Response to Anonymous Referee #1 Interactive comment on

"Stability of temperatures from TIMED/SABER v1.07 (2002–2009) and Aura/MLS v2.2 (2004–2009) compared with OH(6-2) temperatures observed at Davis Station, Antarctica"
by W. J. R. French and F. J. Mulligan

Reviewer comments in Black
Response to comments in Green

General comments:
This is an interesting and well written study dealing with the comparison of groundbased OH rotational temperatures at Davis station with SABER and MLS satellite observations.

The Davis OH temperature data set is one of the best-maintained and characterized data sets of this kind and is well suited for the verification and validation of satellite data. This paper helps in evaluating the quality of satellite data sets, which are also used for studies dealing with long-term trends in mesopause temperatures.

Therefore, the paper is an important contribution, and – in my opinion – should be published subject to minor revisions. Apart from the specific points mentioned below I have three general comments/suggestions:

We thank Referee#1 for these comments and have attempted to answer further specific comments and suggestions below:

a) I suggest including the supplement material in the main paper.

We have reconsidered including the supplemental material in the main paper but overall we believe these items to be somewhat of a distraction to the main thrust of the paper.
The miss-distance and miss-time comparison tables, for example, show no systematic trends over the range of times and distances considered. It would be more appropriate to include them in the main paper if some effect of tightening the selection criteria had been observed.
The table of other studies is useful as supplemental material only, since the bias comparisons include such a wide range of variables (different versions of SABER comparison data, weighting functions, acceptance ranges, transition probabilities, etc.).

b) It would be good to present and discuss the temperature trends derived from the individual time series, and not only the trends seen in the differences between the satellite data sets and the ground-based measurements at Davis.

The long term trends in the Davis data set are the subject of another paper currently in production. The satellite data sets presented here are not sufficiently long to warrant meaningful long term trend analysis which will adequately account for the solar cycle.

c) The effect of the limited vertical resolution of the MLS temperature profiles on the comparison needs to be addressed.

This item is dealt with in the specific question on section 4.3 below

Note : Reviewer #1 appears to have commented on an earlier version of the manuscript. Some of the corrections suggested have already been made and the page and line numbers for the additional comments do not match with the latest manuscript version (either the ACPD formatted PDF file acpd-10-21547-2010.pdf or the submitted PDF document)

Specific comments:

Page 1, line 14: I suggest adding 'miss-distance' to read 'The profile selection criteria

- miss-distance < 500 km ..'

added miss-distance as advised

Page 2, line 11: 'study of (Oberheide et al., 2006)' -> 'study of Oberheide et al. (2006)'

This was corrected in a previous review

Page 4, line 7: '2km' -> '2 km'

This was corrected in a previous review

Page 5, line 1: 'with _13 orbits per day' -> 'with _14 orbits per day'

The ~13 orbits per day was picked up from the EOS MLS website (http://mls.jpl.nasa.gov/index-eos-mls.php), which is incorrect. The 'A train' satellite series (Aqua, CloudSat, CALIPSO, PARASOL and Aura) complete 233 orbits in 16 days, or 14.56 orbits per day. This has been corrected as advised.

Page 5, lines 1-10: It is also important to mention the limited vertical resolution of the MLS temperature profiles of only about 15 km at mesopause altitudes

We have added the statement in section 2.3 "The vertical resolution, as defined by the full width at half maximum (FWHM) of the averaging kernels, varies from 5.3 km at 316 hPa to 9 km at 0.1 hPa and reaches 15 km at 0.001 hPa. (Schwartz et al., 2008). The effect of the considerably lower resolution of the MLS data, compared with a SABER temperature profile, on the results of this study is discussed in greater detail in Section 4.3.."

In this context we have also provided additional information on the along-track resolution of the MLS measurements by adding the following text ", with an along-track resolution of ~165 km (increasing to 220 km in the MLT region)" in lines 7/8.

Page 7, line 28: 'Burns et al. (2003) found good correlation between Davis OH and sodium lidar temperature at Syowa station at a distance of 1500 km.' Can you quantify this 'good correlation'?

