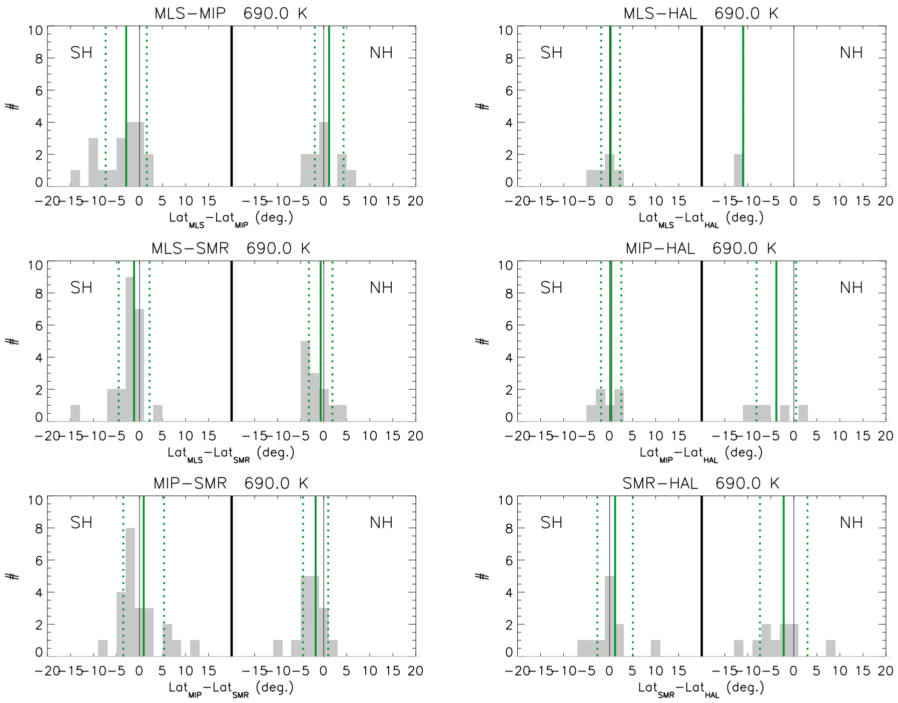

Fig. 3. the same as Fig. 1 at 690 K.

