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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.

Anonymous Referee #1

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1 General remarks

The authors present a long-term comparison of upper stratospheric and mesospheric temperature profiles derived from lidar measurements at Haute Provence with results from the SABER and MLS satellite instruments. They show altitude shifts in the satellite derived temperature profiles, seasonally varying differences that are not understood, and overall long-term consistency.

I believe this is an important and generally well written paper, and well suited for publication in AMT after addressing the following short-comings.

2 Detailed suggestions

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

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.

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.

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.

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^{-6} (or whatever the electronics of the OHP lidar specify).

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

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.

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

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

4, 117-118: Sentence seems to be broken / missing something.

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 retreived profiles here, not the ones of the underlying data acquisition.

5, 143: Drop "Other"?

Figure 2: Good figure. I would 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.

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

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

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.

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".

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

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.

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.

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).

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.

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

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.

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 mentioned, this is basically a good and important paper, and should be made as readable as possible.

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