The correlation analysis for the two years (2000 and 2001) compared in the Burns et al. (2003) study is presented in their figure 2. Maximum correlation coefficients for seasonally de-trended nightly averages were 0.68 and 0.51 respectively and both peaked at zero lag. We are not sure that adding correlation coefficients to quantify the correlation is useful in this context without explicitly stating the details of the cross correlation analysis performed (ie on years 2000 and 2001 seasonally detrended nightly average temperatures and the number of coincident observations). In order to avoid this statement becoming too unwieldy we consider it appropriate for the reader to refer to the paper for the details of the correlation.

Page 8, line 9: This is a really minor point, but I suggest replacing 'warmer' by, e.g., 'larger', because temperatures cannot really be warm or cold, but only high or low.

We have retained 'warmer' in this case

Sections 4.2 and 4.3: The trend of the differences between DAVIS OH and SABER / MLS is of course highly relevant, but it would also be interesting for the reader to know what the temperature trends in the individual time series are.

As stated above, the long term trends in the Davis data set are the subject of another paper currently in production. The satellite data sets presented here are not sufficiently long to warrant meaningful long term trend analysis which will adequately account for the solar cycle.
Nevertheless, for the interest of the reviewer the current solar-cycle and long term trend coefficients for the winter mean temperatures in the Davis data set (1995-2009) are 4.12± 0.79 K/100SFU and -1.03±0.82 K/decade respectively.
For the 2002-2009 (SABER equivalent) interval, the Davis data give values of 4.57±2.13 K/100SFU and -0.68±4.2 K/decade.
By comparison the SABER (2002-2009) data yields a (largely meaningless - due to the 0.7K/year bias drift) solar cycle of 7.1±5.0 K/100SFU and 7.8±7.4 K/decade. .

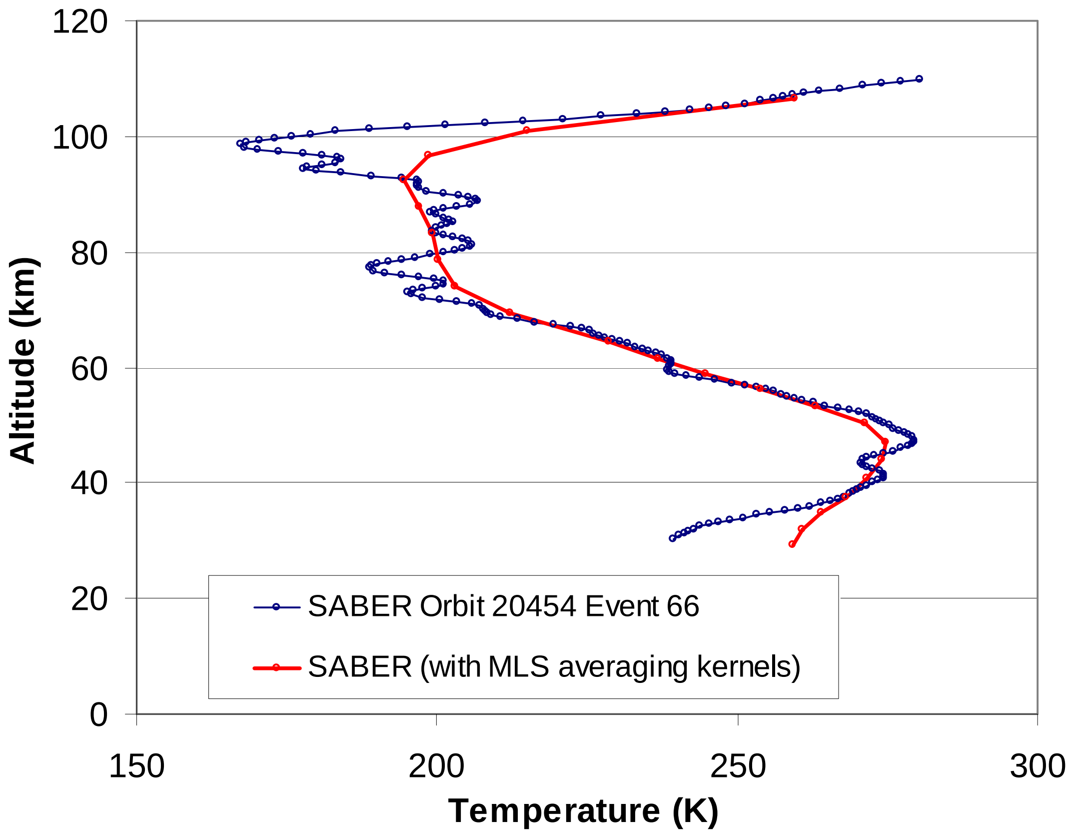
Section 4.3: The vertical resolution of the SABER temperature profiles is with about 2 km quite good. However, this is not the case for the MLS temperature profiles. According to the MLS temperature profile validation paper by Schwartz et al. (2007) (their Fig. 6) the full width at half maximum of the averaging kernels is about 15 km between 0.01 and 0.001 hPa. This means, that the MLS temperature profiles correspond to the actual temperature profiles convolved with a function with about 15 km FWHM. Weighting the temperature profiles with a typical OH VER profile will smooth the profile even more. I'm not sure how to treat the limited vertical resolution of the MLS profiles for this comparison correctly, but this issue needs to be addressed in some way in the paper. The effect of the smoothing can be tested by convolving a sample SABER temperature profile (with high vertical resolution) with the MLS averaging kernels, followed by the weighting with the VER profile. The resulting OH-equivalent temperature should be compared to the temperature obtained from the same SABER profile weighted by the VER profile. Perhaps the difference between these two OH-equivalent temperatures is not that large.

