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Atmospheric Measurement Techniques Discussions

Interactive comment on "Lidar temperature series in the middle atmosphere as a reference data set. Part B: Assessment of temperature observations from MLS/Aura and SABER/TIMED satellites" *by* Robin Wing et al.

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Response Lidar temperature series in the middle atmosphere as a reference data set. Part B: Assessment of temperature observations from MLS/Aura and SABER/TIMED satellites Referee #1

In the following, the numbers x, y refer to page x and line y of the manuscript.

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1, 4: Why only to 2011? We are now in 2018! MLS and SABER and the OHP lidar are still working and providing temperature profiles. Please use the additional 6 years of data since 2012, and provide results that are much more meaningful.

There were two reasons I ended the analysis at 2011: The LTA system underwent significant system upgrades in 2011 and as a result has a few data gaps. In part A of the article I identified a 20 year period for comparison where both lidars remained relatively unchanged. After establishing the lidars as a consistent benchmark measurement I wanted to use the same time period in the satellite comparison. There were several periods after and during system development and change where the lidar data cadence or quality was well below average and I rejected the profiles as candidates for this study. I have extended the analysis from 2011 to 2018 by using the temperature profiles from LiO3S (which were validated in Part A) to fill in the gaps in the LTA data record. It is important to note that LiO3S is a stratospheric ozone lidar and was not designed to measure temperatures high into the mesosphere. As I result I have increased the vertical integration for these profiles.

Text has been added and modified throughout the article to accommodate these changes.

Figures 3, 4, 5, 6, 10 11, 12, 13 have been updated

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At some point in the paper it is absolutely necessary to mention / show, how the lidar temperature analysis presented here relates to the temperature profiles published for many years in the NDACC database. Are there systematic differences between the two? If so, where and how big? Maybe even an additional plot.

This is shown and discussed in Figure 11 (attached) of the companion article (Wing et al., 2018a) The black zero line are temperatures produced by the NDACC lidar algorithm for LTA, coloured lines are the median ensemble temperature differences for the

algorithm presented in the companion article (green), the ozone lidar LiO3S (orange), MSIS-90 (magenta), SABER (blue), and MLS (red). Blue shaded area is the variance of all SABER-lidar comparisons and is given to illustrate the scale of geophysical variations. The systematic differences between the NDACC and modified temperature algorithm (black and green) are negligible below 70 km, -4 K at 80 km, and -20 K at 90 km. The change in character above 84 km is mainly due to species specific Rayleigh backscatter correction and changes to the gravity vector.

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2, 26-36 This does not connect well to the previous paragraph. Before you talked about satellites as primary instruments. Here, suddenly, you talk about alternatives to lidar. Please rework the entire introduction, so that there is a more logical flow.

Removed sentence: In this work we will show the value of our improved Rayleigh lidar temperature profiles, described in (Wing et al., 2018a), as a validation tool in the middle atmosphere .

Reworked the introduction into the following format: Section 1.1 Rayleigh lidar as a validation tool Section 1.2 Precious lidar-satellite studies Section 1.3 Alternative validation techniques Section 1.4 Outline

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2, around 45: Why not say that lidars measure altitude / range via measuring time, and that this is a very precise measurement with relative uncertainty of the order of 10 (or whatever the electronics of the OHP lidar specify).

Replaced this sentence with: Second, lidars measure range by measuring the time required for a backscattered photon to return to the station and be recorded by the photon counting electronics. The current OHP lidar uses a Licel digital recorder and has a sampling 40 MHz which corresponds to a vertical resolution of 7.5 m. The uncertainty on the sampling rate is negligible however, there is the possibility of trigger delay

and jitter in the counting electronics of 50 $\pm$ 12.5 ns \cite{Licel_manual} contributing a maximum possible uncertainty of 18.25 $\pm$ 3.25 m in the raw lidar measurement. This error is constant with altitude which allows us to sample the upper middle atmosphere with the same range resolved confidence as the lower middle atmosphere and troposphere.

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2, around 48: It would be good to give a reference for this claimed distortion of the altitude vector.