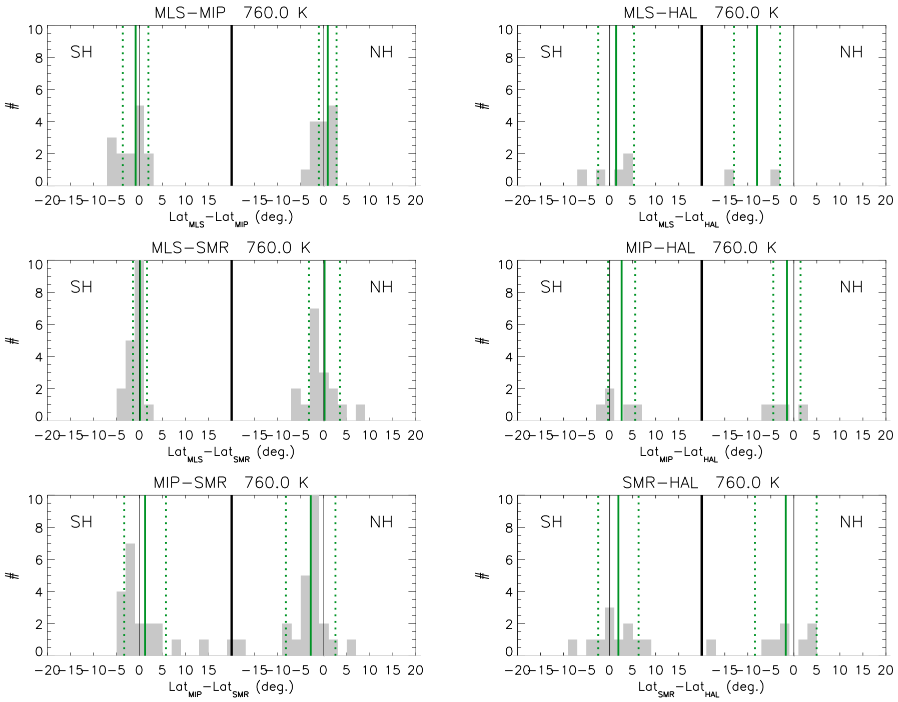

Fig. 4. the same as Fig. 1 at 760 K

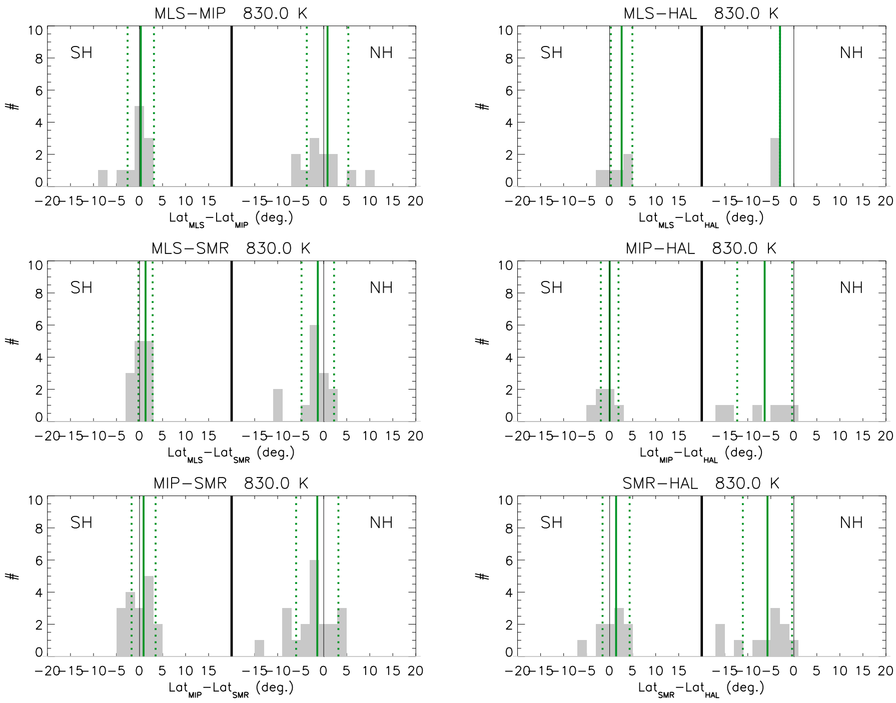

Fig. 5. the same as Fig. 1 at 830 K.