This is an interesting point which required some additional analysis. We tested the effect of the lower vertical resolution MLS temperature profiles by convolving the SABER temperature profiles with the MLS averaging kernels as the referee suggested. On a yearly average basis, the difference in OH-equivalent temperature was less than 2K.

The following paragraphs were added to section 4.3 to describe this investigation.

"One possible consideration for the difference in bias behaviour is the effect of the much lower vertical resolution of MLS (15 km FWHM) compared with SABER (2 km FWHM). This was investigated in the following manner. All of the SABER profiles that passed the selection criteria were convolved with averaging kernels of the altitude and width described in Schwartz et al. (2008) for MLS temperature retrievals, thereby creating SABER profiles with the vertical resolution of an MLS profile (see supplemental figure 4). These 'MLS-like' SABER profiles were then weighted with a Gaussian profile centred at 87 km altitude and FWHM of 8.7 km to calculate OH-equivalent temperatures comparable with SABER T_G87.
Although individual 'MLS-like' profiles produced OH-equivalent temperatures as much as 30 K different from the SABER T_G87 values, averages for each year were within 2 K, and the bias drift compared to Davis OH measurements persisted.
The effect of applying a relatively broad Gaussian weighting to the higher resolution SABER profile and to the lower resolution 'MLS-like' profile tends to attenuate any differences between them. On the basis of this investigation, we conclude that the

lower resolution of the Aura profiles does not significantly change the OH-equivalent temperatures or the bias stability reported."



Supplemental Figure 4. An example of a SABER temperature profile and its lower resolution MLS-like equivalent. The latter was obtained by convolving the SABER profile with the Aura-MLS averaging kernels specified in Schwartz et al. (2008).

Page 11, line 16: '..' -> '.'
This was corrected in a previous review

Page 12, line 1: There are no spaces between the initials for this reference.
Corrected as advised

Page 14, table 1, row 'standard error' of the lower table: the standard errors given are '0.3', '0.4' suggesting only one significant digit (to the right of the decimal place). The weighted temperatures and standard deviations, however, have 2 significant digits. If the standard errors have 2 significant digits they should be presented as '0.30' etc.
Corrected to two significant digits as advised

Page 16, line 4: 'standard error-in-the-mean'. Do you mean 'error-of-the-mean'?
Yes standard error-of-the-mean. Corrected as advised.

Supplement material: I suggest including the supplement material in the paper. The manuscript only has fairly small number of Figures and tables, and there should be enough space for the supplement material comprising 3 tables.

Our explanation for retaining the supplemental material is given above

Supplement Material, page 1, column 4, row ’MTM Hawaii’: ’Gold man’ -> ’Goldman’
This appears to be a typesetting or PDF conversion error, there is no space between d and m in the submitted manuscript.