Replaced this sentence with: Third, as a benefit of active remote sensing raw lidar measurements don't suffer from vertical distortion in the altitude vector. Each altitude level in a lidar measurement corresponds to an independent collection of backscattered photons which are returning at a defined time from a given altitude range. In contrast, passive remote sensors such as limb scanning satellites can suffer biases at high altitudes due to: radiometric and spectral calibration, field of view and antenna transmission efficiency, satellite pointing uncertainty, as well as biases introduced by the forward model \citep{schwartz_2008_MLS_validation}. Additionally, many satellites like MLS are optimized for tropospheric and lower stratospheric measurements and conduct faster scans with fewer channels at higher altitudes \citep{Livesey et al 2006}. These different biases can exist simultaneously in both the retrievals of temperature and pressure and can considered, in part, as distortions in the altitude vector when compared to lidar measurements.

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3, around 63: It would make sense to give pros and cons also for the airglow imagers, similar to what is done for the other techniques. Also: Sodium and other metal layer lidars should also be introduced briefly in this context, including their pros and cons.

Added sentence: These instruments can provide excellent wide field of view measure-

ments over a geographic area but cannot yield vertical profiles of temperature.

Added section: e) Ground-based resonance doppler and Boltzmann lidars can derive temperatures from sodium, iron, and other meteoric metal layers in the upper mesosphere and lower thermosphere (80 - 115 km) \citep{Fe_temperature_lidar_Chu}. These techniques are not only useful in deriving temperature profiles but are also well situated for studies of other middle atmospheric phenomena such as gravity waves and noctilucent clouds. These lidars are restricted to measuring in the altitude band defined by the distribution of each metallic layer.

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4, 90: Siva Kumar or Sivakumar. Many reference callouts, and many references are sloppy. They all need to be checked and corrected.

The 2003 article is listed as V. Siva Kumar and the 2011 article as V. Sivakumar.

Because this is the format of the author's name in the original publications, I have retained that formatting here so that readers can located and access the correct journal articles in each case.

I export my references into BibTex directly from the journal websites.

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4, 94: Is it an "initialization problem" or "initialization related bias"? To me, problem seems the wrong word.

Changed to initialization related bias.

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4, 117-118: Sentence seems to be broken / missing something. The study also found found a systematic difference in the upper mesosphere which was attributed to tidal aliasing, bias in SABER or AO.

Changed to: As well the study found systematic temperature differences in the upper mesosphere which were attributed to tidal aliasing, bias in the SABER temperature retrieval, or temperature differences due to the AO.

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5, 124: Are these the numbers that are relevant for this study? Seems to me that a usual temperature profile is acquired over at least 4 hours (page 6, line 168). It would make more sense to use the more relevant times and altitude resolutions of the retrieved profiles here, not the ones of the underlying data acquisition.

Changed to: The Observatoire de Haute Provence (OHP) Rayleigh lidar has been in operation in southern France since 1978 and routinely produces nightly average temperature profiles of the upper stratosphere and lower mesosphere.

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5, 143: Drop "Other"?

Done

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I would Figure 2: Good figure. suggest, however, to also present average temperature profiles from lidar and MLS before Fig. 2. This will set the stage and help readers who do not have the average temperature profile in their head. It will also lead nicely into the vertical shifts discussed later.

I've added a new figure 2 with an example temperature profile for each instrument. Also added some text: An example of all three temperature profiles for the night of the 25th of July 2012 is shown in figure \ref{fig:mil}. In this comparison the lidar profile was produced over 4 hours and has a vertical resolution of 150 m from 30 km to above 90 km. The large temperature uncertainty above 70 km is a result of the fine vertical resolution required to capture the mesospheric inversion layer present near 77 km.

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Figure 2 caption: "show" should be "shown".

Changed

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Figs. 3, 5, 11, 12: Color scale is missing.

Added.

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7, 175: Could that not be checked, whether there is a bias coming from the initialization, e.g. by using MLS or SABER temperatures, or at least comparing them with the used initialization temperatures. I think more digging into this is required and would be a very important test for this paper.

Yes the idea of initializing the lidar retrieval with an external temperature is a good one.

I had initially considered using OH airglow temperatures to initialize the lidar as well as satellite temperatures and then doing as you suggest and comparing back to the satellites. Unfortunately, this is not a test which I can perform in a timely manner. I started writing the codes to do this analysis and it quickly became clear that this project is not so straight forward. Ensuring that the gridding for the initialization point is correct, error propagation, in both temperature and altitude, for a lidar retrieval using three different instruments for seed pressure, and thinking about what kind of statistics are meaningful to use when comparing the lidar, as a function of satellite temperature and pressure, to the satellite would be both complicated and important. If circumstances permit I'd like to come back to this idea after I complete my thesis and write up the results in as a separate paper.