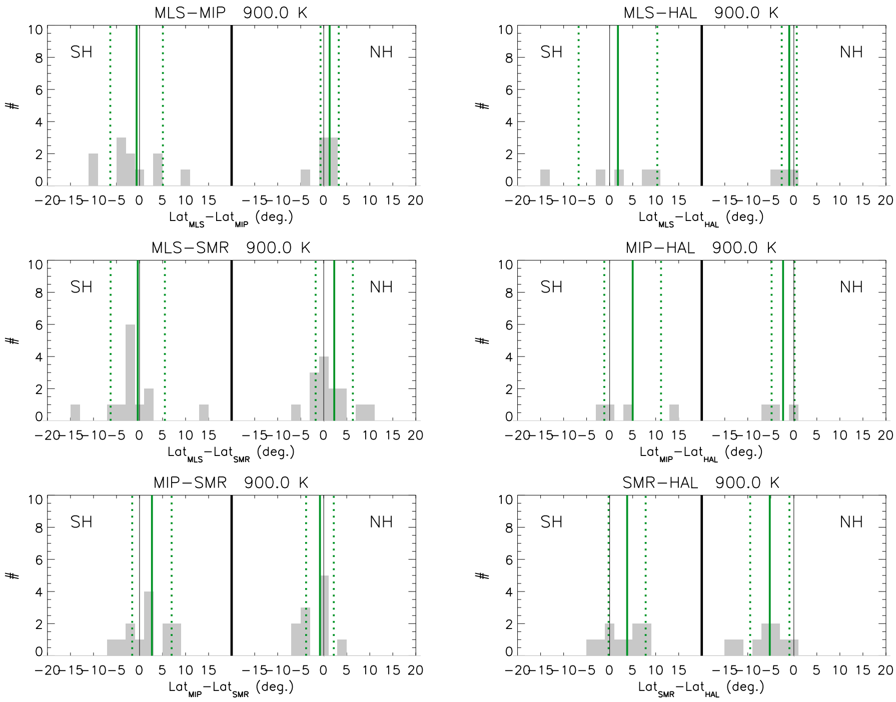

Fig. 6. the same as Fig. 1 at 900 K

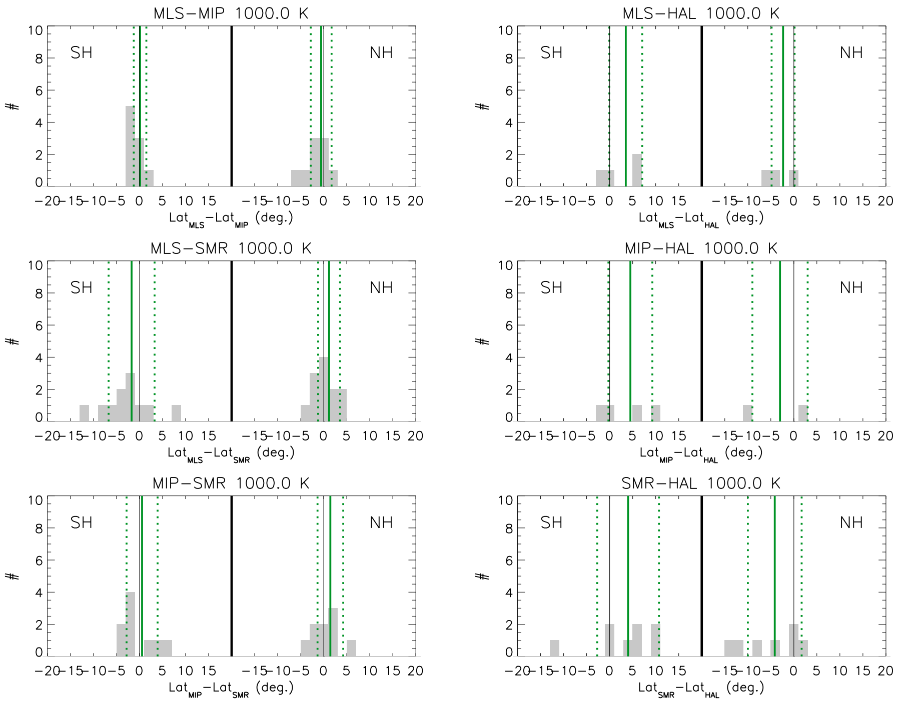

Fig. 7. the same as Fig. 1 at 1000 K

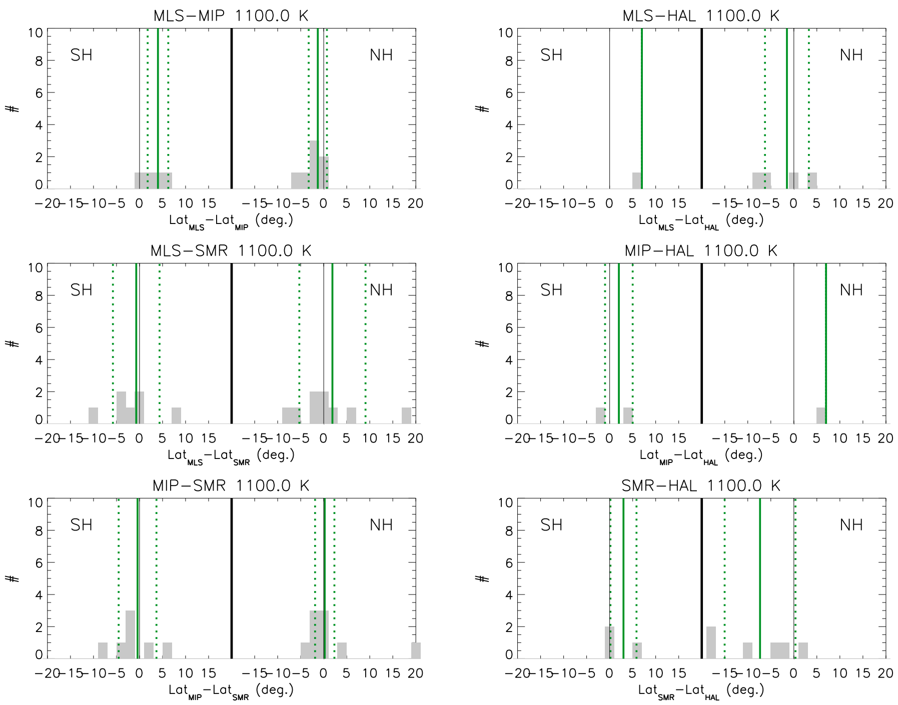

Fig. 8. the same as Fig. 1 at 1100 K