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7, 178: How do you know that lidars are exceptionally accurate there? I think this needs more explanation and / or a reference (e.g. Leblanc et al. AMT 2016). Or do you mean precise, which is easier to show than accurate? What is exceptional? 0.01 K? 0.1 K is typical for radiosondes at lower altitudes around 10 to 20 km, and would not be exceptional. Also, instead of "are" I would prefer "should be".

Point well taken. I've softened the statement and added the relevant Leblanc citations Changed to: . . . a region where lidar uncertainties in both altitude and temperature are well described \citep{leblanc_ndacc1} \citep{leblanc_ndacc3}

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Figure 10 caption: It would be good to say that the underlying color plots are the same as in Figs. 2 and 4.

Done

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Figs. 11, 12: It would be good to also show seasonal difference profiles, similar to Figs. 2, 4. This would be particularly good for showing the oscillations in the lidar - MLS differences.

Added Figure 14 to show the change in the ensemble plots for all temperature comparisons, summer comparisons, and winter comparisons.

I've also added text in support of the figure and to the discussion.

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17, around 240: I am missing plots and a discussion of the time-altitude evolution of the lidar - SABER and lidar - MLS differences after the altitude shift corrections have been applied (Similar to Figs. 2 and 4). In particular it would be good discuss whether there are long-term drifts in these differences, or whether all instruments seem stable over time and thus usable for the temperature trend detection outlined in the introduction.

Probably there needs to be some analysis looking into possible long-term trends in these differences. As mentioned before, this should include data up to 2018.

I have added figure 14 which shows the ensemble medians before and after correcting for stratopause height.

I intend to look at altitude dependant decadal temperature trends in my next article. Cutting this article off with a discussion of seasonal variations seems like a good end point. I have to think carefully about how to extract the seasonal component of the variation from any systematic change over the 16 year period. As well I would like to discuss how best to resolve the disagreements between lidar and the satellites with someone from both the SABER and MLS team.

As noted in earlier in the response, the analysis now includes data up to March 2018.

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17, 258: The "why" for this needs to be discussed, not just shrugged off. Is it really background correction? Is it noise, i.e. are noisier profiles biased more (this could easily tested by comparing e.g. four 1 hour profiles with the corresponding 4 hour profile.) Or is it initialization temperature (test how much it would have to be changed to get rid of the bias, and how consistent that is with e.g. SABER, MLS at high altitudes).

Added: There still remains some residual systematic warm bias between the lidar satellite comparisons in this publication. Further work needs to be done on the problem of lidar initialization to fully address the effects of noise and a priori choice on high altitude Rayleigh lidar retrievals. However, we cannot discount the possibility that some of the remaining temperature difference is due to incorrect altitudes in the satellite data product.

Cited Wing2018A results regarding cooling due to noise filtration at the top of the lidar profile

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18, 275: Other things that come to mind here, and should be mentioned, are multiple scattering effects not considered in the single scattering lidar equation. This could result in enhanced return signals at lower altitudes, which pretends too high density and too cold temperature. Also, smaller rotational Raman bandwidth from the light scattered in colder regions (lower stratosphere) results in enhanced effective system transmission for those altitudes, also pretend too high density and too cold temperatures (She at al. 200x, Whiteman et al. 200x). Also: Is ozone absorption accounted for correctly? I think it would be important to have some numbers for the possible magnitude for all these effects (including the ones currently in the manuscript), for the OHP lidar configuration.

We have discussed in Part A (Wing et al 2018a) our rationale for ignoring multiple scatter effects. Multiple scatter effects are negligible. The probability of a photon backscattering is small, the probability of a photon backscattering twice is vanishingly small, and the probability of a twice backscattered photon being inside the lidar field of view (0.27 mrad see: Table 1 in Wing et al 2018a) is near zero. Ignoring the multiple scatter terms in the lidar equation is standard practice in middle atmospheric studies. As well any multiple scatter effects from water clouds in the troposphere would not be seen in the OHP Rayleigh lidar as the low gain channel is electronically blanked at 12 km and the high gain at 22 km.

OHP lidar has very narrow bandpass filters, either 1 nm for older measurements or 0.3 nm for recent years (Wing et al. 2018a). The rotational raman lines are outside of our bandpass. We are currently working to develop and install a rotational Raman temperature channel for temperatures from the ground to 30 km. This is the current project of another PhD student.

An example O3 correction is attached:

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19, 327: Remove "located". A "spatial" verb seems wrong in this temporal context.

Changed to ‘found’

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19, 331-333: I did not see much discussion of accuracy and precision in this paper (e.g. hardly any standard deviations, uncertainty estimates and their checks.). Largely, the paper looks only at satellite - lidar bias and its temporal evolution. Therefore, I would rather say that the lidar provide good temperature measurements that are consistent with SABER and MLS over a decade (decades only if data up to 2017 or 2018 are analyzed, as suggested at the beginning.

Section 7 has been re-written. Closer attention was paid when using the words “accurate” and “precise”

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The references are rather sloppy and need to be checked carefully. Like many manuscripts, this one would also benefit from reducing redundancies and improving conciseness. I realize that addressing my remarks above will initially tend to make the paper longer. However, I would urge the authors to go through the paper again carefully and remove redundancies and repetitions where possible. As men- tioned, this is basically a good and important paper, and should be made as readable as possible

We have double checked the references. Some (Sivakumar vs. Siva Kumar, for example) appear incorrect but in fact match the author names on the original publications.

We have endeavoured to reduce redundancies where possible, while also incorporating all sections which the reviewers sought to have added to the manuscript.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-139, 2018.

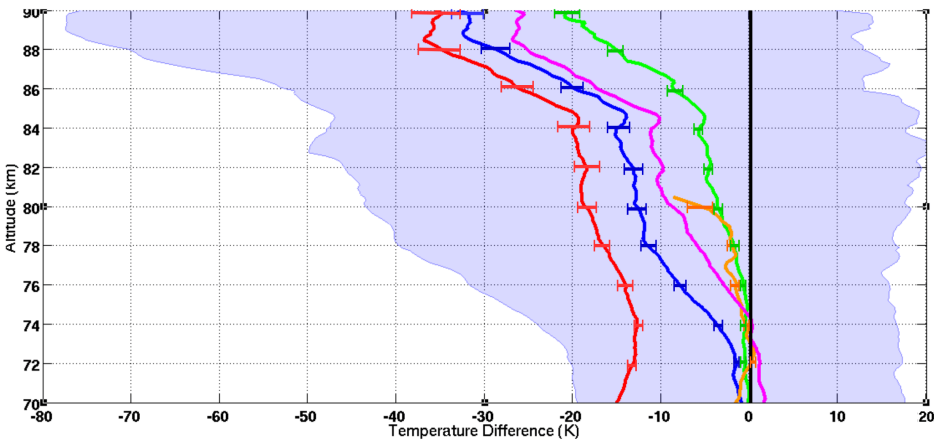

Fig. 1.

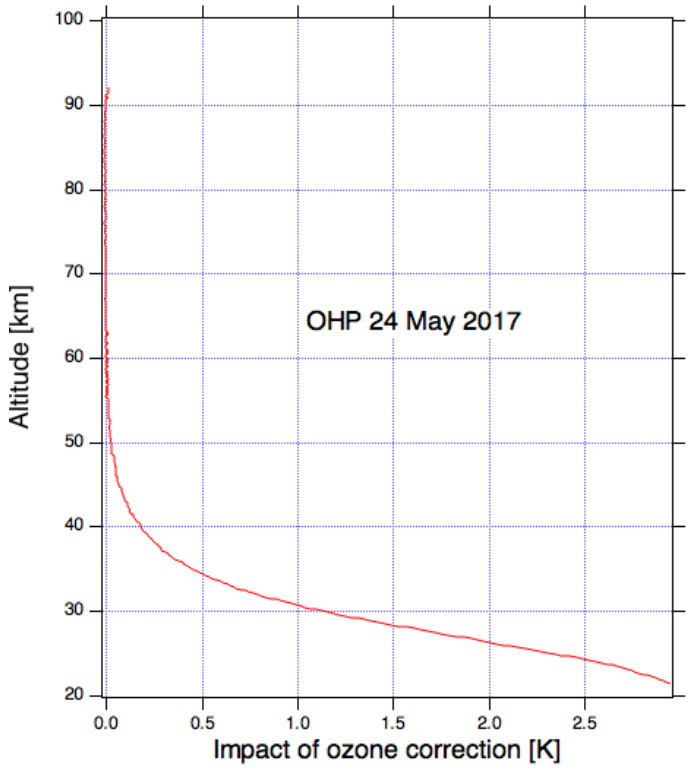
OHP 24 May 2017
Altitude [km]
Impact of ozone correction [K]
100
90
80
70
60
50
40
30
20
0.0
0.5
1.0
1.5
2.0
2.5

Fig. 2.