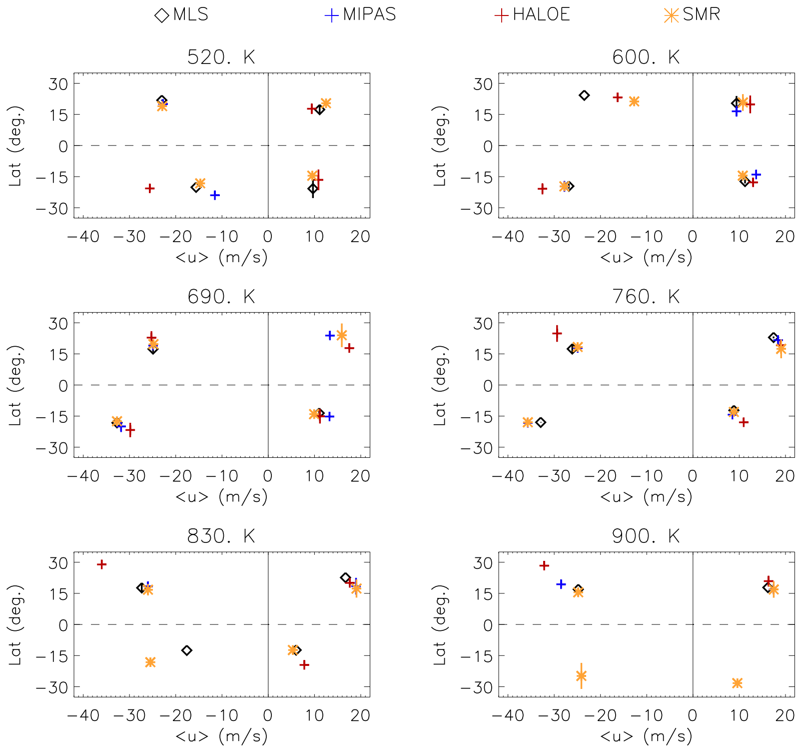

Fig. 9. the same as figure 8 in the paper but with HALOE data restricted to 1992-1999

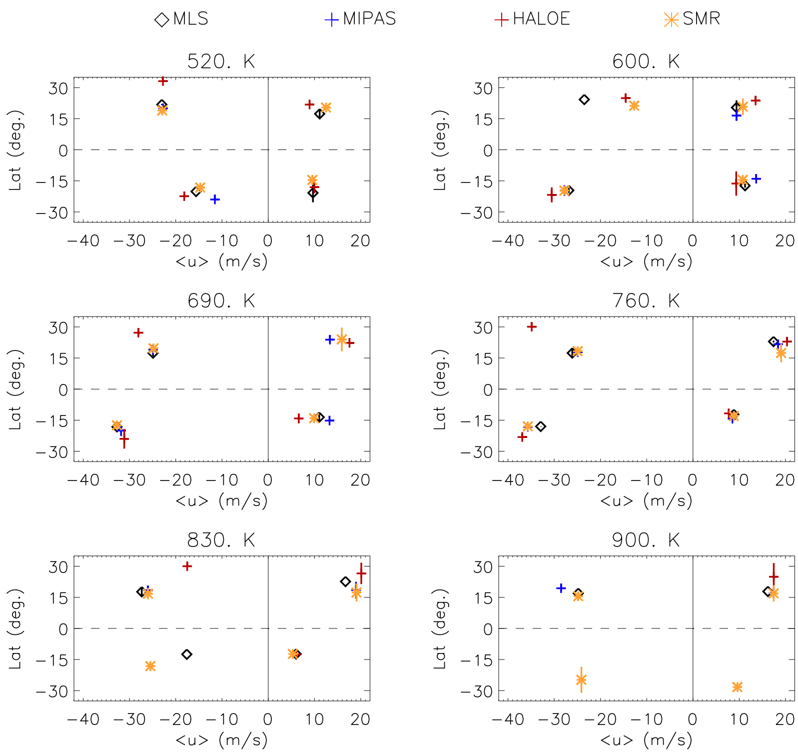

Fig. 10. the same as figure 8 in the paper but with HALOE data restricted to 2000-2005